Essays in Local Public Finance and Political Economy

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Felix Köhler

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Abteilungssprecher	Prof. Dr. Thomas Tröger
Referent	Prof. Dr. Eckhard Janeba
Koreferent	Prof. Dr. Arthur Seibold

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Preface

Rich countries devote a substantial share of their resources to government spending. In the OECD, general government expenditure averaged around 43% of GDP in 2023, up from 39% in 2007 (OECD, 2023). This emphasizes the importance of understanding how policymakers at various levels of government make decisions. Unlike firms, which typically aim to maximize profits, the objectives of government spending are more complex and often multidimensional, for example encompassing equity, efficiency and political stability. These goals can differ across institutions and shift over time in response to political trends. A clearer understanding of the incentives, constraints and processes behind government action in specific contexts can contribute to more efficient and effective public resource allocation.

This thesis advances our understanding of how political institutions respond to reforms and fiscal shocks, with a focus on German states and municipalities. These sub-national governments are particularly well-suited for empirical and causal analysis not only because they account for nearly 46% of general government spending in 2023 (Federal Statistical Office, 2025). They also provide a large number of observations and natural experiments that allow for causal interpretation. Germany provides an ideal setting for this research given its federal structure, degree of decentralization and the availability of high-quality administrative data.

Specifically, Chapter 1 examines the consequences of abolishing minimum tenure requirements for parliamentary pensions. Despite the financial stakes involved and the ubiquity of reforms, they have received little empirical attention in the German context. Chapter 2 analyzes the effects of quasi-exogenous increases in municipal debt on local public finances, providing new evidence on how debt affects fiscal decision making and the role of fiscal oversight in mitigating unsustainable public finances. Chapter 3 investigates the fiscal impact of a breakthrough innovation by focusing on a vaccine producer whose success triggered an unprecedented surge in local business tax revenues. Leveraging the magnitude of this shock, the chapter contributes to the literature on windfall tax revenues and their implications for budget planning and intergovernmental transfers.

Beyond their focus on economic decisions of elected officials within a political framework,

the chapters in this thesis are unified by a shared empirical approach. All baseline results rely on event study designs, either in the form of the traditional two-way fixed effects estimator or a synthetic control group. This reflects not only a methodological preference but also the broader appeal of event studies as a transparent and intuitive way to present dynamic treatment effects. Event study plots were frequently requested in seminars, emphasizing their value in communicating results. The popularity of event studies is also reflected in recent theoretical developments, particularly debates around biases in the standard fixed effects estimator under staggered treatment timing, which is a common feature of event study designs (Roth et al., 2023) and one that is addressed in this thesis.

The following chapters build on these topics and methods to explore how institutions respond to distinct fiscal and political challenges or opportunities. In Chapter 1, I investigate how pension reform, i.e., the abolition of minimum tenure requirements for parliamentary pensions, affects the composition of German legislatures. While previous research has extensively analyzed the effects of changes to politicians' wages, there is limited empirical evidence on how pension system reforms influence political careers. This gap is particularly relevant in light of far-reaching pension reforms implemented in Germany over the past three decades.

Using newly collected and harmonized administrative data on parliamentary careers at both the state and federal levels, this chapter analyzes the effects of pension eligibility rule changes across 17 legislative bodies. The empirical strategy exploits variation in the timing of reforms across states and differences in legislators' exposure to reforms within parliaments.

Despite the sizable financial incentives involved in passing the tenure requirement, its elimination has limited compositional effects. There is no significant change in the probability of running for re-election, nor in the overall number of candidates, suggesting that the reform did not affect political entry or exit decisions in a systematic way. Additionally, no lasting shifts are observed in key characteristics such as age or gender. The only persistent compositional change is a decline in the share of legislators with PhDs, suggesting a modest shift toward a more socially representative legislature, although at the cost of academic quality.

The chapter uncovers an important party-level response. Political parties rank candidates who are no longer subject to the tenure requirement after reform worse on party lists. This strategic repositioning emphasizes the central role of parties in parliamentary systems. There is no evidence of a persistent vote penalty associated with attempts to reach the tenure threshold prior to reforms, implying that candidates were not penalized electorally for this strategic positioning. Overall, these results call into question the reforms' effectiveness in achieving some of their goals like attracting a more diverse

candidate pool.

In Chapter 2, I explore the causal effects of professional football club relegation on local public finances in Germany. Exploiting this natural experiment, it examines how relegation-induced public budget shocks from football, which is caused above all by public stadium financing, translate into municipal debt and budget responses. This analysis contributes novel causal evidence on how debt affects local governments, an area that has so far received limited causal attention. The chapter is based on data of 178 municipalities that host a club in the Bundesliga system over more than two decades.

The main finding is that relegation of a professional football team leads to a substantial and persistent increase in public debt for the host municipality, which on average amounts to €238 per capita or around 10% five years after relegation. This rise in debt occurs without significant changes to public investment or overall spending, suggesting that the additional debt does not crowd out core municipal services. However, municipalities consolidate budgets modestly, mainly by small and persistent increases in local business tax rates by about 0.18 percentage points and some asset sales. Interestingly, municipalities that host clubs experiencing promotion do not enjoy financial benefits, pointing to an asymmetric fiscal impact of athletic performance.

The majority of the eventual observed debt reduction is driven by state-level debt reduction programs, which are effectively bailouts. These programs distribute the financial burden across taxpayers beyond the affected municipalities, potentially encouraging moral hazard. Nonetheless, the evidence suggests that debt accumulation is largely unaffected by the presence or absence of these programs, implying limited moral hazard. The analysis also reveals political economy dynamics. While relegation has little electoral impact, promotion yields modest electoral gains for incumbent parties associated with stadium upgrades, offering policymakers an incentive for risky financial commitments.

In Chapter 3, which is joint work with Eckhard Janeba, Davud Rostam-Afschar and Paul Steger, I investigate how German municipalities respond to large, unexpected revenue shocks by exploiting another natural experiment. Following pharmaceutical company Biontech's breakthrough in developing a COVID-19 vaccine in late 2020, corporate tax revenues increased substantially, especially in municipalities that hosted Biontech facilities. Using a synthetic control group approach, the chapter provides causal evidence on how municipalities adjust tax policy, expenditure and asset management in response to unanticipated and vast positive budget shocks.

The chapter finds that treated municipalities received extraordinary tax windfalls of up to €3,440 per capita annually over four years, which is more than the annual budget before the shock, yet responded conservatively in terms of spending. Rather than increasing discretionary spending or public investment, they primarily used the revenue to repay

debt, accumulate capital reserves and fulfill increased mandatory payments to the fiscal equalization system. Notably, municipalities misjudged the shock's persistence and as a consequence cut local business and property tax rates. As revenues returned to pre-shock levels faster than expected, tax cuts had to be reversed within four years, which illustrates how institutional constraints and expectations shape local fiscal policy.

The chapter also uncovers a disconnect between tax cuts and real economic effects. Despite aggressive tax rate cuts and the stated goal to boost biotechnology clusters, there was no sustained increase in local firm activity within Biontech's industry or overall. Moreover, business tax spillover effects on neighboring municipalities were minimal.

One main contribution of Chapter 3 is studying a large shock to public finances, which occurs more often than perhaps expected. The shock's size allows for precise analysis of responses to non-marginal shocks in contrast to marginal shocks, which are usually the focus of studies in the literature. I document that in the literature the budgetary effect of smaller shocks is difficult to pin down as they tend to elicit more varied budgetary responses, while large shocks like the Biontech shock lead to more cautious fiscal policy. Together with the other chapters, these findings help to build a more nuanced understanding of the responsiveness of public finances to institutional and fiscal changes.

Chapter 1

Pension Eligibility and Parliamentary Composition: Evidence from German Legislatures

1.1 Introduction

Members of parliament enjoy various material and intangible perks, with old age compensation claims in Germany being the second most valuable benefit after monthly allowances. While public and academic discussions on parliamentary compensation often focus on allowances, pension payments are more complex and harder to analyze. Their timing, which sometimes occurs far in the future, and uncertain duration can make pension payments less salient, even to potential claimants. A key feature of parliamentary pensions, particularly in Germany, is that minimum tenure requirements limit generous benefits to legislators who serve a specified period. These thresholds create discontinuities in politicians' lifetime budget sets, influencing legislators' incentives to remain in office.

This study investigates how German legislatures at both the state and federal levels respond to the removal of minimum tenure requirements in parliamentary pension systems. These reforms shift away from generous, tenure-based benefits for career politicians toward more flexible arrangements that offer partial pensions and thus ease legislators' transition into non-political careers. The intended goals include attracting a broader pool of candidates, encouraging shorter-term public service and improving public acceptance. This paper explores whether such reforms lead to more diverse and dynamic political representation. While the federal Bundestag and many state parliaments have removed these thresholds, nearly half of the states continue to apply them, creating rich variation for analysis. Notably, minimum tenure requirements are not unique to Germany and

exist in other parliamentary systems as well (Miglino, 2024), but evidence on their effect remains scarce.

This raises the question of how such reforms affect legislators' decisions and the desirability of thresholds. Given that pensions constitute a significant portion of parliamentary compensation, they go beyond simple taxpayer considerations. The supply of lawmakers may be influenced not only by compensation but also by the design of rules and allocation mechanisms governing parliamentary life like the minimum tenure requirement.

Newly collected, extensive data on German parliamentarians at both the state and federal levels allow me to analyze the impact of pension reforms across 17 different entities. By exploiting the large number of parliaments, varying reform timing and differing exposure within parliaments to rule changes in an event study design, I interpret the results as causal. The endogeneity of reform adoption is a concern, particularly when parliamentarians themselves pass the laws. To address this, I demonstrate flat pre-trends in the event studies and show that reform adoption is not driven by any particular political party. Additionally, reforms' legacy clauses, which leave provisions unchanged for many active legislators, further alleviate this concern.

Although the incentives to meet the eligibility threshold are substantial, my results provide no evidence that legislators respond to the elimination of the threshold on key margins. Specifically, they do not change their probability of running for re-election, nor is there any noticeable change in their average tenure, even among first-term legislators who would be expected to be directly affected. Additionally, the age and gender composition of parliaments remains unchanged in the long run. The overall number of candidates running for office is not affected, either. The only permanent change is a decrease in the share of legislators holding a PhD, which could be interpreted either as a decrease in (academic) quality or an increase in how representative legislatures are with respect to the population.

In contrast, political parties do respond by adjusting their candidate selection strategies. After the threshold is no longer binding, parties stop giving preferential list placements to legislators who would have previously faced the threshold, suggesting that parties take individual members' compensation into account. This constraint on candidate selection may conflict with parties' goal of fielding the most vote-maximizing candidates.

The mixed-member proportional representation system used in the Bundestag and many state parliaments allows for the analysis of personal vote shares for candidates who appear both on the party list and are simultaneously constituency candidates. Although parties may have a broader pool of potential candidates in the reformed regime, as securing members' pensions becomes less of a concern, the selected candidates are more popular only in the short-term and do not receive more votes than those still subject to the tenure

requirement in the long-term. This revealed preference approach calls into question the reforms' popularity with voters.

While the response of party lists suggests that political parties take the reforms into account, the lack of observable changes in legislators' behavior is striking. One possible explanation for this lack of response is that the position of legislator is so inherently attractive that individuals seek to remain in parliament for as long as possible, regardless of the tenure requirement. This is supported by the consistently high probability to run for re-election of over 90%, which indicates that the desire to remain in office is a powerful motivator.

Several factors may contribute to the attractiveness of the job, including the substantial levels of financial compensation, the status associated with holding a parliamentary seat and legislators' political ambition or zeal. Additionally, parliamentary careers in Germany, particularly at the state level, are characterized by relatively few restrictions on outside employment and limited scrutiny of backbenchers, which could further reduce the opportunity costs of remaining a legislator. I also document that when parliamentarians pass reforms reducing their old-age compensation, allowances increase to more than compensate for the loss in pensions' net present value. These factors combined may explain why the reforms have had limited impact on legislators' decisions to continue their careers in parliament.

The paper is structured as follows. The related literature is introduced in Section 1.2 and Section 1.3 summarizes the changes to the institutional frameworks governing legislators' pensions. The data are introduced in Section 2.3, before showing descriptive statistics in Section 1.5. The empirical design and results are presented in Section 2.4 and tied together in a brief discussion in Section 2.5. Section 2.6 concludes.

1.2 Related literature

The most closely related study is a recent paper that analyzes the introduction of a minimum tenure requirement for parliamentary pensions in the Italian national parliament (Miglino, 2024). It finds that the reform led to an increase in affirmative confidence votes, consistent with legislators' incentives to secure pension eligibility. The study also documents shifts in party affiliation toward the majority party, greater observable effort and reduced party dissent. These results suggest that the reform enhanced government stability by strengthening party discipline.

While party dynamics are also important in the German context, key differences exist. Confidence votes are far less frequent in German parliaments and this study focuses on

the removal rather than the introduction of tenure requirements. Moreover, by analyzing multiple legislative bodies across Germany, this paper leverages broader institutional variation to more robustly identify the effects of pension reforms on legislative behavior.

Several studies have analyzed pension arrangements incidentally while investigating legislators' retirement behavior. Pension system peculiarities are included as a covariate in a regression model in Hall and Van Houweling (1995), which highlights the substantial value of pensions in the US House of Representatives in the 1990s and identifies pension entitlements as one of the determinants of retirement behavior. Similarly, Kerby and Blidook (2011) reports a change in the probability of exiting parliament once a pension eligibility threshold is reached in the Canadian parliament.

However, such studies often face endogeneity issues, as the pension system is typically treated as a control variable rather than the primary research focus in a quasi-experimental setting. This suggests that pension reforms provide a more informative setting for understanding the causal effects of pensions on legislators' behavior. A different strand of the literature examines the impact of parliamentary composition on the implementation of overall public pension systems, but the focus here is specifically on elected officials' pensions.

The literature on the effects of changes to elected officials' allowance provides mixed evidence. For example, Fisman et al. (2015) find in a natural experiment in the European Parliament that increased allowance does not lead to more effort or legislative output, but increases the attractiveness and thus competition for parliamentary positions, akin to outcomes for mayors in Peru in Pique (2019). Cunha and Manoel (2019) analyze the Brazilian context and document that higher wages for elected municipal officials were associated with a strong economic response in the form of firm and job creation as well as increased firms' revenues and investments and improved municipal budget surpluses. Similar results in this setting were found in Ferraz and Finan (2009). Besley (2004) provides theoretical explanations for these findings as better pay can mitigate agency problems by attracting competent individuals and motivating incumbents to exert greater effort. Another strand of the literature focuses on the effect of public sector employee pay where incentives differ since they are not elected.

Monetary incentives can change the composition of a legislature. In Kotakorpi and Poutvaara (2011) higher salary coincides with higher education among female candidates for Finnish parliament. Diermeier et al. (2005), with the aid of a calibrated structural model, report modest effects of pay increases, but do not include a policy experiment of changing retirement rules. Many of these studies are confined to a very narrow time window.

Despite extensive research on the effects of changes to politicians' wages and allowances, there is surprisingly little evidence on how pension system reforms impact political be-

havior. This gap is especially relevant given the far-reaching pension reforms enacted in Germany and elsewhere. Understanding the behavioral responses to such reforms is essential, as they can influence political career decisions, government stability and fiscal outcomes. Yet, as this study shows, the responses to these reforms are limited even in the long run across many dimensions. This raises questions about the reforms' effectiveness in achieving their stated goals, such as attracting a more diverse pool of legislators.

1.3 Institutional framework

1.3.1 Federal structure

Germany's federal structure divides legislative authority between the federal government and 16 state governments (Länder). Each level has its own parliaments, the Bundestag at the federal level and state parliaments (Landtage), with independent rules on various matters, including parliamentary compensation. Elections are held every four to five years at different times. Legislators pass laws within their jurisdiction, including those governing their own compensation. While some regulations align across states, others diverge, reflecting different policy choices. This study uses data from all 17 legislatures, along with the European Parliament and the final term of the Volkskammer, the democratically elected parliament of the German Democratic Republic. As tenure in these two bodies counts toward pension eligibility in all the 17 current national and sub-national parliaments, they are included.

1.3.2 Parliamentary pension systems

Old age compensation, like parliamentary allowances, was introduced to offset forgone income during legislative service. Compensation schemes for German parliamentarians in old age can be grouped into three categories, which are outlined here.

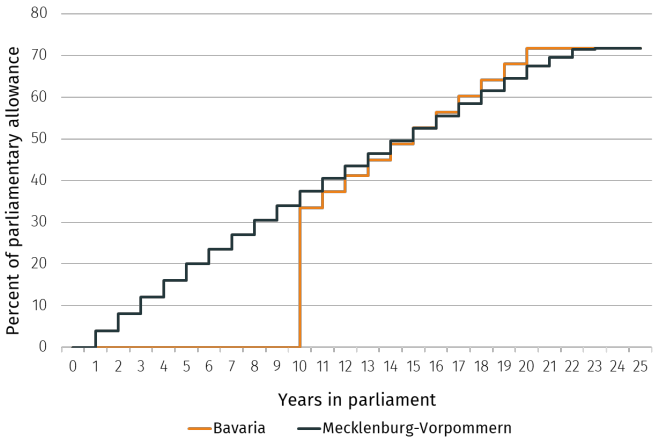
Standard regime

In the standard mechanism to allocate old age compensation, which I call standard regime, parliamentarians are granted monthly payments if they were part of the parliament for at least as long as an eligibility threshold. These payments accrue after a specified retirement age until death, similar to a regular pension. In some cases, the statutory retirement age decreases with tenure exceeding the threshold. Pension payments are always defined as a share of parliamentary allowance. The share is increasing by 2-5 percentage points

per tenure year beyond the threshold. Pension claims accrued outside parliament are not offset, apart from a few exceptions. Importantly, there is variation in the threshold across legislatures and over time within legislatures. It ranges from one to ten years in the time period under consideration. Rules specify that for the purpose of pension eligibility, incomplete years are rounded up to a whole year after six months or more.[1]

Pension schedules for two different legislatures using the standard regime are visualized in Figure 1.1. While the pension schedule in the state of Mecklenburg-Vorpommern provides a more generous pension incrementally as a function of tenure, pensions in the Bavarian state legislature are zero until ten years. At this point, they are almost equally generous in terms of share of parliamentary allowance. In the focus of this paper is the switch from a schedule that exhibits Bavaria's jump to a schedule that allows for small increments in pension payments from the start.

Figure 1.1: Exemplary pension schedules



Notes: This graph depicts pension schedules of two German states in percent of parliamentary allowance as a function of a legislator's tenure in parliament in 2022. Parliamentary allowance is €8,886 per month in Bavaria and €6,466 per month in Mecklenburg-Vorpommern. Allowance for all states over time is presented in Table 1.A.6.

Parliamentarians who fail to pass the threshold in the standard regime can claim a much less generous compensation paid in accordance with the number of months served. Back-of-the-envelope calculations show that the benefit in monetary terms roughly quadruples to a value exceeding half a million Euros when passing the threshold in states that have a threshold of ten years. While in 1990 all state parliaments and the Bundestag used this standard regime, in 2022 there were 11 left. The trend has been either to lower the threshold to one year, effectively abolishing it to amend it to encompass two full legislative terms, i.e., eight or ten years. The wording and spirit of these regulations are very similar across states.

[1]Tables 1.A.2, 1.A.3, 1.A.4 and 1.A.5 in the Appendix collect the old age compensation parameters for each parliament and year.

Pension fund

Another way to organize old age compensation is through a pension fund, requiring parliamentarians to pay a mandatory contribution (e.g., €1,913 per month in 2021 in Baden-Württemberg) to finance future pensions. Pensions are fully funded by contributions without the need for minimum tenure thresholds. This approach makes the associated costs more transparent by shifting expenses to the present and avoiding long-term pension liabilities on the administration's books. States Baden-Württemberg, Brandenburg and North Rhine-Westphalia pool resources by registering their legislators under a joint scheme. All states applying the pension fund model substantially increased monthly allowances at the fund's introduction to offset contribution payments.

One-time payments

The final method employed in German legislatures provides parliamentarians with an additional amount on top of their allowances to make private provisions for old age. These monthly payments are independent of tenure and replace long-term pension liabilities with one-time expenses. Bremen and Schleswig-Holstein adopted this system by shifting from traditional pension schemes to transparent, upfront payments.

1.3.3 Reforms

The goals of reforming old age compensation systems, whether by moving away from the standard regime or reducing eligibility thresholds within it, are varied. The primary motivation is to shift away from regulations that provide generous full-time pension benefits for career politicians, instead focusing on partial pensions that help bridge the gap legislators face in their professional careers outside parliament (Deutscher Bundestag, 2007). This could attract a more diverse pool of candidates and encourage shorter-term public service, which could, in turn, lead to more dynamic and representative governance. The aim of this paper is to put this hypothesis to the test. Reforms are also expected to improve public acceptance, as tenure-based pensions may be seen as favoring only long-serving politicians. Additionally, reforms aim to reduce costs and increase transparency (Landtag von Baden-Württemberg, 2008). In 1990, pension regulations were largely uniform, including tenure requirements. However, a series of reforms, particularly in the 2000s, led to diverging legislation addressing these issues.

Reforms involving one-time payments and funded systems are difficult to compare directly with the standard regime, as they fix pension contributions in the present, while the standard regime fix pension payouts for legislators in the future. This will be ad-

dressed by defining a variable mapping one to the other. As a robustness check, analysis will include only parliaments operating under the standard regime. Another issue of studying these reforms is that they often encompass more than just changes to eligibility thresholds. Increases in retirement age, adjustments to the length of parliamentary terms, changes in pension generosity and alterations to parliamentary allowances are significant confounding factors. For this reason, key reform parameters are collected for all states and the federal level over time (cf. Tables 1.A.6 and 1.A.7). The following analysis will address the issue of mixed treatment by either including confounding reform aspects as control variables or measuring the effect of each reform aspect separately. As will be shown, these factors do not explain the results.

Since not all parliaments opted to switch from the standard regime or lower their thresholds, it is important to understand which legislatures chose to implement reforms. These parliaments will constitute the treatment group. In order to interpret changes across parliaments as a response to eligibility reforms as causal, the adoption of reforms should be arguably exogenous and not driven by confounding characteristics. This is an especially pressing concern in this context. After all, it is lawmakers themselves who, within limits, determine both their own allowances and pension regulations. However, when changes were made they generally enjoyed broad support across political parties. The historically dominant parties, center-left SPD and center-right Union, both voted in favor of the reforms, often with backing from other parties (Landtag von Niedersachsen, 2004; Landtag von Schleswig-Holstein, 2006; Landtag von Mecklenburg-Vorpommern, 2005). These changes occurred under both parties' rule and states from the former German Democratic Republic were just as inclined to implement reforms as the western states. As such, there are no clear or obvious factors that determine selection, but this hypothesis will have to stand quantitative tests below.

Another circumstance that might alleviate some concern is that legacy clauses in pension reforms protect existing claims at the time of reform implementation. This ensures that changes do not apply retroactively to current legislators with accrued pension benefits. As a result, long-serving members remain unaffected by reforms introduced during their tenure. The burden of new rules falls instead on future legislators or those not yet eligible for pensions at the time of reforms. While this reduces political resistance by shielding some current members, it can also delay the full impact of the reforms.

1.3.4 Mixed electoral systems

In Germany and the majority of states, parliamentary seats are allocated through a mixed electoral system, which will be discussed in Section 1.6. Citizens cast two votes. The first is a majoritarian vote for a local constituency candidate and the second is for party lists.

In the latter, seats are allocated based on each party's share of votes and the candidates' position on the party list. The higher a candidate is ranked, the more likely they are to be elected. This gives parties significant influence over a candidate's probability to be elected. Many candidates run in a constituency and are simultaneously placed on the party list, which will allow the analysis of individual popularity among candidates on the party list. Some states use different electoral rules and will be excluded from this part of the analysis. Recent changes to electoral rules at the federal level do not affect the period under consideration.

Hypotheses

Before turning to the quantitative analysis, I propose three hypotheses. First, if some legislators previously stayed longer to benefit from generous pension payments after passing the threshold, removing this requirement may encourage shorter careers, especially for newly elected members. As a result, average tenure and the share of legislators running for re-election after their first term should decrease.

An effect that could offset this response is the net increase in monetary compensation in present value terms following the reforms, as documented in Section 1.6. This shift is driven by higher current allowances at the expense of future pension benefits. As a result, parliamentary membership may become more attractive overall, particularly for individuals who place greater value on immediate income over future payments.

Second, parties strategically rank candidates on their lists based on pension eligibility. With the threshold eliminated, this incentive weakens, leading to less strategic placement. Therefore, candidates who no longer face the threshold should be ranked lower, i.e., worse, than those who still do.

Third, if parties' focus shifts from their members' pension interests to alternative strategies like vote-maximizing as hypothesized above, candidates in their first term running for re-election, who would have faced the threshold before the reforms, may become more popular with voters compared to those who are still bound by eligibility requirements.

1.4 Data

The analysis relies on administrative data on legislators, obtained upon request from the respective parliamentary administrations. The format and quality of the data varied significantly, requiring substantial harmonization. In some cases, administrations provided only unstructured text files or referred to tables on their websites. For the city-states

Berlin, Bremen and Hamburg no administrative data was available and thus information was hand-collected from various sources, most notably Wikipedia. To determine the quality of this data, two states' administrative data, i.e., Rhineland-Palatinate's and Saxony's, were compared to information collected on Wikipedia. Deviations from the administrative data were so minimal that I decided to include the city-states in the analysis.

Since old age compensation is a function of time served in all state parliaments plus the Bundestag, the European Parliament and the last period of the legislative body of the German Democratic Republic, i.e., Volkskammer, the final data set consists of terms in 19 parliaments.

Data availability varies widely by state, but includes at a minimum start and end date of terms served, as well as gender, political affiliation and year of birth. Legislators from different parliaments are matched by first name, last name and year of birth. It is possible that this approach fails to match all instances of persons switching between parliaments, thus underestimating the respective total tenure in parliaments and overestimating the number of distinct legislators.[2] However, to bias estimates this measurement error would have to correlate with treatment, i.e., reform adoption.

This yields the universe of democratically elected legislators in post-war Germany on all levels of government: a total of 46,108 terms served by 17,300 persons. The baseline estimation sample, which is a sub-sample of parliamentarians serving in 1990 or later, used for much of the following analysis consists of 8,384 persons. I merge data from Wikidata for legislator characteristics such as party affiliation for time periods or individuals where this information is missing. Observations are merged on the basis of first name, last name and year of birth.

A caveat of this data set is that it only includes individuals who were elected to parliament, as parliamentary administrations do not systematically collect information on candidates. To address this, data from the German non-governmental organization *Abgeordnetenwatch*, available on their website, is merged with the existing data set based on first and last names, starting in 2002. This supplementary data provides information on who ran for office, their position on the party list and the vote share of constituency candidates. In cases where data coverage begins particularly late, additional information on party lists and constituency results was collected by hand from political party websites and election result data by the respective electoral commission.

To get from legislator-term observations to a yearly panel, i.e., legislator-year observations, I expand legislator-term observations according to the years the respective leg-

[2]This concern is especially pressing for female legislators changing their last name in wedlock. However, less than 8% of individuals switch parliaments according to this procedure.

islative term encompasses. In case of overlaps, the more senior term takes precedence. Variables that do not change within a term, e.g., the position on the party list, take that value until the end of the respective term.

Data on institutional conditions such as parliamentary allowances, old age compensation schemes and their reforms were collected by hand on the basis of laws in the respective legal journals from the year of German reunification in 1990 until 2022. Going back in time, it is increasingly difficult to find changes in laws for some states (cf. missing observations in Table 1.A.2) and going beyond 1990 would imply that states from the former German Democratic Republic are not included. This yields a parliament-level panel that is merged to the legislator-level panel.

1.5 Descriptive statistics

Table 1.1 presents summary statistics of the sub-sample of legislators starting their careers in 1990 or later that will be the basis for subsequent analysis. Average tenure is around eight years. Figure 1.2a breaks down average tenure by parliament, including those legislators who are still in office. States that switched earlier from legislative periods of four to five years (i.e., all except Bremen and the Bundestag) tend do have longer-serving legislators and city-states have among the shortest-serving parliamentarians.

Few legislators change their party affiliation during their parliamentary career. Around 16% of parliamentarians experience the parliament dissolving prematurely compared to statutory length, e.g., due to a no confidence vote, which is relevant in this context because eligibility thresholds are usually tailored to be two statutory legislative periods. Around 7% of individuals are part of more than one parliament during their career.[3] A third of legislators are still in office at the time of data collection. Thus, their entire career path is not yet observed. Later analysis will selectively exclude those legislators.

A substantial share of observations comes from non-state parliaments, particularly the Bundestag (cf. Figure 1.2b). Since the regulations for old age compensation are closely aligned with those of state parliaments and its eligibility reform is nearly identical in timing and intent, the Bundestag will be included in the main estimations. However, it will be selectively excluded to assess the sensitivity of the results.

To gain further insight into legislator behavior and assess the similarity between state legislatures and the Bundestag, Figure 1.A.2 in the Appendix shows the development of several variables over time, separately for state parliaments and the Bundestag. Fig-

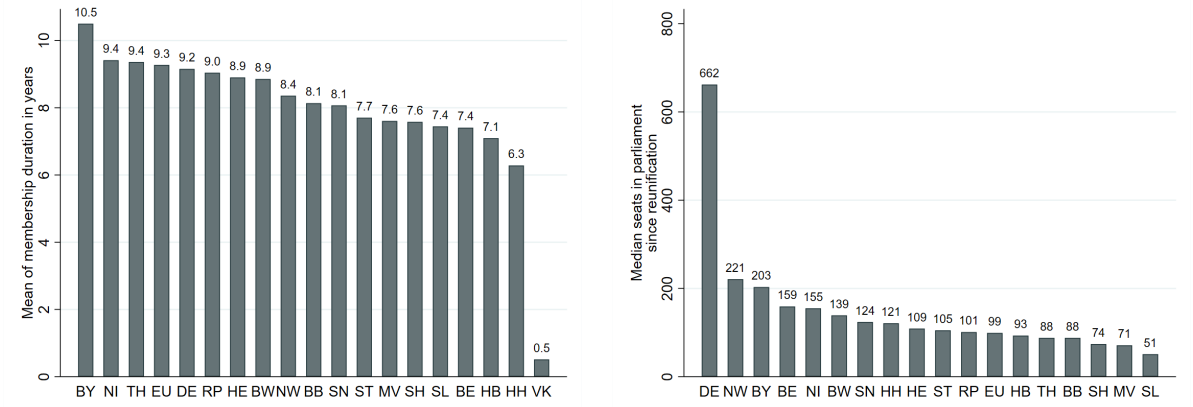
[3]To address the issue of which parliament's rules apply, individuals are either attributed the parliament rules at the time they needed to make the decision to run again in order to pass the threshold or they are dropped from the analysis completely.

Table 1.1: Summary statistics

Variable	N	Mean	Median	SD	Min	Max
Tenure in days	8,384	2,996	2,214	2,311	0	11,757
Total number of terms	8,384	2.29	2	1.51	1	11
Age at entry	8,369	44.50	45	9.95	18	82
Age at exit	8,369	53.17	54	10.71	19	86
Number of party changes	8,384	0.02	0	0.17	0	3
Female	8,384	0.34	0	0.47	0	1
Position on party list	5,272	19.64	12	21.77	1	148
Constituency vote share	4,574	0.30	0	0.15	0	1
First Party						
AfD	8,383	0.05	0	0.22	0	1
FDP	8,383	0.08	0	0.27	0	1
Grüne	8,383	0.11	0	0.32	0	1
Linke/PDS	8,383	0.08	0	0.27	0	1
SPD	8,383	0.29	0	0.46	0	1
Union	8,383	0.33	0	0.47	0	1
Indicators						
1(Improper term)	8,384	0.16	0	0.37	0	1
1(Death in office)	8,384	0.00	0	0.06	0	1
1(Change of parliament)	8,384	0.07	0	0.26	0	1
1(Still in office)	8,384	0.32	0	0.47	0	1
1(former GDR excl. Berlin)	8,384	0.26	0	0.44	0	1
1(National parliament or higher)	8,384	0.27	0	0.44	0	1

Sample of parliamentarians in 1990 or later. Improper terms are defined as those lasting shorter than statutory length.

Figure 1.2: Seats and tenure by parliament since reunification



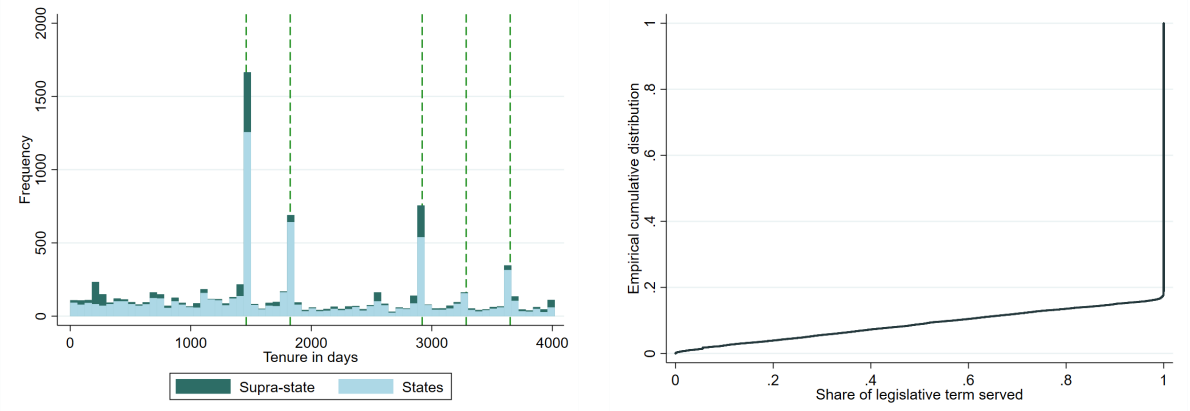
a) Length **b)** Seats

Notes: Abbreviations can be found in Table 1.A.1.

ure 1.A.2a illustrates that the total number of seats remained relatively stable until it increased following reunification in 1990. Since then, the number of seats has trended downward in state parliaments, while the Bundestag has expanded steadily since the early 2000s, culminating in electoral law reform in 2023. Figure 1.A.2b plots the average age of parliamentarians over time, showing similar patterns for both state and federal

levels, with an average age of 51 years today. The female share among legislators, shown in Figure 1.A.2c, follows three distinct phases: below 10% in the post-war years, a steady rise beginning in the 1980s until the late 1990s and then a plateau at around one-third. Again, the Bundestag closely tracks state parliaments. Figure 1.A.2d shows the development of average tenure among parliamentarians, excluding those still in office. This suggests that the following analysis takes place in a context of relatively stable tenure patterns.

Figure 1.3: Term completion



a) Tenure **b)** Term consummation

Notes: The left graph depicts the distribution of parliamentary careers in terms of tenure in days, while the right graph plots the cumulative share of legislators exiting parliament during a legislative term. Both exclude legislators who are still active.

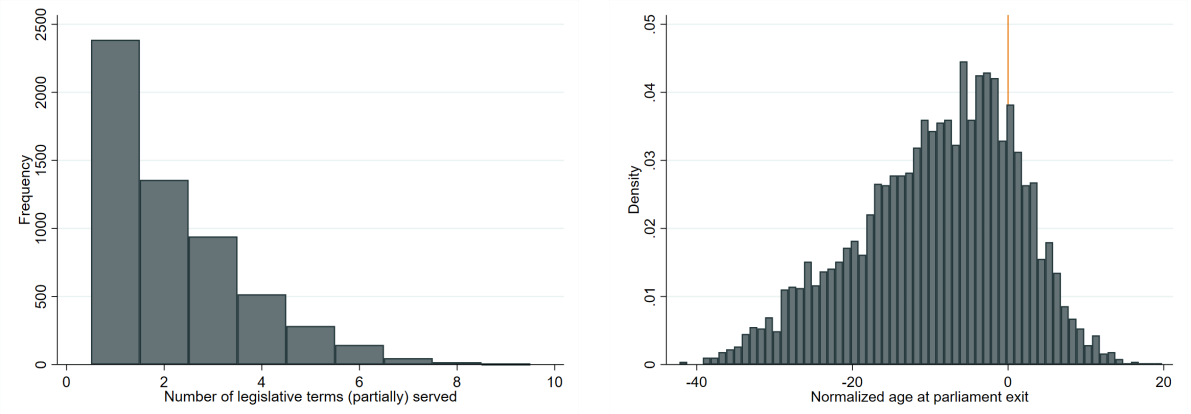
Figure 1.3 provides evidence that parliamentarians tend to serve their full legislative period. The left panel displays a histogram of parliamentarians' total tenure in days, showing substantial bunching at durations corresponding to one statutory term length (four or five years) and two terms (eight, nine or ten years). The right panel presents the empirical cumulative distribution function of the share of a term served relative to its ex-post full length. It indicates that well over 80% of terms are served in full. Legislators exiting prematurely do so in a uniform pattern, with the exit probability remaining constant over time. This contrasts with the idea that exits tend to occur primarily at half-year intervals when pension eligibility is reached.

The distribution of the total number of terms served and age at parliament exit is shown in Figure 1.4. While a significant number of individuals serve only one term, the majority serve two or more terms. Statutory retirement age varies over time and by parliament. Common statutory retirement ages are 60, 63, 65 and 67 years. Figure 1.4b plots the distribution of age at parliament exit, subtracted by the respective statutory retirement age at the time. The graph shows that most parliamentarians exit parliament long before they would be eligible for pensions, which highlights the fact that being a legislator is

often not a lifelong career. There is little noticeable bunching around the statutory retirement threshold. This is expected, as most parliamentarians tend to complete the terms they have started rather than leaving immediately upon reaching retirement age. In fact, many parliamentarians stay beyond the age at which they could retire with a full pension. Since parliamentary allowances and old age compensation are not paid simultaneously, these individuals forgo pension income if they are eligible. While they may still be financially better off by remaining in office, this suggests they may derive low disutility or even utility from continuing to work as legislators.

In light of these stylized facts, parliamentarians appear to adjust their behavior more on the extensive margin, i.e., deciding whether to run for parliament again, rather than on the intensive margin, i.e., deciding when to quit within a term. This is particularly true since eligibility thresholds are typically designed to require two full legislative periods. However, eligibility rules do allow for commenced years to count as a full year as long as more than half of it is served. This subtlety would permit slightly premature exits without forfeiting pension claims. Despite this, such behavior is not observed in the data.

Figure 1.4: Parliament exits



a) Terms served **b)** Normalized age at exit

Notes: The left graph depicts the distribution of number of terms served, while the right graph depicts the distribution of parliamentarians' age at exit, normalized by the respective statutory retirement age at the time. Both exclude legislators who are still active.

1.6 Analysis

Treatment definition

This section examines the effects of eliminating eligibility thresholds. For this analysis, treatment is defined as a binary indicator of whether a reform changes the eligibility

threshold for old age compensation. The threshold must shift from a level that is unattainable after a single parliamentary term to one that can be reached within a single term or the threshold must be abolished entirely. In reformed systems, legislators can qualify for a pension without needing to be re-elected, while in unreformed parliaments re-election remains necessary to meet the threshold.

A reform occurring in parliament p in year t is coded as $d_{p,t} = 1$, while $d_{p,t} = 0$ indicates that no reform has taken place. Table 1.A.8 presents the status quo across parliaments, with a switch from 1 to 0 marking the introduction of a reform. Parliaments that have not implemented such reforms serve as the control group. Each parliament undergoes at most one switch during the observation period. Nearly all reforms involve relaxing eligibility thresholds, with only one parliament moving in the opposite direction.[4]

Empirical method

The baseline estimation method is a conventional two-way fixed effects event study design (Schmidheiny & Siegloch, 2019). An event study is well-suited to the research question because it allows for the estimation and visual presentation of dynamic effects. Changes, if present, are expected to materialize only after some time as parliamentarians adjust and legacy rules expire. The continuous existence of never-treated parliaments and absorbing treatment status simplify technical identification.[5] The empirical model is characterized by

$$y_{ipt} = \sum_{j=\underline{j}}^{\overline{j}} \beta_j b_{pt}^{j} + X'_{ipt}\gamma + \mu_p + \theta_t + \varepsilon_{ipt} \tag{1.1}$$

with event indicators $d_{p,t}$ as defined above coded to

$$b_{pt}^{j} = \begin{cases} \sum_{s=-\infty}^{\underline{j}} d_{p,t-s} & \text{if} \quad j = \underline{j} \\ d_{p,t-j} & \text{if} \quad \underline{j} < j < \overline{j} \\ \sum_{s=\overline{j}}^{\infty} d_{p,t-s} & \text{if} \quad j = \overline{j}. \end{cases}$$

That is, the outcome y_{ipt} such as tenure or decision to run for office of legislator or candidate i in parliament p in year t is regressed on event indicators b_{pt}^{j} for each time period j before and after the event. A vector of control variables X'_{ipt} may be included

[4] To preserve observations, this exceptional reform is assumed to have a symmetrical, opposite effect and is coded as -1. However, for analyses of party lists and vote shares, this reform is treated as non-reformed, given that almost a decade has passed since its introduction.

[5] Absorbing states, a term from the event study and difference-in-differences literature, refer to the fact that treatment status is never reverted, meaning all treated units remain treated throughout the observation period.

to control for potentially confounding factors such as governing parties, term length, premature dissolution, election year and part-time parliament, if not indicated otherwise. Parliament fixed effects μ_p control for possibly unobserved time-invariant, parliament-specific effects and year fixed effects θ_t control for year-specific effects common to all legislators. The error term ε_{ipt} is clustered at the legislative term level which is the level of treatment assignment (Abadie et al., 2023).

The coefficients of interest are the β_j, which represent the dynamic treatment effect over time. The indicator for one year before treatment, b_{pt}^{-1}, is normalized to zero, serving as the reference point. Endpoint indicators $b_{pt}^{\underline{j}}$ and $b_{pt}^{\overline{j}}$ group all periods before $\underline{j}$ and after $\overline{j}$ into two separate bins. This "binning" of endpoints ensures that the dynamic effects β_j are measured relative to the pre-treatment year alone. The dynamic treatment effect is thus to be interpreted relative to the pre-treatment period and parliaments that are not (yet) treated. However, since the binned endpoint indicators combine multiple time periods, their estimates do not have straightforward interpretations.

The choice of leads and lags affects the length of the estimation time window and sample size, as the panel is shortened accordingly. I primarily use five leads and nine lags, covering the period from 1999 to 2017. This selection makes transparent potential pre-trends and anticipation effects while providing a sufficiently long time horizon to observe changes, capturing at least three full or partial legislative terms after the reforms. For data-constrained analyses of party list ranks and vote shares, I use three leads and seven lags.

An alternative specification additionally estimates the differential effect between two groups g within a parliament by introducing the full set of interaction terms to Equation (1.1). One group consists of legislators within the (now obsolete) pre-treatment eligibility threshold. Legislators in this group are (would have been) directly affected by the threshold because at the time they were not eligible for pension payments yet. Such observations within the threshold have indicator variable $1(g = \text{within})_{it} = 1$. All other legislator-year observations that are beyond the (obsolete) threshold, are coded as zero. These two groups might very well be affected differently by the tenure requirement elimination as the latter group exceeded the threshold already. The empirical model is given by

$$y_{iptg} = 1(g = \text{within})_{it} \left(\sum_{j=\underline{j}}^{\overline{j}} \delta_j b_{pt}^{j} + X'_{ipt}\nu \right) + \alpha_{pt} + \lambda_{pg} + \eta_{tg} + \epsilon_{iptg}. \tag{1.2}$$

This specification, akin to a triple difference estimator, compares the development of the outcome variable not only to the pre-treatment period and legislators in untreated parlia-

ments as in Equation (1.1), but additionally compares legislators facing the requirement with those who have already met the requirement within the same parliament.

Self-selection

As legislators initiate and vote on laws, self-selection of parliaments into treatment is particularly contentious in this case. Since parliamentarians decide on treatment status, assignment may not be random, potentially introducing estimation bias. If certain characteristics influence treatment assignment, the results could reflect these confounding factors rather than the effect of threshold elimination. However, this concern is mitigated by the significant time gap between the passage of legislation and its implementation. Additionally, legacy clauses protect pension claims accrued before the reforms, so the new rules apply only to successors and those not yet eligible for pensions at the time of the change.

The brief discussion of circumstances under which reforms were implemented in Section 1.3.3 and a glance at which states conducted reform in Table 1.A.8 might further assuage concerns that an obviously selected group introduced reforms. Figure 1.A.3 and Table 1.A.9 in the Appendix present regression results of party shares before and after reforms. They suggest that, at least in the periods leading up to the reforms, there is no differential development in the party composition of parliaments. A similar picture emerges when looking at parties in power. Shifts in political power thus cannot explain reform adoption.

This is also reflected in Table 1.A.10. It shows balance of a range of characteristics between treated and non-treated parliaments before a wave of reforms starting in 1998. The difference in key characteristics is insignificant.[6]

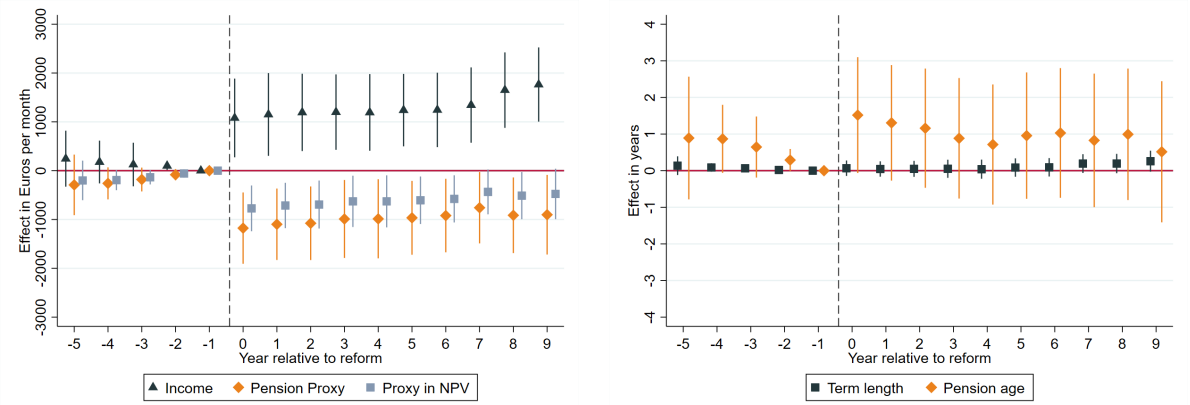
Ultimately the identifying assumption is that absent the reforms, outcomes in treated units would have evolved parallel to those of control units. One way to make this assumption more plausible is to document the absence of significant pre-trends. Below, pre-trends five years prior to reforms will be shown.

[6]The one exception to this are party shares of center-left SPD, center-right Union and category "Other." The share of legislators who are members of SPD was 13.3 percentage points higher in parliaments that eventually eliminated the threshold and 15.4 percentage points lower for members of center-right Union. However, as the event study in Figure 1.A.3 attests, the shares are not on a different trend shortly before the reforms. The significant difference in SPD and Union shares might explain why the dynamic effects in Figure 1.A.3 document an increase in the share of Union seats compared to the SPD. It might simply be a reversion to the mean. Note that reforms were also passed with both parties in power, respectively.

Mixed treatment

Law changes rarely target a single provision but are often part of broader reform packages affecting multiple aspects of parliamentary life at once. As a result, the effect of a reform may be confounded by simultaneous changes introduced within the reform bundle. Figure 1.5, which is based on Table 1.A.11, presents event studies estimated according to Equation (1.1) of variables that also changed during law reforms. Column 6 in Table 1.A.11 shows the effect of reforms on the eligibility requirement in years. It mechanically decreases after reforms as defined above by around eight years on average. This is the reform aspect of interest.

Figure 1.5: Contemporaneous changes



a) Monetary changes **b)** Limit changes

Notes: This graph depicts two event studies, based on Equation (1.1). NPV is net present value. The time on the horizontal axis is years relative to the year when pension eligibility reform was implemented. Confidence intervals are computed at the 95% level.

Turning to contemporaneous changes in other characteristics, Figure 1.5a presents an event study of legislators' monthly allowances. While pre-reform trends are statistically indistinguishable from zero, allowances increase by approximately €1,100 on average during the reforms, compared to both the non-reform group and the pre-treatment period. At the same time, a proxy for old-age compensation generosity decreases by around €1,200 following the reforms or approximately €800 in net present value terms. Note that the latter figure is based on a discount factor of 0.9376 (see below), which places more weight on current income than discount factors typically used. If a higher discount factor were applied, the pension proxy in net present value terms would converge to its nominal value, reducing the reforms' average net monetary benefit in net present value.

The pension proxy serves to make pension entitlements across the different schemes introduced in Section 1.3.2 comparable and to condense pension generosity into a single measure. Under the standard system, pension disbursements as a function of tenure

are directly observable, while the alternative systems specify fixed contributions with uncertain future disbursements. Table 1.A.7 reports the pension proxy for each year and parliament, representing a hypothetical value for the standard regime. That is, the monthly amount in Euro that a parliamentarian serving ten years, retiring at the respective statutory retirement age and dying at age 85 would need to set aside to receive the same pension benefits. This proxy captures the implicit, equivalent pension contributions in the standard regime. In a further step, the net present value of the pension proxy is approximated by discounting pension payments in the future.[7]

The sharp event study results are encouraging that the proxy variable can at least partially account for the change in monetary pension generosity. Note that serving ten years would make a legislator eligible for a pension in the standard system in each parliament and, although lengthy, it is not far from the average time served. The contrary development of allowances and implicit pension contributions depicted in Figure 1.5a roughly balance out only in nominal terms, implying a real net benefit in the order of €300. Legislators on average reformed their monetary compensation to be more generous and near-term. This needs to be kept in mind when interpreting the results below. To address the issue of a contemporaneous compensation change, the subsequent analysis will also include specifications controlling for allowance and implicit pension generosity or, alternatively, try to disentangle the effects of different reform aspects (see below).

Figure 1.5b shows that the reforms of interest often include provisions to increase the statutory retirement age. The point estimates indicate an average initial increase of over 1.5 years, although the effect is not statistically distinguishable from zero. The analysis period also coincides with a widespread shift in parliamentary term lengths from four to five years. In 1990, only two parliaments were elected for five years, while by 2022 only two remained on a four-year term. However, the reforms of interest do not appear to have systematically increased term lengths relative to the control group compared to the pre-reform period.

While the increasing monetary benefit from allowance changes is not fully offset by the declining value of the contribution proxy in net present value, the average reform does not entail statistically significant shifts in term length or pension age. Aggregate findings on the reform effect may mask heterogeneous effects across different aspects of reforms. The subsequent analysis will therefore disentangle reforms that specifically altered each

[7] In order to express the proxy in terms of net present value, some additional simplifying assumptions need to be made. The calculations are based on a time span of 25 years (average age between hypothetical death at 85 years and retirement age at 65 years minus average age in parliament, which is 50 years). The discount factor is based on three rates: Inflation was 1.88% per year in the time period under consideration. Average allowance growth (on which pensions are based) was 2.78% per year. Additionally, I impose an extra "preference" rate of 2%, which altogether yields an effective overall rate of 6.66%. This implies a discount factor of 0.9376.

of these four aspects to assess their individual impact on the respective outcome variable. The results will show that none of these changes, when considered in isolation, account for the observed development of the respective dependent variable. This suggests that the overall effects of the reforms on outcomes studied here are not driven by adjustments to these parameters but rather by abolishing eligibility requirements.

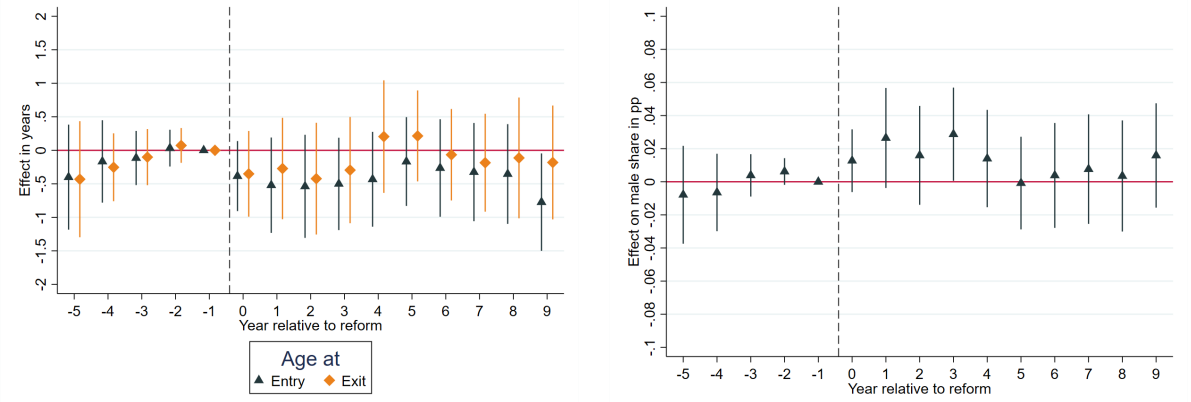
Finally, Column 7 of Table 1.A.11 in the Appendix regresses an indicator variable equal to one if a parliament was dissolved before completing its full statutory term. This tests whether early dissolutions either drive the reforms or are systematically influenced by them. The results show no significant association, suggesting that early dissolutions are neither a cause nor a consequence of the reforms.

Results

Effect on the composition of parliaments

Before assessing the hypotheses posed above, it is necessary to examine whether the results could be driven by changes in parliamentary composition. A key concern is that the age structure of parliamentarians might evolve differently across groups. However, Figure 1.6a challenges this possibility. The figure presents differential developments in age at parliamentary entry and exit, based on event study estimates following Equation (1.1). There is no evidence of significant pre-trends or lasting effects after treatment (cf. Table 1.A.12).

Figure 1.6: Age and gender



a) Entry and exit ages

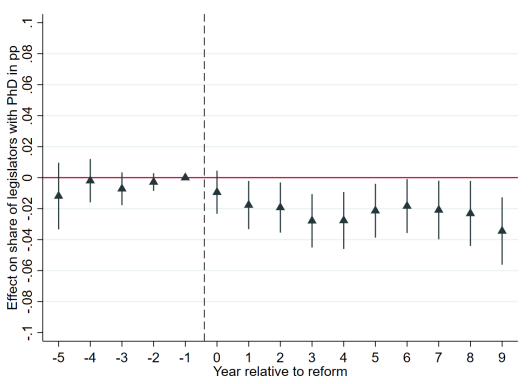
b) Gender: Male share

Notes: This graph depicts two event studies, based on Equation (1.1). The time on the horizontal axis is years relative to the year when pension eligibility reform was implemented. Confidence intervals are computed at the 95% level.

Similarly, Figure 1.6b shows that the reforms did not significantly affect the share of male

legislators. Figure 1.A.4 in the Appendix presents regression results excluding control variables X'_{ipt} in Equation (1.1). In this specification, initial treatment estimates are positive and statistically significant, indicating a temporary effect on the share of male legislators in the order of 4 percentage points that is not lasting, however. These findings suggest that compositional changes in age or gender are unlikely to drive reform effects.

Figure 1.7: Share of legislators with a PhD



Notes: This graph depicts an event study, based on Equation (1.1). The time on the horizontal axis is years relative to the year when pension eligibility reform was implemented. Confidence intervals are computed at the 95% level.

Figure 1.7 shows the effect of the reforms on the share of legislators holding a PhD, revealing a gradual and persistent decline of around 3–4 percentage points in the long run. Given that the average share of PhD holders among legislators in the sample is around 10%, this represents a substantial relative change. Column 5 of Table 1.A.12 presents a similar analysis for the share of professors, though the effect there is not statistically significant.

This decline in the share of PhD holders could be interpreted in two ways. On the one hand, it may signal a drop in formal qualifications among legislators, which might be viewed as a decrease in legislator quality. On the other hand, considering that less than 5% of the general population holds a PhD, the effect may reflect a shift toward more socially representative legislatures. The reforms could have lowered barriers to entry, making parliamentary service more accessible to candidates with diverse professional backgrounds, not just those with advanced academic credentials.

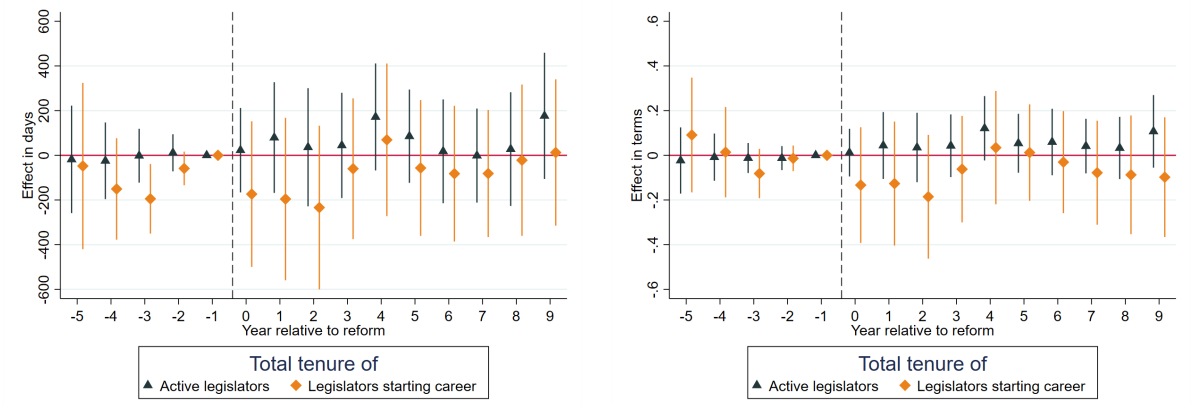
Another relevant aspect of parliamentary composition is the distribution of legislators across terms served. Since pension eligibility is closely tied to reaching and completing a second term, the term distribution holds particular importance in this context. Significant shifts in the share of legislators serving multiple terms could potentially confound the estimated reform effects. However, neither Figure 1.A.6 nor Table 1.A.17 in the Appendix reveal any significant changes in the term distribution following the reforms. Thus, on average there is no lasting compositional effect of the reforms with respect to

age and terms served. The share of male legislators increases temporarily in the specification without control variables. There is a lasting decrease in the share of legislators, who have a PhD. These findings, with the exception of the gender share, are robust to excluding control variables X'_{ipt} or incorporating variables allowance and pension proxy in Equation (1.1).

Tenure and share running for re-election

This section examines whether legislators' tenure is affected by the pension threshold reforms. Figures 1.8a and 1.8b present estimates of tenure changes, based on the regression results in Table 1.A.13. One channel through which abolishing the pension requirement might influence parliamentarians' behavior is by altering the length of their parliamentary careers. Tenure is measured in two different ways here. First, it is defined as the total tenure of all legislators active in parliament at a given point in time, represented by black triangles or second, as the total tenure of all legislators who began their parliamentary careers at a specific point in time, represented by yellow diamonds. The outcome is expressed either in days or in legislative terms, with logarithmic transformations applied in Columns 1-2 of Table 1.A.13. Across all definitions, the results consistently point in the same direction. None of the post-reform coefficients is statistically significant, suggesting that the reforms did not systematically affect legislators' career lengths.

Figure 1.8: Tenure



a) Days **b)** Terms

Notes: This graph depicts two event studies, based on Equation (1.1). The time on the horizontal axis is years relative to the year when pension eligibility reform was implemented. Confidence intervals are computed at the 95% level.

Since the estimated effects could be influenced by concurrent reforms (such as changes to allowances or pension payments, as discussed above) I re-define the treatment variable in Equation (1.1). Rather than focusing on the removal of the tenure threshold, treatment

is now defined as any period featuring either a yearly allowance increase above the 95th percentile or a pension proxy decrease below the 5th percentile in the sample.[8] The results from these alternative reform definitions are shown in Table 1.A.14 in the Appendix. Neither allowance increases nor pension size reductions have a significant effect on tenure. This suggests that the estimated impact of removing the tenure requirement is not biased by overlapping changes in allowances or pension generosity.

Considering there is no evidence of an intensive margin response, as tenure remains unaffected, Figure 1.9, which is based on Table 1.A.15, depicts the extensive margin response. It shows how the share of legislators running for re-election, either as constituency or list candidate or both, differs between the treatment and control groups. The analysis is done separately for first-term legislators considering re-election and more senior legislators. The threshold reforms are expected to primarily affect new legislators, as re-election would have guaranteed them pension payments without the reforms. However, consistent with the intensive margin results, there is no significant response. The large size of the estimated effect window also rules out gradual learning or taking advantage of legacy rules as an explanation.

Column 3 of Table 1.A.15 presents the results of a regression of the total number of candidacies running for election on the reform event indicators, regardless of whether candidates held prior parliamentary membership. The results show no significant change in the total number of candidates following the reforms, suggesting that there is no entry effect associated with the removal of the threshold. This indicates that the reforms do not lead to a broader increase in political participation or candidacy rates, which was one of the reforms' aims.

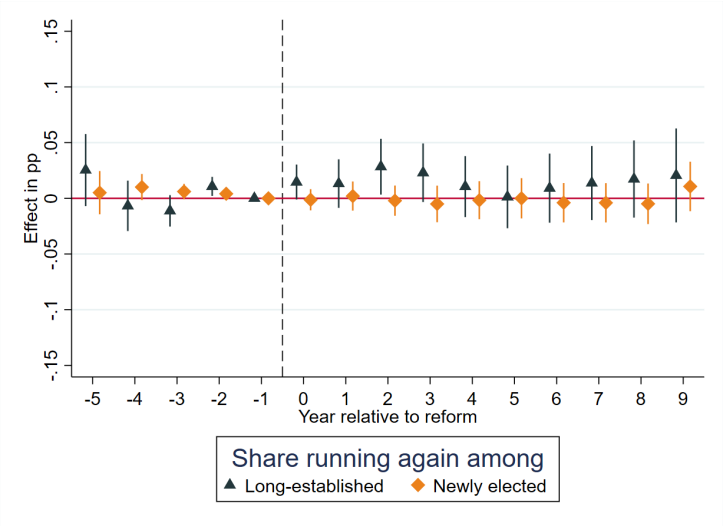
Note that all findings in this section are robust to different specifications, including controls for allowance and pension size changes, either by incorporating them into the covariate vector X'_{ipt} or by excluding control variables entirely from Equation (1.1).

Heterogeneity

The lack of evidence in favor of an average tenure response might hide important heterogeneity of politicians' reaction to changes in incentives (Keane & Merlo, 2010). Two dimensions by which responses might differ could be age and age relative to the statutory retirement age. As retirement approaches, pension payments might become more salient and discounted less by legislators. Figures 1.A.5a and 1.A.5b, based on Table 1.A.16, analyze this heterogeneity. Being faced with imprecisely estimated coefficients again, there is again no evidence of a consistent tenure response. Possible reasons for this finding,

[8] These thresholds correspond to an allowance increase of at least 9% or a pension proxy decrease of at least 4%.

Figure 1.9: Share running for re-election



Notes: This graph depicts an event study, based on Equation (1.1). The time on the horizontal axis is years relative to the year when pension eligibility reform was implemented. Confidence intervals are computed at the 95% level.

coupled with later findings, will be discussed in Section 2.5.

Ideally, heterogeneity in occupations outside of parliament would also be studied. Legislators face varying outside options as they return to previous careers and different levels of previous pension claims. For example, civil servants with secure pension entitlements might be less affected by the removal of a minimum threshold, while the self-employed or full-time politicians may depend more heavily on parliamentary pensions. Unfortunately, the data provided by parliamentary administrations do not allow for a robust heterogeneity analysis.[9]

In conclusion, neither average tenure nor the probability of legislators to run for re-election decreases significantly following reforms that abolish eligibility thresholds, contrary to the initial hypothesis. This holds true even in the sub-sample of first-term candidates, who would be expected to be most affected by the reforms. However, there is considerable uncertainty in terms of large confidence intervals regarding the rejection of the first hypothesis. Therefore, the next section will examine the effect on a variable that is observed more frequently.

[9]Even when supplemented with information from Wikidata and other sources, occupational data are available for only about 12% of legislators. For some parliaments, no such data exist and where it does, it is often self-reported, raising concerns about credibility. As a result, subgroup analysis is not feasible.

Position on party list

This section investigates the effect of the reforms on positions on the party list. Due to data limitations, the effect window in this part of the analysis is restricted to three leads and seven lags, starting in 2002. As in previous analyses, the focus is on candidates elected to parliament, while the construction of the dependent variable, i.e., rank divided by list length, includes all available list candidates. Another sample restriction is the exclusion of the city-state of Berlin, as it features both a state-wide party list and election district lists. While this is not unique to Berlin, data on which specific list a candidate is placed on in Berlin is unavailable. Additionally, lists in some states are determined by voters rather than parties and are therefore excluded from this analysis as well.[10]

To account for variation in list lengths and to prevent the denominator of the normalized rank from driving the results, the logarithm of list length is included as a control variable throughout this and the next section. Additionally, I include party×parliament fixed effects, which control for the average list length of each party within each parliament.

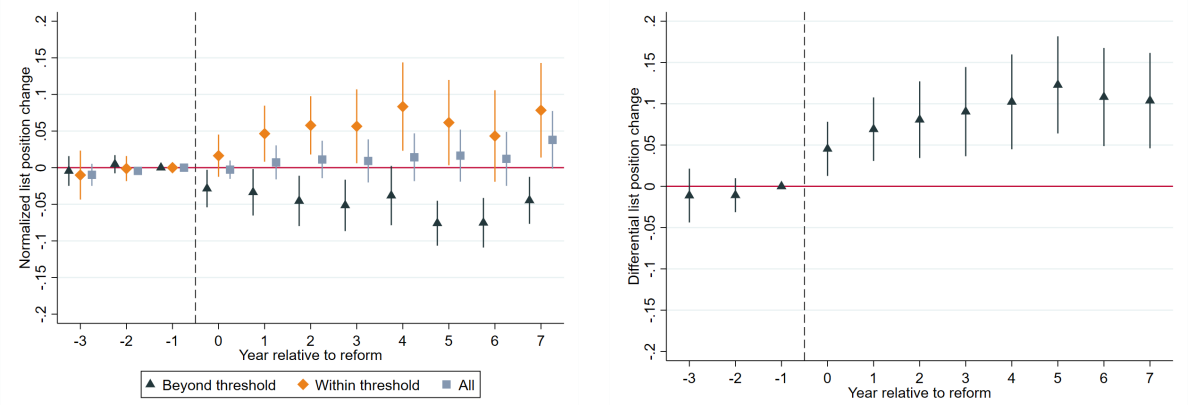
Here, it is insightful to again distinguish between legislators that have passed the (now obsolete) threshold and those that have not. In the case of reformed parliaments, this is the threshold that was in place before the reforms as all parliaments operated under a tenure requirement at some point. Legislators within the threshold face an incentive to reach pension eligibility under unreformed schemes while they do much less so (in terms of pension payments) in reformed schemes.

The primary result is illustrated in Figure 1.10a, which is derived from Table 1.A.18. The outcome variable represents the normalized rank on a party list, calculated as the rank divided by the list length. Following the reforms, the rank of legislators within the threshold becomes progressively less favorable, as a larger rank on the party list corresponds to a lower probability of election. Conversely, the average rank of legislators beyond the threshold improves compared to both the pre-reform period and the control group. Both effects appear to persist over time. However, the overall normalized rank does not exhibit a differential trend. Figure 1.10b displays event study coefficients based on Equation (1.2), estimating the reforms' differential effect on legislators within the threshold relative to those beyond the threshold.

Table 1.A.18 also reports the single estimate for the entire post-reform period. On average, legislators within the obsolete threshold experience a deterioration in rank of approximately 4.7 percentage points compared to those where the threshold remains in place in Column 1. The single post-reform estimate rises to 8.6 percentage points when additionally comparing them to legislators beyond the threshold in Column 3. This find-

[10]This is the case in Baden-Württemberg, Bavaria, Bremen and Hamburg.

Figure 1.10: Place on party list



a) Rank by subgroup **b)** Differential rank

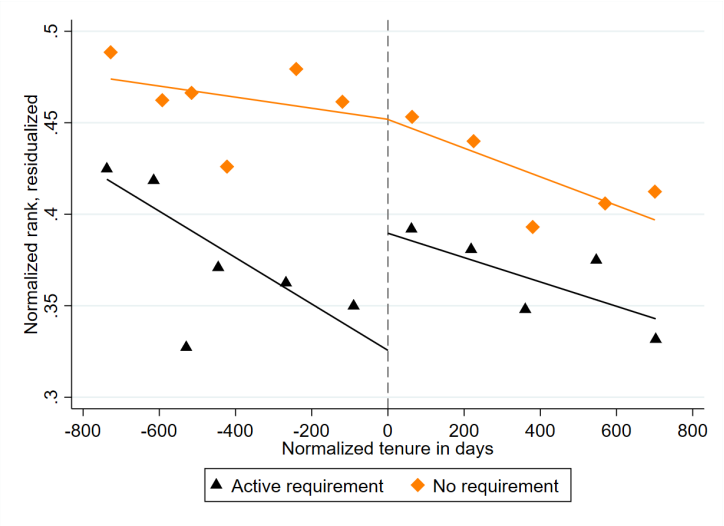
Notes: This graph depicts two event studies. The left graph is based on Equation (1.1), while the right graph is based on Equation (1.2). The time on the horizontal axis is years relative to the year when pension eligibility reform was implemented. Confidence intervals are computed at the 95% level.

ing provides strong evidence in support of the second hypothesis. Party lists appear to incorporate the pension threshold into their candidate placement decisions. Once the threshold is no longer binding, party lists adjust to this new reality, with high-tenured legislators benefiting from more favorable list ranks on average. Positions on the party lists being a zero sum game, the overall effect is zero.

Figure 1.11 supplements the analysis by presenting a binned scatter plot depicting the residualized normalized rank on the vertical axis against the normalized tenure in days on the horizontal axis. Residuals are from a regression of the normalized rank on the control variables and fixed effects as in Equation (1.1) and the logarithm of list length. Tenure is normalized by subtracting from current tenure the corresponding (former) pension threshold. Observations that have not yet secured a pension have a negative value while positive values imply a pension claim exists. In case of parliaments that abolished the requirement, this is the old, hypothetical requirement. This difference is reflected in two graphs: The orange diamond markers represent legislators in parliaments that abolished the tenure requirement and here serve as the comparison group while the black triangles represent legislators in parliaments that operate under an active requirement. Linear approximations of the underlying data are shown separately for both groups and on both sides of the threshold.

The normalized rank distribution shows no visible break around the hypothetical threshold for legislators no longer subject to the requirement. In contrast, for legislators still facing the requirement, rank improves more rapidly as tenure nears the threshold. At the threshold, the linear fit suggests a discontinuity of approximately 6 percentage points, indicating that among similarly experienced candidates, those on the verge of meeting

Figure 1.11: Normalized rank and tenure



Notes: This graph depicts a binned scatter plot depicting the residualized normalized rank on the vertical axis against the normalized tenure in days on the horizontal axis, differentiated by parliaments that have a pension eligibility threshold in place and those that eliminated it. Residuals are from a regression of the normalized rank on the control variables and fixed effects as in Equation (1.1) and the logarithm of list length. Tenure is normalized by subtracting from current tenure the respective pension threshold.

pension requirements are favored. Beyond the threshold, rank patterns evolve in parallel as incentives become comparable.

The analysis establishing a clear effect on candidates' position on the party list does not identify the exact mechanism. It remains unclear whether the result stems from affected candidates lobbying more intensely and successfully for favorable list positions when facing a tenure requirement or whether parties, well informed about parliamentary regulations, more actively incorporate their members' interests when compiling lists. The observed outcome reflects the combined effect of these two possible channels and will be discussed below.

Robustness

Robustness checks in Columns 1-3 in Table 1.A.19 also regress the non-normalized rank on the event indicators and find a strong and significant differential effect. Columns 4-6 report regressions with list length as the dependent variable. There is neither a significant change relative to the control group and pre-reform period nor a differential development between legislators within and beyond the (hypothetical) threshold.

The result is not sensitive to the estimation method, cf. Figure 1.A.7 based on Sun and Abraham (2021) or the selection of control variables (cf. Columns 1-3 in Table 2.A.13). Restricting the sample to those parliaments that constitute a true panel yields even more

pronounced findings and rules out that compositional effects of the estimation sample drive the result (cf. Columns 4-6 in Table 2.A.13). Including parliament-specific linear time trends does not change the results either (cf. Columns 7-9 in Table 2.A.13). Focusing on only those parliaments that stayed in the standard pension regime as discussed in Section 1.3.2 rules out any changes that might be associated with modifications in the way pensions are organized. Results do not change qualitatively for this sub-sample in Columns 10-12.

One might worry that short lists have an out-sized influence since a one rank change translates into a larger percent change compared to long lists. Excluding observations that are part of lists with fewer than 10 members does not change the picture (cf. Columns 1-3 in Table 2.A.14). Instead of considering the position on the party lists of legislators within the threshold, one can focus on those within the threshold in their (obsolete) decisive term. In the vast majority of cases, this is the second term if there has been no premature dissolution of parliament or tenures beginning within a legislative term. Comparing the place on the party list of legislators about to enter the decisive term between treated and non-treated parliaments in Columns 4-6 of Table 2.A.14 yields point estimates comparable to the main specification in the early and later years after reforms.

Finally, local or newly emerging parties could have a differential impact on rank patterns. For this reason, models in Columns 7-9 of Table 2.A.14 include legislators who are members of long-established parties and their predecessor organizations only.[11] The results barely change. Focusing on the historically most popular parties, SPD and Union, that are part of every parliament at every point in time yields similar results.

To assuage concerns that this finding is driven by other aspects of the reforms that are implemented concurrently, I define events where aspects of the reform package have been introduced at other times and construct from these alternative event study coefficients in the same fashion I do with the original events of interest. As discussed above, these include changes to the parliamentary allowance, the pension contribution proxy, statutory pension age and term length. An event is defined as a period with a yearly percent allowance increase larger than the 95th percentile, a pension proxy smaller than the 5th percentile, a pension age change of one year or more and any term length change, respectively.[12]

Results from regressing the normalized rank on the new event study coefficients and control variables are reported in Table 1.A.22. Based on this exercise, reforms to allowances do not have an impact on differential rank developments in Column 3. Reforms to the pension proxy have a significant, positive effect in Column 6, but only shortly after the reforms and dissipating quickly. Changing pension age in Column 9 is even associated

[11] This includes parties FDP, Grüne, Linke, SPD and Union.

[12] The percentiles of allowance increase and pension proxy decrease correspond to an increase of at least 9% and a decrease of at least 4%, respectively.

with an opposite, negative effect in later years. Reforms to term length do not lead to a differential response bar one year, but a significant negative response for legislators beyond the threshold. Note that Figure 1.5b documents that there is no systematic change of term lengths around eligibility threshold reforms. I conclude that none of the contemporaneous changes under consideration seems to drive the full positive effect on differential rank developments and it is indeed the elimination of the tenure threshold that drives the results.

A final exercise is testing the sensitivity of the results to excluding particular parliaments from the analysis. In Figure 1.A.8, Figure 1.10b is replicated by estimating Equation (1.2) repeatedly by omitting each parliament one-by-one. The jackknife estimates paint the same picture throughout. No single parliament drives the overall result. Excluding the Bundestag and the Hessian parliament attenuates the treatment effect estimates, however.

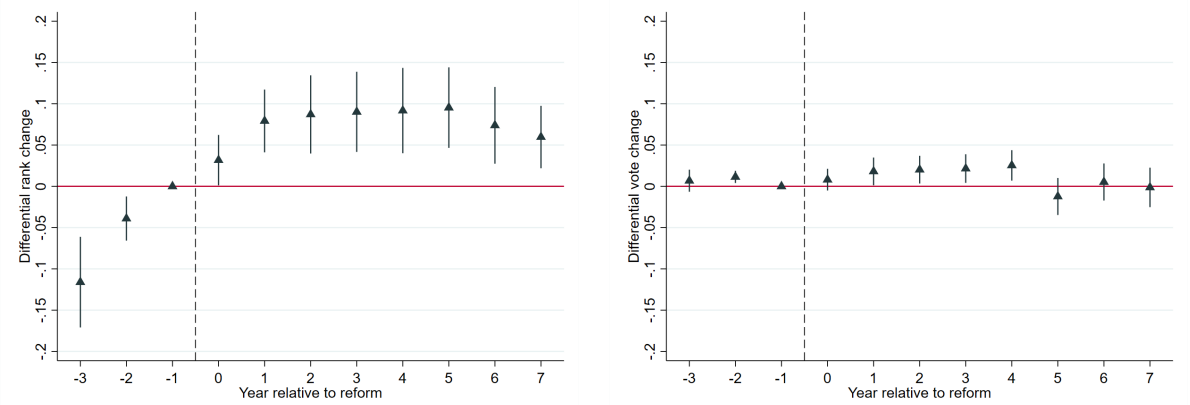
In light of the empirical evidence presented here, I conclude that there indeed is a robust, significant and lasting change in the allocation of positions on the party lists in the aftermath of the elimination of a minimum eligibility threshold. Long-established candidates benefit from this persistent and meaningful shift at the cost of their low-tenured peers, consistent with the reduction of pension incentives outlined in the second hypothesis.

Vote share

To assess how the previous finding resonates with voters, the mixed electoral system in most parliaments introduced in Section 1.3.4 can be leveraged by analyzing the sub-sample of legislators who are simultaneously ranked on a party list (proportional component) and fielded as constituency candidates (majoritarian component). For these individuals, vote shares are observed, allowing for a comparison of average vote shares between treated and non-treated parliaments for ranked candidates.

Since being selected as both a constituency candidate and a party list candidate is not random, it is necessary to establish that the previous finding holds for this particular sub-sample as well. Figure 1.12a, based on regression results of Equation (1.2) in Table 1.A.23, depicts the differential rank patterns of legislators who are simultaneously fielded as constituency candidates. Compared to the main specification, the number of legislator-year observations drops by approximately 6,000 to 20,117. This implies that around three in four ranked parliamentarians are also constituency candidates. The differential rank development of legislators who are constituency candidates closely mirrors the baseline result without this restriction after the reforms. However, a pronounced pre-trend is evident in this sub-sample.

Figure 1.12: Constituency candidates on lists



a) Differential rank **b)** Differential vote share

Notes: This graph depicts two event studies. The graphs are based on Equation (1.2). The time on the horizontal axis is years relative to the year when pension eligibility reform was implemented. Confidence intervals are computed at the 95% level.

In Table 1.A.23, the dependent variable is the vote share of ranked candidates, with Figure 1.12b graphically illustrating the results. The pre-trend is again imperfect. Between one and four years after the reforms, there is a significant increase in the share of votes of approximately 2.5 percentage points. However, this effect is not persistent, vanishing after one legislative term. The non-interacted models in Table 1.A.23 do not yield significant results for either group separately. While Section 2.5 explores several competing explanations, there is some evidence in support of the third hypothesis, i.e., that legislators running for the (obsolete) decisive term become more popular with voters after the reforms.

1.7 Discussion

In light of the empirical evidence presented in this paper, some unifying remarks may help to interpret the results. It is perhaps unsurprising that overall tenure remains unaffected. Tenure reflects the culmination of decisions not only by candidates themselves but also by voters and parties, making it difficult to alter at will. However, the absence of an effect on the arguably more discretionary decision to run for office is less intuitive. One possible explanation is that parliamentary membership is perceived as attractive for various non-pecuniary or non-pension-related reasons, preventing the reforms from tipping the scales on average. Alternatively, the threshold might not be salient to legislators, although this seems unlikely given the observed effects on party list rankings.

A robust finding throughout the analysis of party list placements is the gradual increase

in the (normalized) rank rather than a sudden shift. One possible explanation is the incremental phasing out of legacy provisions embedded in the reforms. As the number of legislators accruing pension claims under the old regime gradually declines, fewer candidates require a helping hand from their party list. Notably, the event study estimates peak four to five years after the reforms, a period when, in most cases, a new parliament has convened and the new rules apply to all legislators below the threshold. The estimates remain at this elevated level, as indicated by the binned endpoint indicator of the final lag.

The mechanism behind this strategic use of the party list may be driven by at least two forces. On the one hand, parties may monitor their members' parliamentary careers and internalize their pursuit of pension eligibility, a pattern that becomes empirically visible when tenure requirements are abolished. On the other hand, the most influential party members are often legislators themselves, who have strong incentives to lobby for better list positions when facing the threshold. The ultimate driving force behind the observed result remains unclear.

Note, however, that legislators facing the threshold are by definition newcomers to parliament and therefore likely to be junior. Given the zero-sum nature of party lists, any improvement in the position of a newcomer would come at the expense of long-established legislators. Senior legislators also have an incentive to remain in parliament and may wield greater influence within the party than newcomers. Consequently, at least some degree of benevolence from senior party members appears necessary to explain the observed rank improvements. As Cerina and Deidda (2017) remarks, parties often dominate parliamentary life, which is true in this context, too.

The temporary surge in popularity of candidates facing the old, non-binding threshold may be linked to their less favorable positions on the party list. These candidates might exert greater effort to secure election. Alternatively, the abolition of tenure requirements could prompt parties to prioritize more popular candidates over those primarily selected to secure pension claims, even if the latter are less popular. This effect dissipates five years or one term after the reforms, despite the persistent rank effect. In terms of voter preferences, the reforms of eligibility thresholds do not make a difference in the long run based on this measure.

1.8 Conclusion

In this paper, I have studied reforms that abolished minimum tenure requirements for generous pension payments in the context of state and national legislatures in Germany. Although the financial incentives to remain in office for at least two full terms were

substantial before the reforms, the elimination of tenure requirements led to surprisingly few compositional changes. A setting in which such thresholds are introduced, as in Miglino (2024), might have produced more predictable responses. The null result on the share of first-term legislators running for re-election is striking, especially since many legislators likely have attractive outside options. However, parliamentary mandates in Germany are well-remunerated and it is difficult to observe effort. Additionally, outside options can often be pursued alongside parliamentary work, making a legislative career attractive regardless of pension incentives. These findings of non-responsiveness align with a broader literature that documents only modest changes in response to adjustments in politicians' compensation.

The finding that party list positions change strategically not only demonstrates that some margins do respond to the reform but also shifts the focus from individual candidates to parties as key actors in the political arena. The elimination of eligibility requirements removes a friction in legislators' budget sets that previously constrained party decision-making and candidate selection.

From a normative perspective, it is difficult to draw strong conclusions that account for all of the reforms' intricate implications. The reforms effectively reallocated resources from future pension payments to present-day allowances at a net financial benefit to them while expanding pension eligibility to more legislators. Assessing the reforms solely based on its fiscal costs is likely inadequate and its implications for representation are more relevant. In this regard, I document that for a large sub-sample of parliamentarians, voters do not penalize efforts to pass the eligibility threshold in the long-term at the ballot box. The overall number of candidates does not change following reforms and the only observed lasting effect is a decrease in the share of legislators holding a PhD. Reforms thus were very limited in their aim to attract a more diverse pool of candidates.

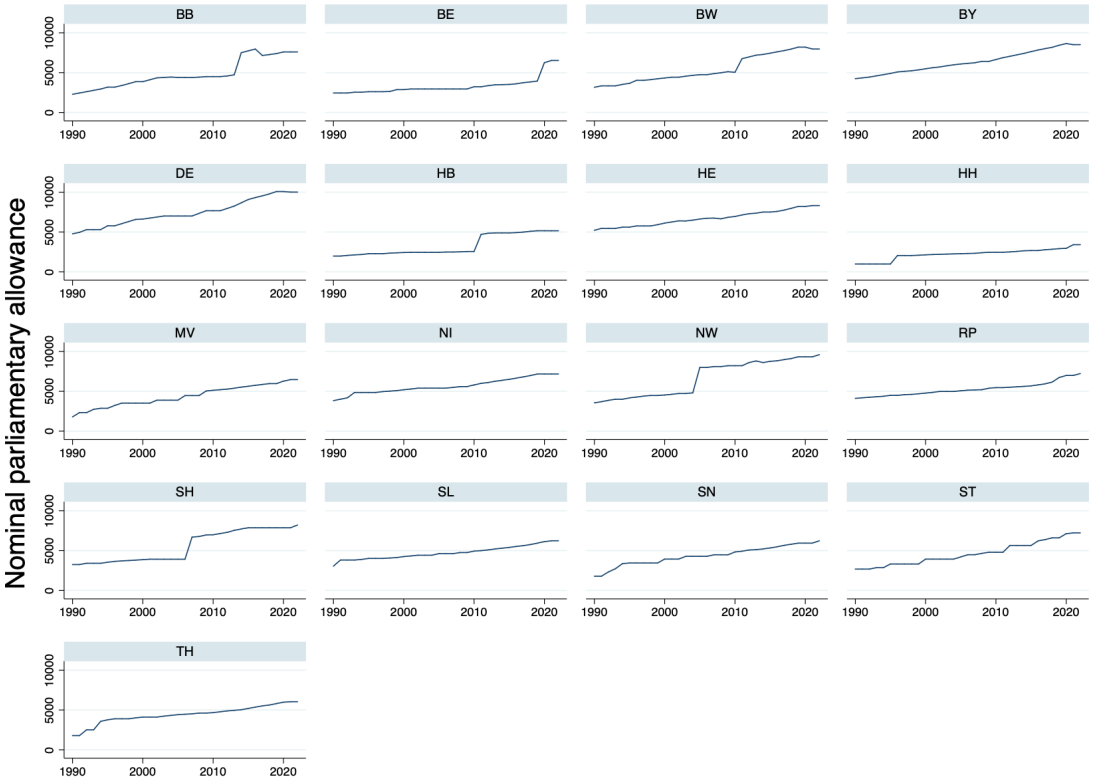
Appendix

1.A General Appendix

Table 1.A.1: Abbreviations

Abbreviation	Parliament (of)
BW	Baden-Württemberg
BY	Bavaria
BE	Berlin
BB	Brandenburg
DE	Bundestag
EU	European Parliament
HB	Bremen
HH	Hamburg
HE	Hesse
MV	Mecklenburg-Vorpommern
NI	Lower Saxony
NW	North Rhine-Westphalia
RP	Rhineland-Palatinate
SL	Saarland
SN	Saxony
ST	Saxony-Anhalt
SH	Schleswig-Holstein
TH	Thuringia
VK	Volkskammer

Figure 1.A.1: Nominal parliamentary allowance over time by parliament



Allowances before 2002 are converted to Euro at a rate of 1.96 DM / Euro. Large increases can be attributed to changes in tax treatment. Note that legislators may receive (tax-free) payments on top of this basic allowance, e.g., a lump sum for costs. Abbreviations according to Table 1.A.1.

Table 1.A.2: Eligibility threshold for standard old age compensation scheme in years

	1990	1991	1992	1993	1994	1995	1996	1997	1998	1999	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014	2015	2016	2017	2018	2019	2020	2021	2022
BW	8	8	8	8	8	8	8	8	8	8	8	8	8	8	8	8	10	10	10	10	10	OP	OP	OP	OP	OP	OP	OP	OP	VW	VW	VW	VW
BY	8	8	8	8	8	8	8	8	8	8	8	8	8	8	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10
BE	7	7	7	7	7	7	7	7	7	9	9	9	9	9	9	9	9	9	9	9	9	9	9	9	9	9	9	9	9	9	9	9	9
BB	8	8	8	8	8	8	8	8	8	8	8	8	8	8	8	8	1	1	1	1	1	1	1	1	VW	VW	VW	VW	VW	VW	VW	VW	VW
HB	8	8	8	8	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	OP	OP	OP	OP	OP	OP	OP	OP	OP	OP	OP	OP
HH	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1
HE	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	8	8	8	8	8	8	8	8	8	8	8	8	8	8	8
MV	8	8	8	8	8	8	8	8	8	8	8	8	8	8	8	8	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1
NI	8	8	8	8	8	8	8	8	8	8	8	8	8	8	8	8	8	8	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1
NW	8	8	8	8	8	8	8	8	8	8	8	8	8	8	8	VW	VW	VW	VW	VW	VW	VW	VW	VW	VW	VW	VW	VW	VW	VW	VW	VW	VW
RP	8	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10
SL	8	8	8	8	8	8	8	8	8	8	8	8	8	8	8	8	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10
SN	8	8	8	8	8	8	8	8	8	8	8	8	8	8	8	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10
ST	6	6	6	6	6	6	6	6	6	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1
SH	8	8	8	8	8	8	8	8	8	8	8	8	8	8	8	8	8	OP	OP	OP	OP	OP	OP	OP	OP	OP	OP	OP	OP	OP	OP	OP	OP
TH	4	4	4	4	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6
DE	8	8	8	8	8	8	8	8	8	8	8	8	8	8	8	8	8	8	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1

OP=one-time payment; VW=pension fund (*Versorungswerk*). Fields colored in blue are extrapolations from later entries due to missing data.

Table 1.A.3: Monthly old age compensation as a share of parliamentary allowance

	1990	1991	1992	1993	1994	1995	1996	1997	1998	1999	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014	2015	2016	2017	2018	2019	2020	2021	2022
BW	35	35	35	35	35	35	30	30	30	30	30	30	30	30	30	30	30	30	30	30	30	EI	EI	EI	EI	EI	EI	EI	EI	VW	VW	VW	VW
BY	35	35	35	35	35	35	35	35	35	35	35	35	35	33.5	33.5	33.5	33.5	33.5	33.5	33.5	33.5	33.5	33.5	33.5	33.5	33.5	33.5	33.5	33.5	33.5	33.5	33.5	33.5
BE	45	45	45	45	45	45	45	45	45	35	35	35	35	35	35	35	35	35	35	35	35	35	35	35	35	35	35	35	35	35	35	35	35
BB	33	33	33	33	33	33	33	33	33	33	33	33	33	33	33	33	3.3	3.3	3.3	3.3	3.3	3.3	3.3	3.3	VW	VW	VW	VW	VW	VW	VW	VW	VW
HB	24	24	24	24	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	EI	EI	EI	EI	EI	EI	EI	EI	EI	EI	EI	EI
HH	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2
HE	29	29	29	29	29	29	29	29	29	29	29	29	29	27.8	27.8	27.8	27.8	27.8	27.8	27.8	27.8	27.8	27.8	27.8	27.8	27.8	27.8	27.8	27.8	27.8	27.8	27.8	27.8
MV	35	35	35	35	35	35	35	35	35	35	35	35	35	35	35	35	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4
NI	38.5	38.5	38.5	38.5	38.5	38.5	38.5	38.5	38.5	38.5	25	25	25	25	25	23.9	23.9	23.9	2.5	2.5	2.5	2.5	2.5	2.5	2.5	2.5	2.5	2.5	2.5	2.5	2.5	2.5	2.5
NW	33	33	33	33	33	33	33	33	33	33	33	33	33	33	33	VW	VW	VW	VW	VW	VW	VW	VW	VW	VW	VW	VW	VW	VW	VW	VW	VW	VW
RP	33	33	33	33	33	33	33	33	33	33	33	33	33	33	33	33	33	33	33	33	33	33	33	33	33	33	33	33	33	33	33	33	33
SL	35	35	35	35	35	35	35	35	35	35	35	35	35	35	35	35	35	35	35	35	35	35	35	35	35	35	35	35	35	35	35	35	35
SN	35	35	35	35	35	35	35	35	35	35	35	35	35	35	35	35	35	35	35	35	30	30	30	30	30	36	36	36	36	36	36	36	36
ST	38.5	38.5	38.5	38.5	38.5	38.5	38.5	38.5	38.5	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3
SH	35	35	35	35	35	35	35	35	35	35	35	35	35	35	35	35	35	EI	EI	EI	EI	EI	EI	EI	EI	EI	EI	EI	EI	EI	EI	EI	EI
TH	29	29	29	29	29	29	29	29	29	26	26	26	26	26	26	26	26	26	26	26	26	26	26	26	26	26	26	26	26	26	26	26	26
DE	32	32	32	32	32	24	24	24	24	24	24	24	24	24	24	24	24	24	2.5	2.5	2.5	2.5	2.5	2.5	2.5	2.5	2.5	2.5	2.5	2.5	2.5	2.5	2.5

OP=one-time payment; VW=pension fund (*Versorungswerk*). Fields colored in blue are extrapolations from later entries due to missing data.

Table 1.A.4: Monthly old age compensation percentage point increase per year

	1990	1991	1992	1993	1994	1995	1996	1997	1998	1999	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014	2015	2016	2017	2018	2019	2020	2021	2022
BW	4	4	3.5	3.5	3.5	3.5	3.5	3.5	3.5	3.5	3.5	3.5	3.5	3.5	3.5	3.5	3.5	3.5	3.5	3.5	3.5	OP	OP	OP	OP	OP	OP	OP	OP	VW	VW	VW	VW
BY	4	4	4	4	4	4	4	4	4	4	4	4	4	3.8	3.8	3.8	3.8	3.8	3.8	3.8	3.8	3.8	3.8	3.8	3.8	3.8	3.8	3.8	3.8	3.8	3.8	3.8	3.8
BE	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3
BB	3.5	3.5	3.5	3.5	3.5	3.5	3.5	3.5	3.5	3.5	3.5	3.5	3.5	3.5	3.5	3.5	3.3	3.3	3.3	3.3	3.3	3.3	3.3	3.3	VW	VW	VW	VW	VW	VW	VW	VW	VW
HB	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	OP	OP	OP	OP	OP	OP	OP	OP	OP	OP	OP	OP
HH	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2
HE	3	3	3	3	3	3	3	3	3	3	3	3	2.75	2.75	2.75	2.75	2.75	2.75	2.75	2.75	2.75	2.75	2.75	2.75	2.75	2.75	2.75	2.75	2.75	2.75	2.75	2.75	2.75
MV	5	5	5	5	5	5	5	5	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4
NI	3.5	3.5	3.5	3.5	3.5	3.5	3.5	3.5	3.5	3.5	3.5	3.5	3.5	3.5	3.5	3.3	3.3	3.3	2.5	2.5	2.5	2.5	2.5	2.5	2.5	2.5	2.5	2.5	2.5	2.5	2.5	2.5	2.5
NW	3.5	3.5	3.5	3.5	3.5	3.5	3.5	3.5	3.5	3.5	3.5	3.5	3.5	3.5	3.5	VW	VW	VW	VW	VW	VW	VW	VW	VW	VW	VW	VW	VW	VW	VW	VW	VW	VW
RP	3.5	3.5	3.5	3.5	3.5	3.5	3.5	3.5	3.5	3.5	3.5	3.5	3.5	3.5	3.5	3.5	3.5	3.5	3.5	3.5	3.5	3.5	3.5	3.5	3.5	3.5	3.5	3.5	3.5	3.5	3.5	3.5	3.5
SL	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	3.5	3.5	3.5	3.5	3.5	3.5	3.5	3.5	3.5	3.5	3.5	3.5	3.5	3.5	3.5	3.5	3.5
SN	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	3.5	3.5	3.5	3.5	3.5	3	3	3	3	3	3.6	3.6	3.6	3.6	3.6	3.6	3.6	3.6
ST	5	5	5	5	5	5	5	5	5	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3
SH	5	5	5	5	5	5	5	5	5	5	5	5	5	4	4	4	4	OP	OP	OP	OP	OP	OP	OP	OP	OP	OP	OP	OP	OP	OP	OP	OP
TH	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3
DE	4	4	4	4	4	3	3	3	3	3	3	3	3	3	3	3	3	2.5	2.5	2.5	2.5	2.5	2.5	2.5	2.5	2.5	2.5	2.5	2.5	2.5	2.5	2.5	2.5

OP=one-time payment; VW=pension fund (*Versorungswerk*). Fields colored in blue are extrapolations from later entries due to missing data.

Table 1.A.5: Statutory retirement age in years

	1990	1991	1992	1993	1994	1995	1996	1997	1998	1999	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014	2015	2016	2017	2018	2019	2020	2021	2022
BW	60	60	60	60	60	60	60	60	60	60	60	60	60	60	60	60	63	63	63	63	63	-	-	-	-	-	-	-	-	67	67	67	67
BY	65	65	65	65	65	65	65	65	65	65	65	65	65	65	65	65	65	65	65	65	65	65	65.1	65.2	65.3	65.3	65.4	65.5	65.6	65.7	65.8	65.8	65.9
BE	63	63	63	63	63	63	63	63	63	63	63	63	63	63	63	63	63	63	63	63	63	63	63	63	63	63	63	63	63	63	63	63	63
BB	65	65	65	65	65	65	65	65	65	65	65	65	65	65	65	65	65	65	65	65	65	65	65.1	65.2	67	67	67	67	67	67	67	67	67
HB	60	60	60	60	63	63	63	63	63	63	63	63	63	63	63	63	63	63	63	63	63	-	-	-	-	-	-	-	-	-	-	-	-
HH	65	65	65	65	65	65	65	65	65	65	65	65	65	65	65	65	65	65	65	65	65	65	65	65	65	65.3	65.4	65.5	65.6	65.7	65.8	65.8	65.9
HE	55	55	55	55	55	55	55	55	55	55	55	55	55	55	55	55	55	55	60	60	60	60	60	60	60	60	60	60	60	60	60	60	60
MV	60	60	60	60	60	60	60	60	60	60	60	60	60	60	60	60	65	65	65	65	65	67	67	67	67	67	67	67	67	67	67	67	67
NI	65	65	65	65	65	65	65	65	65	65	65	65	65	65	65	65	65	65	65	65	65	65	65	65.2	65.3	65.3	65.4	65.5	65.6	65.7	65.8	65.8	65.9
NW	60	60	60	60	60	60	60	60	60	60	60	60	60	60	60	60	65	65	65	65	65	67	67	67	67	67	67	67	67	67	67	67	67
RP	60	60	60	60	60	60	60	60	60	60	60	60	60	60	60	60	60	60	60	60	60	60	60	60	60	60	60	60	60	60	60	60	60
SL	60	60	60	60	60	60	60	60	60	60	60	60	60	60	60	60	65	65	65	65	65	65	65	65	65	65	65	65	65	65	65	65	65
SN	60	60	60	60	60	60	60	60	60	60	60	60	60	60	60	60	65	65	65	65	65	67	67	67	67	67	67	67	67	67	67	67	67
ST	55	55	55	55	55	55	55	55	55	65	65	65	65	65	65	65	65	65	65	65	65	65	65.1	65.2	65.3	65.3	65.4	65.5	65.6	65.7	65.8	65.8	65.9
SH	65	65	65	65	65	65	65	65	65	65	65	65	65	65	65	65	65	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-
TH	55	55	55	55	55	55	55	55	55	60	60	60	60	60	60	60	60	60	60	65	65	65	65.1	65.2	65.3	65.3	65.4	65.5	65.6	65.7	65.8	65.8	65.9
DE	65	65	65	65	65	65	65	65	65	65	65	65	65	65	65	65	65	65	65	65	65	65	65.1	65.2	65.3	65.3	65.4	65.5	65.6	65.7	65.8	65.8	65.9

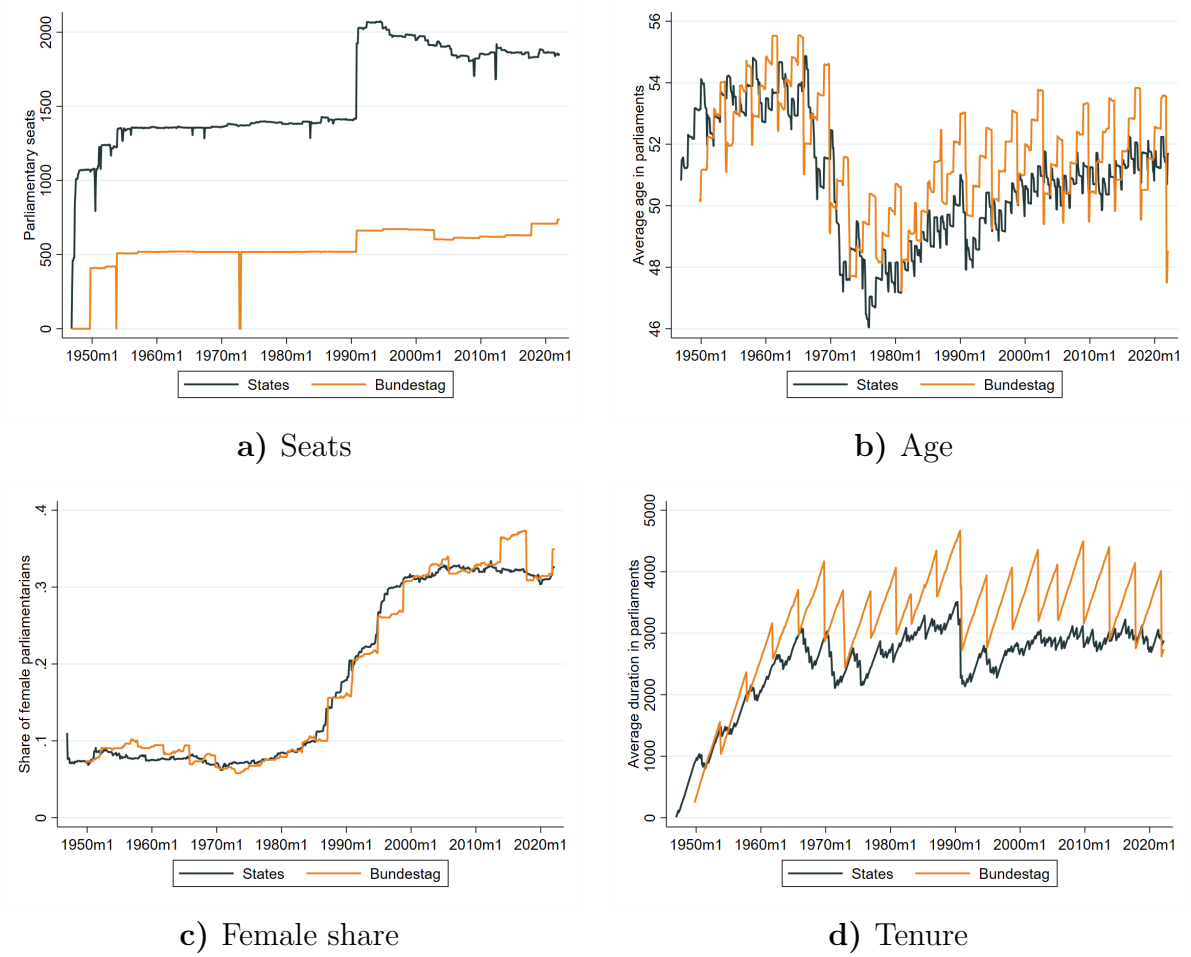
OP=one-time payment; VW=pension fund (*Versorungswerk*). Fields colored in blue are extrapolations from later entries due to missing data. For one-time payments, no limit applies.

Table 1.A.6: Monthly parliamentary allowance in original currency

	1990	1991	1992	1993	1994	1995	1996	1997	1998	1999	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014	2015	2016	2017	2018	2019	2020	2021	2022
BW	6198	6539	6539	6539	6900	7162	7900	7900	8058	8284	8475	8670	4433	4557	4666	4750	4750	4879	4991	5125	5047	6756	6975	7199	7290	7448	7616	7776	7963	8210	8210	7972	7972
BY	8301	8501	8700	8997	9293	9590	9965	10115	10247	10463	10746	10994	5718	5861	5990	6092	6166	6247	6416	6416	6648	6881	7060	7244	7426	7642	7849	8022	8183	8445	8657	8519	8886
BE	4790	4790	4790	4980	4980	5100	5100	5100	5162	5610	5610	5770	2951	2951	2951	2951	2951	2951	2951	2951	3233	3233	3369	3477	3498	3526	3601	3742	3840	3944	6250	6532	6657
BB	4462.6	4786	5110	5434	5757	6230	6230	6627	7086	7576	7576	8037	4351	4399	4448	4399	4399	4390	4438	4504	4504	4504	4585	4732	7510	7743	7967	7159	7274	7397	7605	7605	7605
HB	3872	3872	4037	4158	4241	4457	4457	4457	4591	4660	4735	4783	2446	2446	2446	2446	2485	2485	2510	2550	2550	4700	4846	4884	4884	4884	4919	4988	5087	5154	5154	5150	5150
HH	1920	1920	1920	1920	1920	1920	4000	4000	4000	4080	4160	4240	2196	2224	2252	2280	2303	2326	2396	2456	2456	2456	2500	2566	2641	2668	2668	2777	2833	2907	2951	3401	3555
HE	10200	10660	10660	10660	10970	10970	11266	11266	11266	11582	11969	12238	6401	6384	6490	6628	6725	6745	6657	6843	6946	7141	7294	7366	7508	7508	7583	7750	7975	8206	8206	8319	8297
MV	3500	4550	4550	5350	5620	5620	6310	6880	6880	6880	6880	6880	3890	3890	3890	3890	4465	4465	4465	5040	5136	5198	5276	5393	5526	5636	5749	5864	5967	5967	6278	6466	6466
NI	7500	7840	8200	9500	9500	9500	9500	9700	9825	9970	10160	10340	5403	5403	5403	5403	5403	5485	5595	5595	5800	6000	6108	6261	6386	6501	6655	6810	6973	7176	7176	7176	7176
NW	6958	7230	7570	7833	7833	8165	8370	8605	8752	8752	8875	9053	4722	4722	4807	8000	8000	8112	8112	8216	8216	8216	8612	8809	8612	8765	8837	8981	9122	9330	9330	9330	9603
RP	8061.3	8189	8317	8445	8573	8779	8779	8955	9035	9171	9345	9523	4981	4981	4981	5070	5147	5172	5198	5395	5460	5460	5515	5570	5625	5682	5812	5939	6163	6736	6993	6993	7228
SL	5980	7470	7470	7470	7625	7869	7869	7869	7971	8091	8326	8476	4429	4429	4429	4624	4624	4624	4758	4758	4942	5001	5096	5213	5307	5408	5522	5632	5759	5943	6133	6238	6238
SN	3500	3500	4550	5350	6592	6753	6753	6753	6753	6753	7712	7712	3942	4284	4284	4284	4284	4481	4481	4481	4835	4932	5080	5130	5213	5338	5487	5668	5804	5944	5944	5944	6237
ST	5252	5252	5252	5600	5600	6500	6500	6500	6500	6500	7700	7700	3937	3937	3937	4212	4487	4487	4662	4797	4797	4797	5655	5655	5655	5655	6227	6389	6606	6606	7131	7231	7231
SH	6360	6360	6680	6680	6680	6930	7150	7260	7350	7460	7570	7680	3926	3927	3927	3927	3927	6700	6774	6971	6990	7151	7294	7550	7723	7870	7870	7870	7870	7870	7870	7870	8220
TH	3500	3500	4900	4900	6971	7371	7615	7615	7615	7851	8031	8031	4107	4226	4319	4414	4463	4515	4610	4610	4666	4773	4883	4956	5030	5196	5357	5513	5623	5803	5977	6037	6037
DE	9333	9696	10366	10366	10366	11300	11300	11825	12350	12875	12953	13200	6878	7009	7009	7009	7009	7009	7339	7668	7668	7668	7960	8252	8667	9082	9327	9542	9780	10083	10083	10013	10013

Amounts are rounded to integers. Before 2002, amounts are denoted in DM, otherwise Euro. Fields colored in blue are extrapolations from later entries due to missing data.

Figure 1.A.2: Development over time



a) Seats **b)** Age

c) Female share **d)** Tenure

Notes: This graph depicts average values of parliament characteristics over time for the national Bundestag and the state parliaments, respectively.

Table 1.A.7: Pension generosity proxy in Euros

	1990	1991	1992	1993	1994	1995	1996	1997	1998	1999	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014	2015	2016	2017	2018	2019	2020	2021	2022
BW	3407	3594	3511	3511	3705	3845	3736	3736	3811	3918	4008	4101	4101	4215	4316	4394	3135	3220	3294	3383	3331	1585	1589	1587	1628	1638	1679	1720	1751	1805	1859	1913	1952
BY	3650	3738	3826	3956	4087	4217	4382	4448	4506	4601	4725	4834	4917	4824	4013	4082	4131	4185	4299	4299	4454	4610	4710	4813	4913	5035	5149	5240	5323	5470	5583	5470	5681
BE	2910	2910	2910	3025	3025	3098	3098	3098	3136	2398	2398	2466	2467	2467	2467	2467	2467	2467	2467	2467	2703	2703	2816	2907	2924	2948	3010	3128	3210	3297	5225	5461	5565
BB	1825	1958	2090	2223	2355	2548	2548	2711	2899	3099	3099	3288	3481	3519	3558	3519	2903	2898	2929	2972	2972	2972	3013	3097	1614	1664	1712	1748	1776	1806	1857	1920	1930
HB	1485	1485	1548	1595	1431	1504	1504	1504	1549	1573	1598	1614	1614	1614	1614	1614	1640	1640	1657	1683	1683	750	773	779	779	779	785	796	812	823	844	843	849
HH	393	393	393	393	393	393	818	818	818	834	851	867	878	890	901	912	921	930	958	982	982	982	1000	1026	1056	1049	1045	1083	1100	1124	1136	1304	1357
HE	6415	6704	6704	6704	6899	6899	7085	7085	7085	7284	7528	7697	7873	7421	7545	7705	7818	7841	5534	5688	5774	5936	6063	6123	6241	6241	6303	6442	6629	6821	6821	6915	6897
MV	2013	2617	2617	3078	3233	3233	3630	3958	3782	3782	3782	3782	4182	4182	4182	4182	3572	3572	3572	4032	4109	3742	3799	3883	3979	4058	4139	4222	4296	4296	4520	4656	4656
NI	3490	3648	3815	4420	4420	4420	4420	4513	4572	4639	3325	3384	3458	3458	3458	3308	3308	3358	2798	2798	2900	3000	3054	3104	3153	3196	3258	3320	3385	3468	3453	3438	3423
NW	3558	3697	3871	4005	4005	4175	4280	4400	4475	4475	4538	4629	4722	4722	4807	1263	1263	1281	1281	1297	1297	1297	2114	2114	2114	2114	2114	2114	2114	2290	2290	2290	2453
RP	4122	3455	3508	3562	3616	3703	3703	3777	3811	3869	3942	4017	4109	4109	4109	4183	4246	4267	4288	4451	4504	4504	4550	4595	4641	4687	4795	4899	5084	5557	5769	5769	5963
SL	3287	4106	4106	4106	4191	4325	4325	4325	4381	4447	4577	4659	4761	4761	4761	4971	3237	3237	3331	3331	3459	3501	3567	3649	3715	3786	3865	3942	4031	4160	4293	4367	4367
SN	1924	1924	2501	2941	3623	3712	3712	3712	3712	3712	4239	4239	4238	4605	4605	2999	2999	3137	3137	3137	2611	2663	2743	2770	2815	3459	3556	3673	3761	3851	3851	3851	4042
ST	4713	4713	4713	5025	5025	5833	5833	5833	5833	1994	2362	2362	2362	2362	2362	2527	2692	2692	2797	2878	2878	2878	3379	3365	3351	3336	3658	3737	3848	3831	4118	4158	4140
SH	2927	2927	3074	3074	3074	3189	3290	3341	3382	3433	3484	3534	3533	3377	3377	3377	3377	1500	1500	1500	1500	1500	1500	1500	1500	1500	1500	1829	1829	1829	1829	1829	1829
TH	2523	2523	3533	3533	4384	4636	4789	4789	4789	3813	3901	3901	3902	4015	4103	4194	4240	4290	4380	3504	3546	3627	3695	3735	3775	3883	3987	4085	4149	4263	4372	4397	4378
DE	3818	3966	4240	4240	4240	3467	3467	3628	3789	3950	3974	4050	4127	4205	4205	4205	4205	4065	3670	3834	3834	3834	3963	4092	4279	4465	4566	4652	4748	4874	4853	4798	4777

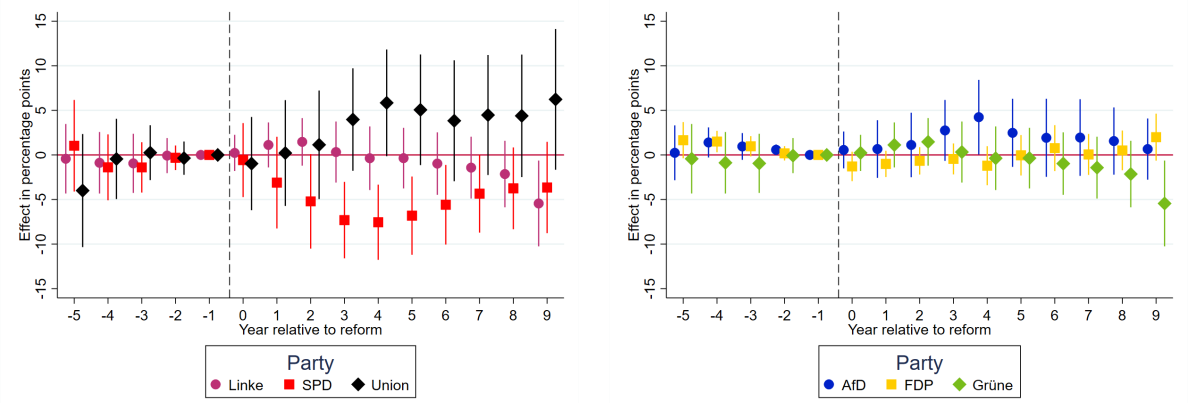
Hypothetical pension in Euro a parliamentarian who served 10 years and retired at the statutory age would receive per month served, ignoring tax and discounting.

Table 1.A.8: Reforms of interest

	1990	1991	1992	1993	1994	1995	1996	1997	1998	1999	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014	2015	2016	2017	2018	2019	2020	2021	2022
TH	0	0	0	0	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1
HH	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
HB	1	1	1	1	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
ST	1	1	1	1	1	1	1	1	1	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
NW	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
BB	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
MV	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
SH	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
DE	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
NI	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
BW	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	0	0	0	0	0	0	0	0	0	0	0	0
SN	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1
BE	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1
BY	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1
HE	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1
RP	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1
SL	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1

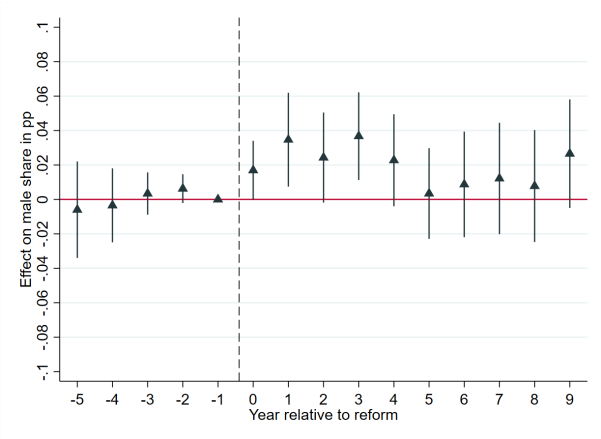
1 indicates a standard old age compensation system, where the eligibility threshold is set such that it cannot be reached after one term in parliament. 0 indicates those periods in which a single term is enough to get a pension. Lines after 1999 and before 2017 indicate the time window.

Figure 1.A.3: Party composition



Notes: This graph depicts two event studies, based on Equation (1.1). The time on the horizontal axis is years relative to the year when pension eligibility reform was implemented. Confidence intervals are computed at the 95% level.

Figure 1.A.4: Gender: Male share without control variables



Notes: This graph depicts an event study, based on Equation (1.1) without control variables X'_{ipt}, unlike in Figure 1.6b. The time on the horizontal axis is years relative to the year when pension eligibility reform was implemented. Confidence intervals are computed at the 95% level.

Table 1.A.9: Parliament-level regression estimates: Parties

	(1) AfD		(2) FDP		(3) Grüne		(4) Linke		(5) SPD		(6) Union	
Lead 5	0.13	(0.47)	0.24	(1.55)	1.65	(1.02)	-0.43	(1.96)	1.03	(2.58)	-4.00	(3.19)
Lead 4	-0.02	(0.37)	1.40	(0.85)	1.50*	(0.60)	-0.88	(1.73)	-1.39	(1.86)	-0.45	(2.26)
Lead 3	0.08	(0.36)	0.95	(0.76)	0.97	(0.57)	-0.95	(1.67)	-1.40	(1.42)	0.25	(1.55)
Lead 2	-0.02	(0.08)	0.58	(0.30)	0.18	(0.41)	-0.08	(0.99)	-0.32	(0.69)	-0.36	(0.94)
Lead 1	0.00	(.)	0.00	(.)	0.00	(.)	0.00	(.)	0.00	(.)	0.00	(.)
Lag 0	-0.41	(0.48)	0.56	(1.04)	-1.29	(0.83)	0.23	(1.02)	-0.58	(2.09)	-0.97	(2.63)
Lag 1	-0.40	(0.52)	0.66	(1.63)	-1.01	(0.74)	1.12	(1.27)	-3.11	(2.59)	0.21	(2.98)
Lag 2	-0.38	(0.54)	1.12	(1.82)	-0.67	(0.77)	1.47	(1.34)	-5.21	(2.67)	1.14	(3.06)
Lag 3	-0.49	(0.64)	2.75	(1.72)	-0.45	(0.87)	0.32	(1.72)	-7.31**	(2.16)	3.97	(2.89)
Lag 4	-0.62	(0.70)	4.23*	(2.10)	-1.21	(1.10)	-0.37	(1.79)	-7.55***	(2.12)	5.84	(3.01)
Lag 5	0.14	(0.73)	2.48	(1.92)	-0.04	(1.14)	-0.36	(1.71)	-6.81**	(2.21)	5.06	(3.12)
Lag 6	0.42	(1.01)	1.92	(2.20)	0.76	(1.30)	-0.97	(1.76)	-5.59*	(2.24)	3.83	(3.41)
Lag 7	-0.63	(0.85)	1.95	(2.16)	0.05	(1.15)	-1.43	(1.74)	-4.34	(2.20)	4.48	(3.39)
Lag 8	-0.98	(1.31)	1.56	(1.90)	0.51	(1.12)	-2.14	(1.87)	-3.75	(2.31)	4.39	(3.46)
Lag 9	-0.07	(1.56)	0.66	(1.73)	1.99	(1.32)	-5.44*	(2.42)	-3.65	(2.58)	6.23	(3.97)
Control variables	Yes		Yes		Yes		Yes		Yes		Yes	
Parl FE	Yes		Yes		Yes		Yes		Yes		Yes	
Year FE	Yes		Yes		Yes		Yes		Yes		Yes	
Adj. R-Squared	0.46		0.44		0.82		0.91		0.78		0.70	
Obs.	323		323		323		323		323		323	

Regression results based on Equation (1.1). Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 1.A.10: Balance table on December 31, 1998

	Control			Treatment			
	Obs.	Mean	SD	Obs.	Mean	SD	Diff
Tenure in days	673	5105.30	2588.26	1136	5049.58	2405.07	-55.727
Tenure in terms	673	3.40	1.71	1136	3.55	1.80	0.151
Age	673	49.38	8.69	1136	49.34	8.48	-0.045
Male	673	0.65	0.48	1136	0.64	0.48	-0.012
Academic title							
Below PhD	636	0.90	0.30	1135	0.84	0.37	-0.057
PhD	636	0.09	0.28	1135	0.14	0.35	0.050
Prof	636	0.01	0.12	1135	0.02	0.14	0.007
Parties							
AfD	672	0.00	0.00	1136	0.00	0.00	0.000
FDP	672	0.02	0.14	1136	0.04	0.20	0.020
Grüne	672	0.08	0.28	1136	0.07	0.26	-0.009
Independent	672	0.00	0.05	1136	0.01	0.12	0.011
Linke/PDS	672	0.09	0.29	1136	0.05	0.22	-0.038
Other	672	0.02	0.13	1136	0.05	0.23	0.037*
SPD	672	0.30	0.46	1136	0.43	0.50	0.133**
Union	672	0.48	0.50	1136	0.33	0.47	-0.154***
Indicators							
1(Improper term)	673	0.20	0.40	1136	0.30	0.46	0.103
1(Death in office)	673	0.00	0.05	1136	0.01	0.08	0.003
1(Change of parliament)	673	0.08	0.28	1136	0.13	0.33	0.043
1(former GDR excl. Berlin)	673	0.31	0.46	1136	0.24	0.43	-0.069
1(National parliament or higher)	673	0.00	0.00	1136	0.39	0.49	0.394

Table 1.A.11: Parliament-level regression estimates: Mixed treatment

	(1)		(2)		(3)		(4)		(5)		(6)		(7)	
	Allowance		Contribution proxy		NPV proxy		Term length		Pension age		Threshold		Premature dissolution	
Lead 5	245	(288)	-291	(311)	-199	(203)	0.1	(0.1)	1.1	(0.8)	0.09	(0.3)	-0.09	(0.2)
Lead 4	177	(220)	-260	(164)	-193	(103)	0.09	(0.06)	0.9*	(0.4)	0.3*	(0.2)	-0.02	(0.2)
Lead 3	126	(224)	-181	(122)	-134	(73)	0.06	(0.05)	0.7	(0.4)	0.3*	(0.1)	-0.2	(0.1)
Lead 2	99*	(43)	-82	(56)	-57	(42)	0.02	(0.02)	0.3*	(0.2)	0.1	(0.07)	-0.005	(0.03)
Lead 1	0	(.)	0	(.)	0	(.)	0	(.)	0	(.)	0	(.)	0	(.)
Lag 0	1079**	(405)	-1176**	(367)	-772**	(235)	0.07	(0.1)	1.3	(0.8)	-7.7***	(0.2)	-0.02	(0.08)
Lag 1	1151**	(426)	-1097**	(367)	-712**	(233)	0.05	(0.1)	1.1	(0.8)	-7.7***	(0.2)	-0.01	(0.09)
Lag 2	1193**	(398)	-1076**	(378)	-693**	(246)	0.06	(0.1)	1.0	(0.8)	-7.8***	(0.2)	-0.06	(0.2)
Lag 3	1199**	(388)	-989*	(401)	-628*	(264)	0.06	(0.1)	0.7	(0.8)	-7.8***	(0.2)	-0.07	(0.1)
Lag 4	1193**	(394)	-986*	(407)	-628*	(268)	0.05	(0.1)	0.5	(0.8)	-7.8***	(0.2)	-0.03	(0.1)
Lag 5	1240**	(373)	-965*	(380)	-605*	(244)	0.09	(0.1)	0.8	(0.9)	-7.7***	(0.3)	0.1	(0.2)
Lag 6	1244**	(383)	-919*	(378)	-577*	(242)	0.10	(0.1)	0.9	(0.9)	-7.7***	(0.3)	0.1	(0.2)
Lag 7	1343***	(387)	-759*	(367)	-434	(231)	0.2	(0.1)	0.8	(0.9)	-7.8***	(0.3)	0.05	(0.2)
Lag 8	1650***	(388)	-913*	(389)	-511*	(242)	0.2	(0.1)	1.0	(0.9)	-7.9***	(0.3)	0.05	(0.2)
Lag 9	1764***	(382)	-901*	(410)	-476	(259)	0.3	(0.1)	0.5	(1.0)	-8.1***	(0.3)	-0.005	(0.1)
Parl FE	Yes		Yes		Yes		Yes		Yes		Yes		Yes	
Year FE	Yes		Yes		Yes		Yes		Yes		Yes		Yes	
Adj. R^2	0.92		0.87		0.88		0.75		0.99		0.98		0.11	
Obs.	323		323		323		323		323		323		323	

Regression results based on Equation (1.1). Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 1.A.12: Effect on legislator characteristics

	(1)		(2)		(3)		(4)		(5)	
	Entry age		Exit age		1(Male)		1(PhD)		1(Professor)	
Lead 5	-0.400	(0.394)	-0.431	(0.435)	-0.006	(0.014)	-0.012	(0.011)	0.002	(0.004)
Lead 4	-0.166	(0.309)	-0.253	(0.255)	-0.003	(0.011)	-0.002	(0.007)	-0.000	(0.004)
Lead 3	-0.115	(0.203)	-0.102	(0.210)	0.003	(0.006)	-0.007	(0.005)	0.001	(0.003)
Lead 2	0.032	(0.138)	0.073	(0.130)	0.006	(0.004)	-0.003	(0.003)	-0.000	(0.001)
Lead 1	0.000	(.)	0.000	(.)	0.000	(.)	0.000	(.)	0.000	(.)
Lag 0	-0.384	(0.263)	-0.351	(0.322)	0.017	(0.009)	-0.009	(0.007)	0.002	(0.002)
Lag 1	-0.521	(0.358)	-0.272	(0.380)	0.035*	(0.014)	-0.018*	(0.008)	0.004	(0.004)
Lag 2	-0.538	(0.387)	-0.423	(0.419)	0.024	(0.013)	-0.019*	(0.008)	0.005	(0.004)
Lag 3	-0.501	(0.347)	-0.295	(0.398)	0.037**	(0.013)	-0.028**	(0.009)	0.005	(0.004)
Lag 4	-0.432	(0.356)	0.205	(0.422)	0.023	(0.013)	-0.028**	(0.009)	0.005	(0.004)
Lag 5	-0.168	(0.333)	0.215	(0.341)	0.003	(0.013)	-0.021*	(0.009)	0.006	(0.004)
Lag 6	-0.264	(0.366)	-0.066	(0.343)	0.009	(0.015)	-0.018*	(0.009)	0.007	(0.005)
Lag 7	-0.325	(0.368)	-0.185	(0.367)	0.012	(0.016)	-0.021*	(0.010)	0.005	(0.005)
Lag 8	-0.354	(0.374)	-0.114	(0.453)	0.008	(0.016)	-0.023*	(0.011)	0.004	(0.005)
Lag 9	-0.773*	(0.365)	-0.182	(0.427)	0.027	(0.016)	-0.034**	(0.011)	0.006	(0.006)
Control variables	Yes		Yes		No		Yes		Yes	
Parl FE	Yes		Yes		Yes		Yes		Yes	
Year FE	Yes		Yes		Yes		Yes		Yes	
Adj. R-Squared	0.02		0.04		0.01		0.04		0.04	
Obs.	42,930		42,930		42,945		42,707		42,707	

Regression results are based on Equation (1.1). Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 1.A.13: Parliamentary tenure change

	ln(Tenure)		Tenure in days		Tenure in terms	
	(1)	(2)	(3)	(4)	(5)	(6)
	Active	Starting	Active	Starting	Active	Starting
Lead 5	0.014	-0.022	-18.249	-47.917	-0.019	0.042
	(0.036)	(0.069)	(120.970)	(187.095)	(0.074)	(0.120)
Lead 4	0.007	-0.046	-24.593	-150.603	-0.006	-0.034
	(0.024)	(0.042)	(86.215)	(114.174)	(0.053)	(0.083)
Lead 3	0.011	-0.066*	-1.825	-194.670*	-0.010	-0.100*
	(0.017)	(0.030)	(60.619)	(78.293)	(0.034)	(0.050)
Lead 2	0.017	-0.011	11.227	-58.689	-0.012	-0.030
	(0.013)	(0.013)	(41.781)	(37.802)	(0.027)	(0.025)
Lead 1	0.000	0.000	0.000	0.000	0.000	0.000
	(.)	(.)	(.)	(.)	(.)	(.)
Lag 0	0.021	-0.015	22.876	-173.582	0.013	-0.163
	(0.030)	(0.053)	(95.041)	(163.842)	(0.053)	(0.122)
Lag 1	0.048	-0.001	79.412	-195.789	0.045	-0.169
	(0.036)	(0.065)	(124.592)	(182.848)	(0.075)	(0.130)
Lag 2	0.018	-0.027	36.131	-233.777	0.036	-0.202
	(0.039)	(0.065)	(133.016)	(184.140)	(0.078)	(0.131)
Lag 3	0.009	0.001	44.297	-60.047	0.045	-0.078
	(0.035)	(0.062)	(118.407)	(158.383)	(0.070)	(0.113)
Lag 4	0.042	0.030	171.730	69.430	0.122	0.004
	(0.034)	(0.068)	(120.418)	(171.724)	(0.072)	(0.122)
Lag 5	0.030	-0.041	85.567	-56.727	0.051	-0.079
	(0.032)	(0.061)	(104.978)	(152.846)	(0.064)	(0.102)
Lag 6	0.013	-0.050	17.903	-82.230	0.056	-0.102
	(0.032)	(0.061)	(116.810)	(152.700)	(0.072)	(0.107)
Lag 7	-0.003	-0.068	-1.265	-81.555	0.040	-0.137
	(0.029)	(0.053)	(105.852)	(142.998)	(0.060)	(0.107)
Lag 8	0.001	-0.046	27.775	-21.875	0.034	-0.125
	(0.037)	(0.062)	(128.006)	(170.281)	(0.069)	(0.122)
Lag 9	0.044	-0.070	176.677	12.539	0.116	-0.117
	(0.041)	(0.061)	(142.087)	(164.680)	(0.079)	(0.123)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
Parl FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R-Squared	0.06	0.07	0.06	0.15	0.08	0.15
Obs.	42,945	16,654	42,945	16,654	42,945	16,654

Regression results are based on Equation (1.1). Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 1.A.14: Parliamentary tenure change after other reforms

	Allowance reform				Pension size reform			
	(1) Active		(2) Starting		(3) Active		(4) Starting	
Lead 5	-120	(101)	40	(131)	209*	(85)	164	(150)
Lead 4	-58	(92)	-57	(112)	116	(66)	22	(111)
Lead 3	-158	(94)	-186	(111)	94*	(47)	-26	(84)
Lead 2	-38	(71)	-72	(89)	48	(34)	8	(56)
Lead 1	0	(.)	0	(.)	0	(.)	0	(.)
Lag 0	-15	(93)	-56	(134)	-40	(40)	-125	(77)
Lag 1	-34	(96)	-120	(131)	-22	(50)	-143	(89)
Lag 2	-34	(84)	39	(112)	-57	(61)	-48	(97)
Lag 3	32	(88)	100	(123)	-6	(70)	81	(114)
Lag 4	-19	(105)	-16	(144)	29	(72)	144	(124)
Lag 5	-85	(107)	10	(174)	1	(74)	147	(127)
Lag 6	-24	(106)	-9	(157)	-13	(76)	130	(132)
Lag 7	3	(116)	43	(164)	-1	(77)	167	(143)
Lag 8	75	(129)	111	(182)	75	(90)	209	(149)
Lag 9	-28	(89)	143	(121)	167	(102)	98	(136)
Control variables	Yes		Yes		Yes		Yes	
Parl FE	Yes		Yes		Yes		Yes	
Year FE	Yes		Yes		Yes		Yes	
Adj. R-Squared	0.06		0.16		0.06		0.16	
Obs.	40,841		15,761		42,850		16,591	

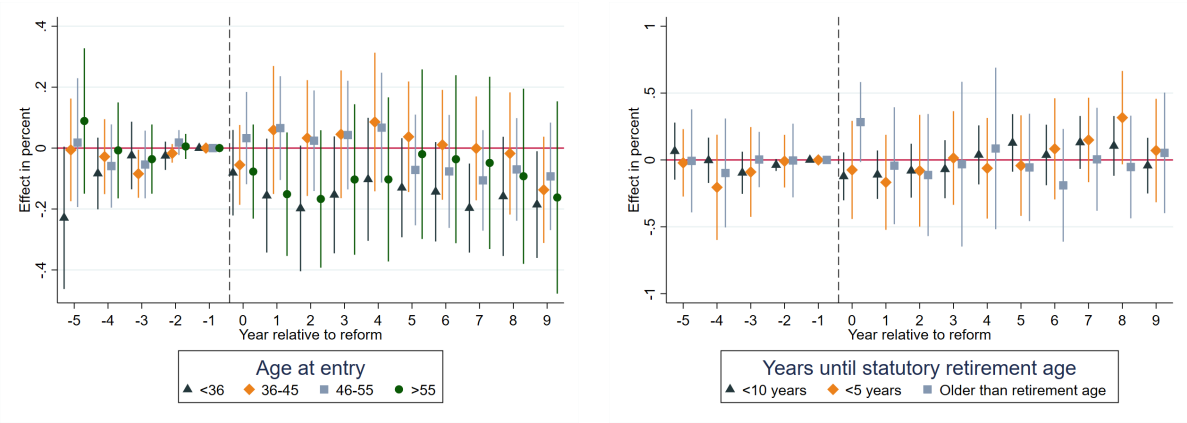
Regression results are based on Equation (1.1). Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 1.A.15: Share of legislators running for re-election

	Long-established		Newly elected		All candidacies	
	(1) 1(Running for re-election)		(2) 1(Running for re-election)		(3) Total number	
Lead 5	0.027	(0.017)	0.005	(0.010)	-8.173	(8.268)
Lead 4	-0.005	(0.012)	0.010	(0.006)	-4.950	(5.900)
Lead 3	-0.010	(0.007)	0.006	(0.003)	-2.529	(2.895)
Lead 2	0.011**	(0.004)	0.004	(0.002)	-0.142	(1.582)
Lead 1	0.000	(.)	0.000	(.)	0.000	(.)
Lag 0	0.015	(0.008)	-0.001	(0.005)	0.285	(3.837)
Lag 1	0.014	(0.011)	0.002	(0.007)	-0.308	(7.129)
Lag 2	0.029*	(0.013)	-0.002	(0.007)	-0.412	(7.911)
Lag 3	0.024	(0.014)	-0.005	(0.009)	0.464	(8.859)
Lag 4	0.012	(0.015)	-0.002	(0.009)	0.558	(9.979)
Lag 5	0.002	(0.015)	0.000	(0.010)	0.858	(10.845)
Lag 6	0.010	(0.016)	-0.004	(0.010)	-0.799	(11.833)
Lag 7	0.015	(0.017)	-0.004	(0.010)	0.143	(12.385)
Lag 8	0.018	(0.018)	-0.005	(0.010)	1.137	(13.684)
Lag 9	0.021	(0.021)	0.011	(0.012)	-4.410	(16.017)
Control variables	Y		Y		Y	
Parl FE	Y		Y		Y	
Year FE	Y		Y		Y	
Adj. R-Squared	0.05		0.02		0.09	
Obs.	121,461		166,177		298,098	

Regression results are based on Equation (1.1). Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

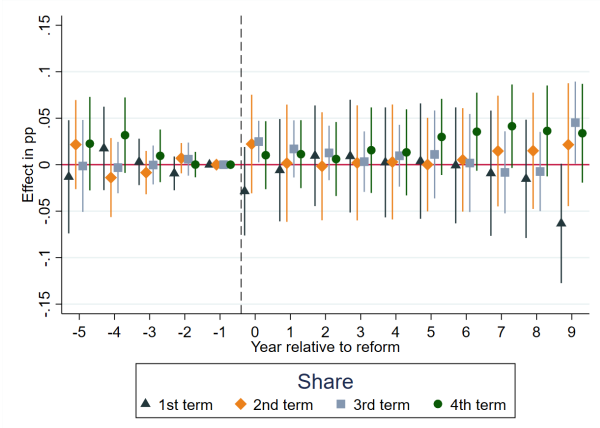
Figure 1.A.5: Heterogeneity



a) Age **b)** Period until statutory retirement age

Notes: This graph depicts two event studies, based on Equation (1.1). The time on the horizontal axis is years relative to the year when pension eligibility reform was implemented. Confidence intervals are computed at the 95% level.

Figure 1.A.6: Share in respective term



Notes: This graph depicts two event studies, based on Equation (1.1). The time on the horizontal axis is years relative to the year when pension eligibility reform was implemented. Confidence intervals are computed at the 95% level.

Table 1.A.16: Parliamentary tenure by (relative) age

	Age groups				Years until retirement		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	<36	36-45	46-55	>55	<10yrs	<5yrs	<0yrs
Lead 5	-0.229	-0.006	0.018	0.089	0.066	-0.022	-0.007
	(0.117)	(0.085)	(0.106)	(0.120)	(0.107)	(0.127)	(0.193)
Lead 4	-0.083	-0.028	-0.059	-0.008	-0.003	-0.205	-0.097
	(0.059)	(0.062)	(0.069)	(0.079)	(0.085)	(0.198)	(0.204)
Lead 3	-0.024	-0.084*	-0.054	-0.036	-0.096	-0.090	0.003
	(0.056)	(0.040)	(0.056)	(0.057)	(0.079)	(0.168)	(0.103)
Lead 2	-0.025	-0.017	0.018	0.005	-0.038	-0.009	-0.004
	(0.023)	(0.015)	(0.021)	(0.021)	(0.022)	(0.099)	(0.138)
Lead 1	0.000	0.000	0.000	0.000	0.000	0.000	0.000
	(.)	(.)	(.)	(.)	(.)	(.)	(.)
Lag 0	-0.081	-0.055	0.033	-0.077	-0.123	-0.075	0.283
	(0.071)	(0.066)	(0.076)	(0.078)	(0.090)	(0.184)	(0.150)
Lag 1	-0.156	0.059	0.066	-0.151	-0.110	-0.167	-0.043
	(0.094)	(0.106)	(0.086)	(0.102)	(0.091)	(0.178)	(0.219)
Lag 2	-0.198	0.033	0.024	-0.167	-0.080	-0.081	-0.113
	(0.104)	(0.096)	(0.083)	(0.113)	(0.101)	(0.210)	(0.228)
Lag 3	-0.154	0.045	0.043	-0.103	-0.069	0.014	-0.032
	(0.096)	(0.105)	(0.090)	(0.124)	(0.109)	(0.176)	(0.308)
Lag 4	-0.103	0.086	0.067	-0.103	0.038	-0.062	0.086
	(0.101)	(0.114)	(0.091)	(0.135)	(0.111)	(0.189)	(0.302)
Lag 5	-0.130	0.037	-0.072	-0.020	0.127	-0.042	-0.056
	(0.082)	(0.091)	(0.091)	(0.140)	(0.108)	(0.189)	(0.201)
Lag 6	-0.143	0.011	-0.077	-0.036	0.037	0.083	-0.190
	(0.082)	(0.091)	(0.093)	(0.139)	(0.114)	(0.190)	(0.211)
Lag 7	-0.197**	-0.001	-0.106	-0.049	0.131	0.149	0.005
	(0.073)	(0.086)	(0.083)	(0.142)	(0.099)	(0.158)	(0.193)
Lag 8	-0.158	-0.018	-0.070	-0.093	0.105	0.316	-0.053
	(0.098)	(0.101)	(0.085)	(0.145)	(0.112)	(0.175)	(0.192)
Lag 9	-0.185*	-0.137	-0.093	-0.162	-0.043	0.070	0.053
	(0.088)	(0.088)	(0.089)	(0.159)	(0.105)	(0.194)	(0.225)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Parl FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R-Squared	0.16	0.10	0.07	0.05	0.07	0.06	0.08
Obs.	3,581	5,024	5,851	2,198	2,931	884	540

Regression results are based on Equation (1.1).
Standard errors in parentheses * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 1.A.17: Composition of parliaments: Share in respective legislative term

	(1) Term 1		(2) Term 2		(3) Term 3		(4) Term 4	
Lead 5	-0.010	(0.031)	0.021	(0.024)	-0.003	(0.025)	0.022	(0.025)
Lead 4	0.019	(0.023)	-0.014	(0.021)	-0.004	(0.014)	0.032	(0.020)
Lead 3	0.004	(0.013)	-0.009	(0.012)	-0.001	(0.011)	0.010	(0.014)
Lead 2	-0.009	(0.009)	0.007	(0.008)	0.006	(0.009)	0.000	(0.007)
Lead 1	0.000	(.)	0.000	(.)	0.000	(.)	0.000	(.)
Lag 0	-0.028	(0.024)	0.022	(0.027)	0.024*	(0.011)	0.011	(0.018)
Lag 1	-0.006	(0.028)	0.002	(0.032)	0.017	(0.016)	0.012	(0.018)
Lag 2	0.009	(0.027)	-0.001	(0.029)	0.012	(0.015)	0.006	(0.020)
Lag 3	0.009	(0.031)	0.002	(0.031)	0.003	(0.017)	0.016	(0.023)
Lag 4	0.002	(0.030)	0.003	(0.031)	0.009	(0.017)	0.014	(0.023)
Lag 5	0.003	(0.031)	0.002	(0.025)	0.012	(0.024)	0.029	(0.020)
Lag 6	-0.001	(0.031)	0.003	(0.028)	0.005	(0.027)	0.035	(0.021)
Lag 7	-0.009	(0.034)	0.013	(0.030)	-0.007	(0.023)	0.041	(0.022)
Lag 8	-0.015	(0.032)	0.014	(0.031)	-0.004	(0.022)	0.033	(0.025)
Lag 9	-0.065	(0.035)	0.024	(0.035)	0.040	(0.024)	0.038	(0.028)
Parl FE	Yes		Yes		Yes		Yes	
Year FE	Yes		Yes		Yes		Yes	
Mean of dep. variable	0.38		0.27		0.19		0.09	
Adj. R-Squared	0.01		0.01		0.01		0.02	
Obs.	42,945		42,945		42,945		42,945	

Regression results are based on equation (1.1). Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 1.A.18: Event study estimates: Normalized place on the party list

	(1) Within		(2) Beyond		(3) Interaction		(4) All	
Lead 3	-0.010	(0.017)	-0.005	(0.010)	-0.011	(0.016)	-0.010	(0.007)
Lead 2	-0.001	(0.009)	0.005	(0.006)	-0.011	(0.010)	-0.004*	(0.002)
Lead 1	0.000	(.)	0.000	(.)	0.000	(.)	0.000	(.)
Lag 0	0.016	(0.014)	-0.029*	(0.013)	0.045**	(0.016)	-0.003	(0.006)
Lag 1	0.046*	(0.019)	-0.034*	(0.016)	0.069***	(0.019)	0.007	(0.012)
Lag 2	0.058**	(0.020)	-0.045*	(0.017)	0.081**	(0.023)	0.011	(0.013)
Lag 3	0.056*	(0.025)	-0.051**	(0.017)	0.090**	(0.027)	0.009	(0.015)
Lag 4	0.083**	(0.030)	-0.038	(0.020)	0.102***	(0.029)	0.014	(0.016)
Lag 5	0.062*	(0.029)	-0.076***	(0.015)	0.123***	(0.029)	0.016	(0.018)
Lag 6	0.043	(0.031)	-0.075***	(0.017)	0.108***	(0.030)	0.012	(0.018)
Lag 7	0.078*	(0.032)	-0.045**	(0.016)	0.104***	(0.029)	0.038	(0.020)
Control variables	Y		Y		Y		Y	
Year FE	Y		Y		N		Y	
Parl × Party FE	Y		Y		N		Y	
Parl × Year FE	N		N		Y		N	
Parl × Within FE	N		N		Y		N	
Year × Within FE	N		N		Y		N	
Parl × Within × Party FE	N		N		Y		N	
Post treatment	0.047		-0.044		0.086		0.017	
p-value	0.045		0.001		0.000		0.265	
Adj. R-Squared	0.30		0.21		0.33		0.22	
Obs.	15,313		11,013		26,326		26,328	

Regression results are based on Equation (1.1) or, in Column 3, on Equation (1.2).
Standard errors in parentheses * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 1.A.19: Plausibility checks: Normalized place on the party list

	Non-normalized rank			List length		
	(1)	(2)	(3)	(4)	(5)	(6)
	Within	Beyond	Interaction	Within	Beyond	Interaction
Lead 3	-1.149	-1.893	1.614	-1.271	-5.441	4.749*
	(1.439)	(1.433)	(1.067)	(2.405)	(3.489)	(2.256)
Lead 2	-0.411	-0.216	0.458	-2.063	-2.815	1.769*
	(0.848)	(0.759)	(0.703)	(1.113)	(1.473)	(0.828)
Lead 1	0.000	0.000	0.000	0.000	0.000	0.000
	(.)	(.)	(.)	(.)	(.)	(.)
Lag 0	0.462	-3.120	4.734**	-2.347	-5.269	2.905
	(1.545)	(1.708)	(1.508)	(3.342)	(4.231)	(1.830)
Lag 1	2.217	-2.032	5.505**	-5.430	-6.438	3.370
	(1.758)	(1.517)	(1.663)	(4.824)	(4.721)	(2.196)
Lag 2	3.515*	-2.567	6.795***	-5.649	-7.419	4.431
	(1.724)	(1.497)	(1.827)	(5.306)	(4.961)	(2.342)
Lag 3	3.766	-2.809	7.549**	-5.257	-6.996	4.240
	(2.113)	(1.512)	(2.299)	(5.712)	(5.196)	(2.622)
Lag 4	5.058*	-2.586	8.871**	-5.815	-6.894	4.812
	(2.322)	(1.638)	(2.625)	(5.976)	(5.392)	(2.915)
Lag 5	5.187*	-3.482*	8.602***	-0.192	1.857	0.524
	(2.445)	(1.670)	(2.419)	(6.339)	(6.551)	(3.315)
Lag 6	3.906	-3.586*	7.983**	-1.800	-0.872	1.519
	(2.545)	(1.659)	(2.579)	(5.979)	(6.023)	(3.232)
Lag 7	4.921	-2.589	8.971**	-4.382	-5.444	4.410
	(2.662)	(1.753)	(2.781)	(6.663)	(6.943)	(3.848)
Control variables	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	N	Y	Y	N
Parl × Party FE	Y	Y	N	Y	Y	N
Parl × Year FE	N	N	Y	N	N	Y
Parl × Within FE	N	N	Y	N	N	Y
Year × Within FE	N	N	Y	N	N	Y
Parl × Within × Party FE	N	N	Y	N	N	Y
Mean of dep. variable	23.72	17.89	21.28	51.69	57.36	54.06
Adj. R-Squared	0.56	0.48	0.53	0.90	0.87	0.94
Obs.	15,313	11,013	26,326	15,313	11,013	26,326

Regression results are based on Equation (1.1) or, in case of interactions, on Equation (1.2). Standard errors in parentheses * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 1.A.20: Robustness checks: Normalized place on the party list

	No covariates			Panel cases			Linear time trends			Standard regime		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Within	Beyond	Interaction	Within	Beyond	Interaction	Within	Beyond	Interaction	Within	Beyond	Interaction
Lead 3	-0.015	-0.022	0.001	-0.002	-0.005	0.007	-0.016	-0.015	-0.011	-0.023	-0.002	-0.009
	(0.013)	(0.012)	(0.012)	(0.016)	(0.020)	(0.016)	(0.024)	(0.015)	(0.016)	(0.026)	(0.015)	(0.022)
Lead 2	-0.003	0.003	-0.009	0.001	-0.001	0.002	-0.006	0.003	-0.011	-0.014	-0.000	0.002
	(0.009)	(0.007)	(0.010)	(0.013)	(0.012)	(0.014)	(0.012)	(0.007)	(0.010)	(0.018)	(0.010)	(0.016)
Lead 1	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
	(.)	(.)	(.)	(.)	(.)	(.)	(.)	(.)	(.)	(.)	(.)	(.)
Lag 0	-0.007	-0.033	0.043**	0.024	-0.037*	0.074***	0.018	-0.025*	0.045**	0.013	-0.026	0.053
	(0.016)	(0.017)	(0.014)	(0.017)	(0.016)	(0.019)	(0.014)	(0.012)	(0.016)	(0.025)	(0.016)	(0.027)
Lag 1	0.035	-0.030*	0.065***	0.059*	-0.041*	0.103***	0.039	-0.028	0.069***	0.079*	-0.022	0.073*
	(0.020)	(0.015)	(0.016)	(0.023)	(0.018)	(0.019)	(0.020)	(0.015)	(0.019)	(0.036)	(0.017)	(0.033)
Lag 2	0.042*	-0.042**	0.080***	0.081**	-0.047*	0.122***	0.053*	-0.034*	0.081**	0.073	-0.032	0.073
	(0.018)	(0.014)	(0.020)	(0.023)	(0.020)	(0.021)	(0.024)	(0.017)	(0.023)	(0.038)	(0.017)	(0.044)
Lag 3	0.039	-0.044**	0.085**	0.090**	-0.053*	0.145***	0.060	-0.040*	0.090**	0.086*	-0.026	0.079
	(0.023)	(0.016)	(0.024)	(0.027)	(0.020)	(0.024)	(0.030)	(0.018)	(0.027)	(0.039)	(0.017)	(0.044)
Lag 4	0.076**	-0.027	0.102***	0.123***	-0.046	0.170***	0.087*	-0.028	0.102***	0.126**	-0.006	0.098*
	(0.027)	(0.017)	(0.025)	(0.030)	(0.023)	(0.027)	(0.037)	(0.021)	(0.029)	(0.041)	(0.020)	(0.046)
Lag 5	0.042	-0.065***	0.091**	0.109**	-0.079***	0.194***	0.084*	-0.067**	0.123***	0.054	-0.077***	0.124**
	(0.025)	(0.014)	(0.027)	(0.032)	(0.021)	(0.029)	(0.038)	(0.022)	(0.029)	(0.038)	(0.018)	(0.041)
Lag 6	0.046	-0.060**	0.094**	0.090**	-0.083***	0.191***	0.063	-0.074**	0.108***	0.055	-0.081***	0.132**
	(0.029)	(0.019)	(0.029)	(0.033)	(0.021)	(0.028)	(0.041)	(0.026)	(0.030)	(0.042)	(0.019)	(0.041)
Lag 7	0.050	-0.037*	0.073*	0.102**	-0.049*	0.162***	0.098*	-0.050	0.104***	0.071	-0.055**	0.125**
	(0.030)	(0.016)	(0.029)	(0.033)	(0.021)	(0.029)	(0.043)	(0.029)	(0.029)	(0.039)	(0.017)	(0.040)
Control variables	N	N	N	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	N	Y	Y	N	Y	Y	N	Y	Y	N
Parl × Party FE	Y	Y	N	Y	Y	N	Y	Y	N	Y	Y	N
Parl time trends	N	N	N	N	N	N	Y	Y	Y	N	N	N
Parl × Year FE	N	N	Y	N	N	Y	N	N	Y	N	N	Y
Parl × Within FE	N	N	Y	N	N	Y	N	N	Y	N	N	Y
Year × Within FE	N	N	Y	N	N	Y	N	N	Y	N	N	Y
Parl × Within × Party FE	N	N	Y	N	N	Y	N	N	Y	N	N	Y
Adj. R-Squared	0.30	0.21	0.33	0.31	0.23	0.35	0.31	0.22	0.33	0.32	0.21	0.36
Obs.	15,313	11,013	26,326	12,577	8,515	21,092	15,313	11,013	26,326	11,306	8,562	19,868

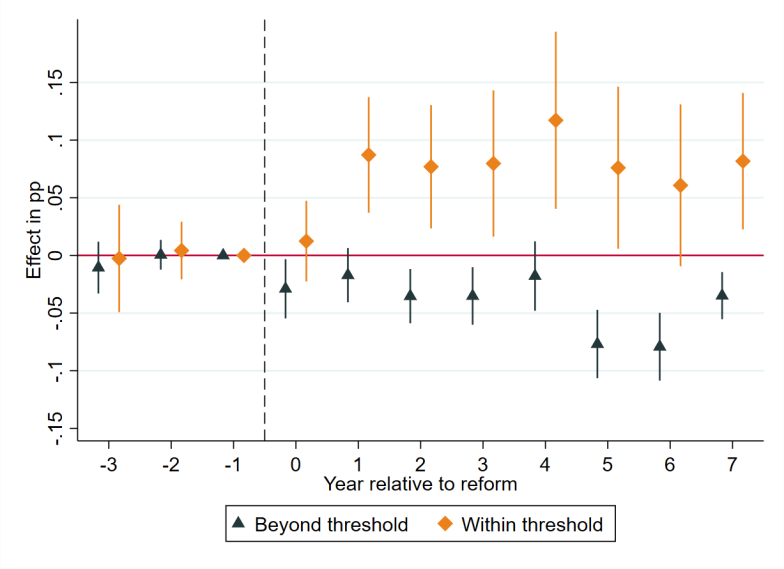
Regression results are based on Equation (1.1) or, in case of interactions, on Equation (1.2). Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 1.A.21: Robustness checks, continued: Normalized place on the party list

	Restrict list length			Decisive term			Main parties		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Within	Beyond	Interaction	Within	Beyond	Interaction	Within	Beyond	Interaction
Lead 3	-0.014	0.000	-0.020	-0.073*	-0.002	-0.059	-0.010	-0.006	-0.009
	(0.017)	(0.011)	(0.015)	(0.030)	(0.007)	(0.032)	(0.016)	(0.010)	(0.015)
Lead 2	-0.001	0.007	-0.014	-0.010	0.001	-0.005	-0.001	0.004	-0.009
	(0.009)	(0.006)	(0.011)	(0.016)	(0.003)	(0.019)	(0.009)	(0.006)	(0.010)
Lead 1	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
	(.)	(.)	(.)	(.)	(.)	(.)	(.)	(.)	(.)
Lag 0	0.017	-0.027*	0.047*	0.036	-0.029*	0.117**	0.016	-0.029*	0.047**
	(0.015)	(0.013)	(0.018)	(0.045)	(0.011)	(0.039)	(0.015)	(0.013)	(0.016)
Lag 1	0.040*	-0.030	0.066**	0.085	-0.044**	0.155**	0.045*	-0.032*	0.070***
	(0.018)	(0.017)	(0.020)	(0.054)	(0.014)	(0.046)	(0.019)	(0.016)	(0.019)
Lag 2	0.053**	-0.042*	0.079**	0.031	-0.053**	0.096*	0.055**	-0.044*	0.080***
	(0.019)	(0.018)	(0.024)	(0.055)	(0.016)	(0.042)	(0.020)	(0.017)	(0.023)
Lag 3	0.051*	-0.049*	0.089**	-0.008	-0.062***	0.076	0.053*	-0.050**	0.092**
	(0.025)	(0.019)	(0.029)	(0.057)	(0.015)	(0.044)	(0.026)	(0.017)	(0.027)
Lag 4	0.075*	-0.038	0.098**	-0.050	-0.061***	0.061	0.079*	-0.037	0.103***
	(0.030)	(0.021)	(0.030)	(0.065)	(0.015)	(0.046)	(0.031)	(0.020)	(0.029)
Lag 5	0.060	-0.085***	0.126***	0.006	-0.067***	0.104*	0.053	-0.074***	0.119***
	(0.030)	(0.017)	(0.032)	(0.065)	(0.015)	(0.051)	(0.030)	(0.015)	(0.029)
Lag 6	0.042	-0.082***	0.110**	0.031	-0.066***	0.106*	0.033	-0.073***	0.102**
	(0.032)	(0.018)	(0.032)	(0.064)	(0.014)	(0.049)	(0.032)	(0.016)	(0.030)
Lag 7	0.072*	-0.047**	0.106**	0.039	-0.048***	0.119*	0.071*	-0.043**	0.099**
	(0.034)	(0.018)	(0.032)	(0.064)	(0.014)	(0.047)	(0.034)	(0.016)	(0.030)
Control variables	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	N	Y	Y	N	Y	Y	N
Parl × Party FE	Y	Y	N	Y	Y	N	Y	Y	N
Parl × Year FE	N	N	Y	N	N	Y	N	N	Y
Parl × Within FE	N	N	Y	N	N	Y	N	N	Y
Year × Within FE	N	N	Y	N	N	Y	N	N	Y
Parl × Within × Party FE	N	N	Y	N	N	Y	N	N	Y
Adj. R-Squared	0.25	0.19	0.29	0.34	0.20	0.23	0.29	0.20	0.32
Obs.	13,274	10,034	23,308	2,527	7,800	10,327	14,069	10,873	24,942

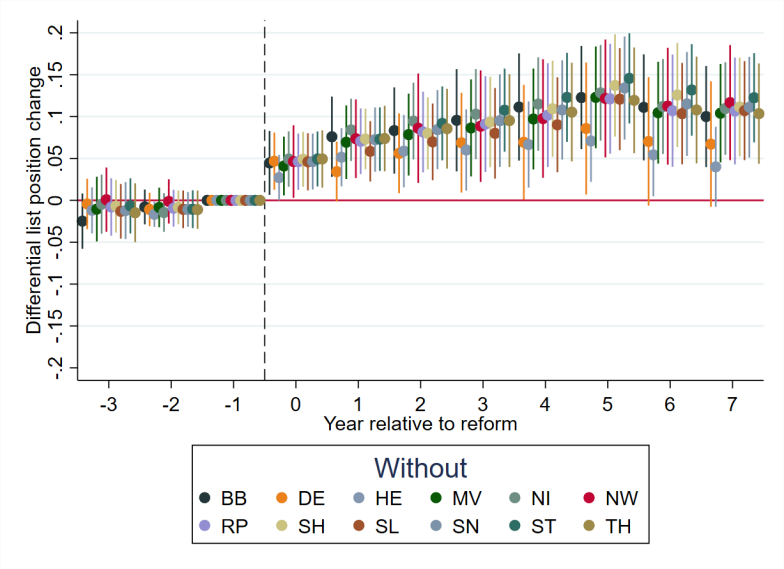
Regression results are based on Equation (1.1) or, in case of interactions, on Equation (1.2). Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Figure 1.A.7: Robustness to method: Sun & Abraham (2021)



Notes: This graph depicts an event study based on Sun and Abraham (2021). The time on the horizontal axis is years relative to the year when pension eligibility reform was implemented. Confidence intervals are computed at the 95% level.

Figure 1.A.8: Jackknife exercise



Notes: This graph depicts an event study, based on Equation (1.2). Each dynamic treatment effect estimate represents a specification omitting each parliament one-by-one. The time on the horizontal axis is years relative to the year when pension eligibility reform was implemented. Confidence intervals are computed at the 95% level.

Table 1.A.22: Sensitivity to other aspects of reform: Normalized rank

	Reform of allowance			Reform of contribution proxy			Reform of pension age			Reform of term length		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Within	Beyond	Interaction	Within	Beyond	Interaction	Within	Beyond	Interaction	Within	Beyond	Interaction
Lead 3	-0.006	-0.005	-0.041	0.018	0.002	-0.011	-0.012	-0.007	-0.036*	-0.034*	-0.032*	-0.026
	(0.023)	(0.015)	(0.021)	(0.018)	(0.010)	(0.012)	(0.019)	(0.012)	(0.016)	(0.015)	(0.013)	(0.020)
Lead 2	-0.022	-0.014*	-0.020	0.006	0.012*	-0.023***	-0.005	-0.008	-0.026*	-0.046***	-0.038***	-0.018
	(0.014)	(0.007)	(0.016)	(0.009)	(0.005)	(0.006)	(0.015)	(0.008)	(0.013)	(0.010)	(0.010)	(0.012)
Lead 1	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
	(.)	(.)	(.)	(.)	(.)	(.)	(.)	(.)	(.)	(.)	(.)	(.)
Lag 0	0.021	-0.026*	0.010	0.013	-0.013	0.020	-0.008	-0.014	-0.012	-0.048	-0.135***	0.074
	(0.021)	(0.010)	(0.018)	(0.012)	(0.009)	(0.011)	(0.016)	(0.013)	(0.016)	(0.055)	(0.025)	(0.040)
Lag 1	0.023	-0.011	0.000	0.023	-0.017	0.034*	-0.013	-0.015	-0.025	-0.077	-0.147***	0.064
	(0.022)	(0.012)	(0.020)	(0.017)	(0.013)	(0.015)	(0.021)	(0.012)	(0.020)	(0.053)	(0.024)	(0.040)
Lag 2	0.044	-0.015	0.012	0.032	-0.019	0.042*	-0.004	-0.005	-0.025	-0.071	-0.147***	0.074
	(0.025)	(0.014)	(0.025)	(0.020)	(0.015)	(0.019)	(0.021)	(0.011)	(0.021)	(0.054)	(0.023)	(0.042)
Lag 3	0.049	-0.000	0.015	0.019	-0.027	0.043*	0.003	0.001	-0.027	-0.045	-0.122***	0.066
	(0.026)	(0.017)	(0.026)	(0.018)	(0.015)	(0.020)	(0.020)	(0.013)	(0.023)	(0.058)	(0.028)	(0.045)
Lag 4	0.066*	0.000	0.003	0.029	-0.012	0.038	-0.004	0.009	-0.042	0.041	-0.088**	0.122*
	(0.029)	(0.013)	(0.031)	(0.021)	(0.015)	(0.019)	(0.024)	(0.015)	(0.026)	(0.058)	(0.028)	(0.053)
Lag 5	0.028	-0.014	-0.020	-0.001	-0.033*	0.020	-0.029	-0.001	-0.052*	-0.069	-0.111***	0.034
	(0.029)	(0.013)	(0.028)	(0.021)	(0.014)	(0.018)	(0.021)	(0.015)	(0.022)	(0.054)	(0.029)	(0.044)
Lag 6	0.006	-0.015	-0.033	-0.023	-0.043**	0.012	-0.052*	0.004	-0.077***	-0.090	-0.135***	0.043
	(0.030)	(0.011)	(0.028)	(0.023)	(0.014)	(0.020)	(0.021)	(0.016)	(0.021)	(0.050)	(0.027)	(0.040)
Lag 7	0.009	-0.003	-0.025	-0.006	-0.015	-0.014	-0.028	0.021	-0.079***	-0.092	-0.109**	0.016
	(0.024)	(0.014)	(0.026)	(0.025)	(0.012)	(0.022)	(0.020)	(0.013)	(0.019)	(0.050)	(0.031)	(0.039)
Control variables	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	N	Y	Y	N	Y	Y	N	Y	Y	N
Parl × Party FE	Y	Y	N	Y	Y	N	Y	Y	N	Y	Y	N
Parl × Year FE	N	N	Y	N	N	Y	N	N	Y	N	N	Y
Parl × Within FE	N	N	Y	N	N	Y	N	N	Y	N	N	Y
Year × Within FE	N	N	Y	N	N	Y	N	N	Y	N	N	Y
Parl × Within × Party FE	N	N	Y	N	N	Y	N	N	Y	N	N	Y
Adj. R-Squared	0.30	0.21	0.33	0.30	0.21	0.33	0.30	0.21	0.33	0.30	0.21	0.33
Obs.	15,313	11,013	26,326	15,313	11,013	26,326	15,313	11,013	26,326	15,313	11,013	26,326

Regression results are based on Equation (1.1) or, in case of interactions, on Equation (1.2). Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 1.A.23: Ranks and vote shares of constituency candidates on lists

	Normalized rank						Vote share					
	(1) Within		(2) Beyond		(3) Interaction		(4) Within		(5) Beyond		(6) Interaction	
Lead 3	-0.087**	(0.025)	0.034	(0.024)	-0.116***	(0.027)	-0.045***	(0.012)	-0.046**	(0.016)	0.007	(0.007)
Lead 2	-0.027	(0.014)	0.020	(0.013)	-0.039**	(0.013)	-0.010**	(0.004)	-0.015***	(0.003)	0.011**	(0.004)
Lead 1	0.000	(.)	0.000	(.)	0.000	(.)	0.000	(.)	0.000	(.)	0.000	(.)
Lag 0	-0.006	(0.017)	-0.026	(0.014)	0.032*	(0.015)	-0.010	(0.005)	-0.011	(0.008)	0.008	(0.006)
Lag 1	0.049*	(0.019)	-0.031	(0.018)	0.079***	(0.019)	0.007	(0.010)	-0.012	(0.013)	0.018*	(0.008)
Lag 2	0.054	(0.028)	-0.037	(0.022)	0.087***	(0.023)	0.008	(0.013)	-0.010	(0.015)	0.020*	(0.008)
Lag 3	0.049	(0.031)	-0.032	(0.019)	0.090***	(0.024)	0.013	(0.011)	-0.004	(0.015)	0.022*	(0.009)
Lag 4	0.071	(0.039)	-0.011	(0.022)	0.092***	(0.025)	0.017	(0.015)	-0.006	(0.016)	0.025**	(0.009)
Lag 5	0.038	(0.032)	-0.051*	(0.020)	0.095***	(0.024)	0.013	(0.012)	0.028	(0.014)	-0.012	(0.011)
Lag 6	0.016	(0.031)	-0.041	(0.022)	0.074**	(0.023)	0.010	(0.011)	0.008	(0.014)	0.005	(0.011)
Lag 7	0.030	(0.028)	-0.019	(0.019)	0.060**	(0.019)	0.003	(0.009)	0.007	(0.013)	-0.001	(0.012)
Control variables	Y		Y		Y		Y		Y		Y	
Year FE	Y		Y		N		Y		Y		N	
Parl × Party FE	Y		Y		N		Y		Y		N	
Parl × Year FE	N		N		Y		N		N		Y	
Parl × Within FE	N		N		Y		N		N		Y	
Year × Within FE	N		N		Y		N		N		Y	
Parl × Within × Party FE	N		N		Y		N		N		Y	
Adj. R-Squared	0.30		0.20		0.32		0.82		0.75		0.81	
Obs.	11,572		8,545		20,117		11,572		8,545		20,117	

Regression results are based on Equation (1.1) or, in case of interactions, on Equation (1.2). Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Chapter 2

Money Pit(ch): The Impact of Football-Induced Municipal Debt

2.1 Introduction

In Germany, local public debt was €2,033 per capita in 2024 and has been increasing steadily in the last years, often to finance permanent budget deficits instead of public investment (Federal Statistical Office, 2024a). Since German municipalities provide elementary public goods such as schools, roads and some forms of social insurance, policymakers and academics alike worry about debt's effect on local public good provision and economic growth. Legacy debt might crowd out usually debt-financed investment and infrastructure spending, impairing core government responsibilities. This is especially prevalent in Germany, where municipalities and counties were responsible for around 38% of total public investment in 2023, but frequently run net investment deficits (Federal Statistical Office, 2024b). Increased interest payments and interest rate risk burden local public finances additionally. For this reason, some states have launched debt reduction programs, which bail out part of municipalities' debt in exchange for mandatory consolidation measures. While these programs have been studied extensively (Fremerey et al., 2022; Merlo et al., 2023), there is scant causal evidence on the effect of public debt accumulation on public good provision and local economic activity.

In this study, I propose a plausibly exogenous shock to German municipalities' debt to examine its impact on local public finances. Due to interdependencies between municipalities and local football clubs regarding stadium financing, a club's quasi-random relegation to a lower, less lucrative tier in the Bundesliga league system represents a shock to the hosting municipality's public finances. This shock materializes as a significant and substantial increase in public debt. Equipped with this natural experiment, I estimate

the dynamic debt effect on the budget and debt persistence with an event study design.

A striking example of this interdependence, discussed in Section 2.2, is the case of Aachen. The municipality backed the construction of a new stadium following the local club's promotion to the first division. However, after a quick relegation, the club struggled with loan payments, prompting strong municipal support despite already high debt levels. Two further relegations worsened the financial strain, ultimately leading the municipality to acquire the stadium at significant additional cost. During this period, public debt rose by approximately €1,000 per capita, reaching €3,471 per capita. This study expands on the Aachen case to systematically examine how football outcomes affect local public finances by analyzing a broader set of municipalities.

To that end, I collect information on the top four divisions of male professional football in Germany, which includes the Bundesliga, for more than two decades. This is supplemented by hand-collected data on stadium ownership and municipalities' participation in stadium financing. German municipalities' high autonomy, especially with respect to tax rate setting and their ability to take on loans, make them an ideal empirical testing ground. Studying debt empirically can be tricky on the national level as opposed to the sub-national level, which avoids some of the thorny issues of cross-country analysis such as different laws, preferences and institutions. The aim of this study is to quantify the effect of public debt on municipalities' budgets and in particular capital expenditure. At the same time, characteristics that are conducive to municipalities' debt accumulation are documented.

My findings reveal a strong and significant average debt response in municipalities which host a professional club that is relegated of around €238 per capita or 10% after five years. This result is robust to different specifications and methods. In terms of the treatment effect size, it is comparable to the average standard deviation of debt over time within municipalities. The shock analyzed here thus serves as a good indicator of typical debt swings.

Excess debt subsequently stabilizes after five years and then begins converging back to the pre-treatment control group gap. Surprisingly, the estimates do not reveal substantial adverse budgetary consequences associated with increased debt levels. This is also true for spending on public investment, which responded strongly to permanent budget shocks in another study on German municipalities (Helm & Stuhler, 2024), alleviating concerns about crowding out of investment (loans). Conversely, there is no beneficial symmetric effect when hosting a team promoted to a more lucrative division: Local public finances benefit–if at all–only in a strongly attenuated way.

These findings contribute to the empirical literature on local government debt by enabling causal claims about its effects and introducing a previously unexamined determinant,

i.e., financial commitments in a volatile sports industry, alongside established factors such as political election cycles, fiscal rules and administrative organization (e.g., Feld & Kirchgässner, 2001; Pettersson-Lidbom, 2001; Benito & Bastida, 2004; Ashworth et al., 2005; Feld et al., 2011; Grembi et al., 2016).

Medium-term responses to increased debt are limited to moderate but persistent local business tax rate increases of around 0.18 percentage points or 1% and some asset sales. In terms of spillover effects, neighboring municipalities follow suit with a one-to-one response in their business tax rate that is limited to a radius of around 30 kilometers, similar to Eugster and Parchet (2019). The property tax rate remains unchanged. This emphasizes large, football-hosting municipalities' leading role in local tax competition as in Janeba and Osterloh (2013).

The analysis also sheds light on the decentralized and multi-tiered institutional framework in which German municipalities operate. Fiscal oversight, which is in place to avoid unsustainable local public finances, has been shown to exert control in Christofzik and Kessing (2018). In this setting oversight authorities impose budget consolidation programs in the year after relegation which, however, do not prevent municipalities from incurring a significant amount of debt, raising questions about oversight effectiveness. Additionally, the interdependence between municipalities and municipality-owned savings banks is highlighted: They play a large role in providing loans to municipalities, underscoring accountability issues.

Long-run economic growth is not significantly affected based on two proxies for local economic activity. How municipalities' economies remain relatively unscathed in the face of such a substantial debt surge can be explained by state-level debt reduction programs. Related work on German programs includes Fremerey et al. (2022) and Merlo et al. (2023), who study municipalities in states North Rhine-Westphalia and Hesse, respectively. Their findings suggest that large municipalities–when mandated to consolidate–decrease debt mainly through tax increases instead of reductions in spending. This is partly the case here as well, even though the focus of this study is debt accumulation instead of consolidation. The debt programs might increase moral hazard since part of the documented reduction of excess debt from football in later years is accounted for by debt programs. This benefits municipalities directly involved in football but is financed by all state taxpayers. My findings suggest municipalities' own modest consolidation measures cannot explain the observed debt reduction by themselves. However, the dynamic treatment effect in the first years of the debt accumulation phase is similar for municipalities in states without debt consolidation programs. This suggests that moral hazard plays a minor role.

The study's findings yield three key insights. First, public investment appears less sen-

sitive to debt than commonly feared. While context matters, the 10% increase in public debt in this study does not significantly crowd out investment loans, even in the long run, suggesting that other factors play a larger role in driving public investment. Second, state bailout funds effectively absorb debt linked to football eventually, implying municipalities in states with debt reduction programs do not fully bear the financial burden of relegation. This creates a fiscal externality, shifting the financial burden onto taxpayers in other municipalities for purely consumptive purposes. Third, the timing of fiscal oversight is crucial. Although oversight intervenes during relegation-induced debt accumulation with mandated consolidation programs, this occurs only after financial damage has been done. To be effective, oversight should focus on financial commitments, such as loans and guarantees, at the time they are made, rather than reacting only when adverse outcomes materialize.

From a theoretical perspective and considering what the literature has shown, it is not clear how exactly municipalities would react to such a particular public finance shock as proposed here. However, both provide guidelines to formulate hypotheses on municipalities' response. The growing literature on local public finance shocks exploits quasi-exogenous variation in tax revenues and trade (e.g., Berset et al., 2023; Besfamille et al., 2023) or utilizes changes to intergovernmental transfers (e.g., Dahlberg et al., 2008; Gadenne, 2017; Helm & Stuhler, 2024) to make causal statements on budgetary decisions. Although institutional settings vary widely across studies, one common conclusion is that it matters how transitory the shock is perceived to be. For example, Christofzik and Schneider (2019) find that German municipalities finance an increase in outgoing intergovernmental transfers they perceive to be temporary mostly by increasing debt. Thus, there seems to be no need to permanently decrease other forms of spending or to increase taxes. In contrast, permanent changes to how grants are allocated in Helm and Stuhler (2024) are directly reflected in spending and, above all, investment. Interestingly, this effect is shown to be symmetric with permanent decreases in grants leading to persistently lower investment. Municipalities thus attempt to smooth expenditure and tax policy by incurring debt or accumulating reserves when a shock is temporary, but make lasting budgetary changes in response to a permanent shock, which is consistent with theoretical considerations in Barro (1979).

Therefore, it matters how the relegation shock is perceived by policymakers in this context. Though, as will be shown empirically, less than one in four clubs achieve promotion within two years after being relegated and the probability is negligible after that, municipal council members pass generous public contributions to stadium financing. Relegation as defined here is, on average, not a temporary but at least medium-term shock lasting several years. It is, however, perceived as temporary ex-ante and thus should translate,

above all, in increased debt as in Christofzik and Schneider (2019) and Berset et al. (2023).

The dissonance between policymakers' initial expectation and reality should lead to policy change eventually as debt balloons and other typically debt-financed spending such as investment might be crowded out. Municipalities are hypothesized to conduct at least temporary consolidation measures to address the excess debt from football, possibly as a response to budgetary pressure and fiscal oversight. Empirical evidence on how consolidation could play out is mixed. While Fremerey et al. (2022) find that, when pressed to consolidate, German municipalities engage both in spending cuts and tax hikes, Merlo et al. (2023), in a similar setting, document that large municipalities tend to make use of tax hikes. One would expect tax rate increases to play a more pronounced role in the context of this paper as most municipalities hosting a professional football club are large.

Interest in football results as drivers of outcomes outside of sports has already uncovered remarkable relationships in the realm of the economics of education and political science. Singleton et al. (2024) use relegation from the English Premier League as treatment, showing that university student enrollment in affected municipalities decreases due to less visibility. The deviation of international football matches' outcome from expectations are found to increase national leaders' popularity when winning, but not when losing in Busby et al. (2017). In this study, I find a similar asymmetric effect also for democratically elected officials. While relegation has an attenuated effect at best, the majority party at the time of stadium modernization benefits electorally from subsequent promotion. This provides one explanation why municipal decision makers provide public support to local clubs. Beyond this rationale, this study illustrates that also public finances can be affected strongly by incidental events like football.

The paper proceeds by introducing the institutional context of municipalities and professional football in Germany in Section 2.2. Data sets and their sources are introduced in Section 2.3. Summary statistics, the empirical model, regression results and heterogeneity analysis as well as exploration of possible mechanisms and robustness checks are presented in Section 2.4. Section 2.5 contains a brief discussion of the results and Section 2.6 concludes.

2.2 Institutional background

German municipalities

Municipalities are the lowest level of government in Germany. The approximately 13,000 entities exhibit a considerable amount of autonomy in terms of public spending and

raising revenue. While municipalities earmark a large share of spending for mandatory task such as some forms of social spending, they enjoy considerable leeway beyond that. Crucially, public investment is an important budget item that is decided by the respective municipality. For example, municipalities are responsible for building and maintaining basic local public goods such as roads, nurseries and schools.

On the revenue side, municipalities rely on intergovernmental transfers, which average 21% of revenues in my sample, and shares of certain federally levied taxes. Municipalities' share of the personal income tax is 25% of municipal revenues in my sample. Municipalities also have the authority to levy two key taxes themselves: The local property tax (LPT) and local business tax (LBT) serve as crucial sources of revenue for funding local services and infrastructure. In my sample both taxes contribute on average 10% and 35% to revenues, respectively. The local tax rate on net business income is determined by multiplying the nationally universal base rate of 3.5% with the locally chosen tax multiplier. If a municipality sets the multiplier to a typical 400%, the resulting tax rate on local net business income would amount to 14%. This is on top of the federally levied corporate tax rate of 15%. The base rate is 0.35% for the property tax.

Though notionally municipalities have to balance their budgets, loans that finance investment are allowed. Municipalities may also take short-term loans with typically low maturity in order to bridge temporary liquidity constraints. They are sometimes referred to as cash or liquidity loans. In practice, municipalities increasingly finance recurring budget deficits with these loans. Since much of that spending is not backed by an increase in investment, many municipalities find themselves in an unsustainable position (Heinemann et al., 2009).

In theory, higher-level governments like counties and states oversee municipal budgets to ensure fiscal sustainability. While budget rules and oversight structures vary by state, they all adhere to this core principle. Fiscal oversight may launch budget consolidation programs (*Haushaltssicherungskonzept*) that mandate consolidation measures (Christofzik & Kessing, 2018). In practice, supervision has not stopped long-term financing of budget deficits with short-term loans for the most part, however.

Some states in which municipalities' misuse of short-term loans and resulting high debt levels is especially rampant, e.g., North Rhine-Westphalia and Hesse, started debt reduction programs. These typically assume at least part of municipalities' debt in exchange for budget cuts, tax rate increases or similar concessions enshrined in budget consolidation programs. The goal of these bailouts is to preserve municipalities' capacity to provide core governmental services, which states are legally required to guarantee.

Since Berlin, Bremen and Hamburg hold a unique status as city-states, they are left out of the following analysis. Their dual role as both municipalities and states at the same

time make comparisons to other municipalities difficult. So-called district-free towns are municipalities which have a dual role as municipality and county at the same time and will be included in the sample.

League system

The German male professional football system constitutes a multi-tiered framework. The first division is the Bundesliga, succeeded by the 2. Bundesliga and the 3. Liga, culminating in the Regionalliga, which is segmented into five regional divisions. League affiliation is determined at the end of each season, usually in May. Each season, a division normally comprises between 18 and 20 teams. A fixed number of usually two or three of the worst performing teams are relegated from higher leagues to lower divisions. Performance is measured by a point system where teams are awarded three points for a victory and one point for a draw. On the other hand, promotion elevates an equal number of top-performing teams from lower tiers to higher divisions. Though this set up has been subject to some reforms over the years, such as the introduction of the 3. Liga in 2008 as well as playing a relegation match to decide league affiliation, the cornerstones of the system remain unchanged.

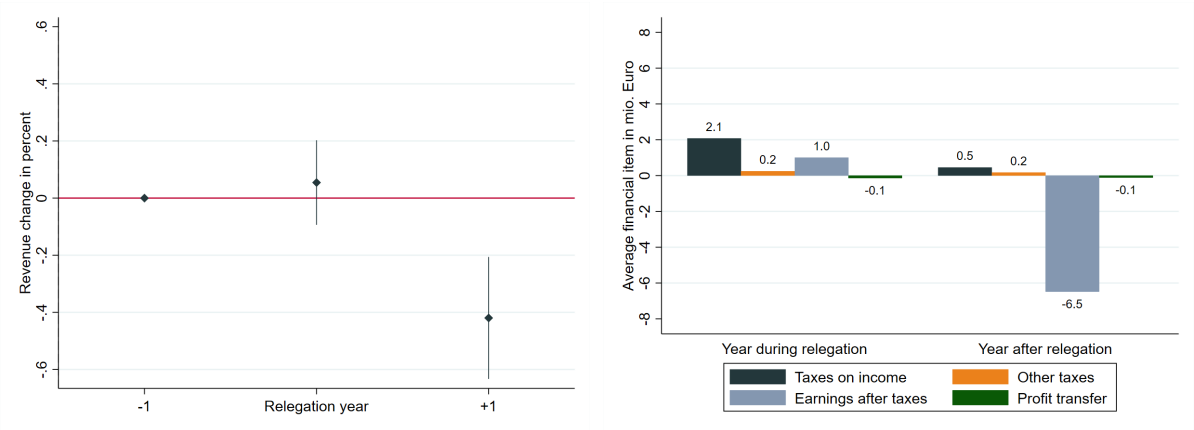
The dynamics of relegation and promotion are characterized by some degree of back and forth. Some clubs routinely switch divisions. This is reflected in Figure 2.A.2 in the Appendix. It presents an event study graph of clubs that were relegated in relative year 0. While almost half of relegated teams were promoted in the last three years such that relegation implies ending up in the original division, it is less common to be promoted after being relegated. Less than one in four clubs achieve this within two years after which the probability to do so becomes negligible. The concrete treatment definition in this paper will exclude some cases of instant switching.

Football clubs' revenue depends heavily on the division their first male professional team plays in. Thus, relegation as well as promotion can have profound financial implications. The loss of television revenue,[1] sponsorship deals and match day income associated with participation in higher leagues usually leads to a significant decline in revenue. This reduction in income may necessitate cost-cutting measures, including player wage reductions, stadium rent renegotiation, staff layoffs and budget constraints on transfer activity and player recruitment.

[1] In general, a club's share of overall television revenue is determined primarily by current league affiliation and past performance. In the season starting in 2024, this implies that clubs in the first division share around €26 million while clubs in the second division share around €7 million. This is complemented by a weighted average of club performance in the last years. More recent years have substantially larger weights. While this formula implies a large drop in revenues on impact, the adverse effect of television revenues worsens for each season in a lower division. See Deutsche Fußball Liga e.V. (2020) for details.

Figure 2.2.1: Average financial responses after relegation from first division

a) Event study **b)** Absolute levels



Notes: The left panel depicts an event study, based on Equation (2.1). It shows the percent revenue change of football clubs relegated from the first to the second division relative to the year before relegation and non-relegated clubs between 2018 and 2023. Confidence intervals are computed at the 95% level. The right panel depicts clubs' absolute levels of financial figures in million Euros in the financial year during and after relegation, respectively.

Deutsche Fußball Liga (DFL), the governing body responsible for the first two divisions, requires clubs to publish key financial figures in the form of balance sheets and income statements starting in 2018. On the basis of this data, I can quantify average responses to relegation to the second division in Figure 2.2.1. Relegated clubs experience a strong 40% decrease in overall revenues in their first season in the second division relative to the year before relegation and non-relegated teams in Figure 2.2.1a. Figure 2.2.1b focuses on levels. While taxes unrelated to income and transfers of capital, i.e., club owners' capital injection or withdrawal, do not change, average taxes on club income decrease from €2.1 million to €0.5 million. Average earnings after taxes turn strongly negative in the first year after relegation. Although most clubs are not primarily geared towards profit, this change emphasizes the dire financial situation relegated clubs find themselves in.

Since *Deutscher Fußball-Bund* (DFB), the governing body responsible for the third division, does not require publication of income statements, it is more difficult to put a revenue number on division changes here. Being demoted to even lower divisions will not have the same impact in absolute terms, but should represent a strong financial pressure, too. Figure 2.A.1 in the Appendix plots the aggregate revenue in the first three divisions over time and illustrates that revenues in the third division are significantly lower than in the second league.

Beyond athletic performance, clubs can also face relegation, sometimes by more than one division, if the DFL finds them in violation of licensing conditions, such as financial misreporting or impending insolvency. In my sample, there are ten such cases across the

top three divisions. The issue becomes more prevalent in the five Regionalligen, where financial instability is more common, resulting in 39 cases. These instances are flagged and analyzed separately.

Mechanism

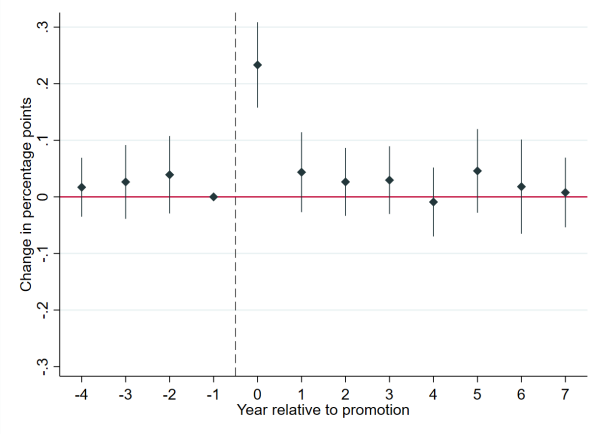
Although football in Germany is generally a private undertaking, there is substantial interdependence between professional clubs and (especially local) government. On the revenue side, municipalities may most directly benefit in the form of business tax revenue, as well as a share of proceeds from the personal income tax and, to a lesser extent, the value-added tax. Since most clubs do not operate with the aim to maximize profit, the business tax, which is levied on net income, might not be as lucrative as it would be for a conventional firm with comparable revenues. In fact, income statements for the years 2018 and 2019 before Covid show that around a third of clubs in the first two divisions had negative earnings before taxes and the median net income is €2.1 million. Beyond this direct effect, football clubs might also stimulate economic activity around match days, e.g., the hospitality industry or publicly-owned parking.

On the other hand, there is public spending related to match days on infrastructure and security, though police presence is financed by states, not municipalities. An important nexus between clubs and municipalities is the public ownership of local stadiums. Of the 177 clubs for which I hand-collected data, 75% played in stadiums that are majority-owned by the municipality in 2020, another 2 (or 1% of) clubs play in a stadium owned by the county. Only 16% own the stadium they use and the remaining 8% of stadiums belong to private investors. Reasons for this high share are historic ownership or implicit local support.

Even in lower divisions, the size of stadiums can be considerable as popularity–and therefore attendance at match days–is substantial. In the season starting in 2022, according to DFL and DFB, approximately 42,000 fans attended each match on average in the first division, decreasing to 22,000 in the second and 8,000 in the third division. Football data website *Transfermarkt.de* still counts between 1,000 and 2,000 visitors on average for the five regional divisions, i.e., the fourth tier. These visitor numbers represent a logistic challenge on-site and require elaborate safety precautions.

For this reason, there are strict stadium criteria in place that need to be met for admission to higher leagues. Detailed requirements on, for example, stadium capacity, roofing, lawn heating as well as quality, sprinkler systems and floodlight drive costs for promoted clubs' stadium modernization (Deutsche Fußball Liga e.V., 2022). At the same time, euphoria surrounding promotion tends to amplify grand projects. This is reflected in the more than

Figure 2.2.2: Construction probability



Notes: This graph depicts an event study, based on Equation (2.1). The time on the horizontal axis is years relative to a promotion event. The dependent variable is an indicator variable equal to one if stadium construction or modernization efforts started in the respective year. Confidence intervals are computed at the 95% level.

20 percentage point increase in the probability to construct or modernize the stadium in the year of being promoted relative to the year before in Figure 2.2.2.

High one-time investments into modernization or new construction represent a lumpy investment financed by long-term loans.[2] In the case of public ownership or municipal support, municipality and club agree on the respective share of the burden.[3] This support may take different forms: Municipalities may issue loan guarantees, participate in financing or lend to clubs directly. With the hand-collected data available to me, I cannot distinguish the latter two and I do not observe loan guarantees. Empirically shown in Figure 2.2.3, the average of municipalities' financing share is 61% and, when adding to this the share other public entities contribute, rises to 77% while clubs carry on average 14% of of the financing share. Municipalities and states may also agree on loan guarantees to secure financing, which are more difficult to observe and measure, but anecdotally play a large role, too.[4] Many modernization projects are plagued by cost overruns that

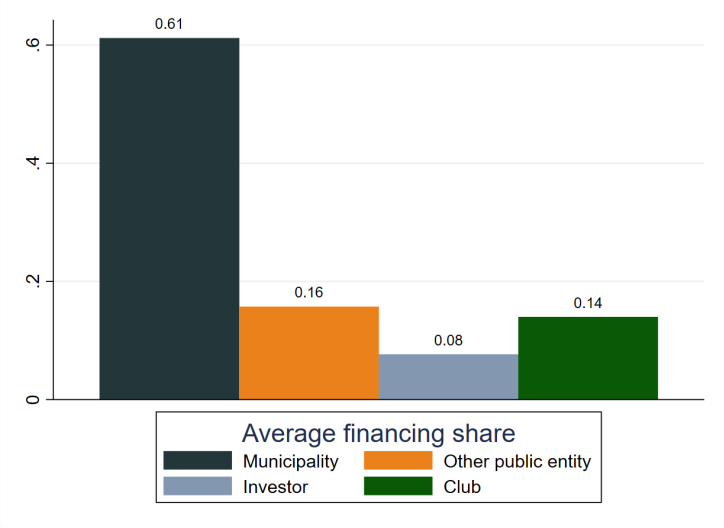
[2]The distinction between stadium construction and modernization is not clear-cut. Although the occasional greenfield project qualifies easily as new construction, many modernization projects are on par with new construction in terms of costs because of limited alternative plots. For this reason, I abstract from the distinction.

[3]The practice to externalize the cost of stadium maintenance seems to be a feature not a bug. The DFB seems to be aware of this when it writes on its website: "The approval process is an important tool–for the league and for the clubs. For many clubs, it is only through the pressure and binding nature of the approval process that they are able to achieve the modernization of the local stadium, which is usually owned by the municipality."

[4]State aid, which might be applicable in this case, is limited by European Union (EU) legislation, i.e., Article 107 (1) of the Treaty on the Functioning of the EU. It includes exceptions for activities which benefit the general public, e.g., arguably sports. However, the European Commission (EC) has intervened to enforce the regulation in the realm of sports at times, once stopping preferential tax treatment of at least one Spanish club. This does not seem to play a large role in Germany, though. I surveyed the EC's state aid database related to keywords football, soccer, stadium and arena which yielded few cases in

additionally inflate financing costs and potentially involvement of taxpayer money. Numbers in Figure 2.2.3 are therefore to be understood as ex-post financing costs whenever available.

Figure 2.2.3: Average financing share of stadium modernization



Notes: This graph depicts the average financing share of stadium construction or modernization efforts by different stakeholders between 2006 and 2023 according to hand-collected data. Data sources are minutes of local council meetings, statements by clubs, press articles and Wikipedia. Please confer Section 2.3 for more detail.

If a tenant club becomes unable to pay the agreed amount after relegation as a consequence of deteriorating revenues outlined above, municipalities face an unbudgeted increase in contributions that needs to be financed. Some rent agreements even specify lower payments in lower divisions or lower attendance ex-ante (Kicker, 2021). Anecdotally, especially prudent municipalities take precautions by building up reserves for such a case. However, these reserves tend to be limited to one or two years in a lower division and far from all municipalities are this foresighted or unconstrained financially (Esslinger Zeitung, 2016).

Even without pre-existing guarantees by municipalities, a public injection of capital might be the only viable option. Besides football, stadiums often offer limited alternative commercial use. While some can be re-purposed for example as concert venues, the characteristic layout and repeated nature of well-attended matches gives the club close to a monopsonist status. Refusal to guarantee financing or rent renegotiation might thus be even more detrimental to public finances in the long run, since an insolvent club, which potentially faces a withdrawal of license by the DFL or DFB, represents an even greater financial loss.

Besides this financial dilemma, reasons for supporting the local club with taxpayer money

Germany. These overwhelmingly resulted in “Decision not to raise objections.”

include the narrative that the increased media attention associated with professional football puts municipalities on the map and might thus benefit the municipality in other ways. Singleton et al. (2024) supports this logic by showing that relegation leads to lower student enrollment in local universities due to the decreased visibility. Municipal councils also debate to what degree aiding the local club represents measures that support youth work and popular sports and are therefore the responsibility of the municipality.

It might also be good politics to get involved in football. Mayors are frequently photographed at stadiums' groundbreaking ceremonies, exploiting football's popularity. In the analysis below, I will document that political dividends from modernization after promotion tend to be larger than the political penalty after relegation. There is also anecdotal evidence of local public officials serving leadership roles at football clubs. Though referenda on public involvement in stadium financing are rare and thus quantifying explicit popular support is challenging, a majority voted in favor in three out of four cases, suggesting it is not only a small group of well-organized and outspoken fans that have corresponding preferences. It might therefore not come as a surprise that in my sample 12 out of 50 clubs playing in a privately majority-owned stadium received explicit stadium-related public financial backing at least once. It may be surprising how involved local governments are, given that professional football is not a traditional public good. However, German municipalities often invest in leisure services beyond core government functions, including theaters, pools and amateur sports facilities.

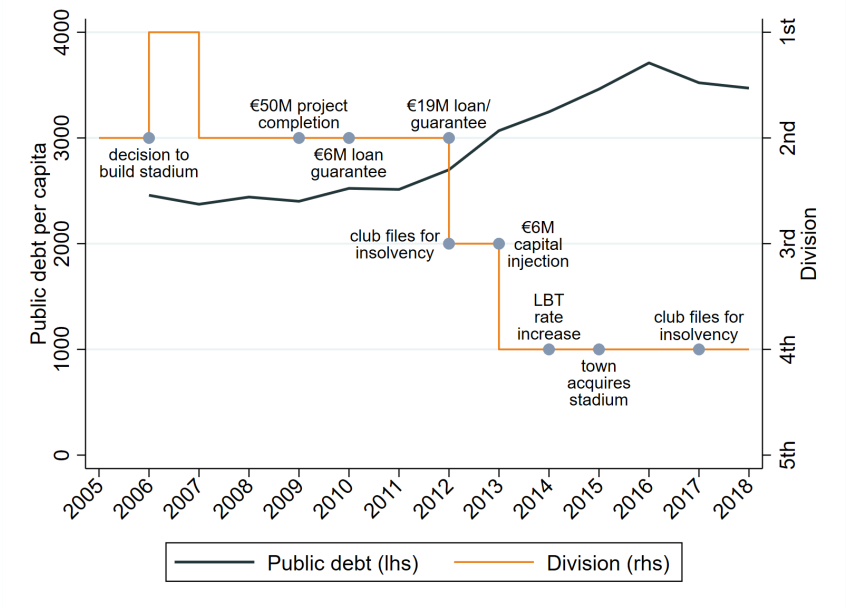
Ultimately, I will not be able to completely disentangle the reasons behind the fiscal effect of relegation. Suffice it to say that there are a wide range of possible explanations for deteriorating public finances following relegation: e.g., direct tax revenue decline, stadium financing and state-owned enterprises' losses of revenue from less economic activity around match days.

Case study

To fix ideas, a brief case study of one municipality, Aachen, which has a population of 245,000 and its local football club, Alemannia Aachen, is depicted in Figure 2.2.4. The horizontal axis represents a timeline. The left axis depicts Aachen's public debt per capita and the right hand side shows which division the local club belonged to at the time. Noteworthy events are added in the graph. They are collected from reports on the city of Aachen's annual financial statements (City of Aachen, 2024).

After the club was promoted to the highest division in 2006, the club decided to build a new stadium. This endeavor was explicitly backed by the municipality, which provided the property and developed the public infrastructure around the area, as well as the state

Figure 2.2.4: Case study: Aachen



Notes: This graph depicts a timeline of city Aachen's public debt development (left-hand side) and the division its most successful football club played in at the time (right-hand side). Notable financial events are added.

of North Rhine-Westphalia, which provided a loan guarantee of €23 million. The club was relegated to the second division after only one year in the first division. In 2009, the stadium was completed at a total cost of around €50 million. The club was the sole owner of the stadium. A year later, the club had difficulties to re-finance stadium loans. Even though the municipality was not directly involved in financing the stadium, it nevertheless decided to provide a loan guarantee of €5.5 million and wrote down assets at a state-owned enterprise related to the stadium. In the beginning of 2012, the municipal council approved further financial assistance in the form of loans and loan guarantees in the order of €19 million. A few months later, the club was relegated to the third division. As a consequence, the club filed for insolvency only a few months later in November 2012. The municipality subsequently wrote off part of its recent €19 million assistance. During this time, the municipality's per capita public debt increased faster than it had before.

Another relegation in 2013 was accompanied by increasing public debt. The municipality of Aachen endowed its stadium firm with another €6 million in order to repay a loan granted to the club by the local savings bank, which the municipality owns. Aachen also agreed to subordinate debt status for €8 million in outstanding debt to the club. In 2015, the municipality acquired the club's stadium for the token amount of €1 and from this point on was responsible for yearly maintenance costs of around €2 million. To generate alternative income from the venue, a casino moved into the premises. Due to lasting athletic failure, the club subsequently remained in the fourth division and filed

for insolvency in 2017 again. During the time period under consideration, public debt increased by approximately €1,000 per capita to €3,471 per capita in 2018, which is entirely made up of short-term liquidity loans as opposed to investment loans.

To shed light on the motives behind granting such generous and lasting public financial support to the club, it is insightful to highlight some remarks from the meeting minutes of the council decision to grant financial assistance on March 7, 2012 (City Council of Aachen, 2012, translation by author): Stressing the symbolic importance, one council member notes that "the city's loss of reputation resulting from the end of professional football would be immense" while another strikes a more aggressive tone by saying that "he who believes [financial assistance should not be granted] simply is not part of this city." Yet another makes the "personal interdependence between city leadership and the leadership of Alemannia" responsible for the dire situation. Stressing the hopes that the city might not end up being on the hook if only things improve, one states that "there is a chance that the burden of [loan guarantees] will never materialize, even if the risk is always looming" while another sees the municipality facing a "lose-lose situation" by either throwing good money after bad or facing the potential (financial) repercussions of a downfall of the club. Another member states the "team's athletic success alone is the fate of the city's budget." Alluding to the prevalence of such a municipality-club nexus, a council member sees "little consolation that we are not the only city council that has to decide on such a case." Finally, the decision to grant financial assistance was passed with 43 votes in favor and 24 against the proposal.

2.3 Data

The main fiscal and demographic variables on municipalities are provided by Bertelsmann Stiftung's *Wegweiser Kommune* between 2006 and 2022, which are derived from administrative data. These include, among others, tax revenues, tax multipliers, debt stocks, expenditure items and intergovernmental transfers for municipalities that have a population of at least 5,000. Number of inhabitants and other population characteristics are also available. To supplement this data set, I use data from the states' statistical offices like local election results between 2002 and 2019. For some states, longer time periods and a wider definition of public debt that incorporates debt held by publicly owned enterprises is available. The statistical office of North Rhine-Westphalia, the most populous state, provides especially detailed information on its municipalities' budgets and is therefore included for closer analysis.

Data by the federal statistical office allow me to identify municipalities that were subject to territorial changes due to municipalities' merging or separating within the estimation

period. The federal statistical office also provides the national consumer price index to convert nominal values into 2020 Euros.

Municipalities' status of being subject to a budget consolidation program was hand-collected from contacting municipalities and minutes of local council meetings.

To explore the role local savings banks (*Sparkassen*) play I hand-collected the information if a bank is headquartered in a municipality and if the savings bank's president of the administrative board is the mayor of the respective municipality.

Regarding data on football outcomes, I collect the top four divisions' final tables, i.e., rank, victories, draws, losses, goals and assigned points, starting in 2000 from websites *Transfermarkt.de* and *Kicker.de*. To be able to account for unusual relegation events, I also include information on nonstandard changes in division: This includes the withdrawal of license due to financial reasons, voluntary departure from a division or a team's dissolution. The current number of club members is also included.

I hand-collect information on stadium ownership between 2006 and 2023 as well as expenditure on the local stadium and the share of funding by municipality, club, investor and other public entities. Sources are the minutes of local council meetings, statements by clubs, press articles and Wikipedia. Information on stadium ownership is rarely missing and available for 170 of 178 clubs, while coverage of stadium funding is patchier. Data on the total costs of stadiums is available for 108 municipalities and the funding shares among stakeholders are available for 99 municipalities. Exact numbers from administrative documents are hard to come by, especially for early years and small municipalities. I cannot rule out a considerable amount of mismeasurement here. However, I have no reason to believe that measurement error is correlated with treatment, which would bias results.

The baseline sample is a panel of 178 municipalities hosting a professional football club at any point between 2000 and 2024 in 13 out of 16 states because three city-states are left out of the analysis.

2.4 Analysis

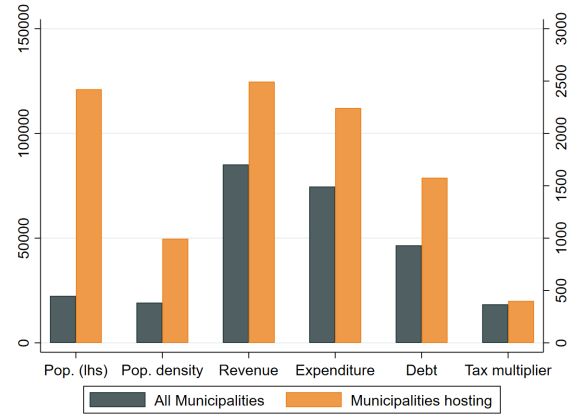
2.4.1 Summary statistics

Municipalities that hosted a football club in one of the first four divisions at any time between 2000 and 2024 constitute my sample. Those naturally differ from the average Germany municipality. In fact, all of the 40 largest German cities–except for city-states–are included. Figure 2.4.1 illustrates these differences by plotting the mean of some

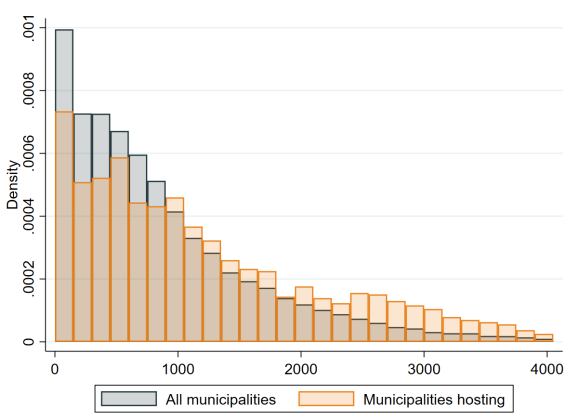
key characteristics for all municipalities above 5,000 inhabitants and contrasting them with averages of the municipalities in my sample. Population and density of sampled municipalities are many times larger. Public finance variables in per capita terms also differ substantially: Average revenue, expenditure and public debt per capita are all larger for sampled municipalities. Public expenditure of around €1,500 and €2,200 per capita, respectively, speak to the relevance of local governments in Germany.

Figure 2.4.1: Summary statistics by sample

a) Means by group



b) Public debt per capita distribution



Notes: The panel to the left depicts municipalities' average values of key characteristics, once for all German municipalities with more than 5,000 inhabitants and once for the estimation sample, i.e., municipalities that host a football club. The panel to the right presents a histogram of the distribution of public debt per capita for both groups, respectively.

The local business tax multiplier that determines the local business tax rate is also slightly larger. The enhanced public finance variables are partly driven by district-free cities that have dual administrative roles as municipality and county simultaneously. Among the approximately 100 district-free cities, 73 are in the sample. Turning to one outcome variable of primary interest, sampled units have a fatter tail in the distribution of debt per capita in Figure 2.4.1b. Figure 2.A.4 in the Appendix presents the same characteristics by division between 2009 and 2022, when the current league structure was in place. More successful clubs play in more (densely) populated municipalities that have larger budgets and debt.

In sum, my sample is characterized by larger and fiscally more involved municipalities that are more indebted than the typical municipality. This potentially limits external validity, i.e., the degree to which results from this paper can be applied to German municipalities in general, though more than half of sampled municipalities have populations smaller than 55,000 in 2021. A more detailed presentation of the sample's summary statistics can be found in Table 2.A.1 in the Appendix.

2.4.2 Empirical model

Treatment definition

For the most part, the analysis will rely on event study estimates due to their straightforward methodology and interpretability as well as visual presentation of dynamic effects. I define relegation as a treatment event if 1) it happened from one of the top four male football divisions.[5] This strikes a good balance between the number of events and the plausibility of an economically significant effect because, as argued above, financial stakes and potential spillovers into public finances decrease at lower tiers. Heterogeneity analysis will reveal that it is above all the first two divisions that drive the results.

Relegations are further restricted to 2) those happening to the first team of the "leading" club in the municipality. This helps keeping the number of events low and measuring the effect of relegations of clubs that have impact by sheer size. Especially larger cities tend to host multiple professional football clubs and some clubs operate a second team. Therefore, I select for each municipality the club observed longest in the sample starting in 2000. If more than one club is identified, I use the number of club members in 2023, i.e., a proxy for number of fans and popularity, as a tie-breaker. This yields a total of 361 potential events.

Furthermore, relegations must 3) not be due to exceptional events. That is, all relegations not due to athletic merit, e.g., withdrawal of license for financial reasons or voluntary withdrawal, are not considered an event. Including those as events would open the door to potential selection issues that could bias the estimates since it is the plausibly exogenous variation in athletic achievement that I want to exploit. Note that a relegation possibly preceding the withdrawal of a license is still considered treatment. This would include in the treatment effect cases where municipalities might have been unable or unwilling to support the club, eventually resulting in a withdrawal of license. For a more detailed discussion, see Section 2.4.7. This treatment selection reduces the number of potential events to 319.

Lastly, 4) relegations must not be immediately preceded or succeeded by a promotion to a higher division. There are quite a number of clubs that routinely switch divisions. These are excluded in order for fiscal pressures to be sufficiently large by not being limited to one season and exceeding potential cautionary measures tailored to a short period in a lower division. Figure 2.A.2 in the Appendix shows that among relegated clubs around 27% were promoted the year before and around 15% are promoted to the same division again the next year. This reduces the number of potential events to 130.

[5] In 2008, *3. Liga* was introduced. Before 2008, there are three divisions in my sample. *Regionalliga* is the lowest division throughout the sample.

Thus, out of 178 clubs 90 are never treated, 58 are treated once, 22 are treated twice, 4 are treated three times and 4 are treated four times. This includes a time span of 23 years since this treatment definition requires observing one lead and one lag of football events observed from 2000 until 2024. Promotion events are defined analogously. To illustrate treatment distribution, Figure 2.A.3 in the Appendix depicts a map of Germany including state borders, which illustrates the sample and treatment frequency. Sampled municipalities are distributed widely across Germany and correspond closely to population density.

Table 2.4.1: Balance of annualized growth rates from 1995 to 2000

	Treatment			Control				
	N	Mean	SD	N	Mean	SD	Difference	p-value
Population	88	-0.001	0.010	90	0.000	0.009	-0.001	0.31
Tax capacity	88	0.022	0.033	90	0.028	0.053	-0.006	0.35
Income tax revenue	88	-0.025	0.048	90	-0.016	0.048	-0.009	0.21
Business tax revenue	88	0.053	0.110	90	0.051	0.094	0.003	0.87

To get a first glimpse of treatment allocation among municipalities, growth rates of key municipality characteristics are presented in Table 2.4.1. The balance table compares annualized growth rates between treated and control municipalities from 1995 to 2000, before the baseline sample starts in 2006 or football events are observed starting in 2000. The selection of these particular characteristics reflects data availability in this time period. Neither growth in population, tax capacity,[6] nor tax revenues from income or business activity are significantly different in the group of municipalities that will be treated at least once and the group of municipalities that are never treated. Based on these observables, treated and control municipalities were not on different growth paths before the estimation period, which is a first indication of quasi-exogenous treatment assignment.

Empirical model

After having defined treatment events, the baseline empirical model is an event study design

[6]Tax capacity (*Steuerkraft*) is a fictitious statistic that is supposed to reflect a municipality's underlying tax capacity from all revenue sources, independent of the actual local business and property tax multipliers, i.e., local tax rates.

$$
\begin{aligned}
y_{it} = \alpha_{\underline{j}} \sum_{j=-\infty}^{\underline{j}} r_{it}^{j} + \sum_{j=\underline{j}+1}^{\overline{j}-1} \alpha_j r_{it}^{j} + \alpha_{\overline{j}} \sum_{j=\overline{j}}^{\infty} r_{it}^{j} \\
+ \beta_{\underline{j}} \sum_{j=-\infty}^{\underline{j}} p_{it}^{j} + \sum_{j=\underline{j}+1}^{\overline{j}-1} \beta_j p_{it}^{j} + \beta_{\overline{j}} \sum_{j=\overline{j}}^{\infty} p_{it}^{j} \\
+ X_{it}'\gamma + \mu_i + \theta_{st} + \varepsilon_{it},
\end{aligned}
\tag{2.1}
$$

which regresses outcome variable y_{it}, most notably debt, of municipality i in year t on two sets of event indicators, r_{it}^{j} and p_{it}^{j}. These are relegation r_{it}^{j} and promotion p_{it}^{j} events of the leading football club in municipality i as defined above.

More specifically, the event indicators are included as leads and lags for time period j relative to the treatment event. This allows estimation of a dynamic effect in the form of the lags of events defined as those for which $j \geq 0$. The leads of events defined as those for which $j < 0$ are informative of pre-trends and anticipation effects. The overall effect window that will be estimated can vary, but in the baseline specification is four years before and seven years after an event, i.e., $\underline{j} = -4$ and $\overline{j} = 7$. This choice of $\underline{j}$ and $\overline{j}$ will lead to the omission of years later than 2019 due to the leads and years before 2008 due to the lags since football treatment data are available between 2001 and 2023. The coefficients of interest will be the set of estimates on the leads and lags of the event indicators, α_j and β_j.

The coefficients on the first lead, i.e., α_{-1} and β_{-1}, are normalized to zero. With a slight abuse of notation, the summations from negative infinity and until infinity sum up all indicators outside or at the corner of the effect window and attach to it a single coefficient. This is what is called "binning" endpoints in the event study literature (Schmidheiny & Siegloch, 2019). It ensures that the estimated dynamic effects $\hat{\alpha}_j$ and $\hat{\beta}_j$ can be interpreted as the difference between treated and control units relative to the year before an event takes place. The interpretation of endpoint coefficients with subscript $\underline{j}$ and $\overline{j}$ is thus different from the estimates within the effect window as it represents the cumulative effect of all events yet to come or already realized.

To give an example, r_{it}^{2} is equal to one if the year is t and the club assigned to municipality i was relegated two years ago and, conversely, is equal to zero if there was no relegation. If a club was relegated in 2001 and the year is 2015, then this event will be part of the binned endpoint variable as well as all other events taking place in 2011, i.e., $2015 - \underline{j}$ or earlier. Since I do not expect relegation and promotion to have a symmetric effect, I include both sets of indicators r_{it}^{j} and p_{it}^{j} separately in the regression equation.

To account for potentially unobservable, time-invariant and municipality-specific char-

acteristics, Equation (2.1) includes municipality-level fixed effects μ_i. State×year fixed effects θ_{st} control for state-specific time-varying shocks as municipalities within a state might be subject to idiosyncratic shocks, e.g., state-level reforms.

One particular worry in this context are states' debt reduction programs that take on part of their municipalities' debt, especially the short-term kind. However, the exact timing and amount of debt forgiveness is not perfectly observable for all states and years. Often only highly indebted municipalities qualify and thus municipalities even within the same state are affected differently, potentially biasing estimates $\hat{\alpha}_j$ and $\hat{\beta}_j$.

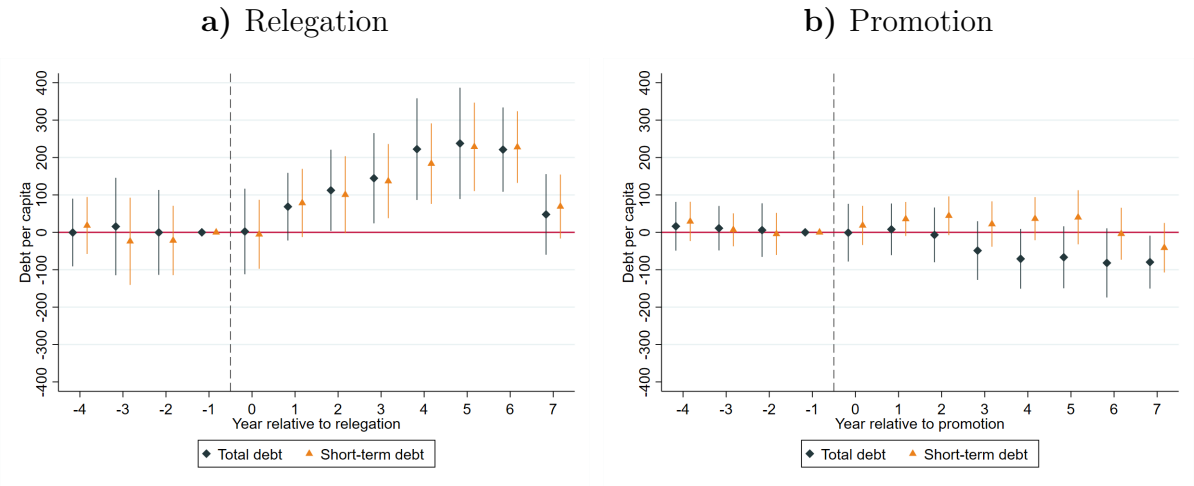
For this reason, the preferred specification of Equation (2.1) includes control variables X'_{it} that aim to address this issue. These take the form of additional, potentially time-varying municipality fixed effects δ_{it}. That is, varying municipality-specific intercepts taking on a different value each time short-term debt decreases relatively by at least 20% and absolutely by at least €200 per capita from one year to the next. This approach allows controlling for the kind of structural breaks associated with debt relief. More gradual debt relief would not be picked up by this approach and instances when municipalities drastically reduced debt on their own in a single year would be flagged incorrectly. However, such misspecification should not be correlated with treatment, i.e., football events. Section 2.4.3 will also present specifications without these fixed effects.

Errors ε_{it} are clustered at level of treatment assignment. According to Abadie et al. (2023), there are two reasons for clustering. One is adjusting for unit sampling uncertainty and the other is adjusting for the variability introduced by the treatment assignment mechanism or design. There is no sampling uncertainty in my setting because in the selected time period, I observe all potentially affected units. However, my estimands depend on potential outcomes and those are determined by the treatment assignment mechanism. That is, I need to adjust errors for division×year×relegation status clusters.

2.4.3 Results

In what follows, visual representations of coefficients estimated according to Equation (2.1) are presented. Accompanying regression tables can be found in the Appendix. Besides regression results, the tables present the outcome variables' mean value one year before treatment. All confidence intervals throughout the paper are 95% confidence intervals. Since a number of key public finance variables can take values of zero and can even be negative, I refrain from taking the logarithm of variables and instead focus on per capita values. Especially with respect to debt, it is each citizen's average financial burden that matters. Worries that results might be driven by population changes in variables' denominator will be addressed in Section 2.4.7.

Figure 2.4.2: Debt response



Notes: This graph depicts two event studies, based on Equation (2.1). The time on the horizontal axis is years relative to a relegation event in the panel to the left and a promotion event in the panel to the right. The dependent variables are total public debt per capita and short-term debt (or liquidity loans) per capita, which is a sub-category of total debt. Confidence intervals are computed at the 95% level.

Figure 2.4.2, which is based on regression results in Table 2.A.2 in the Appendix, presents results for public debt per capita as the outcome variable. For relegation on the left-hand side, there are flat pre-trends and a consecutive increase in public debt per capita starting one year after relegation. An incremental increase in debt appears plausible because the financial strain from clubs' potentially lower loan payments, stadium rent or public asset write-offs needs to be absorbed each season. The surge in debt is driven by an increase in short-term debt, which is a sub-category of overall public debt. Results for capital expenditure loans, which is the other sub-category, is shown in Column 2 of Table 2.A.3. There is no statistically significant effect on investment loans. It is plausible that the debt response is driven by short-term debt since there is, as will be shown, no capital expenditure taking place, but instead a shortfall materializes that needs to be addressed quickly. Note that these loans are not backed by any asset as investment loans would be. In this regard, the excess debt from football is unproductive as opposed to, say, taking on debt to repair roads.

The debt response reaches its maximum after five years at €238 per capita and then plateaus. Considering longer post-treatment horizons below will reveal a trend reversal in debt accumulation after five years. At an average pre-treatment debt level of €2,226 per capita (cf. Column 1 of Table 2.A.2), this would represent a 10.7% increase after five years. Note that this relative increase compared to the control group is due to excess debt of treated municipalities and only minor increases of debt in control units. The insignificant binned endpoint estimate in year seven after treatment hints at long-term debt consolidation, but cannot be interpreted easily. Longer time horizons are considered

in Section 2.4.6.

In order to contextualize the treatment effect's size, summary statistics of residualized debt per capita are presented in Table 2.4.2. Residuals are based on regressions controlling for municipality fixed effects μ_i and state×year fixed effects θ_{st} over 12 years, as in Figure 2.4.2. This way, the variation within municipalities while controlling for state-specific shocks can be studied.

Table 2.4.2: Standard deviation of residualized debt per capita

	N	SD
Full sample	2,136	325
Treated	1,056	355
Never treated	1,080	293

Residuals are based on regressions on municipality-level fixed effects μ_i and state×year fixed effects θ_{st}.

The standard deviation is around €325 per capita for residuals of the entire sample. The treatment effect estimate five years after relegation of €238 is not statistically different from this value (p-value 0.21). Distinguishing by treatment status, treated municipalities have a larger within-unit variation of debt conditional on the fixed effects than control group municipalities, which might also be driven by treatment itself. Thus, the debt shock measured here is neither substantially larger nor smaller than typical variations in public debt, which supports external validity in terms of treatment size. Alternatively, Figure 2.A.5 in the Appendix shows the distribution of within-municipality six-year debt changes per capita. The treatment effect is illustrated by the dashed line and it does not stand out from the distribution, representing the 76th percentile of six-year changes.

Turning to clubs' promotion to higher divisions on the right-hand side of Figure 2.4.2, both debt variables evolve in different directions, but remain statistically insignificant except for the binned endpoint estimate on total debt. This result would suggest that adverse events are at least partially absorbed by municipalities while they do not share the benefits of positive events in terms of debt. Although municipalities do not seem to benefit based on other variables like business tax revenue as well, later specifications will sometimes find a negative effect of promotions on public debt. At no point does this effect reach the scope of the relegation response, however.

2.4.4 Debt response by relegation closeness

Above, all municipalities that host a club that has not been relegated comprise the control group. However, one threat to causal identification is that the estimated effect could be

confounded by characteristics changing by clubs' relative success within a division. If, for example, some municipalities have the resources to provide out-sized support, which results in more athletic success, this could bias my estimates upwards as municipalities who struggle financially might not be able to do so.

As the relegation decision rule is observable, i.e., points scored during a season, I can corroborate the event study design results based on Equation (2.1) by refining the control and treatment groups further to those municipalities which barely escaped or were closely relegated in a given year. Defining groups in such a way enhances the plausibility of the exogeneity assumption further. Since there is a plausibly random element in which team exactly is relegated with respect to public finance figures, teams (and corresponding municipalities) that score a similar amount of points might be even better suited to provide a valid counterfactual than, for example, perpetually successful ones.

To that end, I normalize clubs' final points at the end of each season according to

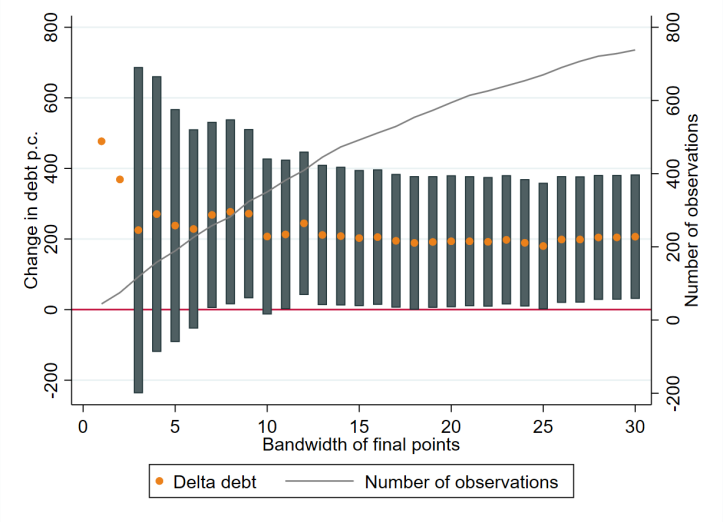
$$P'_{itl} \equiv P_{itl} - \frac{1}{2}\left(\max_{i \in R_{tl}} P_{itl} + \min_{i \in S_{tl}} P_{itl}\right), \tag{2.2}$$

where P'_{itl} is defined as normalized points of football club in municipality i in year t in league l, P_{itl} are original points, R_{tl} is the set of relegated teams in league l in year t and S_{tl} is the set of non-relegated teams. That is, normalized points are a team's final points subtracted by the number of mean points that the most successful team which was still relegated and the least successful non-relegated team scored in the same division and year. Teams that were relegated have a negative or zero normalized score in that year. Teams that were not relegated have a positive or zero normalized score in the respective year.[7] I can therefore test if using non-relegation in general as a control group as used above in Section 2.4.3 leads to the same results as using only marginally decided relegations as treatment events.

I plot differences in per capita debt changes over five years between closely relegated and closely remaining clubs' municipalities in Figure 2.4.3 as a function of normalized score bandwidth on the horizontal axis. The sample is incrementally expanded to include more instances by increasing the bandwidth of the normalized score, starting with very narrow, marginal cases. Figure 2.4.3 plots each respective estimate (left vertical axis) along the number of observations (right vertical axis) for each bandwidth. If only closely decided treatments are considered, the sample size is low, the point estimates are comparatively large and confidence bands are so wide that they are only shown for bandwidths above 2 points. As more and more cases of clubs are included, point estimates converge to an effect of slightly above €200 per capita, without an obvious gradient. This number

[7] Recall that relegations due to non-athletic reasons are excluded to begin with according to the treatment definition in Section 2.4.2. In this sense, final points scored are a perfect predictor of relegation.

Figure 2.4.3: Change in public debt per capita



Notes: This graph depicts differences in debt per capita 5-year changes from one year prior to relegation between municipalities hosting a closely relegated club and those that narrowly avoided relegation (left-hand side) as a function of relegation closeness. Relegation closeness on the horizontal axis is measured as the bandwidth of normalized points according to Equation (2.2). The axis on the right-hand side illustrates the number of observations included at each bandwidth.

is close to the event study estimate, confirming further the validity of the more general approach above.

Although not depicted here, the main results for other outcome variables estimated according to Equation (2.1), which will be presented in Section 2.4.5 below, remain intact qualitatively when estimating effects by focusing on close treatments, with only minor quantitative deviations. See, for example, results for liquidity loans and the business tax multiplier in Figure 2.A.6 in the Appendix. As the event study design is more informative with respect to pre-trends and the effect dynamics as well as incorporating repeated treatment, below the focus will again be on event study estimates. Thus, even municipalities hosting clubs far from being relegated seem to be a good control group for municipalities hosting relegated teams.

An additional reason to employ alternative methods like this non-parametric estimation is that the standard fixed effects estimator with staggered treatment assignment as in Equation (2.1) uses units that have already been treated as comparison units for units that have not yet been treated. This can introduce bias in the presence of treatment effect heterogeneity (Roth et al., 2023). Additionally, some municipalities are treated multiple times. In such cases, fixed effects estimators are not robust to heterogeneous effects and may be contaminated by the effects of other treatments (De Chaisemartin & d'Haultfoeuille, 2024). Section 2.4.7 will therefore employ the estimator that is unbiased in this setting, which was introduced in De Chaisemartin and d'Haultfoeuille (2024).

Another potential threat to causal identification, beyond the closeness of relegation, is that clubs may anticipate municipal support. Clubs in more generous municipalities might therefore take greater risks to avoid relegation. If clubs in fiscally strong municipalities benefit more often, this could introduce a positive bias in the estimates, as these municipalities are more likely to fall into the control group due to their sustainable public finances. Conversely, if anticipated support depends more on political willingness than on fiscal capacity, the effect could be biased downward. As shown in Figure 2.A.8 in the Appendix, the debt effect is more pronounced in highly indebted municipalities, suggesting that public support is driven more by political will than by available fiscal space. On top of this, additional funding does not easily translate into athletic success, particularly in the short-term. In fact, Table 2.4.3 in Section 2.4.6, which focuses on public contributions to stadium financing, indicates that financial support is not significantly associated with improved athletic performance even in the medium to long-term. Estimation bias is therefore not likely to play a large role and might even lead to underestimation of the debt effect.

2.4.5 Other fiscal variables

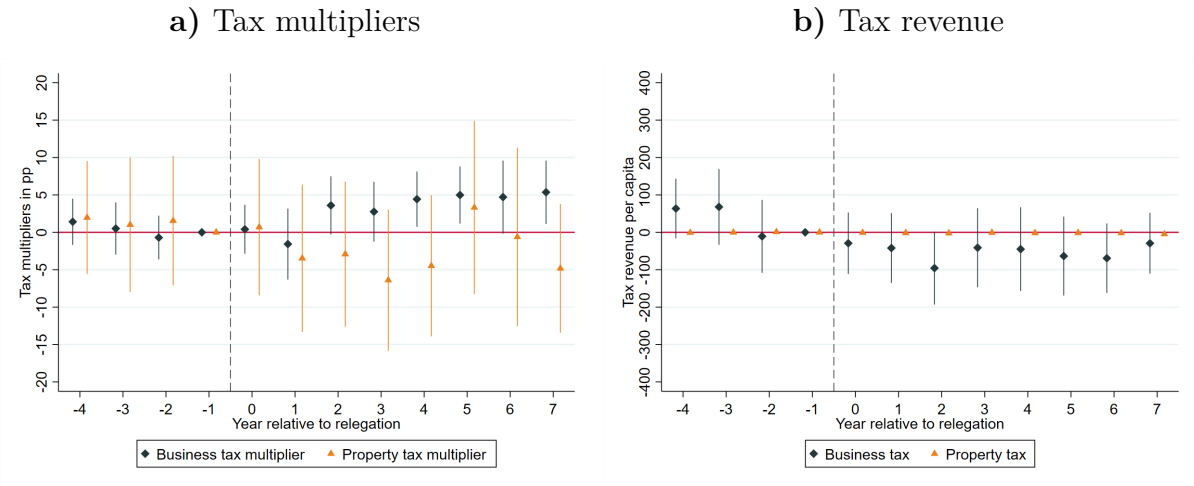
Focusing on tax multipliers and tax revenue, the left-hand side of Figure 2.4.4 depicts a significant and persistent increase in the business tax multiplier by 5 percentage points four years after relegation. This translates into a, considering the severity of the debt response, rather modest business tax rate change of approximately 0.18 percentage points or 1.2%.[8] Estimates for promotion are not shown, but included in the regression results in Table 2.A.2. There seems to be no effect in response to promotion. The other main tax rate, which taxes property, does not change significantly.

Since the tax base of the business tax is firms' net income, which can vary widely over the business cycle, municipalities' business tax revenue is highly volatile. This is reflected in the large confidence bands in the graph to the right in Figure 2.4.4. All estimates are not distinguishable from zero at the 5% level. The potential direct effect of less economic activity surrounding the football club in the first year after relegation does not seem to materialize in business tax revenues. In the medium-term estimates stabilize, which might be caused by the modest tax rate increase that becomes significant four years after treatment. In contrast, the property tax base is stable and since the property tax multiplier does not seem to change on average, revenue from this tax does not evolve differently.

Since the tax response appears to be modest, does spending change while or after incurring

[8] 5 percentage points $\times$ 3.5% = 0.175 percentage points and 5 percentage points/416% = 1.2%

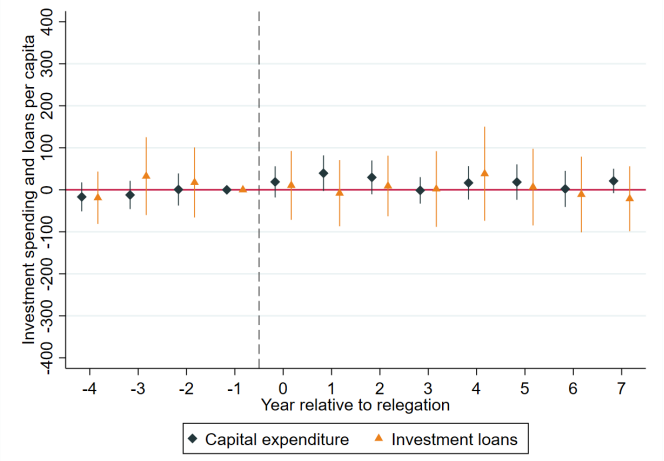
Figure 2.4.4: Tax multipliers and revenue



Notes: This graph depicts two event studies, based on Equation (2.1). The time on the horizontal axis is years relative to a relegation event. The dependent variables are the local business and property tax multipliers, which determine the tax rates in the panel to the left and tax revenues from both sources in the panel to the right. Confidence intervals are computed at the 95% level.

excess debt from football? Figure 2.4.5, which is based on regression results from Column 1 of Table 2.A.3, finds no effect on capital expenditure. This is also in line with unaffected investment loans as municipalities rely on loans to implement large investment projects. It is not the case that the excess debt from football crowds out spending on or borrowing for investment, even in the long-term.

Figure 2.4.5: Capital expenditure and investment loans



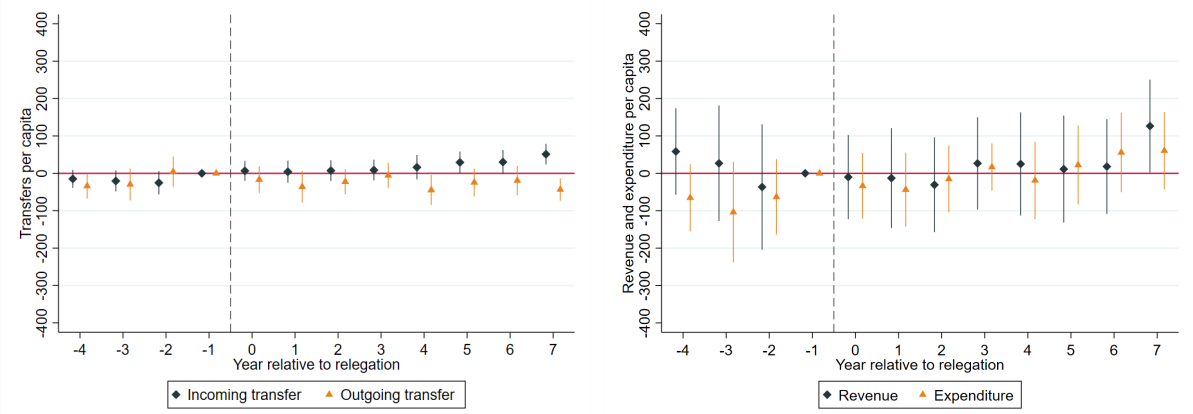
Notes: This graph depicts an event study, based on Equation (2.1). The time on the horizontal axis is years relative to a relegation event. The dependent variables are public capital expenditure per capita and the stock of public investment loans (as opposed to liquidity loans) per capita. Confidence intervals are computed at the 95% level.

Germany's federal structure includes a variety of vertical and horizontal intergovernmental transfers between jurisdictions, which often constitute a substantial part of mu-

nicipalities' budget. Municipalities are also obliged to share part of their revenue with upper levels of government. The left graph in Figure 2.4.6 plots incoming and outgoing government transfers, respectively. Since debt is not a determinant of intergovernmental transfer allocation mechanisms and it seems to be primarily debt that is affected after relegation, it comes as no surprise that incoming intergovernmental transfers do not change significantly. Point estimates of outgoing intergovernmental transfers are negative, but significant only sporadically. This could be driven by (insignificantly) lower business tax revenues because outgoing intergovernmental transfers are a function of tax revenues. Intergovernmental transfers thus do not seem to confound the results on debt.

Figure 2.4.6: Intergovernmental transfers & overall budget

a) Intergovernmental transfers **b)** Overall revenue & spending



Notes: This graph depicts two event studies, based on Equation (2.1). The time on the horizontal axis is years relative to a relegation event. The dependent variables are per capita incoming government grants and grant payments in the panel to the left and per capita total revenues and spending to the right. Confidence intervals are computed at the 95% level.

Overall budget revenue and spending are depicted in the right graph in Figure 2.4.6. There seems to be no differential development of these aggregate figures. Note that exceptional financing transactions such as a possible injection of capital into a stadium-holding enterprise are not included in this definition of spending. It merely reflects cash-based, current administrative expenditure. The difference between revenue and expenditure is the primary balance, reflecting a larger budget surplus four and three years before relegation compared to the pre-treatment year. This is also reflected in Column 5 of Table 2.A.3. A decrease in the budget surplus relative to the control group before treatment would pose a threat to causal identification. This seems to be driven by a statistically insignificant decrease in business tax revenue. As those tax revenues are highly volatile and this is a finite sample, this might reflect chance more than any underlying structural problem of the identification strategy.

Not depicted here, but presented in the last Columns of Table 2.A.3 are results for different

spending categories to study if particular spending items might be affected directly or later on as a consequence of excess debt from football. Social spending and spending on staff and youth do not respond significantly, with social and youth spending exhibiting positive point estimates some years after relegation. This might reflect mandatory spending, which the municipality has only limited discretion over. Alternatively, municipalities possibly assume some youth work carried out by the club prior to relegation. There is no evidence for a pronounced shift in the spending mix, at least considering the limited set of variables of still highly aggregated spending items observed here.

One state, North Rhine-Westphalia, which contributes 38 municipalities to the sample, provides very detailed spending items between 2009 and 2020. This allows analysis beyond the rather broad spending items available for all states. Table 2.A.4 in the Appendix replicates the debt response for this state. As can be seen in Column 1, the debt response is even more pronounced within this state than in general. The remaining columns show regression results of the five largest spending items, followed by four spending categories where municipalities enjoy considerable discretion and which are less related to mandated livelihood security. For none of the nine categories is there a significant and lasting negative effect. At least in North Rhine-Westphalia, there is no spending austerity even in very detailed and supposedly flexible sub-categories and long-time horizon, emphasizing the lack of a spending response in the more general case above.

In sum, most budget items are not affected significantly by relegation, even in the short term. The business tax rate is increased modestly, but persistently. In contrast, the shock to public finances materializes almost exclusively as debt, which increases ultimately to more than €200 per capita. Potentially increased financing costs in the form of interest payments do not show up significantly in aggregate spending, perhaps due to low interest rates. There also seem to be no second-round effects such that excess debt from football causes austerity measures further down the line. One might expect excess debt from football to limit the fiscal scope for public spending as taking up more loans might be more difficult and interest on excess debt burdens the budget. Aside from the modest tax rate increase, budgetary effects on the real economy seem to be rather limited. As the next section will show, state-level debt reduction programs help explain the limited impact on other budget items. Further analysis explores heterogeneous groups with particularly strong debt responses and finds qualitatively similar results within those subgroups.

2.4.6 Mechanisms and Heterogeneity

Public debt definition

Extensive definitions of public debt include not only debt in the core budget, but loans held by all entities that are controlled by (local) governments as well. These state-owned enterprises (SOEs) might be used as a vehicle to conceal the true degree of indebtedness. The other way around, it could be the case that following a relegation, municipalities merely swap one kind of debt with the other if they infuse fresh capital in their stadium-owning enterprise, which happened in the case study in Section 2.2. If SOE debt is not taken into account, the estimated debt response could be under- or over-estimated, respectively.

States Baden-Württemberg, Bavaria, North Rhine-Westphalia and Saxony make municipalities' SOE debt available. Figure 2.A.7 in the Appendix displays regression results for those states by different debt definitions, where extensive debt is the sum of core and SOE debt. At least in this sample, which produces similar point estimates for core debt compared to the baseline estimates in the full sample, the extensive definition of debt increases slightly more than core debt and SOE debt changes minimally. Thus, incorporating a wider definition of public debt does not fundamentally change the result. If anything, the debt response is under-estimated in the baseline.

Response by prior fiscal performance

In order to test for heterogeneous treatment effects, I follow two strategies. To test for time-invariant factors like the effect of legacy debt at sample onset, median sample splits are conducted. To test for time-varying characteristics, treatment is re-defined to be conditional on the respective criterion. This criterion might be, for example, above median primary balance in the year before relegation.

Figure 2.A.8 in the Appendix conducts a sample split between those municipalities that had public debt below and above the median value in 2006, i.e., the first year when debt is observed. Those municipalities with below-median debt in 2006, which are in a more comfortable budgetary position to begin with, do not experience a significant debt treatment effect while those with above-median legacy debt exhibit an effect that is larger than the baseline result. The effect thus seems to be driven by municipalities which have already taken on considerable debt. This could be driven by states that are more lenient in their handling of municipal debt. Alternatively, there might be high persistence where a few municipalities take on more and more debt.

As with all simple heterogeneity analysis, differential results might not be driven by

the particular variable analyzed and can instead be driven by confounding factors, e.g., different degree of fiscal oversight. Unfortunately, low sample size within states does not allow for a detailed analysis across states. However, it does not seem to be the case that all municipalities take on debt as a response to relegation to the same degree.

In another exercise, treatment is re-defined as having a below- and above-median primary balance in the year before relegation, respectively, in Figure 2.A.9 in the Appendix. This should not be driven solely by overall economic effects that heavily impact the primary balance because the median is taken in each year separately. Since fiscal data is only available between 2006 and 2022, the effect window is restricted to three leads and five lags to end up with a similar sample size as before. The debt response is pronounced for those exhibiting below-median primary balance the year before relegation, which indicates relative budgetary constraints even before treatment. There is no significant effect for those with above-median primary balance.

One possible explanation for these results is that fiscally more constrained municipalities already have exhausted other means of financing an adverse shock. If a municipality can finance a shock from the current budget, there is no need to take on extra loans. Variables legacy debt, primary deficit and population, cf. Columns 1 and 2 in Table 2.A.6, are highly correlated and drive the results. It is rather large municipalities that already have and are in the process of accumulating debt which are subject to the debt response.

Response by division

As explored in Section 2.2, absolute financial stakes are highest in the higher divisions. However, professional clubs from smaller municipalities tend to be smaller themselves and play in lower divisions. Thus, in per capita terms it is not clear how the public debt response changes by division. In Figure 2.A.10 in the Appendix treatment is conditional on being relegated from each of the top four divisions, respectively. Speaking broadly, there seems to be a decrease in the per capita treatment effect by division. While being relegated from the top two divisions evokes a similar effect, it is barely significant at any point when being relegated from the third division, similar to the fourth division. This is in line with the third division generally being considered the lowest professional football division in Germany.[9]

[9]Note that the treatment definition in this case represents a "horse race" as treatment effects from other divisions are grouped in with the control group and estimated treatment effects here should therefore represent a lower bound estimate.

Relationship to club

Municipalities differ in their relationship with the local club not only by public ownership of the local stadium, but also if municipalities participated in financing construction or modernization. For this reason, Table 2.A.7 in the Appendix repeats the event study analysis, splitting the sample according to both characteristics. Point estimates are comparable to the baseline specification in Column 1 when focusing on municipality ownership in Column 2. Estimates are attenuated when looking at cases when the stadium is not owned by the municipality. Therefore, municipalities are plausibly more involved if they own the stadium. There is, however, an even stronger contrast when distinguishing by contribution status. Municipalities that contribute to financing–and thus explicitly have skin in the game–experience a larger shock to debt in Column 4 than both the baseline and cases without contributions. The results imply that it is above all financial involvement that increases a municipalities' financial dependence on the local club's athletic achievement. This is in line with the case study of Aachen, where the municipality council approved financial assistance long before the stadium finally ended up in the municipality's hands.

Connection to savings banks

In Germany, *Sparkassen* are public savings banks that play a pivotal role in the country's financial landscape. Savings banks are wholly owned by municipalities, counties or local government associations. Municipalities often rely on local savings banks for financing and are, in turn, seen as attractive borrowers due to their low-risk status as their debt is implicitly backed by the state. The interdependence between municipalities and savings banks raises the question if the debt response differs by the particular relationship between the two entities. One observed proxy for closeness is the savings banks' appointed president of the administrative board, who holds considerable sway. As a consequence of consolidation, savings banks are usually owned by multiple municipalities and counties at the same time. This results in a rotating allocation of the board president position, which is usually filled by a mayor or county commissioner (*Landrat*) of the largest municipality in the ownership association. A reason for a differential debt response could be, on the one hand, that municipalities have easier access to borrowing if their mayor is influential in local savings bank matters. An adverse shock such as relegation can thus be financed with loans easily by the bank. On the other hand, savings banks might also be involved in financing the local football club. The dual role as mayor and board president might lead to mobilization of resources at the municipality to safeguard loans from the bank.

To analyze if the relationship matters, treatment is defined conditional on this job at the

time of relegation. Unfortunately, president switches over time within municipalities are rare in my sample so that they cannot be exploited empirically. To prevent this analysis from being driven by larger municipalities having a larger probability of filling the job of president and thus confounding the results, the sample is additionally restricted to a population range where the share of municipalities occupying the job is evenly split. As shown in Figure 2.A.11 in the Appendix, this happens to be a range between 45,000 and 245,000 inhabitants in 2006. This range comprises 35 treatment events without the mayor being president of the local savings bank and 37 with president job. There remains a population imbalance within this range: Average population for those with and without a mayor-president is 134,600 and 109,782, respectively. Note that the share of municipalities with district-free status is comparable across president status, at 66% among no president status and 70% among president status.

In Figure 2.A.12 in the Appendix the debt response is more pronounced when the mayor was administrative board president at the time of relegation. This differential response does not seem to be driven by larger municipalities because qualitatively the results hold in the restricted sample as well. If anything, focusing on similarly sized municipalities only increases the differential. Note that the differential response in debt is also accompanied by a differential increase in the local business tax multiplier. That is, a significant response is only observed for those who have president status in Columns 5 and 10 of Table 2.A.8, but not if mayors were not president. The fact that the tax response is also only present for connected municipalities and emerges only with a lag might suggest that this is done as a response to an increased debt burden instead of a direct reaction to relegation.

The severity of the differential response suggests that close ties to the local lender is an enabler of public debt indulgence. This analysis remains suggestive and cannot disentangle the direction of causation. Either savings banks provide an easily accessible credit line to the controlling mayor-president or savings banks have considerable legacy loans at stake with the local club and the mayor as the board president increases the probability of a bailout with taxpayer money as was the case in the case study in Section 2.2. Either way, this raises questions about the desirability of such a high degree of interdependence.

Long-run development

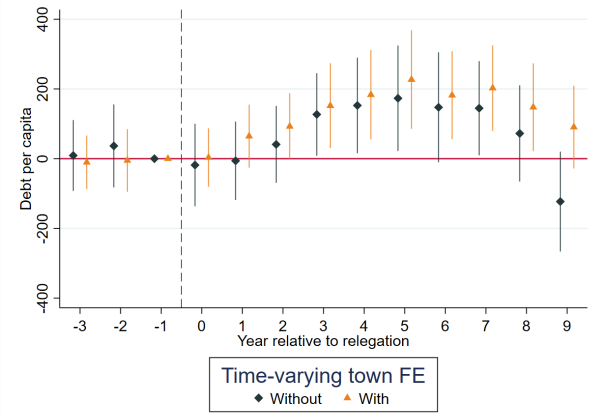
As illustrated by extending the port-treatment estimation window to 9 years and decreasing the number of leads to 3 in Figure 2.4.7, the excess debt incurred following relegation converges back to the difference in debt to the control group in the pre-treatment period eventually. This section explores potential reasons and mechanisms.

Municipalities' own debt consolidation measures were shown to be limited to a modest

business tax rate increase, which became significant roughly at the time the debt trend switched to a downward trajectory. A back-of-the-envelope calculation of the revenue effect corresponding to such a 0.2 percentage point business tax rate increase yields additional annual revenue of approximately €7.5 per capita. This, possibly coupled with additional revenues from net asset sales in later years (cf. below), cannot by itself explain the degree of debt consolidation observed five years after relegation.

One factor driving this could be the aforementioned debt reduction programs, in which states take on part of their municipalities' debt in order to stabilize local public finances. Typically, highly indebted municipalities qualify for such a procedure in exchange for mandated consolidation measures. If relegation increases debt, this might make affected municipalities more likely to be subject to a debt program.

Figure 2.4.7: Longer horizon of debt



Notes: This graph depicts an event study, based on Equation (2.1). The time on the horizontal axis is years relative to a relegation event. The dependent variable is public debt per capita. Compared to baseline estimations above, one lead is omitted and two lags are added. Two specifications include and exclude time-varying fixed effects as outlined in Section 2.4.2, respectively. Confidence intervals are computed at the 95% level.

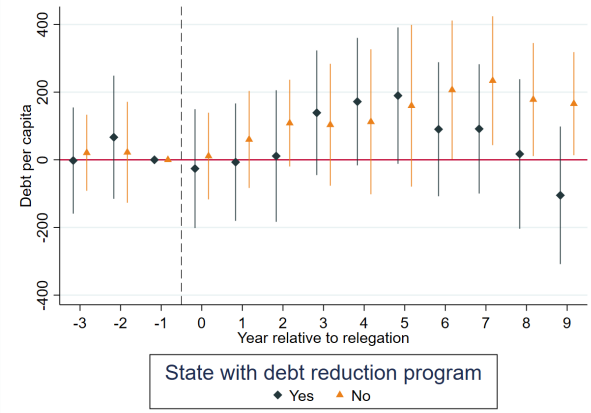
One way to study this is to control for debt reduction programs. As pointed out, data on which municipality was forgiven how much debt at what point in time is not available. For this reason, I employ time-varying municipality fixed effects that provide a new municipality-specific intercept each time a municipality might have been subject to a program.

There is aggregate information on which states conducted debt reduction programs in which years. This information can be combined with extraordinary yearly decreases in short-term debt such as 10%. Including a new, municipality-specific fixed effect for each unit that was in a state conducting a debt reduction program and additionally reduced short-term debt by at least 10% compared to the last year can partly control for the program. Results of including these targeted fixed effects are marked as triangles in

Figure 2.4.7. The treatment effect peaks at a higher debt level than the specification without debt program fixed effects and only starts to decrease in earnest in year 8 after treatment. Accounting for debt relief results in slower debt convergence. The binned endpoint estimate in year 9, that incorporates all subsequent debt development, does neither differ significantly from zero, but with a larger point estimate.

Another piece of evidence in support of the hypothesis that debt reduction programs play an important role is provided by doing a simple sample split. Regressions are run separately for municipalities in states that conducted a large program and those that did not.[10] Figure 2.4.8 illustrates the results of this exercise. Although confidence bands are mechanically much wider, point estimates paint a similar picture to above. Excess debt from football accumulates similarly in both groups of states, but does not converge to the pre-treatment figure in states that did not conduct a debt reduction program. Here, debt levels stay persistently elevated. This suggests there is at least some degree of debt communalization, constituting a fiscal externality as taxpayers in the same state but not necessarily from the particular municipality are afflicted. Municipalities in states with debt reduction programs ultimately do not bear the full financial burden incurred by relegation.

Figure 2.4.8: Debt by debt reduction program status



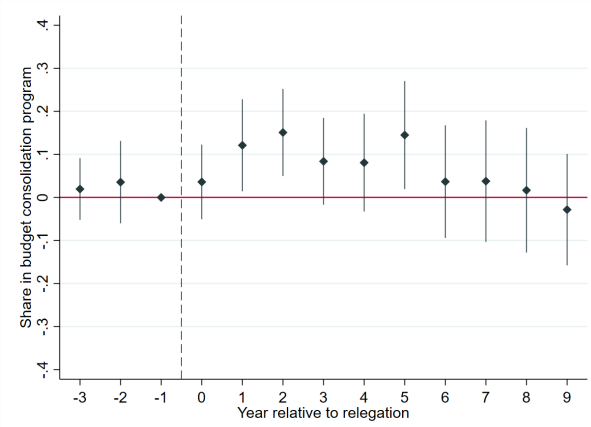
Notes: This graph depicts an event study, based on Equation (2.1). The time on the horizontal axis is years relative to a relegation event. The dependent variable is public debt per capita. Compared to baseline estimations above, one lead is omitted and two lags are added. The sample is split by states that conducted comprehensive debt reduction programs and those that did not. Confidence intervals are computed at the 95% level.

Turning to constraints on public borrowing, municipalities are subject to fiscal oversight by, depending on the state, the county, district or state. Besides confirming budgets, overseeing bodies have–in the interest of sustainable public finances–the authority to

[10] Only Baden-Württemberg, Bavaria, Brandenburg, Saxony and Thuringia did not have a large-scale debt reduction program in this period, cf. `https://kommunalwiki.boell.de/index.php/Konsolidierungshilfen`.

launch a tailored budget consolidation program (*Haushaltssicherungskonzept*), which is different from the debt reduction program above, but is often a precondition to enter one. Consolidation programs contain specific, mandatory austerity measures that the municipality has to pass and fulfill in the future. In some cases, municipalities also impose such a program on themselves. Information on exact measures taken is not observed. To get an idea which role fiscal oversight plays in this context, I rely on hand-collected data from municipalities' council information portals and information requests. Since many set the portals up only in the 2010s, there is some missing data especially in the earlier years.

Figure 2.4.9: Budget consolidation program probability



Notes: This graph depicts an event study, based on Equation (2.1). The time on the horizontal axis is years relative to a relegation event. The dependent variable is an indicator variable equal to one if the respective municipality was subject to a budget consolidation program at the time and zero otherwise. The sample is restricted to municipalities for which program status is observed in all relevant years. Confidence intervals are computed at the 95% level.

Though the analysis is limited to the extensive margin, i.e., if a consolidation program was opened and not its specific measures, Figure 2.4.9 is nevertheless insightful by showing the differential probability to be subjected to a budget consolidation program. The event study estimates exhibit a significant increase in the differential probability of around 16-17 percentage points one, two and five years after relegation and dissipating afterwards. This is indicative of fiscal oversight being on alert quickly in the first two years even though the full extent of debt accumulation is yet to come. The budget consolidation programs are not able to stop the debt surge on average in the short-term, but measures would need time to be implemented and debt might have been even more pronounced absent the programs.

Columns 6 and 7 in Table 2.A.9 in the Appendix show no significant change in the probability of entering a consolidation program around promotion events. However, Figure 2.2.2 documents a sharp increase in stadium modernization probability during the promotion year. This reflects the institutional framework, as higher divisions impose

stricter stadium requirements. If fiscal oversight seeks to prevent excessive municipal debt from football, this is the key intervention window. Municipalities commit substantial funds to stadium projects at this point in time. Once committed, it is difficult to reverse these obligations if relegation happens.

Given that debt levels converge back to the pre-treatment gap on average, which margins do treated or control municipalities employ to decrease the debt gap between them after 5-6 years? Besides responses outlined in Section 2.4.3 such as a modest, but permanent local business tax increase, other average consolidation measure responses remain elusive. Table 2.A.9, which contains this section's regression results, additionally focuses on the primary balance and net asset sales. While the primary balance does not improve for treated municipalities, there is a significant amount of net asset sales in a later year relative to control municipalities, which could contribute to consolidation.

In conclusion, there does not seem to exist a unique consolidation template. Initially, municipalities do not address galloping public debt and have a higher probability to incur a budget consolidation program. Debt relief dominates the consolidation path taken after 5-6 years, with modest measures taken by the municipalities themselves. Based on the previous results, municipalities fare comparatively well after a substantial medium-term debt increase, with slightly higher taxes, some net asset sales and possibly a temporary loss of autonomy in the form of the budget programs. Austerity in the form of expenditure cuts is not observed.

Adverse economic effects

This section explores if the local economy is adversely affected either by relegation directly or later through increased debt or the tax rate increase. The effects on local population levels, number of personal income taxpayers and local tax bases of the personal income and local business tax are estimated and presented in Table 2.A.10 in the Appendix. The tax bases serve as proxies for local economic activity because there is no data on local GDP. Note that data on the personal income tax is limited to eight years.

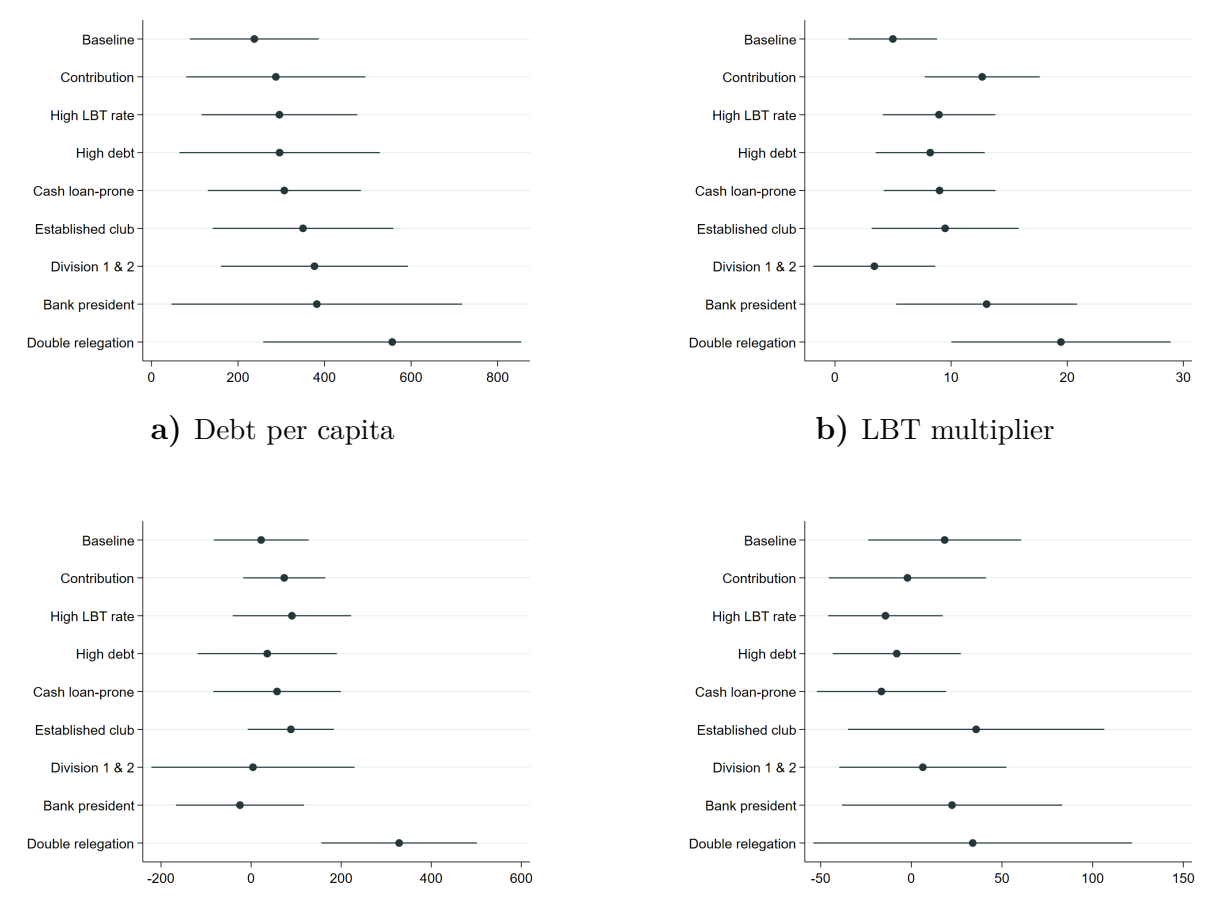
There is a positive, significant and lasting effect on population levels, both in absolute and relative terms in the aftermath of promotion, but not after relegation, though point estimates are negative in some years. Baseline results do not seem to be driven by this partial population change as confirmed in Section 2.4.7. The same pattern is observed for the number of personal income tax payers. Turning to the tax bases of the volatile local business tax and the personal income tax, there is a negative though insignificant development for the business tax and a decrease in the income tax base, which is sig-

nificant three years after relegation as well as the binned endpoint indicator seven and more years after relegation. Thus, there might be an economic contraction associated with relegation, but the picture is not clear. Since municipalities studied here tend to be large municipalities, the local economy might be large enough not to be adversely affected overall. Also, the average business tax rate increase of around 0.18 percentage points does not seem large enough to do significant and lasting damage.

Response heterogeneity

After documenting which club and municipality characteristics correspond to a stronger debt response, this section shows that there is little evidence for a heterogeneous consolidation response. It could be the case that heterogeneity in consolidation policy is hidden in the broad average treatment effect and some groups of municipalities react differently.

Figure 2.4.10: Response heterogeneity of $\hat{\alpha}_5$ with respect to increasing debt effect



a) Debt per capita

b) LBT multiplier

c) Spending per capita

d) Capital expenditure per capita

Notes: This graph depicts heterogeneity analysis for four different dependent variables in each panel, respectively. Effect sizes in the form of $\hat{\alpha}_5$, i.e., the estimated responses five years after treatment, are shown on the respective horizontal axis and different characteristics are listed on the respective vertical axis. They are explained in more detail in the text.

To analyze if this is the case, Figure 2.4.10 presents four panels showing estimates of $\hat{\alpha}_5$, i.e., the effect five years after relegation, which are based on regressions of outcome variables debt, business tax multiplier, spending and investment, respectively. Each panel includes the baseline specification as well as a set of characteristics that are shown to be especially conducive to debt, in ascending order of debt response shown in Figure 2.4.10a: Municipalities that contributed to stadium modernization financing, municipalities that had a local business tax multiplier above the median in 2005, municipalities that had debt per capita above the median in 2006, municipalities in states which are prone to indulge in short-term cash loans,[11] municipalities hosting a club that was in the first four divisions for at least 20 of the observed 25 years, treatment constrained to relegations from the first two divisions, municipalities headed by a mayor who was board president at the local savings bank at the time of relegation (restricted sample like above) and treatment constrained to cases where the club was relegated a second time within three years.

In sub-samples or treatment definitions where debt increased more than in the baseline, the increase of the local business tax multiplier is more pronounced in Figure 2.4.10b, with the exception of relegations from the first two divisions. Note that, approximately, the tax rate response is stronger the larger the debt response is. This cannot be said for the effect on spending and investment in Figures 2.4.10c and 2.4.10d, however. Estimates of the effect five years after relegation are not significantly different from zero in both panels across all specifications, except for the spending response when looking at double relegation events. This treatment definition is based on only 10 events. To sum up, there is no evidence for consolidation measures of especially exposed municipalities here being qualitatively different from the entire sample presented above.

Political outcomes

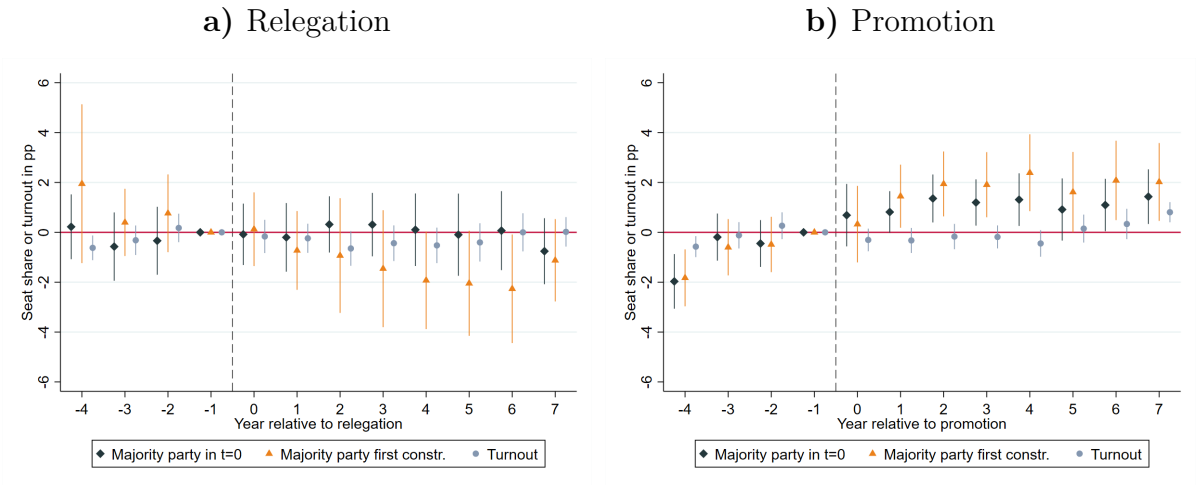
Do citizens react to football outcomes in local elections? If this is the case, then shifts in political power might drive the debt effect. In this section, local election (*Kommunalwahlen*) outcomes are regressed on the treatment variables. Local elections are typically held every five years to determine the municipal council seat distribution and the job of mayor. Data is available on the former, which for all substantial (financial) decisions needs to pass a proposal by the majority of council members. In order to observe political parties' share of council seats every year, all years following an election until the next election take the seat share of that election.

Figure 2.4.11 is based on key insights from regression results in Table 2.A.11. First, one

[11] Brandenburg, Hesse, Lower Saxony, North Rhine-Westphalia, Mecklenburg-Vorpommern, Rhineland-Palatinate, Saarland, Saxony-Anhalt, Schleswig-Holstein

could expect that parties which controlled the majority of council seats at the time of the first observed relegation are subsequently punished at the ballot if they are seen as responsible.[12] While this does not seem to be the case for relegation, the majority party enjoys a significant boost of around 1.5 percentage points after a promotion.

Figure 2.4.11: Local election outcomes



Notes: This graph depicts two event studies, based on Equation (2.1). The time on the horizontal axis are years relative to a relegation event in the panel to the left and relative to a promotion event to the right. The dependent variable is the share of the seats in the municipal council of the majority party, either at the time of the first stadium modernization or one year prior to the event or turnout. Confidence intervals are computed at the 95% level.

An alternative hypothesis is that the party with most seats during the start of the first observed stadium modernization might be held accountable. The estimates in the event study graph suggest there is a barely significant vote penalty of 2.3 percentage points six years after relegation, but the picture is much clearer after promotions. With the ex-post knowledge that construction paid off, majority parties enjoy a significant increase of up to 2.4 percentage points in the municipal council four years after promotion. Note that pre-trends might not be parallel judging from the binned event indicators four years prior to treatment and the analysis is limited to municipalities that experienced a division change or municipality where stadium modernization is observed.

Voter disaffection in the form of overall turnout might be affected following either event, but this it not supported by the estimates in circle symbols. Table 2.A.11 also presents election results for the major parties. Left-leaning parties might be expected to spend more and have less regard for frugal public finances, which could in theory drive the debt response, but this does not seem to be the case when looking at the collective seat share of SPD, Grüne and Die Linke. Relatively small parties like Grüne and Die Linke seem

[12] Focusing on the last observed relegation instead of the first yields the same results. Note that never-treated units are not part of this analysis because there exists no majority party at the time of relegation. Thus, the result is based on a smaller sub-sample.

to profit to some degree at the cost of the larger and established SPD. However, it seems unlikely that increases in the order of less than 1 percentage point from a low base of 10% and 6%, respectively, shift the balance of power strongly.

To summarize, although single parties might benefit from relegation, the effects seem to be more pronounced for those parties which happened to be in power at the time of stadium modernization. This is above all true for promotions. Thus, political outcomes do not seem to play a large role as a mediating variable between relegation and debt. These results do, however, contribute to explaining politicians' motivation for contributing to stadium financing. There is a clear political dividend if promotion takes place.

Effect of public stadium contributions

This section analyzes if municipalities' involvement in stadium financing pays off for the local club by estimating if public contributions have an effect on clubs' future athletic success. If financial support is associated with athletic success, then municipalities would have some control over treatment assignment, i.e., relegation and promotion, violating the assumption of quasi-exogenous treatment assignment.

Table 2.4.3 presents regressions of a football outcome variable on the public contribution to stadium financing in Euros per capita. The football outcome variable is the net division change with respect to the year the contribution took place. It measures the net promotion (or, for negative values, relegation) events a club experienced within a certain time period. Each column of Table 2.4.3 represents a different time span. The mean non-zero public contribution is around €51.9 per capita and has a median of €18.7 per capita.

Table 2.4.3: Municipal contribution to stadium financing in Euros per capita

	(1) Same year	(2) Within 1 year	(3) Within 2 years	(4) Within 3 years	(5) Within 4 years	(6) Within 5 years
Contribution p.c.	0.0011*	-0.0000	0.0003	0.0002	0.0004	0.0008
	(0.0005)	(0.0004)	(0.0004)	(0.0004)	(0.0005)	(0.0006)
Mean(DV)	0.10	0.10	0.15	0.18	0.21	0.23
Years	17	17	17	16	15	14
Obs.	3,139	3,139	3,139	2,960	2,775	2,590

Standard errors in parentheses * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Only the coefficient in Column 1 is significantly different from zero. That is, public contributions are only significantly associated with athletic success if looking at the same year. This reflects the fact that there is a significant probability to modernize the stadium in the year of promotion, either due to increased optimism or stricter stadium requirements

in higher divisions (cf. Figure 2.2.2), in which municipalities are greatly involved. Beyond this, there is no positive association. Including only instances when the stadium was modernized or measuring the effect of the share or absolute amount of public financing instead of the per capita amount also yields insignificant results.

Thus, the nature of public spending on stadium projects is rather municipalities aiding in times of athletic success when a promotion has already happened and visitor demand is surging instead of pushing for future promotion. This, together with evidence from comparisons of close relegation events in Section 2.4.4 and flat pre-trends in the event studies in Section 2.4.3, supports the claim that treatment assignment is quasi-exogenous.

Spillover effects

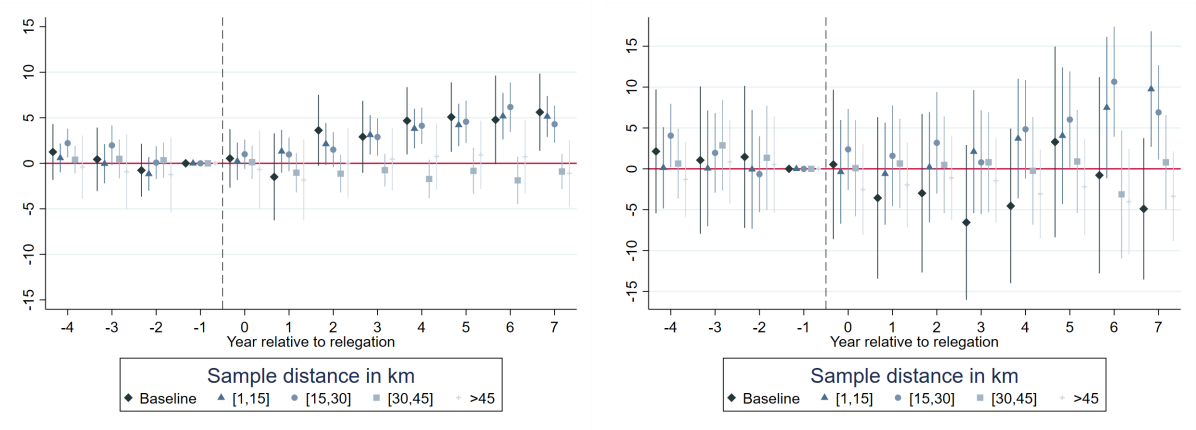
Considering the sample consists mainly of large towns that on average increase the local business tax multiplier by 5 points in the aftermath of relegation, an open question is whether close municipalities respond to the tax increase. As discussed, football club-hosting municipalities tend to be large and regional economic hubs that might influence tax policy in the region through tax competition channels (Janeba & Osterloh, 2013). Since it is only club-hosting municipalities that bear the public finance burden of football events, it is only through the tax channel that close municipalities are affected.

To study spillover effects, I assign each German municipality with at least 5,000 inhabitants a municipality from the football sample based on minimum geographical distance between each others' centroids. Surrounding municipalities are assigned the same treatment status as their closest football municipality. Then, the local business and property tax multipliers of municipalities within certain geographic ranges are regressed on the treatment indicators. That is, the tax multiplier response of municipalities close to a town that experienced relegation and increased the multiplier on average is compared to municipalities close to a football-hosting town that did not experience relegation and a tax hike.

Dynamic tax multiplier spillover estimates by geographical distance to football-hosting municipalities are presented in Figure 2.4.12, which is based on Table 2.A.12. The black diamond markers represent the baseline estimates from above, followed by spillover treatment effects based on geographical distance in steps of 15 kilometers. For the local business tax multiplier in the left panel, municipalities relatively close to treated municipalities increase their multiplier by approximately the same amount compared to municipalities where no relegation happened nearby. Although this analogous effect is from a lower base of around 370 percent for neighboring municipalities compared to almost 420 points for directly affected municipalities as shown in Table 2.A.12, these results show that

Figure 2.4.12: Tax multiplier spillovers by distance

a) Business tax multiplier **b)** Property tax multiplier



Notes: This graph depicts two event studies, based on Equation (2.1). The time on the horizontal axis is years relative to a relegation event. The dependent variables are the local business tax multiplier on the left-hand side and the property tax multiplier on the right-hand side. On top of the baseline specification, estimation samples are defined by municipalities' distance to treatment and control municipalities hosting a football club. Confidence intervals are computed at the 95% level.

there are spillover effects on neighboring municipalities and, considering that they are approximately one-to-one, very large ones at that.

This is also supported by the fact that municipalities within and beyond the 30-45 kilometer range exhibit no statistically significant effect, which is broadly in line with previous results on the geographic extent of fiscal competition effects among local governments (Eugster & Parchet, 2019). There is also no consistently significant effect in neighboring municipalities for the local property tax in the right panel. This is not surprising given that the tax base is less mobile and there is no significant baseline effect at directly affected municipalities. The large business tax spillovers relative to baseline changes, though small in absolute terms, speak to the regional importance of large municipalities and imply that indirect effects of football events go beyond redistribution via states' debt reduction programs. Surrounding municipalities jump at the opportunity to raise more tax revenue without impairing their relative regional competitiveness.

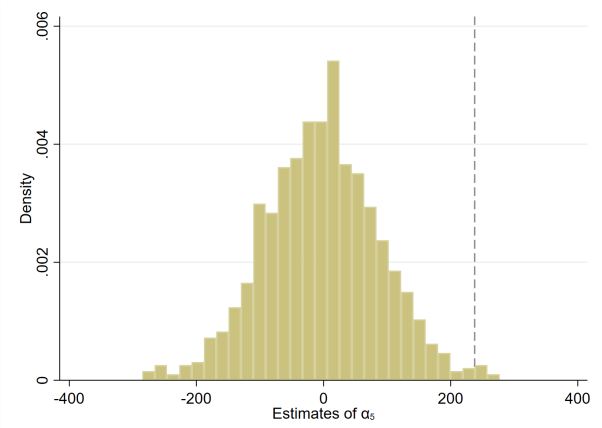
2.4.7 Robustness checks

Placebo tests

To explore whether the baseline results could be due to chance, I conduct a placebo test. In 1,000 replications, treatment is allocated randomly to units and years, holding the number of treatment events constant. Each replication, a set of dynamic treatment effects is estimated according to Equation (2.1). Figure 2.4.13 plots the distribution of

estimates $\hat{\alpha}_5$ from all 1,000 replications along with the estimated effect in the baseline as a dashed line. Placebo estimates are centered around zero and distributed like a normal distribution. The baseline estimate of €238 per capita is in the upper tail, with 99.3% of placebo estimates below it.

Figure 2.4.13: Placebo test for coefficient $\hat{\alpha}_5$



Notes: This graph depicts the distribution of estimates of the debt response five years after placebo events, $\hat{\alpha}_5$. Placebo events are allocated randomly in 1,000 replications. The dashed line illustrates the estimate from the baseline model.

Focusing on more plausible cases where a debt effect might arise, but should not in order to identify a causal effect, I estimate treatment effects of municipalities hosting a club that was relegated and contrast this to clubs that just escaped relegation, pretending that they did. It might be the case that the baseline treatment effect is not entirely driven by relegation, as postulated, but that financially struggling municipalities are less able to support the local football club which, in turn, is less successful, inflating the estimated treatment effect. I want to rule out the possibility that it is relative athletic success rather than relegation that drives my results.

To do this, I compare the debt response in the form of $\hat{\alpha}_5$ from Equation (2.1) for relegated teams and (closely) non-relegated teams in Table 2.4.4, respectively. The table presents the respective p-values of $\hat{\alpha}_5$ by bandwidth around the normalized score in Column 1, which was introduced above in Equation (2.2).[13] That is, I define treatment as a being within a varying distance to zero of the normalized score, once for relegated teams below zero and once for non-relegated teams above zero. Columns 2 and 3 present the respective p-value and number of treatment events for actual relegations while Columns 4 and 5 present the placebo results for clubs that were not actually relegated, but plausibly might have been.

While p-values are mostly below the conventional 5% test size threshold in Column 2,

[13] A test on the joint significance of all post-treatment coefficients yields similar results.

Table 2.4.4: Placebo tests: p-value of coefficient $\hat{\alpha}_5$

Distance to	Below		Above	
0 normalized points	p-value	Treatments	p-value	Treatments
0	0.015	51	0.244	8
1	0.086	58	0.121	27
2	0.017	63	0.470	56
3	0.004	68	0.734	89
4	0.005	73	0.802	127
5	0.005	80	0.213	176
6	0.004	87	0.051	217
7	0.004	94	0.065	254
8	0.012	99	0.595	295
9	0.018	103	0.868	333
10	0.013	106	0.828	377

placebo estimations do not yield a significant coefficient for the tested ranges. Note that teams that closely avoided relegation might very well be relegated in the future, potentially leading to a treatment effect further in the future, but not accounted for here. Even considering this, there is no evidence of a significant treatment effect. Note that the original event study design, however, controls for the entire history of actual relegation and promotion events. These results further support the claim that it is indeed relegation that drives the result and not merely hosting a relatively unsuccessful club.

Robustness to specifications

Tables 2.A.13 and 2.A.14 in the Appendix explore how sensitive the main result is to different specifications and definitions of debt. Columns 1 to 3 of Table 2.A.13 use different numbers of leads and lags to verify that it is not the particular baseline estimation window in Column 4 that drives the result.

Increasing the estimation window by one lag thus reduces the number of observations slightly, but the results hold. Point estimates are slightly attenuated compared to the baseline depicted in Column 4. Conversely, omitting all leads except for one yields significant results, too. The debt response after a promotion becomes more pronounced as more coefficients are significantly different from zero. Note that endpoint estimates, i.e., the first lead and last lag, have different interpretation from the other estimates, indicating that this specification estimates a significantly lower long-run debt level for municipalities with a promoted team. Observations increase even further when additionally limiting lags to 6 in Column 3. Results do not change qualitatively.

Columns 5 and 6 explore whether changing population in the denominator of the outcome variable changes the result. Population is fixed at the level in 2000, the first year football

outcomes are observed and in 2006, the first year when the outcome variable debt is observed, respectively. Coefficients are slightly attenuated and the number of observations drops in the former case due to the lack of complete population data for 2000. Column 7 changes the outcome variable to debt in 2020 Euro terms.

Instead of asking how the average municipality fares after football outcomes, it is ultimately citizens who bear the burden of debt. Thus, Column 1 of Table 2.A.14 is a variant of the baseline specification that puts weights on panel units, i.e., municipalities, according to their inhabitants. Not only do relegation coefficients become slightly larger, but also promotion estimates in this specification come closer to the relegation coefficients in absolute terms.

As municipalities may be subject to jurisdictional changes over the relatively long period under consideration, regression in Column 2 excludes all those that experienced any change to their territory, e.g., due to mergers or separations. Coefficients do not differ much from the baseline, but are less significant, potentially mechanically through lower sample size.

Since excluding municipalities whose club experienced a "double" relegation event (treated twice in at most four years) strongly attenuates the debt effect and significance in Column 3, the baseline effect is strongly driven by those cases. This happened 10 times in the sample and presents an especially large shock to public finances. Excluding municipalities that hosted matches during the 2006 football world cup and for this reason had to implement comprehensive stadium modernization measures makes results stronger in Column 4.

During the time period under consideration, reforms from cash-based to accrual accounting were implemented. Depending on the state, the date at which accrual accounting became mandatory varied. Columns 5 and 6 run regressions separately on the sample that have not implemented accounting reform and those that have to see if different accounting standards drive the results. Point estimates are large, but fewer are significant in the former while in the latter the debt response is stronger than in the baseline specification.

Finally, Column 7 presents results when the 49 license withdrawals are included in the treatment definition. Estimates are strongly attenuated compared to the baseline specification and only statistically significant at the 5% level in two post-treatment years. This might be the result of municipalities unable or unwilling to spend (further) resources on clubs such that a withdrawal becomes more likely but public budgets are burdened less. If a preceding relegation was the cause of financial trouble resulting in the withdrawal of a license (and potentially less generous public support), these cases are included in the baseline specification. Note that 39 of the 49 recorded cases of withdrawal happened

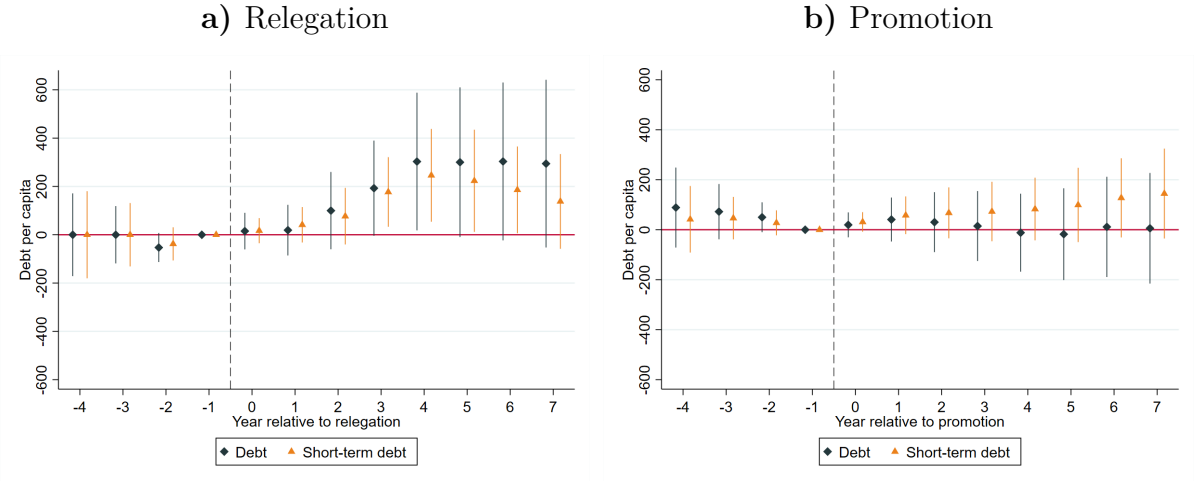
in the lowest division, i.e., Regionalliga. Therefore, this result might be partly due to lower financial stakes in lower divisions. Indeed, Column 8 excludes from treatment only withdrawals from division four and point estimates are larger. Column 9 is the same specification as in Column 7, but excluding from treatment the 33 withdrawals that would not have resulted in relegation based on athletic performance anyway. Here, point estimates are comparable to the baseline specification. If withdrawal of license is indeed associated with less supportive municipalities, this observable characteristic can be used to exclude municipalities from estimation which did not prevent license withdrawal from happening. This is done in Column 10. Here, again, point estimates are very close to the baseline specification.

Overall, the main result is quite robust qualitatively, with the exact magnitude varying by specification. The response to a promotion event, however, is less robust.

Robustness to method

As the two-way fixed effects estimator that is applied in the event study design can be biased in the presence of heterogeneous treatment effects across treatment groups and time, the literature has come up with alternative estimators to address the issue. Although other issues raised in the literature are not applicable here, e.g., the presence of never-treated units is guaranteed, there is no reason to believe treatment effects should be homogeneous.

Figure 2.4.14: De Chaisemartin and D'Haultfoeuille (2024)



Notes: This graph depicts two event studies, based on De Chaisemartin and d'Haultfoeuille (2024). The time on the horizontal axis is years relative to a relegation event on the left-hand side and relative to a promotion event on the right-hand side. The dependent variables are public debt per capita and short-term debt (or liquidity loans) per capita, which is a sub-category of total debt. Confidence intervals are computed at the 95% level.

Therefore, I test robustness with respect to alternative estimation methods. One esti-

mator that is particularly well suited in this context was developed by De Chaisemartin and d'Haultfoeuille (2024).[14] Figure 2.4.14 presents the main result estimated according to their method. The debt response after relegation is qualitatively similar to the two-way fixed effect estimator in Figure 2.4.2, with two exceptions. While the debt response reaches around €238 after five years in the baseline specification, it reaches around €300 per capita here. Short-term debt is similarly inflated. Additionally, confidence bands are much wider, encompassing estimates between around €0 and €600. This result and the exercise in Section 2.4.4 comparing differences in changes non-parametrically underline that the debt response is not a statistical artifact produced by the two-way fixed effects estimator.

2.5 Discussion

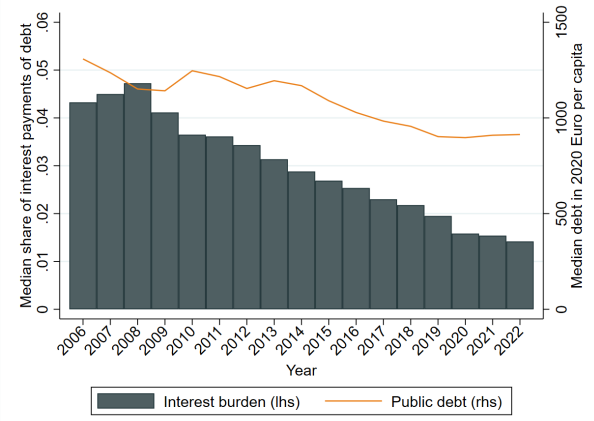
The lack of second-round effects after debt accumulation is striking and merits further discussion. With the exception of a moderate business tax rate increase and some net asset sales six years after relegation, there is no evidence that the excess debt from football significantly impacts other public finance variables adversely, even in the long run. Instead of restricting municipalities' leeway, for example by crowding out investment loans, excess debt from football appears to be added without obvious far-reaching consequences in the budget or the local economy.

One possible explanation is the time period under consideration. In the majority of years monetary policy was expansionary, resulting in low interest rates and easy access to loans. Figure 2.5.1 plots municipalities' median share of interest payments of debt for each year. The median interest burden peaks in 2008 at below 5% and decreases from there monotonically, reaching substantially less than 2% in the last three years. At the same time, median public debt was on a downward trend, helped also by states' debt reduction programs. Under these beneficial circumstances for municipalities, taking out loans might not immediately result in difficult budgetary trade-offs. It seems plausible that more binding constraints on debt, for example imposed by fiscal oversight or economic conditions, would make municipalities sitting out the debt surge less likely.

Fiscal oversight, though mandated to ensure sustainable public finances and indeed imposing budget consolidation programs early on, is not able to prevent excess debt. Note that to be effective in this case, oversight intervention would have to take place when municipalities issue loan guarantees and commitments well in advance of potential relegation. Since counties and states, which are tasked with fiscal oversight, themselves often

[14] This methodology can incorporate repeated treatment. Note that this estimator does not allow for the flexible fixed effects structure as in the two-way fixed effects estimator. That is, state×year and time-varying municipality-specific fixed effects are not included.

Figure 2.5.1: Median interest burden & debt



Notes: This graph depicts municipalities' median share of interest payments among debt for each year on the left axis and median public debt per capita in 2020 terms on the right axis.

provide loan guarantees for stadium modernization this seems questionable. As pointed out above, it might actually be financially sound to accept reduced rent payments after relegation conditional on public stadium ownership. Reconsidering public stadium ownership would most likely not solve the issue either because there is no credible commitment device to avoid bailouts in the time of need, potentially reflecting preferences.

This directs the attention at underlying political considerations. Given that municipalities themselves and overseeing authorities apparently do not rein in public support, what can be said about the desirability of this outcome? From the perspective of municipal decision makers, getting financially involved in local football amounts to a bet on the local club's success. Majority parties in the municipal council at the time of stadium modernization benefit electorally when a club is promoted. Punishment at the ballot is less certain when a club is relegated. This is a bet politicians might be willing to make considering that relegation as defined in this study happens on average only to around 3.5% of clubs annually. Although voters' myopic behavior and the principal-agent problem might be at play here, it is difficult to make the case citizens are not on board with public football involvement. Anecdotal evidence on successful referenda on public stadium financing emphasizes this. Even with states taking on municipalities' debt as part of debt reduction programs and thus sharing the burden across all state taxpayers there is more than meets the eye. First, municipalities surrounding football-hosting ones usually constitute a large part of the fan base and thus enjoy stadium benefits without sharing the associated public costs. Second, if relegation is indeed randomly distributed, state involvement might serve as an insurance against relegation which benefits everyone in expectation. Nevertheless, the debt dynamics documented here represent a case of moral hazard that is often discussed in the context of debt reduction programs. Though the debt accumulation response is similar in municipalities within states that did and those

that did not have debt reduction programs in Figure 2.4.8, it seems likely that decision makers anticipate future financial help.

2.6 Conclusion

In this study, I propose plausibly exogenous variation in shocks to public finances of a sample of German municipalities. A municipality's local football club being (closely) relegated to a lower division presents an unanticipated financing need. Municipalities' short-term response to this shock is mainly driven by an increase in debt. This result is robust to different specifications and methods. In terms of the treatment effect size, it is comparable to the average standard deviation of debt within municipalities. Spending across a wide range of observed items is unaffected. Proxies for local economic activity do not suggest significant adverse effects right away.

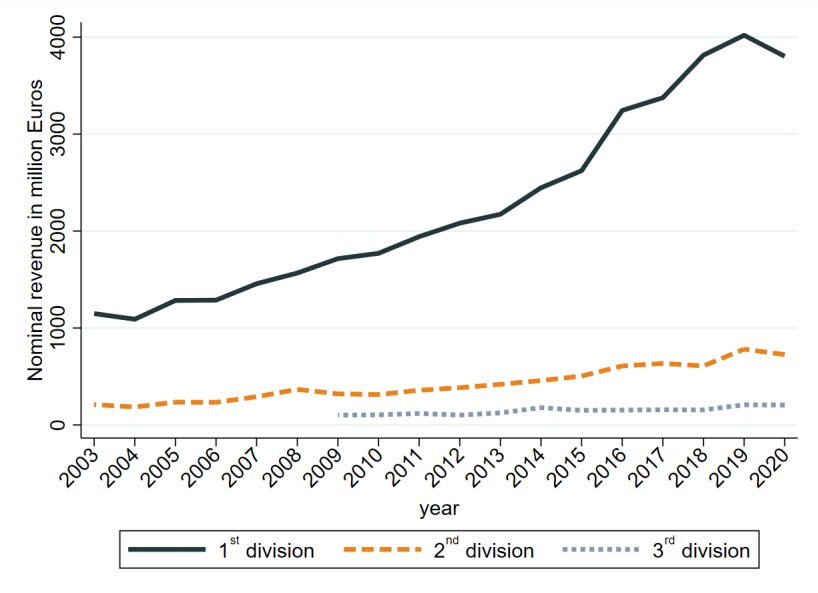
The fact that municipalities primarily react by incurring debt allows analysis of debt's impact on the budget. Surprisingly, estimates do not reveal major economic repercussions in the long run. The worry that more public debt leads to lower public investment cannot be established. There is no crowding out of investment (loans). Expenditure of consumptive nature is not impacted either. This is even true for municipalities that are shown to experience extraordinarily large debt effects. A modest business tax rate increase around the time the debt effect peaks cannot explain debt consolidation alone, but is imitated by municipalities in the region one-to-one.

Finally, the results shed light on the institutional framework in Germany. Although fiscal oversight gets involved in the form of an increased probability to be subject to a budget consolidation program, no significant budget-cutting or revenue-raising measures are observed and the debt response continues to accumulate until five years after relegation. In light of this, states' debt reduction programs seem to carry much of the burden since the debt effect dissipates after around nine years. Municipalities where the mayor serves as board president at the local savings bank experience a much stronger debt increase, emphasizing interdependence not only between municipalities and clubs as well as states, but also between municipalities and savings banks.

Appendix

2.A General Appendix

Figure 2.A.1: Revenue by division

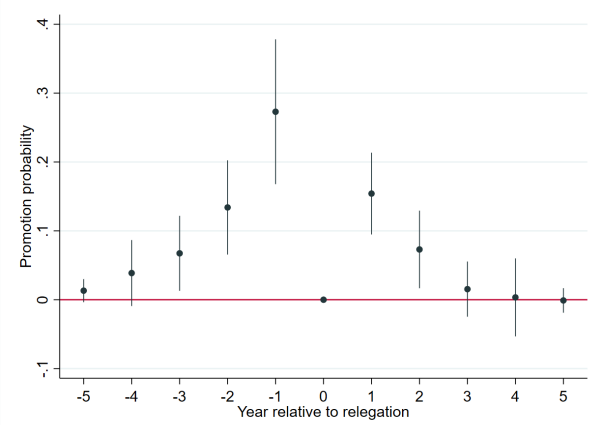


Notes: This graph depicts football clubs' total nominal revenue by division over time in million Euros.

Table 2.A.1: Summary statistics in 2021

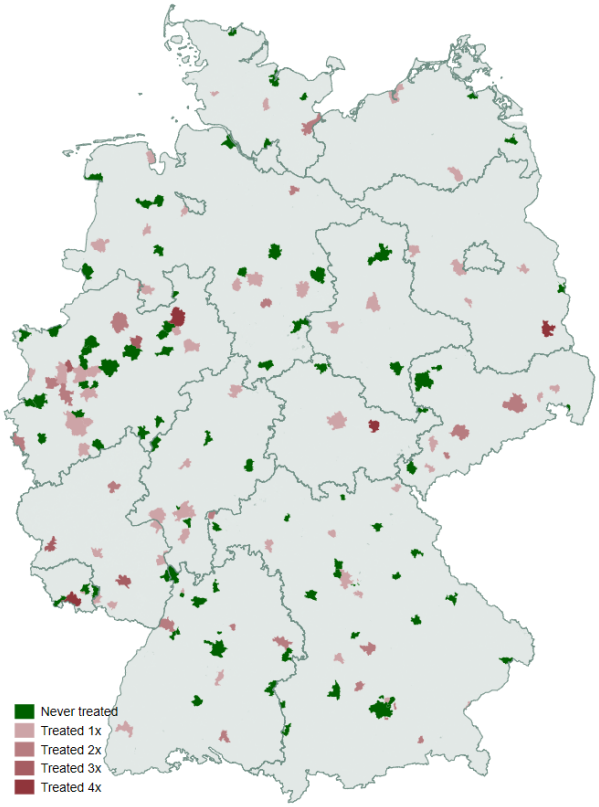
	Obs.	Mean	Median	SD	Min	Max
Population	178	123,526	54,358	175,075	5,645	1,294,608
Per capita						
Primary balance	178	381	255	674	-583	5,777
Revenue	178	3,258	3,122	1,420	1,398	10,881
Expenditure	178	2,877	2,862	1,039	1,147	7,153
Public debt	178	1,548	976	1,770	0	10,241
Short-term debt	178	515	0	1,267	0	8,892
Investment loans	178	1,026	792	858	0	3,854
Business tax revenue	178	929	595	1,416	163	12,892
Property tax revenue	178	179	178	52	82	330
Public investment	178	394	357	301	7	3,292
Net asset sales	178	-15	0	213	-2,572	558
Local multipliers in %						
Business tax	178	412	420	53	250	580
Property tax	178	467	460	121	140	895

Figure 2.A.2: Probability to be promoted



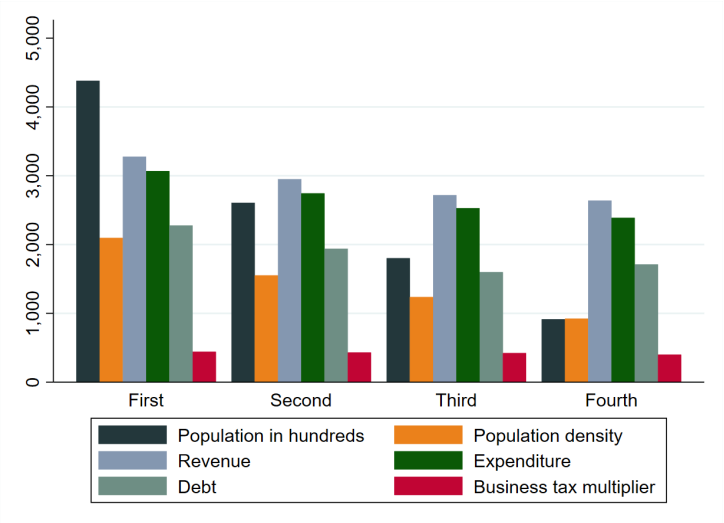
Notes: This graph depicts an event study, based on Equation (2.1). The time on the horizontal axis is years relative to a relegation event. The dependent variable is an indicator variable equal to one if the club is promoted to a more lucrative division. Confidence intervals are computed at the 95% level.

Figure 2.A.3: Sample map and treatment frequency



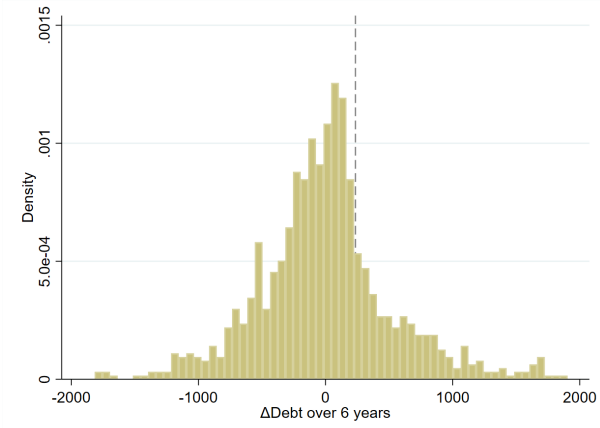
Notes: This graph depicts a map of Germany and its 16 states. Municipalities that are part of the sample are highlighted in green or red, depending on their treatment status. Green municipalities are never treated and red municipalities are treated at least once.

Figure 2.A.4: Summary statistics by division



Notes: This graph depicts mean values of football-hosting municipality characteristics between 2009 and 2022 by football division.

Figure 2.A.5: Distribution of six-year debt changes



Notes: This graph depicts the distribution of within-municipality six-year debt changes in Euros per capita. The estimated baseline treatment effect is illustrated by the dashed line. The horizontal axis is in terms of Euros per capita.

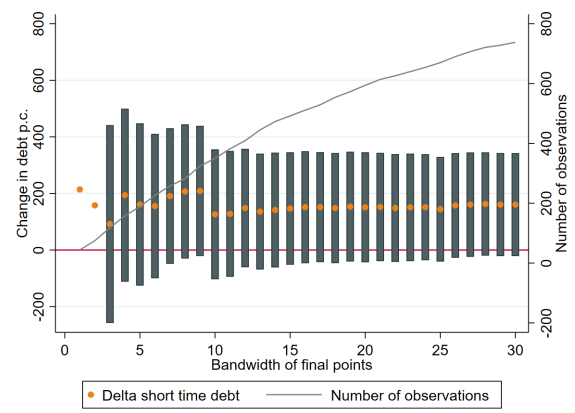
Table 2.A.2: Event study regression results

	(1)		(2)		(3)		(4)		(5)		(6)		(7)		(8)	
	Debt		Short term debt		Business tax multiplier		Property tax multiplier		Business tax revenue		Property tax revenue		Revenue		Spending	
Relegation																
-4	-1	(46)	18	(39)	1	(2)	2	(4)	64	(40)	-1	(2)	58	(59)	-65	(46)
-3	16	(66)	-24	(59)	1	(2)	1	(5)	68	(51)	-0	(3)	27	(78)	-104	(68)
-2	-0	(58)	-22	(47)	-1	(1)	2	(4)	-11	(49)	1	(3)	-37	(85)	-63	(51)
0	2	(58)	-5	(47)	0	(2)	1	(5)	-29	(42)	-1	(2)	-10	(57)	-34	(44)
1	69	(46)	79	(46)	-2	(2)	-3	(5)	-42	(47)	-2	(3)	-13	(68)	-44	(50)
2	112*	(55)	101	(52)	4	(2)	-3	(5)	-96	(49)	-2	(3)	-31	(64)	-15	(45)
3	145*	(61)	137**	(50)	3	(2)	-6	(5)	-41	(54)	-1	(2)	27	(63)	17	(32)
4	222**	(69)	184***	(55)	4*	(2)	-4	(5)	-45	(57)	-2	(3)	25	(70)	-19	(53)
5	238**	(76)	229***	(60)	5*	(2)	3	(6)	-64	(54)	-1	(3)	11	(73)	22	(53)
6	221***	(57)	228***	(49)	5	(2)	-1	(6)	-69	(47)	-2	(3)	18	(64)	56	(54)
7	48	(55)	69	(43)	5*	(2)	-5	(4)	-29	(41)	-5	(3)	126*	(63)	61	(52)
Promotion																
-4	16	(33)	29	(27)	-0	(1)	2	(3)	1	(30)	1	(1)	-20	(36)	7	(24)
-3	11	(30)	7	(22)	-0	(1)	0	(3)	5	(48)	-0	(1)	-19	(46)	-21	(23)
-2	6	(36)	-4	(29)	1	(1)	2	(3)	-31	(65)	1	(1)	-54	(71)	-21	(16)
0	-1	(39)	18	(27)	-1	(1)	-3	(3)	24	(34)	-1	(1)	3	(40)	-15	(18)
1	8	(35)	36	(23)	-1	(1)	-1	(3)	13	(32)	1	(2)	23	(45)	9	(22)
2	-7	(37)	44	(26)	-2	(1)	-5	(3)	-23	(34)	-2	(1)	-64	(41)	-8	(23)
3	-49	(40)	22	(31)	-2	(1)	-6*	(3)	-30	(34)	-2*	(1)	-48	(45)	-23	(21)
4	-71	(41)	36	(29)	-1	(1)	-3	(3)	4	(37)	-3*	(1)	-19	(41)	-45*	(19)
5	-67	(42)	40	(37)	-2	(1)	-5	(3)	-14	(52)	-3**	(1)	-72	(63)	-58*	(24)
6	-82	(47)	-4	(35)	-1	(1)	-4	(3)	-81*	(39)	-4**	(1)	-96	(49)	-46	(24)
7	-80*	(36)	-41	(34)	-3*	(1)	-9**	(3)	-64	(33)	-6***	(1)	-64	(46)	-47	(24)
Municipality FE	Yes		Yes		Yes		Yes		Yes		Yes		Yes		Yes	
State × Year FE	Yes		Yes		Yes		Yes		Yes		Yes		Yes		Yes	
Mean(DV)	2,226		1,161		416		448		535		157		2,535		2,360	
Years	12		12		12		12		12		12		12		12	
Obs.	2,136		2,136		2,134		2,133		2,136		2,136		2,098		2,098	

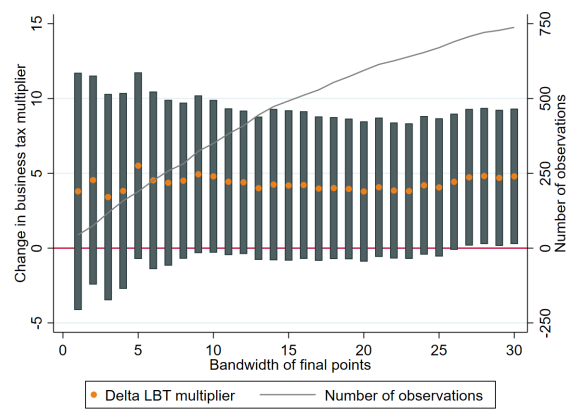
Standard errors in parentheses * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Figure 2.A.6: Changes by bandwidth

a) Change in liquidity loans per capita



b) Change in business tax multiplier



Notes: This graph depicts differences in per capita 5-year changes from one year prior to relegation between municipalities hosting a closely relegated club and those that narrowly avoided relegation (left axis). The differences are of short-term debt (or liquidity loans) in the left panel and the business tax multiplier in the right panel. The differences are plotted as a function of relegation closeness. Relegation closeness on the horizontal axis is measured as the bandwidth of final points around the average points of the highest-ranking relegated team and the lowest ranking non-relegated team in each division and year. The axis on the right-hand side illustrates the number of observations included at each bandwidth.

Table 2.A.3: Event study regression results, continued

	(1) Capital expenditure		(2) Investment loans		(3) Incoming gov. transfer		(4) Outgoing gov. transfer		(5) Primary balance		(6) Spending Social		(7) Spending Staff		(8) Spending Youth		(9) Population	
Relegation																		
-4	-17	(17)	-19	(32)	-18	(13)	-26	(20)	127*	(57)	-9	(12)	-15	(13)	-3	(4)	1735*	(854)
-3	-12	(17)	33	(47)	-19	(15)	-27	(24)	133*	(65)	-15	(13)	-17	(17)	-9	(5)	-135	(1287)
-2	1	(19)	17	(42)	-24	(17)	1	(23)	30	(78)	-5	(10)	-15	(16)	-7	(6)	420	(1144)
0	19	(19)	10	(42)	8	(14)	-17	(19)	26	(58)	7	(12)	-14	(14)	-0	(5)	-124	(999)
1	40	(22)	-8	(40)	-1	(17)	-39	(25)	33	(71)	22	(12)	-20	(15)	1	(5)	-556	(935)
2	30	(20)	9	(37)	8	(15)	-30	(19)	-14	(62)	27*	(13)	-17	(15)	8	(7)	-830	(936)
3	-1	(16)	2	(46)	9	(13)	-12	(19)	11	(60)	18	(11)	-11	(11)	8	(6)	-782	(1325)
4	17	(20)	38	(57)	10	(17)	-49*	(23)	46	(70)	18	(13)	-17	(15)	10	(7)	-612	(1279)
5	18	(21)	6	(46)	26	(16)	-22	(21)	-10	(67)	21	(12)	-24	(16)	13	(8)	97	(1448)
6	2	(22)	-11	(46)	23	(18)	-24	(21)	-35	(63)	36**	(13)	-25	(17)	15*	(8)	531	(1580)
7	21	(15)	-21	(39)	50**	(16)	-45*	(18)	68	(54)	58***	(13)	-13	(17)	20**	(8)	825	(1097)
Promotion																		
-4	-15	(20)	-22	(19)	-1	(9)	4	(14)	-29	(39)	-3	(10)	1	(6)	0	(3)	209	(699)
-3	-5	(19)	-7	(23)	2	(8)	-10	(19)	-0	(50)	-4	(10)	-7	(4)	-3	(3)	-258	(914)
-2	-10	(22)	6	(22)	-4	(10)	-9	(17)	-46	(65)	-8	(10)	-2	(4)	-3	(3)	-584	(841)
0	-15	(15)	-13	(29)	2	(9)	-29	(16)	16	(46)	4	(8)	2	(4)	5	(5)	395	(721)
1	-30	(18)	-25	(29)	-4	(9)	-10	(16)	13	(48)	4	(7)	6	(5)	4	(3)	743	(912)
2	-33	(19)	-53*	(27)	-9	(9)	-8	(18)	-57	(45)	11	(9)	-2	(6)	3	(4)	1364	(877)
3	-34	(19)	-75*	(33)	-11	(8)	-27	(17)	-26	(51)	6	(8)	6	(6)	5	(4)	2122	(1211)
4	-39	(23)	-113***	(28)	-15	(10)	-40*	(19)	19	(41)	8	(10)	-0	(5)	6	(6)	2093	(1065)
5	-31	(18)	-114***	(27)	-12	(10)	-51**	(19)	-13	(62)	4	(9)	-1	(7)	6	(5)	3241**	(1106)
6	-27	(20)	-81*	(34)	-2	(13)	-54**	(17)	-51	(54)	15	(10)	-0	(5)	9	(6)	3005**	(1034)
7	-21	(19)	-45	(25)	-0	(10)	-73***	(18)	-19	(47)	24*	(9)	6	(5)	15**	(5)	5312***	(1012)
Municipality FE	Yes		Yes		Yes		Yes		Yes		Yes		Yes		Yes		Yes	
State × Year FE	Yes		Yes		Yes		Yes		Yes		Yes		Yes		Yes		Yes	
Mean(DV)	288		1,062		420		292		175		488		657		119		164,070	
Years	12		12		12		12		12		12		12		11		12	
Obs.	2,098		2,136		2,098		2,098		2,098		2,098		2,098		1,529		2,136	

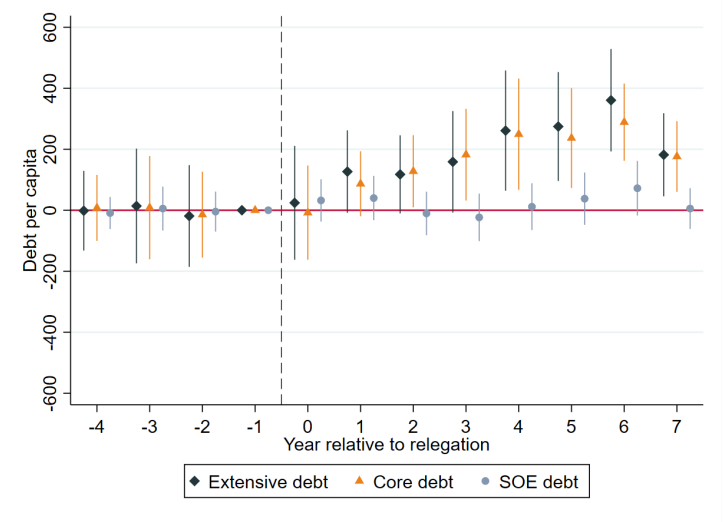
Standard errors in parentheses * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 2.A.4: Event study regression results: Spending categories

	Debt NW		Largest spending items										Discretionary spending items							
	(1)		(2)		(3)		(4)		(5)		(6)		(7)		(8)		(9)		(10)	
	Debt		Administration		Daycare		Support Families		Roads		Elementary schools		Theaters		Sport facilities		Public transport		Libraries	
Relegation																				
-4	11	(132)	-62	(39)	-15	(13)	11	(10)	-18*	(8)	-12	(6)	-5	(6)	-5	(4)	-7	(7)	2**	(1)
-3	53	(165)	-51	(46)	-25	(15)	5	(10)	-13	(10)	-18**	(7)	-18*	(7)	-4	(5)	-3	(12)	1	(0)
-2	7	(141)	5	(54)	-27	(16)	3	(11)	-4	(11)	-10	(7)	-6	(7)	-2	(4)	3	(8)	-0	(1)
0	-23	(130)	-45	(64)	4	(12)	-17	(13)	-7	(11)	1	(6)	1	(4)	-3	(4)	-3	(10)	-0	(1)
1	92	(84)	-25	(54)	-7	(14)	-25	(19)	-10	(11)	-2	(6)	0	(4)	-5	(5)	8	(10)	-1	(1)
2	106	(93)	6	(53)	9	(15)	-6	(18)	-8	(9)	-7	(6)	1	(5)	-8	(4)	-0	(10)	-0	(1)
3	261**	(93)	9	(57)	11	(13)	16	(17)	12	(9)	-9	(8)	2	(5)	-3	(6)	-4	(11)	-1	(1)
4	323**	(98)	29	(56)	17	(12)	34*	(15)	0	(9)	-3	(8)	10*	(5)	-9	(5)	30	(23)	-1*	(1)
5	276**	(89)	28	(57)	20	(14)	33*	(14)	3	(9)	-3	(6)	7	(4)	-6	(5)	0	(8)	-1	(1)
6	309***	(87)	-7	(64)	-3	(21)	16	(13)	-3	(10)	-3	(7)	5	(4)	-0	(5)	2	(7)	-2	(2)
7	256**	(75)	6	(53)	9	(11)	27*	(12)	-6	(9)	1	(7)	7*	(3)	-9*	(4)	2	(8)	-0	(1)
Promotion																				
-4	73	(76)	-87	(55)	13	(14)	1	(8)	-4	(9)	6	(6)	3	(3)	-6	(4)	1	(5)	-0	(1)
-3	44	(93)	-72	(60)	4	(14)	-2	(10)	-14	(10)	-4	(6)	-3	(3)	-4	(6)	7	(7)	0	(1)
-2	33	(100)	-51	(102)	14	(14)	-3	(11)	-5	(9)	-1	(6)	-0	(3)	1	(5)	25	(22)	1	(1)
0	1	(75)	-94	(53)	3	(18)	-1	(10)	-17	(11)	-7	(6)	-3	(4)	-5	(4)	5	(6)	0	(1)
1	40	(80)	-92	(65)	9	(17)	1	(13)	-31***	(8)	-4	(7)	1	(4)	-3	(5)	-4	(6)	1	(1)
2	104	(94)	-101	(61)	8	(17)	2	(11)	-27**	(10)	-10	(8)	-1	(4)	1	(6)	-6	(6)	1	(1)
3	52	(135)	-89	(64)	12	(17)	-13	(12)	-26*	(11)	-7	(9)	1	(4)	-1	(6)	-5	(6)	1	(1)
4	-63	(92)	-127*	(62)	8	(18)	-13	(15)	-30**	(11)	-2	(8)	7	(9)	-1	(7)	-0	(9)	1	(1)
5	-121	(93)	-82	(66)	1	(19)	-9	(15)	-19	(14)	25	(33)	6	(7)	10	(15)	-4	(9)	0	(1)
6	-91	(112)	-85	(60)	23	(19)	-6	(14)	-29**	(11)	-3	(10)	-4	(5)	-1	(6)	14	(23)	1	(1)
7	-86	(105)	-91	(65)	29	(19)	19	(11)	-37***	(10)	-7	(9)	-8	(5)	-6	(5)	-7	(9)	1	(1)
Municipality FE	Yes		Yes		Yes		Yes		Yes		Yes		Yes		Yes		Yes		Yes	
State × Year FE	Yes		Yes		Yes		Yes		Yes		Yes		Yes		Yes		Yes		Yes	
Mean(DV)	3,082		393		229		141		78		65		44		36		17		12	
Years	11		11		11		11		11		11		11		11		11		11	
Obs.	418		418		418		418		418		418		418		418		418		418	

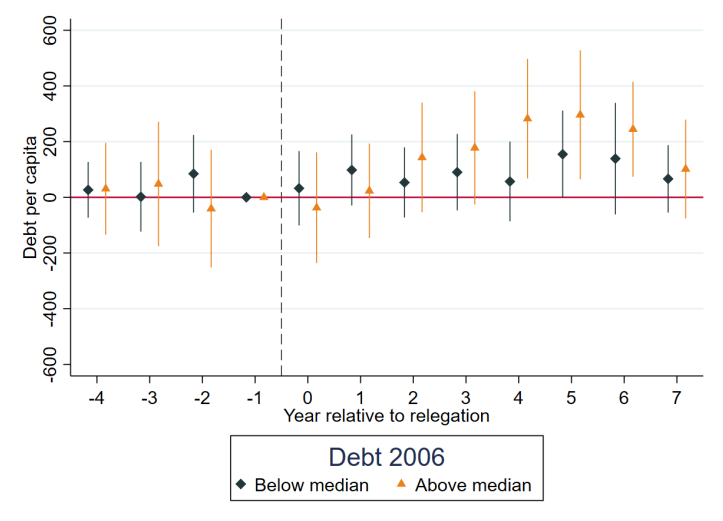
Standard errors in parentheses * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Figure 2.A.7: Extensive definition of public debt



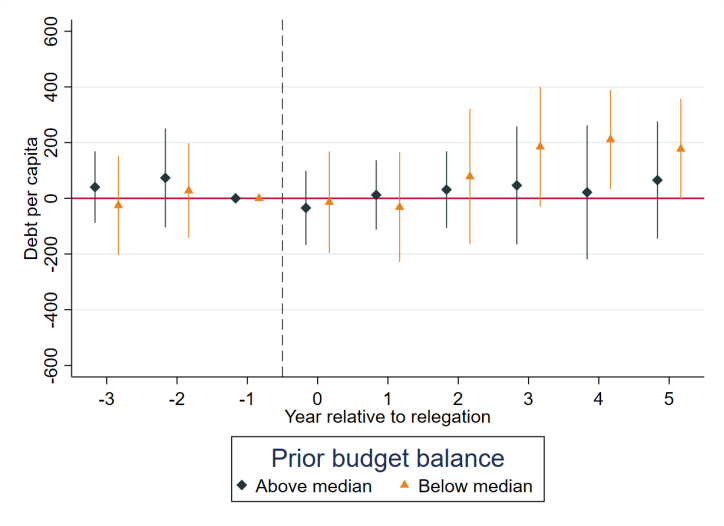
Notes: This graph depicts an event study, based on Equation (2.1) and a restricted sample consisting of municipalities from only four states. The time on the horizontal axis is years relative to a relegation event. The dependent variables are different definitions of per capita public debt. Core debt denotes total debt as before while state-owned enterprise (SOE) debt is debt by firms controlled by the municipality and extensive debt is the sum of the two. Confidence intervals are computed at the 95% level.

Figure 2.A.8: Legacy debt



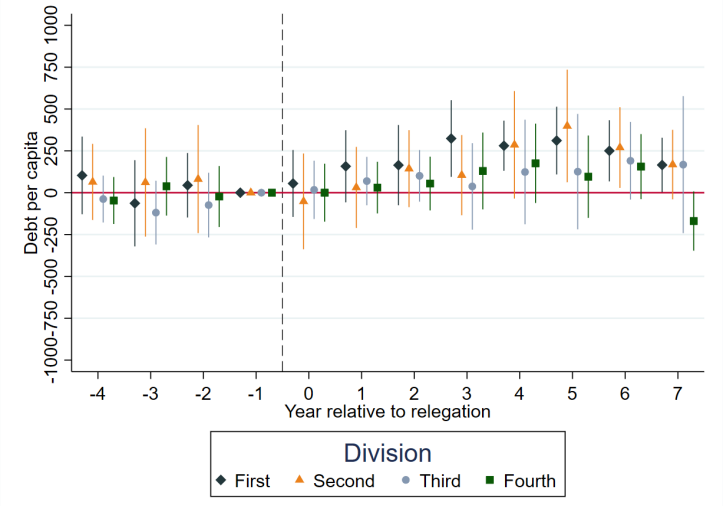
Notes: This graph depicts an event study, based on Equation (2.1). The time on the horizontal axis is years relative to a relegation event. The dependent variable is public debt per capita. The sample is split by below- and above-median public debt in 2006. Confidence intervals are computed at the 95% level.

Figure 2.A.9: Prior primary balance



Notes: This graph depicts an event study, based on Equation (2.1). The time on the horizontal axis is years relative to a relegation event. The dependent variable is public debt per capita. Treatment is re-defined as having a below- and above-median primary balance in the year before relegation, respectively. Confidence intervals are computed at the 95% level.

Figure 2.A.10: Debt response by division



Notes: This graph depicts an event study, based on Equation (2.1). The time on the horizontal axis is years relative to a relegation event. The dependent variable is public debt per capita. Treatment is re-defined as being relegated from one of the top four divisions, respectively. Confidence intervals are computed at the 95% level.

Table 2.A.5: Debt definition and fiscal performance

	Debt definition						Legacy debt				Prior primary balance			
	(1) Total debt		(2) Core debt		(3) SOE debt		(4) Below median 2006 debt		(5) Above median 2006 debt		(6) Below median primary balance		(7) Above median primary balance	
Relegation														
-4	-2	(66)	8	(55)	-9	(27)	27	(51)	31	(84)				
-3	14	(95)	8	(86)	6	(36)	2	(63)	48	(113)	40	(65)	-25	(90)
-2	-19	(85)	-14	(71)	-4	(33)	85	(71)	-41	(107)	73	(90)	27	(86)
0	24	(95)	-8	(78)	32	(35)	32	(68)	-37	(101)	-34	(68)	-14	(92)
1	127	(68)	87	(54)	40	(37)	98	(65)	23	(86)	12	(63)	-32	(100)
2	118	(65)	128*	(60)	-10	(36)	53	(64)	143	(100)	31	(70)	78	(123)
3	159	(84)	182*	(76)	-23	(39)	90	(70)	178	(103)	46	(108)	185	(109)
4	261**	(100)	249**	(92)	12	(39)	57	(72)	283*	(109)	22	(122)	211*	(90)
5	275**	(90)	237**	(83)	38	(43)	155	(79)	296*	(117)	65	(107)	177	(92)
6	361***	(85)	289***	(64)	72	(45)	139	(102)	245**	(86)				
7	182**	(69)	176**	(59)	6	(34)	66	(61)	102	(90)				
Promotion														
-4	-15	(33)	-21	(32)	6	(22)	0	(38)	25	(62)				
-3	-22	(40)	-7	(30)	-15	(32)	9	(41)	15	(57)	-20	(31)	-19	(33)
-2	2	(47)	13	(43)	-10	(32)	-9	(42)	27	(61)	-45	(39)	-51	(40)
0	-15	(42)	-18	(37)	4	(26)	-35	(48)	52	(66)	-9	(44)	-18	(43)
1	-9	(42)	-15	(36)	7	(31)	-81	(48)	123*	(61)	25	(41)	10	(40)
2	-37	(42)	-40	(37)	4	(24)	-93*	(41)	111	(71)	3	(39)	-17	(38)
3	-65	(49)	-60	(42)	-5	(37)	-102*	(43)	23	(89)	-41	(43)	-57	(41)
4	-156***	(46)	-106***	(30)	-50	(37)	-128**	(46)	-16	(76)	-72	(46)	-82	(47)
5	-150***	(45)	-116***	(34)	-35	(38)	-102*	(47)	-64	(89)	-63	(45)	-85*	(41)
6	-121**	(45)	-99*	(40)	-22	(35)	-119*	(54)	-99	(86)				
7	-95*	(42)	-101**	(31)	6	(34)	-67	(39)	-198*	(95)				
Municipality FE	Yes		Yes		Yes		Yes		Yes		Yes		Yes	
State×Year FE	Yes		Yes		Yes		Yes		Yes		Yes		Yes	
Mean(DV)	2,559		1,779		780		530		3,498		1,136		3,086	
Years	12		12		12		12		12		9		9	
Obs.	1,296		1,296		1,296		1,068		1,068		1,519		1,519	

Standard errors in parentheses * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 2.A.6: Heterogeneity by population and division

	Population 2006				Division							
	(1) Below median		(2) Above median		(3) First division		(4) Second division		(5) Third division		(6) Fourth division	
Relegation												
-4	-82	(77)	17	(60)	103	(118)	64	(115)	-38	(71)	-47	(71)
-3	-56	(101)	22	(100)	-63	(131)	61	(164)	-119	(97)	38	(89)
-2	-43	(108)	21	(79)	44	(98)	81	(164)	-74	(98)	-23	(92)
0	26	(97)	-3	(79)	55	(101)	-52	(145)	17	(88)	0	(88)
1	16	(97)	89	(68)	158	(109)	31	(123)	69	(73)	30	(78)
2	-28	(107)	180*	(73)	165	(122)	144	(117)	101	(78)	55	(81)
3	34	(160)	165*	(68)	324**	(116)	105	(122)	37	(131)	130	(116)
4	72	(198)	221**	(74)	281***	(76)	286	(163)	124	(158)	176	(120)
5	-37	(176)	269**	(84)	311**	(103)	399*	(171)	126	(175)	95	(125)
6	63	(145)	212**	(72)	250**	(93)	270*	(122)	191	(118)	156	(99)
7	-189	(113)	144*	(62)	166*	(83)	168	(105)	168	(208)	-169	(90)
Promotion												
-4	-25	(42)	41	(62)	5	(33)	2	(34)	8	(34)	30	(34)
-3	-18	(53)	34	(57)	-5	(30)	-7	(30)	-5	(30)	15	(32)
-2	-36	(45)	24	(60)	-10	(36)	-5	(36)	-10	(36)	3	(38)
0	-8	(46)	-18	(68)	-2	(41)	-3	(40)	3	(41)	-1	(41)
1	-7	(49)	-20	(54)	6	(37)	8	(36)	13	(37)	4	(35)
2	-74	(39)	3	(63)	-18	(35)	-18	(37)	-13	(36)	-14	(37)
3	-85*	(38)	-68	(65)	-58	(40)	-52	(40)	-50	(39)	-56	(40)
4	-90	(52)	-102	(65)	-79	(41)	-69	(41)	-71	(42)	-79	(42)
5	-63	(49)	-124	(70)	-85	(44)	-71	(44)	-78	(44)	-78	(44)
6	-88	(49)	-99	(73)	-83	(48)	-74	(49)	-76	(49)	-78	(49)
7	2	(43)	-81	(65)	-76*	(37)	-85*	(37)	-80*	(37)	-60	(37)
Municipality FE	Yes		Yes		Yes		Yes		Yes		Yes	
State × Year FE	Yes		Yes		Yes		Yes		Yes		Yes	
Mean(DV)	1,209		2,605		2,758		2,243		2,521		1,941	
Years	12		12		12		12		12		12	
Obs.	1,068		1,068		2,136		2,136		2,136		2,136	

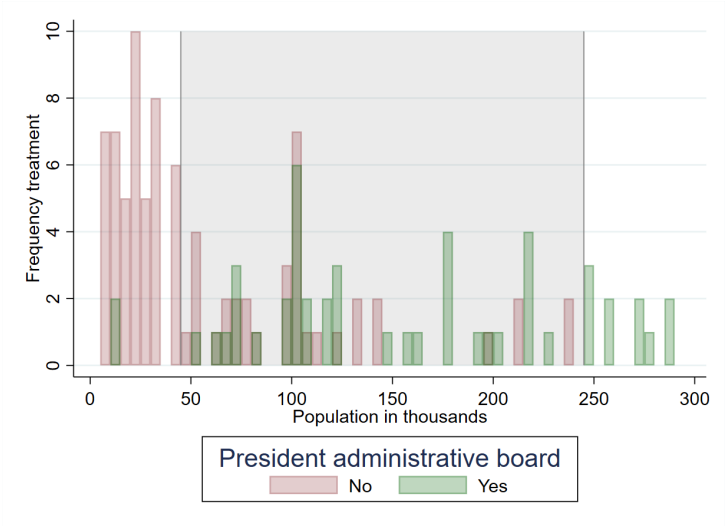
Standard errors in parentheses * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 2.A.7: Conditional on relationship

	(1) Baseline		(2) Ownership		(3) No ownership		(4) Contribution		(5) No contribution	
Relegation										
-4	-1	(46)	-59	(52)	-21	(94)	-61	(62)	55	(74)
-3	16	(66)	-26	(71)	31	(128)	-29	(93)	48	(82)
-2	-0	(58)	-56	(66)	120	(104)	-18	(90)	1	(74)
0	2	(58)	-13	(66)	76	(111)	-19	(79)	37	(82)
1	69	(46)	49	(56)	113	(111)	57	(70)	80	(67)
2	112*	(55)	116	(64)	140	(92)	181*	(73)	21	(68)
3	145*	(61)	122	(78)	206*	(95)	184*	(93)	75	(72)
4	222**	(69)	232**	(89)	126	(102)	278**	(93)	110	(75)
5	238**	(76)	239*	(104)	137	(100)	287**	(105)	142	(89)
6	221***	(57)	201**	(76)	202*	(97)	276***	(80)	92	(93)
7	48	(55)	55	(68)	113	(95)	125	(72)	6	(69)
Promotion										
-4	16	(33)	47	(37)	-112	(77)	-42	(55)	94*	(43)
-3	11	(30)	23	(35)	-32	(80)	-7	(62)	22	(53)
-2	6	(36)	26	(41)	-40	(77)	8	(62)	-14	(47)
0	-1	(39)	-0	(45)	-23	(81)	-42	(69)	22	(34)
1	8	(35)	32	(40)	-91	(76)	-50	(64)	34	(42)
2	-7	(37)	4	(47)	-72	(90)	-80	(61)	40	(49)
3	-49	(40)	-38	(53)	-71	(82)	-123	(70)	2	(48)
4	-71	(41)	-73	(52)	-80	(100)	-171**	(66)	27	(49)
5	-67	(42)	-64	(56)	-84	(89)	-179**	(66)	19	(50)
6	-82	(47)	-102	(56)	-52	(96)	-196**	(68)	11	(53)
7	-80*	(36)	-169***	(48)	72	(87)	-249***	(58)	49	(50)
Municipality FE	Yes		Yes		Yes		Yes		Yes	
State × Year FE	Yes		Yes		Yes		Yes		Yes	
Mean(DV)	2,226		2,399		1,762		2,774		1,405	
Years	12		12		12		12		12	
Obs.	2,136		1,498		638		984		1,152	

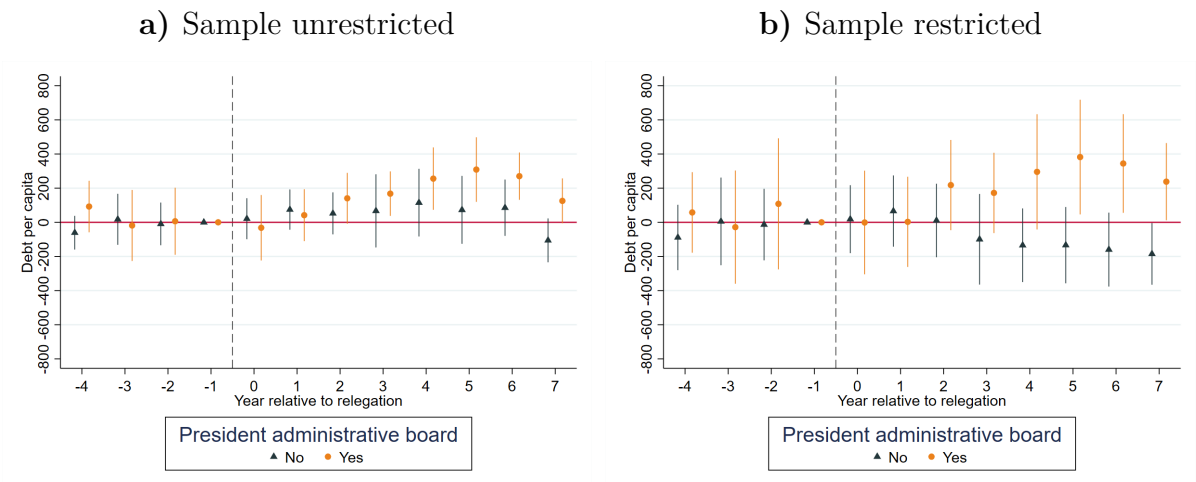
Standard errors in parentheses * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Figure 2.A.11: Sample restriction: Savings bank analysis



Notes: This graph depicts the distribution of municipalities' population. Municipalities are distinguished by their mayors' status as president of the administrative board at the local savings bank. The shaded area illustrates the population range where this status is roughly balanced and therefore determines the restricted estimation sample.

Figure 2.A.12: Debt response by savings bank relationship



Notes: This graph depicts two event studies, based on Equation (2.1). The time on the horizontal axis is years relative to a relegation event. The dependent variable is public debt per capita. Regressions in the panel to the left are based on the whole sample while the sample is restricted to municipalities between 45,000 and 245,000 inhabitants in the panel to the right. In each panel, treatment is re-defined according to if the mayor was president of the administrative board at the local savings bank at the time of relegation or not, respectively. Confidence intervals are computed at the 95% level.

Table 2.A.8: Response by savings bank relationship

	Unrestricted sample										Restricted sample									
	(1)		(2)		(3)		(4)		(5)		(6)		(7)		(8)		(9)		(10)	
	Debt All		Debt No president		Debt President		Business tax rate No president		Business tax rate President		Debt All		Debt No president		Debt President		Business tax multiplier No president		Business tax multiplier President	
Relegation																				
-4	-1	(46)	-61	(50)	92	(77)	0	(2)	2	(2)	-14	(79)	-89	(97)	58	(120)	-0	(0)	0	(0)
-3	16	(66)	18	(76)	-19	(106)	0	(2)	0	(3)	-6	(125)	5	(130)	-28	(168)	-0	(0)	0	(0)
-2	-0	(58)	-9	(63)	6	(100)	-0	(2)	-1	(2)	39	(111)	-13	(106)	108	(195)	0	(0)	-0	(0)
0	2	(58)	21	(61)	-32	(97)	1	(2)	-0	(3)	6	(88)	18	(101)	-1	(154)	0	(0)	0	(0)
1	69	(46)	75	(60)	42	(77)	-4	(4)	1	(3)	46	(86)	66	(106)	3	(134)	0	(0)	0	(0)
2	112*	(55)	52	(62)	141	(76)	-0	(2)	6*	(3)	105	(89)	11	(109)	218	(134)	0	(0)	0	(0)
3	145*	(61)	67	(109)	168*	(66)	-2	(2)	6*	(3)	53	(98)	-99	(135)	172	(119)	0	(0)	0	(0)
4	222**	(69)	115	(101)	256**	(93)	-0	(2)	7**	(3)	100	(108)	-134	(109)	296	(171)	0	(0)	0**	(0)
5	238**	(76)	73	(101)	309**	(96)	0	(2)	7**	(3)	158	(131)	-134	(113)	382*	(170)	-0	(0)	0**	(0)
6	221***	(57)	85	(84)	270***	(70)	-1	(3)	7*	(3)	125	(119)	-160	(110)	344*	(146)	-0	(0)	1***	(0)
7	48	(55)	-106	(65)	126	(67)	-3	(2)	10***	(3)	52	(93)	-185*	(92)	238*	(115)	-0	(0)	1***	(0)
Promotion																				
-4	16	(33)	15	(34)	10	(34)	-0	(1)	-0	(1)	63	(67)	65	(68)	47	(67)	0	(0)	0	(0)
-3	11	(30)	9	(31)	-2	(31)	-0	(1)	-0	(1)	20	(60)	25	(63)	9	(62)	0	(0)	0	(0)
-2	6	(36)	2	(37)	-6	(37)	1	(1)	1	(1)	-9	(71)	-8	(74)	-19	(73)	0	(0)	0	(0)
0	-1	(39)	-5	(41)	3	(41)	-1	(1)	-1	(1)	-3	(78)	12	(79)	9	(79)	-0	(0)	-0	(0)
1	8	(35)	0	(36)	17	(37)	-2	(1)	-1	(1)	-6	(67)	15	(67)	-6	(69)	-0	(0)	-0	(0)
2	-7	(37)	-24	(37)	-3	(36)	-3*	(1)	-2	(1)	30	(65)	41	(64)	32	(65)	-0	(0)	-0	(0)
3	-49	(40)	-68	(40)	-38	(40)	-2	(1)	-2	(1)	-34	(63)	-24	(64)	-17	(67)	-0	(0)	-0*	(0)
4	-71	(41)	-87*	(42)	-58	(42)	-1	(1)	-1	(1)	3	(68)	20	(71)	23	(72)	-0	(0)	-0*	(0)
5	-67	(42)	-87*	(43)	-61	(45)	-2	(1)	-2	(1)	11	(71)	38	(76)	17	(78)	-0	(0)	-0*	(0)
6	-82	(47)	-89	(48)	-67	(49)	-1	(1)	-1	(1)	60	(76)	92	(78)	53	(77)	-0	(0)	-0*	(0)
7	-80*	(36)	-71	(38)	-70	(38)	-2	(2)	-3*	(1)	53	(66)	112	(66)	25	(66)	-0	(0)	-0***	(0)
Municipality FE	Yes		Yes		Yes		Yes		Yes		Yes		Yes		Yes		Yes		Yes	
State × Year FE	Yes		Yes		Yes		Yes		Yes		Yes		Yes		Yes		Yes		Yes	
Mean(DV)	2,226		2,092		2,377		399		435		2,751		2,880		2,608		14		15	
Years	12		12		12		12		12		12		12		12		12		12	
Obs.	2,136		2,136		2,136		2,134		2,134		900		900		900		899		899	

Standard errors in parentheses * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 2.A.9: Long run results

	(1)		(2)		(3)		(4)		(5)		(6)		(7)		(8)		(9)	
	Debt		Debt		Debt		Debt w.o. budget program		Debt with budget program		HSK All		HSK Fully Observed		Winsorized primary balance		Net asset sales	
Relegation																		
-3	9.2	(51.6)	0.5	(49.9)	-10.5	(38.9)	20.8	(56.8)	-2.2	(79.5)	0.0	(0.0)	0.0	(0.0)	36.0	(25.0)	32.8	(16.7)
-2	36.6	(60.4)	36.9	(58.2)	-5.2	(45.5)	22.2	(75.4)	66.7	(92.2)	0.0	(0.0)	0.0	(0.1)	-30.3	(33.4)	28.3	(18.2)
0	-18.3	(60.1)	-8.7	(55.3)	3.2	(42.6)	11.1	(64.8)	-26.1	(89.0)	0.0	(0.0)	0.0	(0.0)	-33.5	(30.9)	-6.9	(18.3)
1	-6.0	(57.1)	20.7	(55.2)	64.6	(45.9)	60.1	(72.5)	-6.9	(87.9)	0.1*	(0.1)	0.1*	(0.1)	-47.2	(32.2)	-3.8	(19.0)
2	41.0	(56.0)	57.1	(55.8)	92.7	(48.3)	108.5	(64.9)	11.2	(98.5)	0.2**	(0.1)	0.2**	(0.1)	-44.9	(33.7)	31.0	(18.4)
3	127.0*	(60.0)	132.4*	(61.8)	152.0*	(61.8)	103.6	(91.1)	139.1	(93.3)	0.1	(0.1)	0.1	(0.1)	-47.2	(30.0)	16.3	(21.0)
4	152.7*	(69.6)	167.7*	(64.7)	183.5**	(65.0)	112.4	(108.4)	171.9	(95.4)	0.1	(0.1)	0.1	(0.1)	-46.1	(39.3)	11.9	(18.2)
5	173.3*	(76.8)	194.9**	(72.3)	227.1**	(71.7)	159.8	(120.9)	189.7	(102.1)	0.1*	(0.1)	0.2*	(0.1)	-85.5*	(36.8)	22.1	(18.5)
6	147.4	(80.1)	142.1*	(68.4)	182.0**	(64.0)	206.7*	(103.7)	90.3	(100.3)	0.0	(0.1)	0.0	(0.1)	-83.3*	(34.2)	44.9*	(21.2)
7	144.7*	(68.6)	131.2*	(63.9)	202.5**	(62.3)	233.8*	(96.4)	91.4	(96.8)	0.0	(0.1)	0.0	(0.1)	-35.8	(33.5)	44.0	(24.1)
8	72.4	(69.9)	62.8	(70.5)	147.7*	(63.8)	178.1*	(84.5)	17.1	(112.0)	0.0	(0.1)	0.0	(0.1)	-19.5	(35.5)	29.7	(31.6)
9	-123.0	(72.7)	-26.8	(61.7)	90.6	(60.2)	165.9*	(77.1)	-104.9	(103.0)	-0.0	(0.1)	-0.0	(0.1)	-14.8	(32.5)	20.2	(21.2)
Promotion																		
-3	-4.4	(33.9)	-12.9	(31.1)	-25.6	(24.8)	-66.6*	(31.8)	18.8	(48.9)	0.1	(0.0)	0.1	(0.0)	-12.3	(17.7)	25.6	(13.5)
-2	-2.9	(43.8)	-28.4	(38.6)	-29.1	(33.6)	-23.7	(45.8)	-37.8	(48.7)	0.1	(0.0)	0.1	(0.0)	-6.4	(26.9)	-2.9	(21.4)
0	-9.8	(42.9)	-17.1	(38.1)	-7.8	(25.8)	-6.8	(32.4)	-34.2	(55.1)	-0.0	(0.0)	-0.0	(0.0)	18.1	(19.3)	-17.0	(16.0)
1	14.7	(37.5)	20.0	(33.4)	1.1	(28.6)	-14.3	(36.9)	54.1	(53.4)	0.0	(0.0)	0.0	(0.0)	40.6	(22.2)	-5.0	(13.3)
2	-2.8	(36.0)	-14.3	(33.7)	-20.1	(29.8)	-63.5	(43.1)	33.4	(54.1)	0.0	(0.0)	0.0	(0.0)	-7.7	(22.3)	5.2	(12.5)
3	-61.2	(43.9)	-65.9	(39.4)	-86.1**	(29.8)	-105.2**	(33.7)	-22.0	(71.3)	0.0	(0.0)	-0.0	(0.0)	28.4	(21.9)	-8.4	(11.8)
4	-80.2	(46.7)	-75.2	(40.5)	-98.0**	(29.7)	-115.9**	(39.5)	-20.7	(72.4)	0.0	(0.0)	0.0	(0.0)	39.7	(23.3)	-13.8	(17.0)
5	-99.4*	(50.1)	-104.1*	(41.4)	-106.3**	(38.1)	-97.4*	(38.6)	-97.3	(78.9)	0.1	(0.0)	0.1	(0.0)	24.6	(25.8)	-4.6	(13.1)
6	-91.8	(56.2)	-103.3*	(48.5)	-113.8**	(38.2)	-106.6**	(38.7)	-73.5	(83.6)	0.1	(0.0)	0.1	(0.0)	30.9	(25.6)	-3.2	(16.5)
7	-70.3	(46.9)	-91.3*	(37.6)	-80.1*	(33.6)	-107.2**	(40.0)	-48.2	(79.3)	0.1	(0.0)	0.1	(0.0)	40.1	(24.7)	10.6	(14.7)
8	-104.2	(56.5)	-114.9*	(49.2)	-80.2*	(38.8)	-158.3***	(38.9)	-34.2	(93.0)	0.1	(0.0)	0.1	(0.0)	38.7	(29.8)	-2.7	(16.6)
9	-80.0	(47.8)	-121.3**	(41.5)	-68.5	(35.4)	-143.4***	(35.0)	-85.0	(86.2)	-0.0	(0.0)	-0.0	(0.0)	26.0	(25.6)	-6.0	(16.9)
Municipality FE	Yes		No		No		No		No		No		No		No		No	
Muni×progr.	No		Yes		No		Yes		Yes		Yes		Yes		Yes		Yes	
Muni×targ. progr.	No		No		Yes		No		No		No		No		No		No	
State×Year FE	Yes		Yes		Yes		Yes		Yes		Yes		Yes		Yes		Yes	
Mean(DV)	2,067		2,067		2,067		772		3,205		0		0		185		-19	
Years	11		11		11		11		11		11		11		11		11	
Obs.	1,958		1,958		1,958		825		1,133		1,840		1,771		1,929		1,929	

Standard errors in parentheses * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 2.A.10: Adverse economic effects

	(1) Population	(2) log Population	(3) PIT payers	(4) Taxable business income	(5) Taxable personal income
Relegation					
-4	1734.5*	0.0007	700.2	882.3*	-28.5
	(853.9)	(0.003)	(543.6)	(407.0)	(71.5)
-3	-135.1	-0.0002	-389.7	980.0	-45.6
	(1287.1)	(0.004)	(567.8)	(508.1)	(80.8)
-2	420.3	0.003	-515.4	75.9	25.8
	(1143.7)	(0.004)	(986.2)	(474.3)	(85.1)
0	-124.3	-0.002	-401.1	-109.3	-51.5
	(999.2)	(0.003)	(590.7)	(409.7)	(77.0)
1	-555.5	-0.004	-1396.4	-159.8	-132.7
	(934.7)	(0.004)	(780.9)	(504.5)	(85.7)
2	-829.6	-0.005	-669.1	-738.4	-167.0
	(935.8)	(0.004)	(765.2)	(517.4)	(93.0)
3	-782.4	-0.006	-773.6	-283.3	-236.2**
	(1324.7)	(0.004)	(856.8)	(535.8)	(88.9)
4	-611.6	-0.006	-159.0	-138.7	-164.5
	(1279.0)	(0.004)	(1034.7)	(551.4)	(108.5)
5	97.0	-0.004	497.9	-508.1	-89.1
	(1447.7)	(0.004)	(1233.2)	(559.5)	(132.8)
6	530.6	-0.002	608.0	-463.2	-100.5
	(1580.3)	(0.005)	(1294.1)	(489.6)	(166.0)
7	825.1	0.002	1444.5	-21.6	-252.8*
	(1097.3)	(0.004)	(985.1)	(427.8)	(122.2)
Promotion					
-4	208.8	0.0009	527.4	-34.8	-26.6
	(698.7)	(0.002)	(1219.8)	(303.7)	(101.6)
-3	-258.5	0.003	-287.7	-238.6	-19.0
	(913.9)	(0.003)	(932.6)	(484.0)	(90.8)
-2	-583.7	0.00008	1472.1	-480.5	-41.0
	(840.8)	(0.004)	(826.4)	(744.6)	(74.6)
0	395.3	-0.0007	345.8	251.0	-85.8
	(720.8)	(0.004)	(592.4)	(456.6)	(82.7)
1	743.2	0.0009	1526.2	172.4	19.6
	(912.3)	(0.003)	(865.2)	(314.6)	(90.3)
2	1364.2	0.002	1609.9	12.3	-11.1
	(876.9)	(0.003)	(889.5)	(310.9)	(84.5)
3	2122.0	0.004	2766.5*	-179.5	-100.0
	(1211.3)	(0.003)	(1067.5)	(322.4)	(91.5)
4	2092.6	0.004	2264.1*	192.9	-167.2
	(1065.5)	(0.004)	(1092.9)	(422.0)	(90.3)
5	3240.8**	0.01**	2930.0*	-43.8	-92.8
	(1106.0)	(0.004)	(1292.5)	(656.9)	(79.0)
6	3005.2**	0.01**	2755.5*	-812.3*	-170.0
	(1034.1)	(0.004)	(1364.8)	(361.3)	(93.3)
7	5312.0***	0.02***	4698.6**	-768.0*	-118.4
	(1012.2)	(0.004)	(1405.5)	(345.4)	(96.9)
Municipality FE	Yes	Yes	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes	Yes	Yes
Mean(DV)	164,070	11	68,217	4,427	18,869
Years	12	12	8	12	8
Obs.	2,136	2,136	1,394	2,099	1,394

Standard errors in parentheses * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 2.A.11: Local election results

	(1) Majority party in $t_r = 0$		(2) Majority party in $t_p = 0$		(3) Majority party constr.		(4) Turnout		(5) Left parties		(6) CDU		(7) SPD		(8) FDP		(9) Grüne		(10) AFD		(11) Die Linke		(12) Other parties	
Relegation																								
-4	.2	(.7)	.5	(.8)	1.9	(1.6)	-.6*	(.3)	-.4	(.7)	-1.6	(.9)	-.3	(.5)	.0	(.2)	-.1	(.3)	-.3	(.3)	-.0	(.2)	2.2	(1.4)
-3	-.6	(.7)	.1	(.6)	.4	(.7)	-.3	(.3)	-.5	(.5)	-.3	(.5)	-.2	(.4)	.1	(.3)	-.5	(.3)	-.2	(.3)	.2	(.2)	.9	(.7)
-2	-.3	(.7)	.7	(.8)	.8	(.8)	.2	(.3)	-.1	(.5)	-.4	(.7)	-.2	(.4)	.2	(.2)	.0	(.2)	.2	(.3)	.1	(.2)	.2	(.8)
0	-.1	(.6)	.8	(.8)	.1	(.7)	-.2	(.3)	.0	(.4)	-.6	(.7)	-.2	(.4)	.1	(.2)	.3	(.2)	.5	(.3)	-.0	(.2)	.0	(.8)
1	-.2	(.7)	.5	(.7)	-.7	(.8)	-.2	(.3)	.6	(.5)	-.0	(.8)	-.1	(.4)	.3	(.3)	.5*	(.2)	.2	(.4)	.1	(.2)	-1.0	(.9)
2	.3	(.6)	1.2	(.9)	-.9	(1.2)	-.6	(.4)	.3	(.5)	-.1	(.8)	-.9*	(.4)	.6	(.4)	.7**	(.2)	.1	(.3)	.5*	(.2)	-.9	(1.1)
3	.3	(.6)	.6	(.8)	-1.5	(1.2)	-.4	(.4)	.7	(.6)	.2	(.7)	-.9*	(.4)	.5	(.3)	.8**	(.3)	.0	(.3)	.8**	(.3)	-1.4	(1.1)
4	.1	(.7)	.2	(.8)	-1.9	(1.0)	-.5	(.4)	.5	(.6)	.3	(.8)	-1.1*	(.4)	.5	(.3)	.9***	(.2)	.1	(.3)	.6*	(.3)	-1.3	(1.1)
5	-.1	(.8)	.1	(.8)	-2.0	(1.1)	-.4	(.4)	.7	(.6)	-.1	(.8)	-.8	(.5)	.5	(.3)	.8*	(.3)	.1	(.4)	.7**	(.3)	-1.3	(1.1)
6	.1	(.8)	.4	(.8)	-2.3*	(1.1)	.0	(.4)	.8	(.7)	.9	(.7)	-.6	(.5)	.7	(.3)	.7*	(.3)	-.4	(.5)	.8**	(.3)	-1.9	(1.2)
7	-.8	(.7)	.6	(.9)	-1.1	(.8)	.0	(.3)	-.2	(.4)	-.0	(.7)	-1.4***	(.4)	.4	(.3)	.3	(.3)	.5	(.3)	.9***	(.2)	-.7	(.8)
Promotion																								
-4	-1.7**	(.6)	-2.0***	(.6)	-1.8**	(.6)	-.6**	(.2)	-1.0	(.7)	.5	(.3)	-.6	(.3)	.1	(.1)	.0	(.3)	-.2	(.2)	-.4*	(.2)	.5	(.7)
-3	-.5	(.8)	-.2	(.5)	-.6	(.6)	-.1	(.3)	-.5	(.5)	.4	(.5)	-.3	(.4)	.0	(.2)	-.1	(.3)	-.0	(.3)	-.2	(.2)	.2	(.7)
-2	-.2	(.7)	-.4	(.5)	-.5	(.6)	.3	(.3)	-.6	(.6)	.3	(.4)	-.4	(.4)	-.1	(.2)	.0	(.3)	.2	(.2)	-.2	(.3)	.2	(.7)
0	.9	(.7)	.7	(.6)	.3	(.8)	-.3	(.2)	-.3	(.6)	-.0	(.3)	-.5	(.5)	-.2	(.2)	.1	(.2)	-.1	(.2)	.1	(.1)	.6	(.7)
1	1.4*	(.7)	.8	(.4)	1.4*	(.6)	-.3	(.3)	.2	(.4)	.4	(.4)	-.3	(.3)	-.1	(.2)	.4	(.2)	-.1	(.2)	.1	(.2)	-.5	(.5)
2	1.5*	(.7)	1.4**	(.5)	1.9**	(.7)	-.2	(.3)	.7	(.4)	.7	(.4)	.1	(.3)	.1	(.2)	.5*	(.2)	-.2	(.3)	.2	(.2)	-1.3**	(.5)
3	1.6*	(.8)	1.2*	(.5)	1.9**	(.7)	-.2	(.2)	.6	(.5)	.7	(.3)	-.2	(.4)	-.0	(.2)	.5*	(.2)	-.3	(.3)	.2	(.2)	-1.0	(.6)
4	.9	(.8)	1.3*	(.5)	2.4**	(.8)	-.4	(.3)	.7	(.5)	.5	(.4)	.3	(.3)	-.1	(.2)	.3	(.2)	-.2	(.3)	.0	(.2)	-.9	(.7)
5	.4	(.7)	.9	(.6)	1.6*	(.8)	.1	(.3)	.9	(.5)	.8	(.5)	.5	(.4)	-.0	(.2)	.4	(.3)	-.2	(.3)	.0	(.2)	-1.6*	(.7)
6	-.0	(.7)	1.1*	(.5)	2.1*	(.8)	.3	(.3)	.6	(.6)	.0	(.4)	.4	(.4)	-.1	(.2)	.3	(.3)	-.1	(.3)	-.0	(.2)	-.4	(.7)
7	-.1	(.7)	1.4*	(.6)	2.0*	(.8)	.8***	(.2)	.5	(.5)	.2	(.4)	.1	(.4)	-.2	(.2)	.1	(.3)	-.1	(.2)	.3	(.2)	-.5	(.8)
Municipality FE	Yes		Yes		Yes		Yes		Yes		Yes		Yes		Yes		Yes		Yes		Yes		Yes	
State×Year FE	Yes		Yes		Yes		Yes		Yes		Yes		Yes		Yes		Yes		Yes		Yes		Yes	
Mean(DV)	39		40		37		48		45		33		28		4		10		2		6		16	
Obs.	707		1,236		1,440		2,114		2,115		2,115		2,115		2,115		2,115		2,115		2,115		2,115	

Standard errors in parentheses * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 2.A.12: Tax multiplier spillovers by distance in kilometers

	LBT multiplier										LPT multipier									
	(1) Baseline		(2) [1,15]		(3) [15,30]		(4) [30,45]		(5) >45		(6) Baseline		(7) [1,15]		(8) [15,30]		(9) [30,45]		(10) >45	
Relegation																				
-4	1	(2)	1	(1)	2**	(1)	0	(1)	-0	(2)	2	(4)	0	(3)	4*	(2)	1	(2)	-1	(2)
-3	0	(2)	-0	(1)	2	(1)	0	(1)	-1	(2)	1	(5)	0	(4)	2	(2)	3	(3)	1	(3)
-2	-1	(1)	-1	(1)	0	(1)	0	(1)	-1	(2)	1	(4)	-0	(4)	-1	(2)	1	(3)	1	(3)
0	1	(2)	0	(1)	1	(1)	0	(1)	-1	(2)	1	(5)	-0	(3)	2	(3)	0	(3)	-3	(3)
1	-1	(2)	1	(1)	1	(1)	-1	(1)	-2	(2)	-4	(5)	-1	(3)	2	(3)	1	(3)	-2	(3)
2	4	(2)	2	(1)	2	(1)	-1	(1)	0	(2)	-3	(5)	0	(3)	3	(3)	0	(3)	-1	(3)
3	3	(2)	3**	(1)	3**	(1)	-1	(1)	0	(2)	-7	(5)	2	(4)	1	(3)	1	(3)	-1	(3)
4	5*	(2)	4***	(1)	4***	(1)	-2	(1)	1	(2)	-5	(5)	4	(4)	5	(3)	-0	(3)	-3	(3)
5	5**	(2)	4***	(1)	5***	(1)	-1	(1)	1	(2)	3	(6)	4	(4)	6*	(3)	1	(3)	-2	(3)
6	5	(2)	5***	(1)	6***	(1)	-2	(1)	1	(2)	-1	(6)	7	(4)	11**	(3)	-3	(4)	-4	(3)
7	6**	(2)	5***	(1)	4***	(1)	-1	(1)	-1	(2)	-5	(4)	10**	(4)	7*	(3)	1	(3)	-3	(3)
Promotion																				
-4	-0	(1)	-2**	(1)	0	(1)	-0	(1)	1	(2)	2	(3)	4*	(2)	4**	(1)	2	(1)	-4**	(1)
-3	-0	(1)	-2*	(1)	-0	(1)	0	(1)	-2	(2)	0	(3)	0	(2)	-0	(1)	-0	(2)	0	(2)
-2	1	(1)	-1	(1)	-1	(1)	-0	(1)	-0	(2)	2	(3)	-1	(2)	-1	(1)	-1	(2)	-1	(2)
0	-1	(1)	-0	(1)	-0	(1)	1	(1)	-0	(1)	-3	(3)	2	(2)	1	(1)	-1	(2)	0	(2)
1	-1	(1)	-1	(1)	0	(1)	-0	(1)	-2	(1)	-1	(3)	3	(2)	3	(2)	-0	(1)	0	(2)
2	-2	(1)	-0	(1)	0	(1)	0	(1)	-3*	(1)	-5	(3)	6**	(2)	3	(2)	2	(2)	2	(2)
3	-2	(1)	-0	(1)	0	(1)	0	(1)	-1	(1)	-6*	(3)	6**	(2)	3	(2)	2	(2)	3	(2)
4	-1	(1)	-1	(1)	-1	(1)	0	(1)	1	(2)	-3	(3)	6*	(3)	2	(2)	3	(2)	3	(2)
5	-2	(1)	-1	(1)	-1	(1)	0	(1)	2	(2)	-5	(3)	4	(2)	1	(2)	4*	(2)	4	(2)
6	-2	(1)	-1	(1)	-2***	(1)	0	(1)	0	(2)	-4	(3)	2	(2)	-1	(2)	6**	(2)	7***	(2)
7	-3*	(1)	-3***	(1)	-2***	(1)	-1	(1)	1	(1)	-10**	(3)	-2	(2)	-3	(2)	5**	(2)	6**	(2)
Municipality FE	Yes		Yes		Yes		Yes		Yes		Yes		Yes		Yes		Yes		Yes	
State × Year FE	Yes		Yes		Yes		Yes		Yes		Yes		Yes		Yes		Yes		Yes	
Mean(DV)	417		372		371		366		365		449		369		371		379		380	
Years	12		12		12		12		12		12		12		12		12		12	
Obs.	2,099		8,442		11,960		6,452		2,671		2,098		8,442		11,960		6,453		2,671	

Standard errors in parentheses * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 2.A.13: Robustness checks

	Different estimation window													
	(1)		(2)		(3)		(4)		(5)		(6)		(7)	
	Debt		Debt		Debt		Baseline debt		Debt per 2000 capita		Debt per 2006 capita		Debt in 2020 Euros	
Relegation														
-4	7	(48)					-1	(46)	-2	(46)	4	(44)	-6	(48)
-3	37	(67)					16	(66)	24	(62)	26	(62)	10	(62)
-2	14	(61)					-0	(58)	18	(55)	19	(54)	1	(57)
0	17	(57)	32	(46)	46	(51)	2	(58)	-5	(57)	-5	(57)	-1	(61)
1	64	(47)	14	(42)	36	(43)	69	(46)	40	(47)	46	(46)	71	(49)
2	115*	(53)	60	(48)	73	(45)	112*	(55)	80	(52)	83	(52)	109	(56)
3	142*	(63)	103	(58)	116*	(58)	145*	(61)	108	(60)	108	(60)	137*	(64)
4	218**	(66)	167***	(49)	191***	(57)	222**	(69)	184**	(68)	185**	(68)	208**	(71)
5	222**	(72)	187**	(60)	215**	(65)	238**	(76)	203**	(75)	207**	(74)	205**	(79)
6	213***	(57)	156*	(62)	78	(52)	221***	(57)	183**	(56)	188***	(55)	175**	(61)
7	126*	(61)	136	(79)			48	(55)	57	(53)	56	(53)	-39	(56)
8	16	(57)	12	(57)										
Promotion														
-4	4	(34)					16	(33)	18	(33)	17	(33)	21	(34)
-3	4	(30)					11	(30)	17	(31)	16	(32)	15	(32)
-2	-2	(35)					6	(36)	7	(35)	8	(35)	6	(37)
0	15	(36)	7	(26)	-22	(30)	-1	(39)	-7	(40)	-4	(39)	-3	(41)
1	13	(33)	21	(29)	21	(28)	8	(35)	3	(36)	7	(35)	6	(36)
2	-24	(36)	-12	(29)	-16	(29)	-7	(37)	-13	(37)	-7	(37)	-15	(38)
3	-46	(41)	-35	(32)	-43	(33)	-49	(40)	-50	(41)	-43	(40)	-63	(39)
4	-88*	(41)	-65	(39)	-60	(36)	-71	(41)	-76	(39)	-67	(40)	-88*	(41)
5	-78	(43)	-78*	(32)	-90**	(31)	-67	(42)	-72	(40)	-58	(41)	-85*	(41)
6	-82	(47)	-63	(37)	-79**	(25)	-82	(47)	-79	(44)	-67	(46)	-103*	(48)
7	-97*	(39)	-66*	(30)			-80*	(36)	-51	(33)	-44	(34)	-89*	(37)
8	-97*	(41)	-84*	(34)										
Municipality FE	Yes		Yes		Yes		Yes		Yes		Yes		Yes	
State × Year FE	Yes		Yes		Yes		Yes		Yes		Yes		Yes	
Mean(DV)	2,216		1,689		1,683		2,150		2,159		2,148		2,317	
Years	11		14		16		12		12		12		12	
Obs.	1,958		2,492		2,848		2,136		2,112		2,136		2,136	

Standard errors in parentheses * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 2.A.14: Robustness checks, continued

	(1) Debt pop.-weighted		(2) Debt w.o. changes		(3) Debt w.o. double		(4) Debt w.o. WC hosts		(5) Before budgeting reform		(6) After budgeting reform		(7) Incl. withdrawals		(8) Excl. division 4 withdrawal		(9) Relegation anyway		(10) Excl withdrawal municipalities	
Relegation																				
-4	-17	(72)	-14	(50)	-15	(50)	19	(47)	59	(65)	-47	(79)	-31	(48)	-10	(52)	-13	(44)	-34	(48)
-3	48	(103)	11	(72)	50	(66)	18	(66)	-6	(90)	65	(94)	-40	(62)	-34	(68)	5	(63)	-20	(68)
-2	14	(81)	3	(63)	19	(64)	2	(61)	9	(91)	4	(80)	-49	(53)	-47	(59)	-3	(55)	-3	(61)
0	-18	(86)	-12	(65)	-23	(66)	3	(61)	-42	(90)	11	(70)	-13	(61)	-19	(69)	-14	(55)	9	(56)
1	89	(67)	53	(53)	13	(56)	73	(48)	18	(94)	72	(60)	74	(59)	77	(63)	46	(45)	91	(48)
2	143	(75)	90	(63)	57	(68)	120*	(57)	49	(84)	158*	(66)	66	(61)	73	(69)	90	(54)	103*	(51)
3	171*	(68)	120	(69)	103	(79)	148*	(64)	144	(106)	195**	(64)	81	(56)	100	(67)	121*	(59)	153*	(73)
4	222**	(79)	208**	(78)	172*	(83)	238**	(76)	202	(132)	245***	(64)	129	(68)	162*	(76)	196**	(68)	206**	(77)
5	240**	(89)	222*	(88)	175	(94)	259**	(80)	275	(165)	228**	(73)	128*	(63)	143*	(72)	225**	(73)	213**	(76)
6	212**	(78)	205**	(66)	108	(64)	251***	(62)	226*	(106)	246***	(72)	137*	(68)	165*	(74)	207***	(56)	242***	(58)
7	130	(68)	17	(60)	-58	(65)	68	(59)	49	(94)	95	(77)	16	(50)	9	(57)	54	(52)	99	(60)
Promotion																				
-4	4	(50)	21	(37)	-18	(33)	-9	(34)	-0	(38)	32	(68)	61	(36)	59	(35)	17	(34)	17	(43)
-3	32	(50)	18	(33)	-0	(31)	3	(30)	24	(41)	-25	(53)	30	(36)	31	(36)	11	(30)	6	(46)
-2	40	(54)	5	(39)	7	(35)	-4	(38)	-8	(35)	8	(55)	16	(41)	16	(41)	6	(36)	-9	(43)
0	0	(65)	-0	(42)	-6	(39)	-2	(40)	3	(40)	51	(53)	-5	(44)	-6	(44)	-3	(40)	16	(44)
1	8	(41)	2	(38)	-7	(36)	9	(37)	11	(44)	60	(56)	22	(38)	21	(38)	4	(35)	10	(45)
2	4	(60)	-6	(40)	-27	(39)	-3	(38)	-22	(48)	76	(59)	11	(40)	11	(39)	-10	(38)	-6	(46)
3	-53	(66)	-50	(44)	-62	(42)	-45	(41)	-60	(43)	54	(73)	-43	(41)	-39	(43)	-49	(40)	-53	(44)
4	-121*	(53)	-67	(47)	-83*	(42)	-60	(42)	-50	(53)	-9	(70)	-52	(47)	-50	(46)	-71	(41)	-84	(49)
5	-156**	(60)	-60	(47)	-79	(43)	-59	(43)	-43	(48)	-35	(79)	-50	(49)	-46	(49)	-69	(42)	-72	(52)
6	-184***	(55)	-67	(52)	-78	(48)	-58	(49)	-62	(49)	-79	(80)	-60	(49)	-56	(48)	-83	(47)	-116*	(54)
7	-138*	(55)	-62	(40)	-51	(36)	-58	(37)	-32	(50)	-151	(77)	-48	(36)	-38	(35)	-85*	(36)	-100*	(45)
Municipality FE	Yes		Yes		Yes		Yes		Yes		Yes		Yes		Yes		Yes		Yes	
State × Year FE	Yes		Yes		Yes		Yes		Yes		Yes		Yes		Yes		Yes		Yes	
Mean(DV)	2,150		2,120		1,888		1,915		838		2,990		2,150		2,259		2,077		2,209	
Years	12		12		12		12		12		11		12		12		12		12	
Obs.	2,136		1,968		2,028		2,016		1,123		1,013		2,136		2,136		2,136		1,656	

Standard errors in parentheses * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Chapter 3

The Fiscal and Economic Effects of the Biontech Shock

Joint with Eckhard Janeba, Davud Rostam-Afschar and Paul Steger.

3.1 Introduction

Municipal governments play a crucial role in delivering public services, including childcare, energy and water supply and housing. They are also responsible for a significant share of public investment. As emphasized by Agrawal et al. (2022), understanding how local governments shape policy decisions is essential. A key empirical challenge, however, is that changes in local revenues are often endogenous to broader economic and fiscal conditions (Knight, 2002; Dahlberg et al., 2008). While the literature has made progress in addressing this issue, as discussed in more detail below, causal evidence on the dynamic spending response remains limited. We contribute to this literature by analyzing a large and plausibly exogenous shock to municipal revenues, allowing us to examine in detail the tax, net asset position and expenditure responses of affected municipalities in Germany. Additionally, we assess the shock's spillover effects by studying tax rate adjustments in neighboring municipalities and the real economic response in treated cities, as reflected in firm entry and exit dynamics.

In late 2020, the biotechnology company Biontech, headquartered in Mainz, Germany, successfully developed one of the first effective vaccines against the novel coronavirus, leading to a dramatic surge in corporate profits for a company that had previously struggled to break even. Since corporate profits are subject to local business taxes, the municipality of Mainz, along with Idar-Oberstein and Marburg, both home to Biontech production facilities, experienced an unprecedented windfall in tax revenues. These gains

exceeded their pre-shock annual business tax revenues, with Mainz's revenue increasing from €150 million in 2020 to €650 million in 2021 and reaching €1,250 million in 2022. We argue that the extraordinary size of the budget shock holds important lessons.

As a first step, we examine how local public finances respond to windfall tax gains by estimating the Biontech-induced tax windfall and the resulting dynamic budgetary adjustments. To do so, we compare the responses of treated municipalities to a control group of other municipalities in the same state of Rhineland-Palatinate using a synthetic difference-in-differences approach. Our findings are further validated using standard difference-in-differences methods and comparisons to Tübingen, home to the biotechnology firm Curevac, which was initially a promising but ultimately commercially unsuccessful contender in the race to develop a coronavirus vaccine. Compared to the existing literature, we extend the analysis of budgetary responses by incorporating municipal balance sheets, with a particular focus on public debt and capital reserves. Our results highlight the importance of considering municipalities' net asset positions in understanding the effects of such a large fiscal shock.

We document substantial revenue increases of approximately €3,440 per capita per year for the four years after 2020, driven entirely by higher local business tax revenues. This represents a significant gain compared to the pre-shock average annual expenditure of around €2,600 per capita and occurred despite the fact that treated municipalities substantially reduced their local property and business tax rates in 2022 relative to the control group. Around half of the windfall revenue was allocated to debt repayment and capital reserve accumulation, while we find no detectable effects on discretionary spending, such as public investment or the local public workforce. The remaining revenue gains were absorbed through mechanically higher contributions to the fiscal equalization system. Overall, our findings suggest a fiscally cautious response to the Biontech shock.

This is somewhat surprising, as even if policymakers expected the shock to be temporary, its magnitude would have allowed for increased discretionary spending distributed over several years. This suggests that higher tax revenues do not necessarily translate into immediate increases in spending. However, reduced debt and higher equity create fiscal space for future spending. We also find that fiscal oversight may constrain municipalities' ability to implement intertemporal resource shifting. Oversight mechanisms focus strictly on budget deficits rather than municipalities' overall net asset positions, making it harder to allocate funds over multiple years and potentially incentivizing the immediate spending of windfall gains.

In the next part of the analysis, we examine the real economic response in Mainz. While the tax cut and Biontech's success may have attracted new firms, particularly in the biotechnology sector, we find no evidence of an increase in the share of firm entries within

Biontech's industry, despite this being one of the stated goals of the city's generous tax policy. We observe a statistically significant rise in overall firm entries only initially which reverses after two years. However, relative to other similarly sized cities, we find a reduction in firm exits following the tax cut.

Going beyond the direct effect of the Biontech shock, we examine the spillover effects of the windfall gains. In 2022, Mainz significantly reduced its local business tax rate by 4.55 percentage points, an almost unprecedented decline of approximately 30%. Compared to the control group, treated municipalities lowered their business tax rates by 4.38 percentage points and, to a lesser extent, their property tax rates by 0.7 percentage points. These reductions may prompt neighboring municipalities, which often compete directly for firms and business activity, to adjust their tax rates in response.

Using data on local tax rates through 2023, we document that municipalities in close geographical proximity to Mainz reduced their local business tax rates by an average of 0.26 percentage points or roughly 2% relative to more distant municipalities, suggesting that local business tax rates are strategic complements. However, the magnitude of this response is very small, both in absolute terms and relative to the large tax cuts induced by Biontech. We find no or only delayed tax rate responses in the neighborhoods of the other two smaller treated municipalities, Idar-Oberstein and Marburg, likely because these municipalities have less influence on the regional economy compared to the larger city of Mainz. Our analysis contributes to the literature on the spatial dynamics of fiscal competition (Janeba & Osterloh, 2013; Eugster & Parchet, 2019; Parchet, 2019) and the nature of strategic tax interactions (Allers & Elhorst, 2005; Devereux et al., 2008; Parchet, 2019).

Finally, the size of the Biontech shock raises the question how such breakthrough shocks are linked with smaller, more common revenue shocks. Local governments can use additional revenue to increase expenditure, change their net asset position or decrease taxes. One empirical question is whether a tendency to spend the full amount of additional revenue found for smaller shocks in the literature also exists for larger revenue shocks. To answer the question whether this so-called flypaper effect disappears with shock size, we analyze more systematically the relationship between budget shock size and the spending response by comparing our estimate to those of 45 estimates from 18 studies in the literature.

We find that for smaller shocks, the budgetary response is more varied, with spending potentially going in any possible direction. In contrast, for larger shocks, although fewer in number, the responses are more concentrated around a value around 1, indicating that an additional unit of revenue leads to an additional unit of spending. Our results suggest a negative relationship between shock size and the spending response, as a significant

portion of the additional revenue in our case is used for tax cuts and improving the net asset position. As will be discussed later, massive shocks to public finances may perhaps be more frequent than commonly recognized. Although in principle many such instances could be studied, we focus on one case that was particularly large and unanticipated.

This paper is structured as follows. Section 3.2 briefly discusses business breakthroughs and their relevance for municipal budgets, the background of COVID-19 vaccine development and the fiscal institutional framework in Germany. In Section 3.3, we present the data sources used for empirical analysis. Section 3.4 discusses our empirical strategies, Section 3.5 reports our results on the budgetary response in the treated municipalities and Section 3.5.4 and Section 3.5.5 discuss the economic and competition effects of the Biontech shock. Section 3.5.6 situates our findings within the broader literature. Finally, Section 3.6 provides concluding remarks.

3.2 Businesses, breakthroughs and municipal budgets

Municipal tax revenues in Germany are closely tied to local business profits, making them highly sensitive to business cycles, bankruptcies and breakthroughs. There are several instances where a single firm experiences large and unexpected profit gains, often as a result of exploding global demand in a health crisis or geopolitical confrontation. In the former scenario, breakthrough innovations, such as an effective vaccine, might emerge from a single firm, leading to geographically concentrated profits or regional windfall tax gains. In the latter case, national governments may, for example, supply foreign nations with military equipment, causing a sudden increase in production and profits for related firms. Beyond increased production, firms may also profit from royalties derived from their intellectual property, even if they do not engage in production themselves. Table 3.I.1 presents several examples of prominent breakthrough cases.

Large profits can directly benefit municipal finances through local corporate taxation, as in Germany, or indirectly through fiscal equalization payments or increased local economic activity. Potential channels include increased income and sales tax revenues from higher employment or rising property tax revenues due to increased property values. The Biontech shock serves as a prime example of an unexpected breakthrough that impacts municipal budgets both directly and indirectly through spillover effects.

3.2.1 The Biontech shock

As the global COVID-19 pandemic unfolded, research into developing an effective vaccine against the SARS-CoV-2 virus rapidly intensified worldwide. One of the successful developers was the biotechnology company Biontech, headquartered in Mainz, Germany. Before the pandemic, the company primarily focused on cancer research using the relatively novel mRNA method. However, on the eve of the pandemic, Biontech pivoted to developing an mRNA-based COVID-19 vaccine, collaborating with the US pharmaceutical company Pfizer to scale up production and distribution.

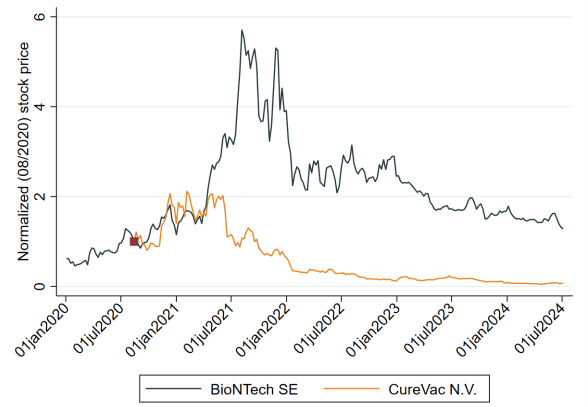
First clinical trials started in April 2020 and by November, phase III clinical trials were concluded. In the latter trials, the vaccine showed high efficacy rates and was approved for clinical use by the US, the UK and European health authorities by the end of 2020. Biontech's vaccine production initially took place in three locations across Germany: The headquarters in Mainz (state of Rhineland-Palatinate) and two additional production sites in the municipality of Idar-Oberstein (Rhineland-Palatinate) and the Hessian municipality of Marburg. Figure 3.3.1 shows a map of the three municipalities and their surrounding municipalities.

With 665 million administered doses in the European Union and 402 million in the United States by 2023 (Our World in Data, 2024), the vaccine quickly became one of the most widely used around the globe. As depicted in Figure 3.2.1, this translated into a strong increase in Biontech's revenues and profits. Net income rose dramatically from €15.2 million in 2020 to €10.3 billion and €9.4 billion in 2021 and 2022, respectively. This translated into a strong increase of local economic activity as measured by gross domestic product per capita in Mainz. According to the Federal Statistical Office, it increased by around 70% in 2021, while the average county-level GDP per capita in Germany stayed roughly flat.

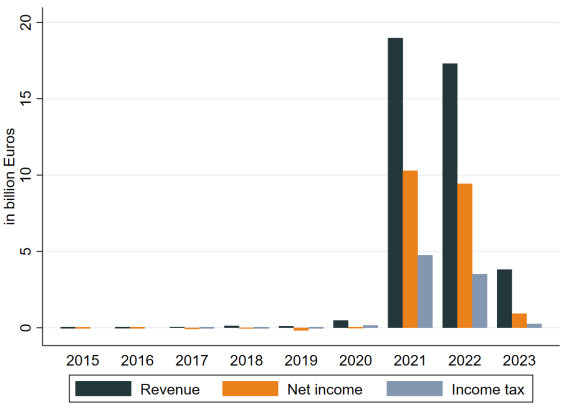
This setting is particularly suitable for examining the fiscal response due to Biontech's status as a relatively young and small start-up at the time. At first, it had few foreign subsidiaries and likely minimal tax planning capabilities. According to its financial statements, Biontech's effective tax rate was 31.6% in 2021 and 27.2% in 2022, which is close to Germany's statutory rate on corporate income, which emphasizes the importance to Biontech of municipalities' eventual tax rate cuts. However, by 2023, its effective tax rate had already declined to 21.6%, aligning with those of established multinationals like Boehringer Ingelheim and Merck, whose effective rates according to their financial statements were 22.0% and 21.9% in 2021 and 22.6% and 22.1% in 2022, respectively. Had Biontech been an established firm, municipal tax revenues could have been smaller at the same level of profit.

Figure 3.2.1: The Biontech shock and distribution of tax revenue growth

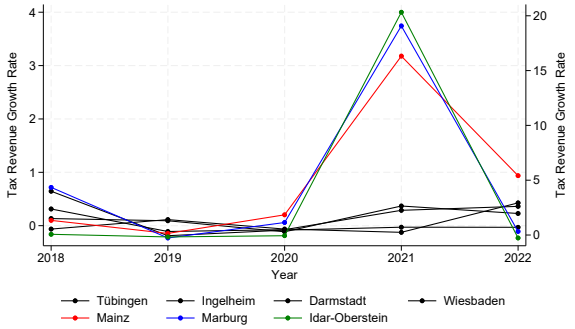
(a) Weekly normalized closing stock prices



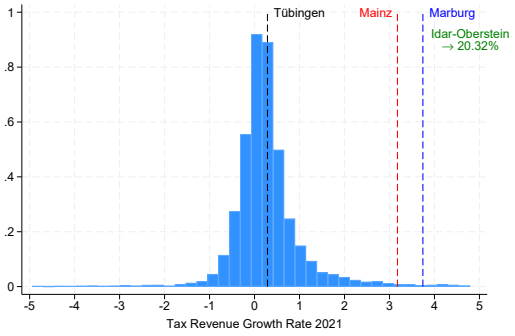
(b) Biontech key financial figures



(c) Tax revenue growth rates 2018-2022



(d) Tax revenue growth rates in 2021



Notes: Figure (a) shows the weekly normalized closing stock prices of Biontech (since January 2, 2020) headquartered in Mainz with establishments in Idar-Oberstein and Marburg and Curevac (since August 14, 2020) headquartered in Tübingen. The stock prices start diverging on April 1, 2021, when Biontech released results on the efficacy of its vaccine of 91.3% and had delivered more than 450 million vaccine by May 6, 2021. On June 16, 2021 Curevac results showed a vaccine efficacy of 47%. Figure (b) shows the increase in Biontech's net income growth from €15.2 million in 2020 to €10.3 billion in 2021 and €9.4 billion in 2022. Figure (c) shows increases in tax revenue growth rates in selected municipalities with biotechnology firms. Figure (d) shows how the Biontech tax revenue shock compares to the growth rates in tax revenues among approx. 11,000 municipalities in Germany in 2021 in the range of -500% and 500%.

A cautionary tale of what could have happened in Mainz instead is provided by Tübingen, a university town in the south of Germany. It hosts biotechnology company Curevac, which, at the time, seemed equally well positioned to launch a viable vaccine against COVID-19. Figure 3.2.1 plots its share price, normalized to its price at its initial public offering in August 2020, along with that of Biontech's. It suggests that market valuations started diverging only in April 2021, implying that markets correctly anticipated increasing profits at Biontech compared to Curevac only in 2021, the year when Mainz's budget was already impacted by increased business tax revenues.

3.2.2 Municipal public finances in Germany

In Germany, three taxes are levied on firm profits. The first is the national corporate income tax ("*Körperschaftsteuer*"), currently set at 15% of taxable profits. Half of the revenues from this tax flow to the federal budget, while the remainder is distributed across the 16 states according to a specific formula. The second tax is the solidarity surcharge ("*Solidaritätszuschlag*"), which is a 5.5% tax rate on the corporate income tax, whose revenue is allocated solely to the federal level. The third tax is the local business tax ("*Gewerbesteuer*"), which varies locally but is typically another 15% of taxable firm profits. While the tax base and deductions are determined at the federal level, municipalities have the autonomy to set a tax multiplier, which determines the ultimate tax rate.[1] The distribution of local business taxes paid by a firm operating in multiple municipalities is roughly proportional to the firm's local economic activity (Federal Ministry of Finance, 2023).[2]

While the federal and state governments experienced significant tax revenue increases due to Biontech's higher corporate income tax payments, amounting to €4.8 billion in 2021 and €3.5 billion in 2022, the local business tax directs approximately half of revenues from overall taxable profit to the municipalities where the firm's production sites are located. As a result, all three of Biontech's sites have seen strong increases in local tax revenues, while other municipalities have benefited only indirectly, if at all. For municipalities, the local business tax is typically the most important revenue source. In total, it generated around €70 billion in Germany in 2022, accounting for approximately 50% of total municipal tax revenue.

3.2.3 Theoretical considerations

To understand how the Biontech shock impacts municipalities' budgets, it is crucial to consider how temporary the shock is perceived to be. According to tax-smoothing theory, fluctuations in tax rates can exacerbate distortions caused by taxation and should be avoided. Instead, governments are advised to keep tax rates stable over time by running temporary budget deficits or surpluses to smooth over temporary shocks (Barro, 1979). However, this approach is not applicable for permanent shocks, as governments would incur a perpetual increase in debt or, in the case of positive shocks, accumulate capital reserves. In the latter case, it may be beneficial to reduce taxes, as the additional resources

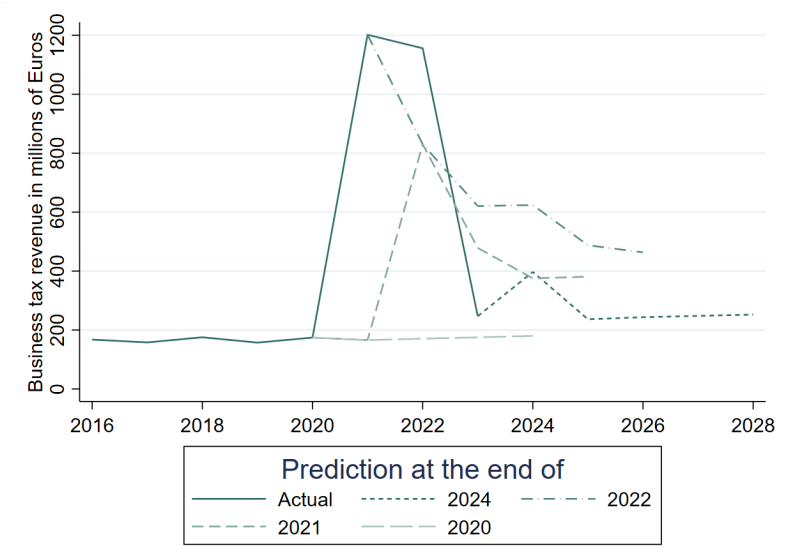
[1] The ultimate local business tax rate is calculated by multiplying the nationally set base rate of 3.5% by the local multiplier, e.g., $3.5\% \times 440\% = 15.4\%$. The local property tax rate is similarly calculated by multiplying the nationally set base rate of 0.35% by the local property tax multiplier.

[2] In brief, if a firm has multiple production sites in Germany, the local business tax is allocated based on the share of the wage bill in each municipality.

could be more productively used in the economy rather than being stored in government savings. Therefore, understanding decision makers' expectations regarding the nature of the shock is essential.

Figure 3.2.2 illustrates Mainz's expectations of future business tax revenues at different points in time based on its budget plans. When the town council voted to decrease the local business tax rate for 2022 at the end of 2021, the budget plans predicted a revenue spike to approximately €800 million in 2022, with revenues settling at around €400 million in the following years, roughly double the levels seen before 2021. An updated forecast in 2022 incorporated the actual revenue increase to €1,200 million in 2021, as well as the diminishing effect of the business tax rate reduction in 2022. At that time, Mainz predicted that revenues would eventually stabilize above €400 million in the following years. However, by 2023, business tax revenues had already dropped back to around €200 million. It turned out that the shock was larger than initially expected, but also more temporary in nature than Mainz had predicted.

Figure 3.2.2: Business tax revenue and predictions in Mainz



Notes: This figure depicts Mainz's yearly tax revenue from the local business tax in millions of Euros over time as well as the city's prediction of future revenue development at different points in time. Source: Mainz's budget plans.

Given these expectations, a modest tax rate decrease could be justified, as revenues are projected to at least double in the medium term. However, this decision should be re-evaluated at the end of 2024, when tax revenues were expected to return to previous levels. The theoretical effect on debt is less clear. As mentioned, the shock was larger than initially predicted. For a municipality like Mainz, which was highly indebted at the time, it may have been prudent to use the unexpected positive shock to reduce debt. This would lower subsequent interest payments and provide more fiscal room to smooth tax policy in the future. Capital reserves should only be built up after outstanding debt

is fully repaid. In terms of expenditure, permanently higher revenues could open the door to increased public spending, especially if there are (investment) projects that were previously constrained by budget limitations. Therefore, we hypothesize that the shock led to a reduction in tax rates and debt, as well as an increase in expenditure, with the exact magnitude of each being an empirical question.

3.3 Data

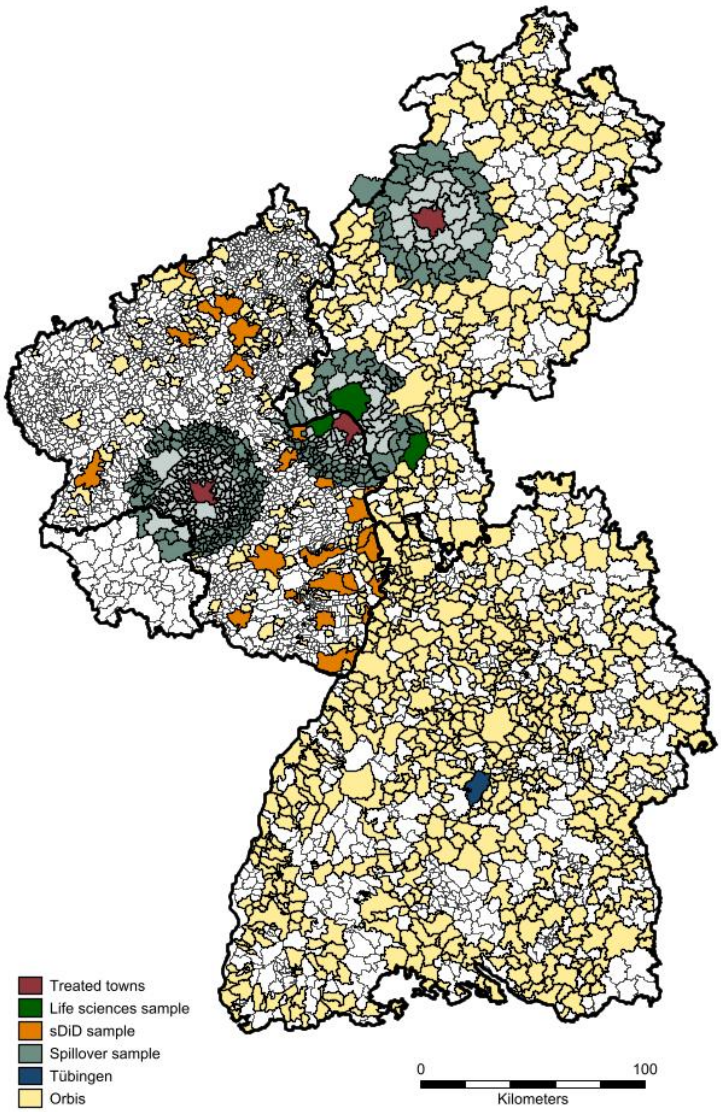
3.3.1 Municipal Balance Sheet and Tax Data

To study the budgetary response of the Biontech shock, we rely on comparable data across municipalities. As municipal accounting rules' details are a state matter, data of municipal budgets are not fully comparable across states. A comparison is further complicated by the switch from traditional cash-based to accrual accounting (Christofzik, 2019), which was mandated in 2009, but implemented at different points in time between states.

To make use of fully comparable data across budget items and time, we restrict our baseline sample to the state of Rhineland-Palatinate and exclude Marburg. Later analysis will employ more expansive data than this baseline sample. We choose Rhineland-Palatinate because two out of the three treated municipalities (Idar-Oberstein and Mainz) are located in this state, including the Biontech headquarter in Mainz. The control group is made up of the largest 33 municipalities in the same state. Although the small sample poses problems in terms of inference, it is beneficial in other aspects. Given the relatively small sample size, we digitized municipal budgets by hand, including municipal balance sheets. This is an improvement over official, ready-to-use municipal finance data sets, which typically do not contain information on assets, liabilities and equity (i.e., capital reserves) or budget forecasts. For our context, balance sheet data are particularly important because under accrual accounting rules any budget surplus which is not spent currently enters the balance sheet as a capital reserve. Given the size of the fiscal shock in question, large parts of the tax revenues would not be accounted for without balance sheet data.

Another advantage of our data is its coverage from 2016 to 2024. While the data for 2023 and 2024 are based on budgetary planning and are therefore preliminary, they provide useful indications of likely values and allow us to examine how municipalities perceived the aftermath of the Biontech shock in real time. Our data set includes detailed budget items and municipal balance sheet data, which are described in detail in Appendix A.

Figure 3.3.1: Map of samples



Notes: The figure depicts municipalities used in the respective estimation samples. Going from north to south, treated municipalities are Marburg, Mainz and Idar-Oberstein. Orange municipalities are the donor pool for the baseline synthetic control group. Neighboring municipalities within 20 kilometers and between 20 and 30 kilometers are shaded in light and dark teal. Thick black lines indicate state borders. Going from north to south, the states are Hesse, Rhineland-Palatinate, Saarland and Baden-Württemberg. Tübingen is indicated in blue. Yellow municipalities are part of the Orbis sample that had at least one new firm each year between 2017 and 2024.

3.3.2 Firm entry and exit data

Data on firm exits and entries at the level of district-free cities ("Kreisfreie Städte") between 2016 and 2023, disaggregated by firm relocations and new firms, are provided by the Federal Statistical Office.

To empirically test whether the tax policy achieved its goal of fostering a biotechnology sector in Mainz, we use Orbis data on newly established firms and compare the develop-

ment in Mainz to other municipalities in the states of Rhineland-Palatinate, Hesse and Baden-Württemberg, conditional on having at least one new firm per year between 2017 and 2024.[3]

If more biotechnology firms are drawn to Mainz, its share of biotechnology firms among new firms should increase following the Biontech shock. During this entire time period, a total of 2,900 new firms across all industries were established in Mainz and 190,997 in the entire sample, but these numbers contain only 14 and 853 firms in the field of "Scientific research and development" (NACE industry code 72), respectively. Biontech spin-offs are excluded by dropping all observations that have Biontech in the company name in any form.

3.3.3 Spillover municipalities

To analyze tax competition effects, we focus on municipalities neighboring those affected by the Biontech shock within 20 and 30 kilometers, respectively. In the case of Mainz, these include Ingelheim, Wiesbaden and Darmstadt, municipalities that actively compete in the life science sector. Data on tax multipliers are provided jointly by the Federal Statistical Office and the State Statistical Offices until 2023.

3.4 Empirical strategy

3.4.1 Synthetic difference-in-differences

Identifying a causal effect relies on a credible counterfactual of what would have happened to public finances in treated municipalities after 2020 absent the shock. A necessary condition for establishing causality is the parallel trends assumption, i.e., the parallel development of key public finance outcomes compared to the control group prior to the shock. To establish a suitable control group and parallel trends, we turn to the method of synthetic difference-in-differences (Arkhangelsky et al., 2021). While we also run a canonical difference-in-difference regression on all budget items based on Equation (3.2), the fairly small sample size makes synthetic difference-in-differences more appealing as a baseline specification in our setting. The average treatment effect on the treated (ATT)

[3] A sample of arguably more comparable municipalities, which also host a significant share of life sciences firms, i.e., Ingelheim, Tübingen, Wiesbaden and Darmstadt yields similar results.

is computed as

$$(\hat{\beta}^{sdid}, \hat{\mu}, \hat{\alpha}, \hat{\xi}) = \underset{(\beta,\mu,\alpha,\xi)}{\arg\min} \sum_{i=1}^{N} \sum_{t=1}^{T} (Y_{it} - \mu - \alpha_i - \xi_t - D_{it}\beta)^2 \hat{\omega}_i \hat{\lambda}_t. \tag{3.1}$$

We run Equation (3.1) on all available expenditure and balance sheet categories described in Table 3.C.1 separately. The intercept is denoted by μ, while α_i and ξ_t denote municipality and year fixed effects, respectively. The binary treatment indicator D_{it} is equal to one for the treatment municipalities starting in 2021 and zero otherwise. Synthetic difference-in-differences yields an average treatment effect on the treated (ATT) that is simply the average of $\hat{\beta}^{sdid}$ in the post-treatment period, which are the coefficients of interest.

Synthetic difference-in-differences combines the standard canonical difference-in-differences approach with a single treatment period (non-staggered implementation) and the synthetic control period developed by Abadie et al. (2010) using a synthetic control group for the standard difference-in-differences approach. The control group is assembled by assigning strictly positive weights $\hat{\omega}_i$ to untreated units so that the synthetic control group most closely resembles the treated units in terms of pre-treatment characteristics and outcomes. We report the average, minimum and maximum estimated unit weights across all estimations for each municipality in the donor pool in Table 3.C.3 in the Appendix. No unit ever contributes more than 12% to the synthetic control group. The majority of municipalities have non-negligible weights. Therefore, our results are not driven by any particular municipality.

Moreover, time weights $\hat{\lambda}_t$ are used to assign more weight to pre-treatment time periods that are similar to post-treatment periods. Thus, the weighted average of pre-treatment outcomes is supposed to be a good predictor for the average treatment period outcomes for control units. The intuition behind time weights is that there is a data-driven approach that determines the pre-treatment reference periods in constructing the ATT. In contrast, the canonical difference-in-differences estimator implicitly gives equal weight to all pre-treatment periods while event studies normalize to the single time period preceding treatment, implying a weight of one. By being more flexible which time periods to choose and by reducing the influence of time periods that are very different from the post-treatment periods, the methodology can remove bias and improve precision.

We estimate standard errors by using 5,000 bootstrap replications and cluster on the municipality level, which is the level of treatment assignment.[4]

[4]When we run the regression for the treated units separately, we rely on placebo estimation (Arkhangelsky et al., 2021).

3.4.2 Difference-in-Differences

As an alternative method, we estimate treatment effects using a simple difference-in-differences model with fixed effects according to regression equation

$$Y_{it} = \beta \cdot Municipality_i \times Post\ 2020_t + \theta_i + \mu_{st} + \epsilon_{it}, \tag{3.2}$$

where Y_{it} is an outcome variable measuring business activity, industry composition or tax rates in municipality i and year t, $Municipality_i$ is an indicator variable equal to one for the respective municipality under consideration and zero otherwise. This will in most cases be Mainz. $Post\ 2020_t$ is an indicator variable equal to one for years between 2021 and 2024, when municipalities were fiscally affected by Biontech's success. In an alternative specification, we expand the indicator variable to include an estimated effect for each post-treatment year separately. Fixed effects θ_i and μ_{st} control for municipality and state×year-specific effects and errors ϵ_{it} are clustered at the municipality level.

3.5 Results

3.5.1 Tax revenue response

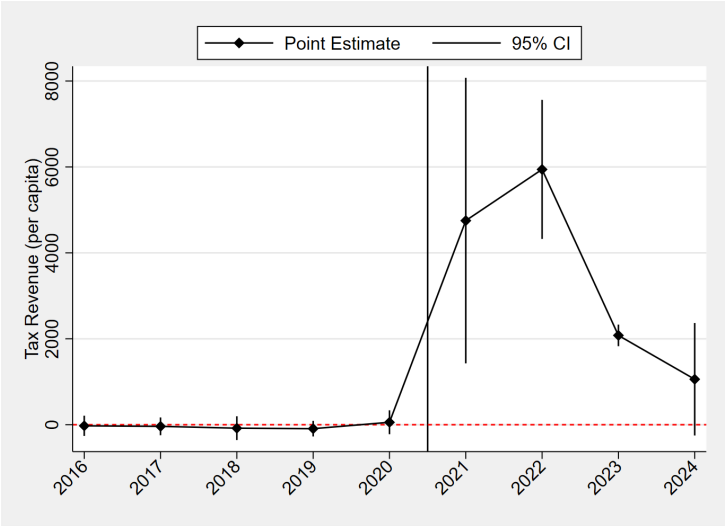
We report estimates of the windfall tax gain from hosting Biontech facilities in Figure 3.5.1, which depicts an event study plot of tax revenues in Euros per capita based on Equation (3.1). The dynamic treatment effect estimates show that tax revenues increased dramatically in the post-treatment years, peaking at around €6,000 per capita in 2022 before converging back to the synthetic control group after that, underlining that the boost was short-lived.

To provide some context, Mainz and Idar-Oberstein had total revenues per capita of around €3,400 and €2,500 in 2020, respectively. The average treatment effect on the treated (ATT) among all post-treatment years is €3,440 per capita, which is statistically different from zero for every conventional significance level (p-value < 0.001).

Exogeneity Check

Before analyzing the spending response, we conduct an exogeneity check in Figure 3.5.2. We regress pre-treatment changes (2016–2020) in economic covariates, budget items and local tax multipliers on the treatment variable within the synthetic difference-in-differences sample, without constructing the synthetic control group. The results show no statistically significant differences in trends between Biontech municipalities and the

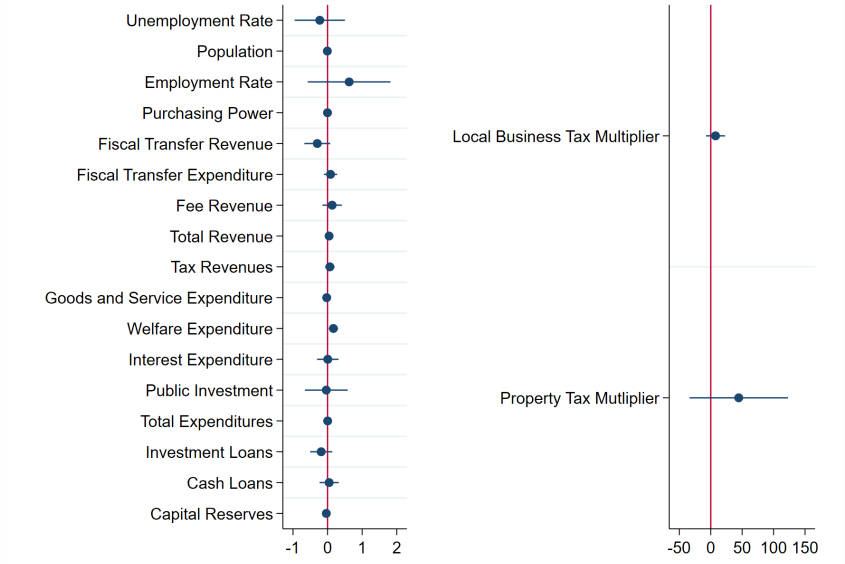
Figure 3.5.1: Tax revenues in Euros per capita



Notes: The figure compares treated municipalities (Mainz and Idar-Oberstein) to a weighted synthetic control group with similar pre-treatment trends. The results show a sharp increase in tax revenues starting in 2021, peaking at around €6,000 per capita in 2022, before returning toward control group levels in 2023. The average treatment effect on the treated (ATT) is €3,440 per capita. Confidence intervals are reported at the 95% level.

unaltered control sample, reinforcing the notion that the Biontech shock was exogenous to local policymakers and not driven by prior fiscal policy decisions.

Figure 3.5.2: Biontech Shock and pre-treatment trends



Notes: The figure represents bivariate regression estimates: We regress the pre-treatment changes (2020-2016) in a wide range of economic, budgetary and fiscal variables on the treatment variable. Standard errors are computed by 5,000 bootstrap replications. Confidence intervals are reported at the 95% level.

Comparison to Biontech's Pre-Tax Profit

A back-of-the-envelope calculation using Biontech's financial statements provides a plausibility check for our treatment effect estimates. Assuming all income tax payments in Figure 3.2.1 (b) accrued in Germany and local business tax revenue was distributed among the three municipalities based on population and statutory tax rates, the hypothetical per capita business tax revenue from 2021 to 2023 sums to approximately €12,200. This closely aligns with our cumulative excess tax revenue estimate of around €12,600 per capita in Figure 3.5.1, reinforcing the validity of our baseline results.

3.5.2 Expenditure response

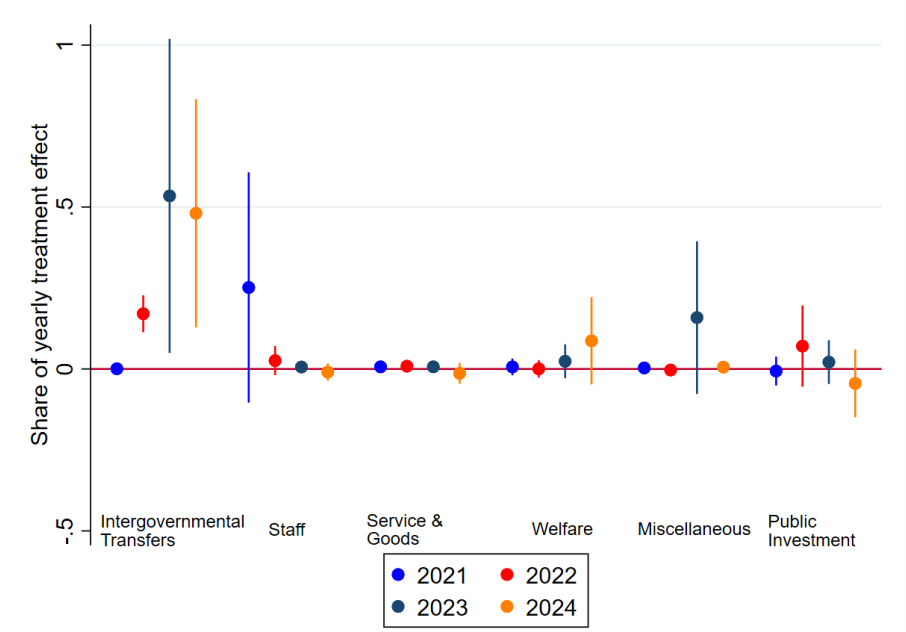
With our detailed budget data, we estimate expenditure responses by applying Equation (3.1) separately to each budget item. Figures 3.5.3 and 3.5.4 display annual expenditure responses as a share of the total annual ATT, while Figures 3.D.1 and 3.D.2 in the Appendix provide absolute effect sizes and Tables 3.D.1 and 3.D.2 show aggregate shares over all years. Since investment, cash loans and capital reserves are stock variables, we analyze their year-on-year changes. For clarity, we distinguish between current spending in Figure 3.5.3 and spending on debt repayment and capital reserves in Figure 3.5.4. To gauge deviations from pre-treatment fiscal policy, Figure 3.5.5 shows the share of these budget items in the total 2020 budget.

It becomes clear that intergovernmental transfers, debt repayment and building up capital reserves dominate the budgetary response in the treated municipalities. While the first is mechanical due to the workings of fiscal equalization systems, the latter two indicate that local policy makers pursue an at least initially fiscally cautious strategy.

Regarding current spending, except for mechanically increasing intergovernmental transfers,[5] we see statistically insignificant responses on discretionary spending such as staff or public investment, which represent the largest and fourth largest share, respectively, in the 2020 budget in Figure 3.5.5. However, even though the confidence bands contain zero, the estimates might still be economically fairly significant. For instance, we estimate the spending response on personnel to be around €263 per capita. Interestingly, this is mainly driven by the municipality of Mainz paying into the staff pension fund upfront, which again resembles a precautionary spending response. We note that the public investment response is heterogeneous in Mainz and Idar-Oberstein. When running Equation (3.1) on both municipalities separately, we see that public investment increases significantly in

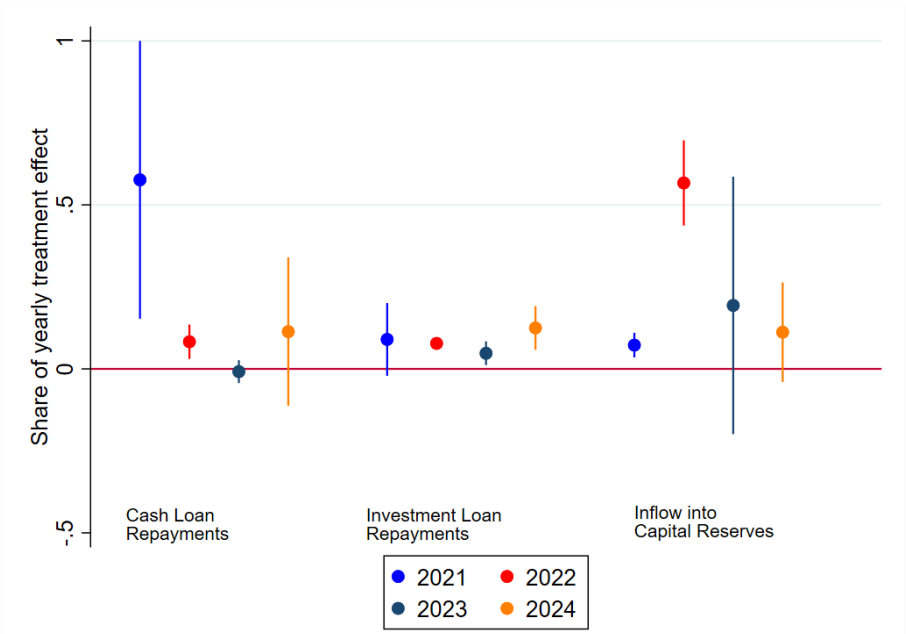
[5]The significant response in 2022 is driven by the business tax levy, which municipalities have to pay to higher levels of government. Increased contributions to the fiscal equalization scheme happen for insitutional reasons only two years after treatment in 2023.

Figure 3.5.3: Budgetary response: Annual treatment effect shares (current spending)



Notes: The estimates represent the spending response of major budget categories in every post-treatment year as a share of the respective annual treatment effect. Confidence intervals are reported at the 95% level.

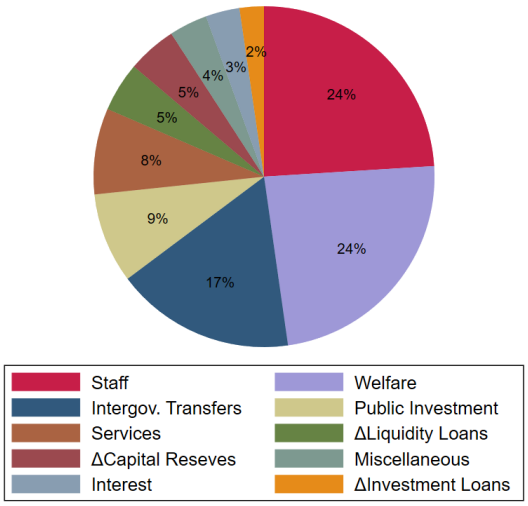
Figure 3.5.4: Budgetary response: Annual treatment effects shares debt & reserves



Notes: The estimates represent the spending response of major budget categories in every post-treatment year as a share of the respective annual treatment effect. Confidence intervals are reported at the 95% level.

Mainz in 2022 and 2023, but converges to the synthetic control group mean in 2024 (see Figure 3.D.10b).

Figure 3.5.5: Budget shares 2020



Notes: This figure shows 2020 budget shares among the entire budget in treated municipalities. In the absence of the Biontech shock, the treated municipalities spend almost half of their budget on staff and welfare whereas debt repayments or inflows into capital reserves only make up for small shares of overall spending. This is in stark contrast to the fiscal response to the Biontech shock shown in Figure 3.5.3 and Figure 3.5.4.

Both highly indebted municipalities experienced a significant reduction in public debt, especially in short-term liquidity loans. Despite the substantial decrease, they still carried outstanding debt throughout the period studied, partly because long-term loans could not be repaid immediately.

Moreover, we estimate a large inflow into capital reserves. Hence, approximately 50% of the windfall tax gains are used to improve the fiscal position of the municipality in the long run. This compares to a 2020 budget share of approximately 5%.

The single biggest share, however, flows into the fiscal equalization system. Municipalities hand over a portion of local business tax revenues to the state and federal level via the business tax levy ("*Gewerbesteuerumlage*"). Municipalities belonging to a county like Idar-Oberstein additionally cede some of their revenue to the county level via the county levy ("*Kreisumlage*"). But more importantly, municipalities with large own tax revenues are likely to no longer qualify to receive intergovernmental transfers from the municipal fiscal equalization system and hence become net contributors. Payments from or contributions to the fiscal equalization system in year t are determined by a municipality's fiscal position in year $t-2$. This explains the fiscal response materializing only in 2023 (see Figure 3.D.5b in the Appendix). This could also be one reason for building up capital reserves in the first place. As municipalities anticipate large mandated intergovernmental transfers to the equalization system, they need to bridge the time until payment occurs.

Comparison to Tübingen

The fact that another German municipality hosted an equally promising company at the time can be exploited to confirm results produced by the synthetic control group. As mentioned above, Curevac of Tübingen was also heavily invested in COVID-19 vaccine research, but ultimately failed commercially. In this sense, Tübingen might be viewed as a good example of what might have happened to Mainz absent Biontech's success. Since Tübingen is located in the state Baden-Württemberg, budget reporting requirements differ from those in Rhineland-Palatinate and not all variables studied above are observable. Despite these difficulties, the case of Tübingen confirms our results based on four public finance variables.[6]

3.5.3 Tax rate response

We estimate the response on the local business tax and the property tax multiplier, which are set by the municipalities themselves. Local tax multipliers are measured in percentage terms such that any change needs to be interpreted as percentage point changes. Note that the final tax rate is determined by multiplying by the baseline rate of 3.5% for the local business tax and by 0.35% for the property tax.

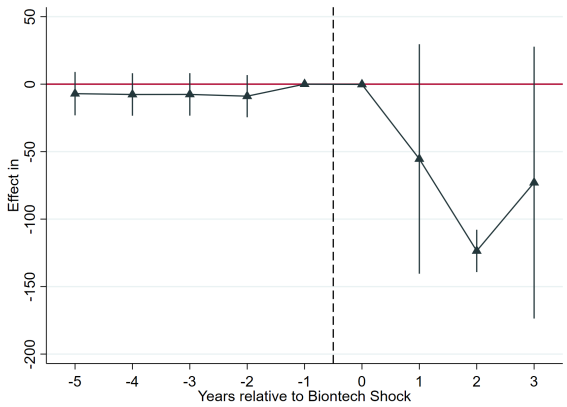
Treated municipalities reduced their tax rates relative to the control units, as depicted in Figure 3.5.6 (a) and 3.5.6 (b). Two years into the shock, the treatment group reduced their local business tax multiplier by about 125 percentage points whereas the property tax multiplier declined by almost 200 percentage points relative to the control group. This corresponds to a tax rate decrease of 4.38 percentage points and 0.7 percentage points, respectively.

On average, the tax cuts are temporary, because as of 2024 tax rates start converging to the control group. This effect is driven by the response of Idar-Oberstein, which reverted its local business tax multiplier back to 420% after two years. Mainz and Marburg only reverted their local business tax multiplier back to 440% and 380%, respectively, in 2025 after three years. The reason why the municipalities of Mainz, Marburg and Idar-Oberstein swiftly took back tax cuts is mostly due to the fact that in 2024, it failed to balance its budget. This was not least due to Biontech's deteriorating net income after vaccine demand collapsed.

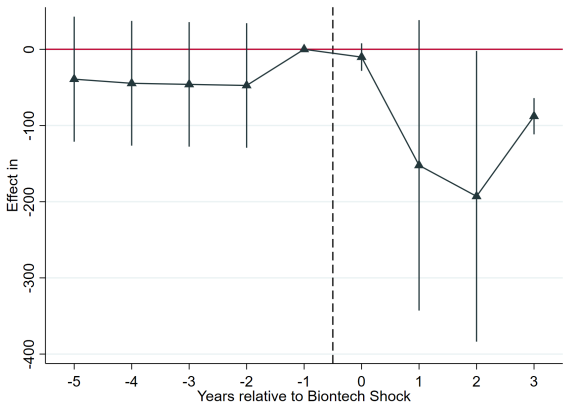
[6] In Figure 3.D.21 in the Appendix, we plot the difference between selected budget items in Mainz and Tübingen, normalized to zero in 2020. Although there are some differences compared to the synthetic difference-in-differences estimates studying for Mainz alone, for example in Figure 3.D.9, differences in terms of timing can be attributed to different accounting standards in the data used here (i.e., "*Ergebnishaushalt*"), since Tübingen only provides this. Although pre-trends before 2021 are not perfect, this can hardly be expected when comparing just two units. Overall, these simple differences seem to be in line with our main results.

Figure 3.5.6: Tax policy

(a) Local business tax multiplier



(b) Property tax multiplier



Notes: Figure shows the changes in (a) local business tax multiplier and (b) the property tax multiplier. Both estimates compare treated municipalities to a synthetic control group, estimated using a synthetic difference-in-differences (SDID) approach. Both types of taxes have been reduced in response to the Biontech shock but revert after three years. Confidence intervals are reported at the 95% level.

Why did municipalities undo tax rate decreases so quickly even though they built up substantial capital reserves that could have financed continued budget shortfalls? One reason is that some of the reserves were already allocated to specific purposes like debt repayment of loans with longer maturity. Another reason is that the responsible fiscal oversight authority in Rhineland-Palatinate insisted balancing the budget irrespective of the favorable net asset position. For example, the sheer size of Mainz's growing deficit of €103 million in 2023 and around €200 million in 2024 led the fiscal authority to refuse budget approval, especially as it could not see any measures being taken to contain the deficit. Thus, tax smoothing in this case is constrained by institutional frictions.

Political Considerations

Political factors might have played a role in shaping the tax cuts. While the mayor can propose changes to local tax multipliers, the power to change them sits with the local council. Idar-Oberstein held mayoral elections in 2022 and Mainz in 2023. State-wide council elections were held in 2024. However, the mayoral election in Idar-Oberstein only saw one candidate and in both municipalities the reductions in local tax multipliers were passed unanimously by the local council. Also, the mayoral election in Mainz was the result of the incumbent mayor moving into state politics in 2022, which does not suggest a politically motivated tax cut in 2022.

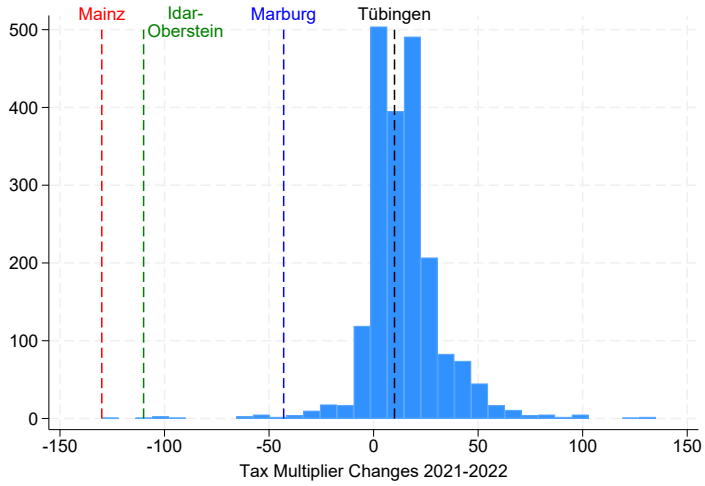
Another possible motivation for the substantial tax rate cuts could be a reluctance among policymakers to see a large portion of the windfall gains redirected into the fiscal equalization scheme. Treated municipalities might prefer reducing the local tax burden over

contributing to horizontal intergovernmental transfers. The equalization mechanism attempts to adjust for varying tax rates by applying a hypothetical uniform rate to all municipalities' tax bases, but this approach only accounts for mechanical differences and not behavioral responses to tax rate changes. Consequently, fiscal strength is overestimated for very low tax rates and underestimated for very high ones, creating incentives for municipalities to maintain higher tax rates, contrary to the observed reductions here.

Robustness

To put the business tax multiplier response's magnitude into perspective, we plot the empirical distribution of non-zero year-on-year business multiplier changes for all of the approximately 11,000 German municipalities between 2021 and 2022 in Figure 3.5.7. Mainz's and Idar-Oberstein's tax multiplier cuts of 130 and 110 points in 2022, respectively, were the largest tax cuts in this time period. Marburg's cut of 43 points still put it in the 99th percentile of negative tax changes. Also, tax decreases play a minor role in this time period, emphasizing the large response in treated municipalities.

Figure 3.5.7: Local business tax multiplier changes (2021-2022)



Notes: This graph depicts the distribution of non-zero year-on-year local business tax multiplier changes in 2021 and 2022 among all of the approximately 11,000 German municipalities. 2022 tax cuts in Mainz, Idar-Oberstein and Marburg are marked in dashed lines. Tübingen increased its tax multiplier in 2021 from 380% to 390%.

3.5.4 Firm responses

This section analyzes firm exits and entries in Mainz. Table 3.5.1 presents difference-in-differences regressions based on Equation (3.2). Columns 1 and 5 estimate the impact on total firm entries and exits per 1,000 inhabitants, respectively, while subsequent columns examine subcategories of entries and exits. Column 9 focuses on net firm entries. The

analysis is limited to approximately 100 district-free cities in Germany, which function as both counties and municipalities, as the Federal Statistical Office's data (2016–2023) is only available at the county level.

Table 3.5.1: Per capita business entries and exits after 2021

	Entries				Exits				Net
	(1) Total entries	(2) New firms	(3) Reloca-tions	(4) Acquisi-tions	(5) Total exits	(6) Clo-sures	(7) Reloca-tions	(8) Acquisi-tions	(9) Per capita
Mainz×2021	1.26***	1.43***	-0.04	-0.14*	0.61	0.71*	0.16***	-0.26***	0.64*
	(0.31)	(0.30)	(0.06)	(0.06)	(0.33)	(0.34)	(0.04)	(0.04)	(0.27)
Mainz×2022	0.67*	0.89***	0.03	-0.26***	-0.92**	-0.51	-0.03	-0.38***	1.59***
	(0.27)	(0.26)	(0.05)	(0.06)	(0.31)	(0.32)	(0.04)	(0.05)	(0.32)
Mainz×2023	-1.84***	-1.33***	-0.09*	-0.42***	-1.82***	-1.22***	-0.25***	-0.35***	-0.03
	(0.35)	(0.32)	(0.04)	(0.06)	(0.36)	(0.36)	(0.06)	(0.06)	(0.23)
City FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State×Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean(Dep Var)	8.12	6.84	0.66	0.62	7.76	6.30	0.86	0.60	0.35
N	808	808	808	808	808	808	808	808	808

This table examines the business activity response in Mainz to the Biontech shock, estimated using a difference-in-differences (DiD) approach with municipality and state-year fixed effects. Standard errors are clustered at the municipality level. Regressions estimated according to Equation (3.2). Outcomes are defined as the respective variable per 1,000 inhabitants. Standard errors in parentheses * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Compared to other large municipalities and the time period before 2021, Mainz experienced a reversal of total firm entries in Column 1. While 2021 and 2022 saw significantly more firm entries per 1,000 inhabitants, this strongly and significantly reversed in 2023. This is driven above all by new firms instead of relocation decisions or acquisitions.

Total exits are significantly lower in the later two years in Column 5. Disentangling the total exit effect reveals that there were less acquisitions away from Mainz throughout while closures and relocations are significantly smaller than zero only in 2023.

Total firm exits show a significant decline in the later two years, as indicated in Column 5. A more detailed breakdown of this effect reveals that acquisitions of firms moving away from Mainz decreased consistently over time. Meanwhile, firm closures and relocations out of Mainz were significantly lower than zero only in 2023.

The regression of net firm entries per 1,000 inhabitants in Column 9 shows a positive overall net effect in the first two years. In 2021, the number of new firm entries more than offset the exits while entries and exits both led to more firms in 2022. However, by 2023, the effects went in opposite directions, canceling each other out. These results suggest that, compared to other large municipalities, Mainz did not experience a lasting boost in new businesses. Nevertheless, the improved municipal budget and lower taxes may have played a role in retaining existing firms, helping to reduce closures and relocations.

One stated goal of Mainz's business tax rate decrease was to build on Biontech's success and create a cluster of biotechnology firms by aligning its local business tax rate

with its neighboring municipality of Ingelheim. In addition, Mainz pursued a marketing campaign and formed a strategy of finding designated areas for establishing new firms (Landeshauptstadt Mainz, 2022).

Table 3.5.2: Mainz: Industry share change among new firms

	(1) NACE 72	(2) NACE 721	(3) NACE 7219	(4) NACE 7211
Mainz × Post 2020	-0.0008	0.0001	0.0004	-0.0004
	(0.0010)	(0.0008)	(0.0008)	(0.0002)
City FE	Yes	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes	Yes
Mean(Dep Var)	0.005	0.003	0.003	0.001
N	190,997	190,997	190,997	190,997

Regressions estimated according to Equation (3.2). Standard errors in parentheses * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Results of the effect on new biotechnology firms as a share of all new firms are presented in Table 3.5.2. This analysis helps assess whether Mainz's tax policy and the Biontech shock led to a relative increase in biotechnology start-ups compared to other places. It relies an a very low number of instances since the yearly number of biotechnology firm entries is low. Table 3.F.1 in the Appendix lists annual instances of start-ups in "Scientific research and development" in Mainz and Ingelheim, respectively.

Here, Y_{it} is an indicator variable equal to one if a new firm is part of the respective industry indicated in the column header and zero otherwise. The outcome variable is defined ever more narrowly going from Column 1 to 4, starting with "Scientific research and development" (NACE industry code 72), followed by "Research and experimental development on natural sciences and engineering" in Column 2, "Other research and experimental development on natural sciences and engineering" in Column 3 or, alternatively, "Research and experimental development on biotechnology" in Column 4. All four specifications suggest that Mainz's share of science-based firms among new firms has not significantly increased.

Biontech's success may have influenced other municipalities beyond Mainz. To explore this, we replicate the analysis from Table 3.5.2 for Ingelheim in Table 3.5.3. Ingelheim, which neighbors Mainz, is home to several life sciences firms, most notably the pharmaceutical giant Boehringer Ingelheim. Agglomeration effects, such as access to research institutions, talent and networks that contributed to Biontech's success, can play a crucial role in firm location decisions. While Mainz may be an attractive hub for biotechnology start-ups, firms might also prioritize historically stable, lower tax rates, making Ingelheim a competitive alternative that still benefits from its proximity to Mainz.

Regressions in Table 3.5.3 paint a mixed picture. Although the share of life sciences firms

Table 3.5.3: Ingelheim: Industry share change among new firms

	(1) NACE 72	(2) NACE 721	(3) NACE 7219	(4) NACE 7211
Ingelheim × Post 2020	0.0008	0.0002	0.005***	-0.005***
	(0.0009)	(0.0007)	(0.0007)	(0.0002)
City FE	Yes	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes	Yes
Mean(Dep Var)	0.005	0.003	0.003	0.001
N	190,997	190,997	190,997	190,997

Regressions estimated according to Equation (3.2). Standard errors in parentheses * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

among new firms has not changed compared to the control group in aggregated industries in Column 1 and 2, there is a positive change in the share of more disaggregated NACE code 7219 firms and a negative change in the share of NACE code 7211 firms. Overall, there is no evidence for a significant agglomeration effect as measured by the share of biotechnology firms among new firms.

Robustness

While the analysis indicates that Mainz did not experience a disproportionate rise in the share of new firms in biotechnology-related industries, the time period may be too short to draw definitive conclusions. An alternative specification in Table 3.5.4, which treats all years after 2021 as post-treatment presents significantly negative point estimates across all four industries, suggesting that biotechnology firms may have been more likely to establish themselves in 2021, coinciding with Biontech's first commercial breakthrough. This could reflect an influx of suppliers, though Biontech subsidiaries themselves are excluded from the analysis, ruling them out as a direct driver of this effect.

Table 3.5.4: Mainz: Industry share change among new firms

	(1) NACE 72	(2) NACE 721	(3) NACE 7219	(4) NACE 7211
Mainz × Post 2021	-0.004***	-0.003***	-0.002**	-0.001***
	(0.0010)	(0.0007)	(0.0006)	(0.0002)
City FE	Yes	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes	Yes
Mean(Dep Var)	0.005	0.003	0.003	0.001
N	190,997	190,997	190,997	190,997

Regressions estimated according to Equation (3.2). Standard errors in parentheses * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 3.F.2 in the Appendix presents results comparing industry shares in Mainz to Tübingen only, since both harbored a promising vaccine manufacturer. Compared to

Tübingen, we observe differential effects in the more narrowly defined industries, going in both directions. Thus, the previous results warrant caution.

3.5.5 Tax competition

Next, we examine whether the tax cuts in the three directly affected municipalities triggered tax rate spillovers in neighboring municipalities. The three directly affected municipalities decreased their tax rates in 2022 by 4.55 percentage points to 10.85% (Mainz), 3.85 percentage points to 10.85% (Idar-Oberstein) and 1.51 percentage points to 12.50% (Marburg). Neighboring municipalities might have felt pressured to lower their tax rates as well in order to stay competitive for local businesses. Figure 3.5.8 focuses on potential tax rate spillovers. Specifically, Figure 3.5.8a plots the share of municipalities that increased their business tax rate in 2022 as a function of their geographical distance to the treated municipalities.

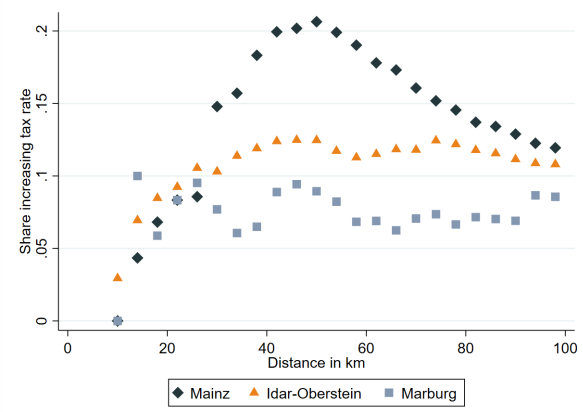
We focus on tax rate increases in neighboring municipalities instead of tax cuts because empirically at the time tax cuts played only a very minor role outside the treated cities. In fact, no municipality closer than 20 kilometers to treated municipalities decreased taxes in 2022. For distances beyond that no clear picture emerges in Figure 3.G.1 in the Appendix.

The graph in Figure 3.5.8a remains descriptive in nature, as a number of factors could confound the share of municipalities increasing taxes. In 2022, there is a positive gradient in the tax hike frequency in terms of distance to the three treated municipalities. Closer municipalities are less likely to increase taxes than those farther away. The gradients are broadly similar for all three municipalities until around 25 kilometers. At this point, municipalities only near Mainz continue to have a higher probability of increasing taxes with distance. Reasons for this could be the magnitude of Mainz's tax rate decrease, which was the most substantial among the three, as well as its regional importance.

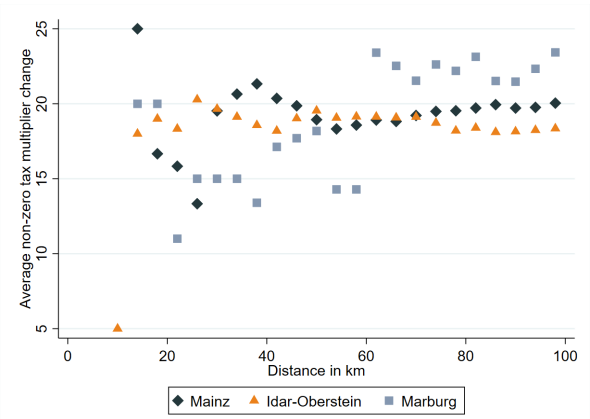
Figure 3.G.2 in the Appendix repeats this exercise for the year 2019 before COVID-19, when no tax rate changes were implemented in the three municipalities. Here, the hump for Mainz is not present and, if anything, the gradient seems to be reversed in the vicinity of Mainz and Idar-Oberstein, with less frequent tax increases overall. For this reason, it is plausible that the tax rate reductions in treated municipalities had a moderating effect on tax increases in the neighborhood, which is in line with the presence of local tax competition.

Figure 3.5.8b depicts the average tax rate change, conditional on a change taking place. In contrast to Figure 3.5.8a there seems to be no clear pattern of a gradient. Instead, irrespective of distance the average tax rate change was around 20 multiplier points, which

Figure 3.5.8: Business tax rate changes by radius in 2022



a) Share of municipalities increasing tax rates



b) Average non-zero tax rate change

corresponds to a business tax increase of 0.7 percentage points. Therefore, if there is a tax response at all, it is on the extensive margin instead of the intensive margin. Progressively fewer municipalities in the vicinity of affected municipalities decide to increase taxes. Those that do, however, increase on average by the same amount as more distant ones.

The reluctance to increase taxes close to tax-cutting municipalities might be explained by the role of advance business tax payment notices. These stipulate a firm's quarterly tax payment. In case of a tax rate change, the notices need to be altered to reflect the new tax rate and municipalities are obliged to send these out to all local taxpayers. This procedure makes tax rate increases, even very small ones, salient to taxpayers. Taxpayers, in turn, might be more likely to look for alternative locations to take their business if their attention is drawn to the local tax increase. In the absence of such a reminder, they might be rather oblivious to the surrounding tax environment such as Mainz's dramatic tax cut. This might also be reflected in the lack of documented discussions in municipal councils surrounding their neighbors' tax cut. The dominant response to neighbors' large tax cuts might thus have been to let sleeping dogs lie.

This spillover result is corroborated by regression evidence in Table 3.5.5 where the dependent variable is the business tax multiplier. It incorporates a counterfactual tax multiplier development by using an alternative version of the empirical design introduced in Equation (3.2): Here, the difference-in-differences estimator compares business tax multipliers in 2022 and 2023, respectively, to 2016-2021 of close municipalities (center closer than 20 kilometers) to municipalities beyond this radius (radius between 20 and 30 kilometers). As both areas are of comparable size, the number of observations in the spillover treatment and control group is roughly equal. This is done for each treated municipality individually, including all neighboring municipalities within the outlined area and, as a robustness check, including only those municipalities in this area belonging to

the same state as the respective treated region. The samples are visualized in the map in Figure 3.3.1.

Table 3.5.5: Business tax multiplier within 20 km vs outside

	Mainz		Idar-Oberstein		Marburg	
	(1)	(2)	(3)	(4)	(5)	(6)
	All	Same state	All	Same state	All	Same state
Distance<20 km×Year 2022	-3.11*	-5.06**	0.73	0.73	-1.80	-1.80
	(1.41)	(1.61)	(0.96)	(0.97)	(2.49)	(2.49)
Distance<20 km×Year 2023	-5.08**	-7.35**	-3.41*	-3.48*	1.36	1.36
	(1.78)	(2.32)	(1.47)	(1.50)	(4.23)	(4.23)
City FE	Yes	Yes	Yes	Yes	Yes	Yes
State×Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean(Dep Var)	373	367	372	372	379	379
N	1,136	744	2,640	2,592	408	408

Regressions estimated according to Equation (3.2). Standard errors in parentheses * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Compared to the years before 2022 and municipalities beyond the 20-kilometer radius, municipalities close to Mainz exhibit a significant, negative effect on the tax multiplier. The effect is more pronounced for 2023 compared to 2022, possibly due to tax adjustment frictions. The estimate of -7.35 percentage points in Column 2 for year 2023 corresponds approximately to a 0.26 percentage point lower business tax rate. As pointed out above, this is driven by less increases rather than more decreases compared to the control group.

Forgone tax increases as a response to tax cuts materialize only in 2023 close to Idar-Oberstein. A possible explanation is the timing of the decision to decrease the local business tax multiplier, which happened only in the beginning of 2022, leaving neighboring municipalities little time to respond in 2022.

Overall, tax rate spillovers appear to be limited, even for Mainz and Idar-Oberstein. The estimated spillover effect is modest relative to the substantial tax cuts in the directly affected municipalities. In 2023, the average spillover response of -7.35 percentage points in Column 2 accounts for just 5.6% of Mainz's 130 percentage point tax rate reduction. However, even small spillover effects may influence tax competition if fewer neighboring municipalities adjust their rates, reinforcing differences in tax attractiveness through the outlined salience mechanism.

3.5.6 The shock size in context

To place the results in this study in context, we compare our spending response to those found in a non-exhaustive survey of the literature on the effects of local government budget shocks. To that end, we collect 45 causally identified spending estimates from 18 studies as well as key study characteristics. The studies are listed in Table 3.I.2 in

the Appendix. Collected characteristics include the respective shock permanence, timing, sign, source, a proxy for the shocks' size and local government level.[7] Spending estimates are normalized to correspond to the per capita change in local currency relative to a 1-unit local currency budget shock. We estimate the empirical model

$$Y_{ip} = \beta X_{ip} + \gamma_p + \delta_c + \varepsilon_{ip}. \tag{3.3}$$

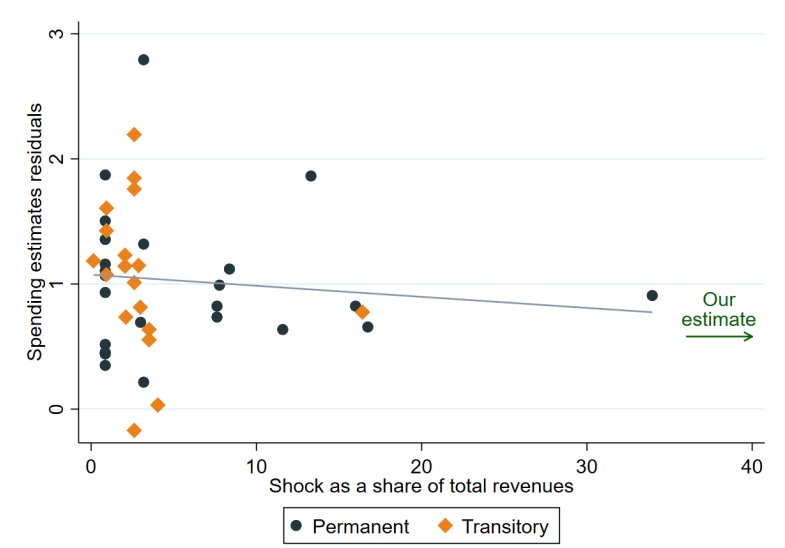
The ith normalized spending estimate Y_{ip} in paper p is regressed on the estimate characteristics X_{ip} outlined above as well as paper fixed effects γ_p and country fixed effects δ_c to control for potentially unobserved, time-invariant paper and country characteristics. An alternative version weights estimates by the inverse of their reported standard error, if available. This is a common methodology in the meta-analysis literature to give more weight to more precisely estimated coefficients (e.g., Neisser, 2021). The error term ε_{ip} is clustered at the paper level to account for potential error dependence within papers.

Table 3.I.3 and Table 3.I.4 in the Appendix report regression results of different specifications based on Equation (3.3). Of most interest to us is the relationship between the spending response and size of the shock. We document that the larger the shock the smaller the proportion of it is spent, but not always significantly. This insight is revisited in Figure 3.5.9, which plots residuals of a regression in the form of Column 7 of Table 3.I.3, excluding shock share, against the shock size. It distinguishes estimates by their persistency. There is a wide dispersion of estimates at the low end of shock sizes, while larger shocks, though fewer in number, concentrate closer to 1. The gray line represents a linear fit and implies a slight negative slope that is, however, not statistically distinguishable from zero.

To make our spending estimate comparable to the ones found in the literature, we need to account for the tax rate cuts in our setting and also for spending items that are not observed in the literature. The former is done by scaling the budget response by the hypothetical, mechanical budget response if tax rates had stayed constant. In both Mainz and Idar-Oberstein, tax revenues mechanically were around 80% of what they could have been absent the tax cuts when scaling observed business tax revenue by the original tax rates. Since we see a small initial firm response in Section 3.5.4 and behavioral responses to the tax rate change seem likely, this number is a lower bound estimate.

[7] Shock permanence is defined according to authors' own classification or, if not available, categorized as permanent if there is a lasting and significant effect on the budget for at least five years and categorized as transitory if not. Typical sources of budget shocks are local resource booms or changes to allocation in fiscal equalization schemes. The proxy for shock size is calculated by dividing the budget shock by the average budget size in the sample and multiplying by 100 to be interpreted in percent. Budget size is either average total annual revenues in the sample or, if not available, average total annual spending. Levels of government include municipalities, counties and school districts.

Figure 3.5.9: Spending response residuals and shock size



Notes: This graph depicts residuals of expenditure estimates from the local public finance literature as a function of shock size, i.e., shock share of total budget, and a linear fit. Expenditure estimates are residualized according to Column 7 of Table 3.I.3. Estimates are taken from studies listed in Table 3.I.2. The graph distinguishes between shocks that are considered permanent and transitory by the authors. Our estimate represents a shock of around 160% of total revenues and 58% of the shock is spent.

In a second step, we subtract the part of our spending estimate that is not observed in the literature, i.e., the change in capital reserves, yielding a spending response of $1 - 0.27 = 0.73$ on average over four years in Figure 3.D.3. Taken together, this results in a comparable spending response of approximately 0.58 at a shock size of 160% of the pre-treatment total annual budget. It is depicted as a green arrow in Figure 3.5.9 and fits into the overall picture of the literature's estimates, though not precisely the linear fit. Our large shock emphasizes the vague relationship between spending response and shock size in the literature. While general statements on the effects of smaller shocks are difficult to make, larger shocks yield spending responses strictly below one. As the fiscal stakes increase, the budgetary response is less concentrated on spending.

Stripping our estimate further of mechanical elements, such as fiscal equalization payments or intertemporal resource shifting such as debt repayment, reduces it to an active spending response of 0.14. Since in the literature expenditure items are not disaggregated to the degree we are able to do, the comparative analysis remains at the aggregated level from above.

What are implications and lessons from the Biontech shock? Although the sheer size of the shock seems to be somewhat of an outlier, our estimates of the fiscal responses are not far from other, more moderate estimates found in the literature. On top of this, municipalities not only in Germany often find themselves in the position of hosting one dominant business, opening themselves up to the same opportunities, incentives and

dependencies studied in this paper (cf. Table 3.I.1). This shock's magnitude can help to accentuate prevalent mechanisms.

3.6 Conclusion

We examine the impact of a sudden tax revenue windfall driven by Biontech's vast and surprising commercial success in three municipalities, using detailed budget data, including current and preliminary figures. Affected municipalities significantly lowered local tax rates, yet public budgets still approximately doubled in the years following the shock. Much of the excess revenue was allocated to debt reduction and capital reserves, supporting long-term fiscal smoothing, unlike the tax cuts. Ultimately, the revenue surge shifted these municipalities from net beneficiaries to net contributors within the fiscal equalization system. However, as tax revenues declined back to pre-treatment levels faster than expected, municipalities reversed tax rate cuts three to four years after the shock. This occurred despite substantial savings, as fiscal oversight authorities pushed to narrow budget deficits.

Beyond public finances, there is little evidence that Mainz, the primary beneficiary, was able to build on this one-off success. While we find no lasting increase in business entries, significantly fewer firms relocated or shut down following the tax cut. As of 2024, there is no indication of a rising share of life sciences firms among new businesses, tempering expectations of a biotechnology cluster emerging. However, structural changes may take longer to materialize.

In terms of tax competition, neighboring municipalities responded subtly, primarily by reducing the probability of raising tax rates rather than lowering rates. This limited response may be attributed to fiscal constraints during the pandemic or the expectation that tax cuts would be temporary. Indeed, two municipalities directly affected by the Biontech shock have already reinstated their pre-shock tax rates in 2024 and 2025, respectively.

Our study offers valuable lessons for policymakers. Large fiscal shocks can occur unexpectedly and in a concentrated manner, emphasizing the importance of learning from past cases. The shock examined here proved more transitory than initially anticipated, with no lasting impact on the economic outlook or industry cluster formation. However, the windfall enabled substantial public savings, which improves municipalities' ability to weather future downturns. An additional challenge was the uncertainty surrounding payments into the fiscal equalization scheme years after excess revenues were recorded. These considerations emphasize the need for cautious fiscal planning. Even with cautious planning, fiscal oversight can limit municipalities' ability to allocate funds over multiple

years, potentially incentivizing immediate spending rather than long-term planning. A more holistic approach to assessing municipalities' fiscal positions could help address these constraints.

Appendix

3.A Budget item description

Budget Item	Description
Municipal Revenues	
Tax Revenues	Contains all tax payments to municipalities from income, value-added, local business and property tax and other minor municipal taxes.
Fiscal equalization transfer, levy and grant revenues	Revenues from fiscal equalization schemes.
Welfare revenues	Direct welfare-earmarked transfers from state and federal levels.
Public-oriented fees	Fees for administrative processes.
Private-oriented fees	Childcare fees, cemetery fees, etc.
Interest Revenue	Revenues from interest payments.
Municipal Expenditures	
Staff and personnel expenditure	Expenditures on staff and personnel-related costs.
Service expenditure	Spending on material, energy costs, maintenance of municipal assets.
Transfer, levy and grant expenditures	Payments into the fiscal equalization system, transfers to local associations.
Welfare Spending	Payouts of welfare spending that is carried out by municipalities (payments to refugees, housing and heating subsidies, disability payments).
Interest Expenditure	Expenditures on interest payments.
Public Investment Expenditures	Spending on public infrastructure and capital projects.
Capital Reserves	Reserves held for investment or emergency purposes.
Investment Loans	Specific loans that can only be used to finance public investment.
Cash-loans	Loans taken out by municipalities designed to meet short-run liquidity constraints.

Note: From municipal balance sheets, we collect the stock variables capital reserves, investment loans and cash-loans. Investment loans are specific loans that can only be used to finance public investment, while cash-loans are taken out by municipalities to meet short-term liquidity needs.

3.B Local public finance

Besides revenues from the local business taxes, German municipalities receive 15% of national income tax revenue and 2% of national value-added tax revenue. These make up around 38% of overall municipal tax revenue. The property tax, whose tax multiplier is also set by municipalities, plays a relatively small role compared to other revenue sources. The biggest source of revenues for municipalities in general are intergovernmental transfers from the municipal fiscal equalization scheme (Federal Statistical Office, 2023).[8]

While some municipal spending is mandated by state and federal government (e.g., welfare spending), municipalities enjoy considerable autonomy in other areas of public policy. For instance, childcare and maintaining public schools are municipal responsibilities. The extent and quality at which these public services are provided is largely left to municipalities. Other areas, such as cultural and recreational activities, are fully optional. Moreover, German municipalities carry out around 55% of public investment in Germany (Federal Statistical Office, 2023). Hence, gaining insights into the marginal propensity of public investment is important for policymakers, who want to boost public investment.

[8]While these systems are designed by each state individually, its mechanics are widely similar: On an annual basis, the state governments define an amount that is to be redistributed across municipalities according to a pre-defined formula. This formula weighs fiscal need (mostly measured by the number of inhabitants) against fiscal strength (a combination of tax revenues). If fiscal need exceeds fiscal strength, the difference is compensated to some extent. So-called "abundant" municipalities, whose fiscal strength is larger than their need, receive nothing and sometimes contribute to the overall amount to be redistributed through municipal levies. If municipalities experience windfall tax gains like the three firm sites of Biontech, their fiscal strength shoots up and are likely to become abundant and hence a net contributor to the fiscal equalization system.

3.C Descriptive statistics

Table 3.C.1: Descriptive statistics: Before treatment (averages 2016-2020)

	Treatment Group		Control Group		Mean Difference	
	Mean	SD	Mean	SD	Difference	t-statistic
Revenues per capita (€)						
Total Revenue	2645.12	497.65	3000.31	1521.13	355.19	(1.68)
Tax Revenue	1389.67	306.84	1743.05	1504.42	353.38*	(2.07)
Intergovernmental Grant Revenue	518.34	149.95	544.11	310.68	25.77	(0.46)
Welfare Revenue	289.47	172.57	357.23	261.06	67.76	(1.13)
Public-related Fees	76.46	16.30	99.39	47.25	22.93**	(3.38)
Private-related Fees	43.55	16.24	69.95	50.29	26.40***	(3.80)
Interest Revenue	31.55	29.92	44.20	77.81	12.65	(1.06)
Current Spending per capita (€)						
Total Expenditures	2543.48	446.40	2722.48	1183.65	179.00	(1.00)
Staff and Personnel Expenditures	742.97	120.60	761.69	174.31	18.72	(0.45)
Service Expenditures	262.53	62.32	376.38	129.30	113.86***	(4.93)
Welfare Expenditures	766.33	419.89	623.50	521.97	-142.83	(-1.01)
Intergovernmental Grant Expenditures	594.44	122.07	808.76	1167.31	214.32	(1.86)
Interest Expenditures	117.53	18.18	75.53	62.84	-42.00***	(-5.12)
Public Investment	241.01	118.50	355.37	311.07	114.36*	(2.41)
Balance sheet per capita (€)						
Liquidity Loans	3099.86	270.48	1738.81	2261.79	-1361.06***	(-5.98)
Investment Loans	1824.99	772.84	1233.85	732.12	-591.14*	(-2.33)
Capital Reserves	1961.74	2258.95	4528.79	4425.77	2567.05**	(3.11)
Population	123199.20	98136.67	51217.58	39956.13	-71981.62*	(-2.30)
Observations	10		115		125	

Table 3.C.2: Descriptive statistics: After treatment (averages 2021-2024)

	Treatment Group		Control Group		Mean Difference	
	Mean	SD	Mean	SD	Difference	t-statistic
Revenues per capita (€)						
Total Revenue	7562.99	2492.05	3480.97	1228.65	-4082.02**	(-4.58)
Tax Revenue	5136.96	2435.56	1930.18	1165.46	-3206.78**	(-3.69)
Intergovernmental Grant Revenue	506.15	213.34	720.68	382.80	214.52*	(2.51)
Welfare Revenue	377.02	231.39	418.34	313.38	41.31	(0.47)
Public-related Fees	89.87	22.06	116.28	53.35	26.41*	(2.76)
Private-related Fees	46.45	17.28	80.83	74.43	34.38**	(3.48)
Interest Revenues	37.61	31.60	28.16	34.40	-9.45	(-0.81)
Current Spending per capita (€)						
Total Expenditures	5159.80	2494.02	3329.91	1340.21	-1829.89	(-2.05)
Staff and Personnel Expenditures	1212.76	553.50	1001.39	223.05	-211.37	(-1.07)
Services Expenditures	409.06	112.82	510.17	169.95	101.11*	(2.32)
Intergovernmental Grant Expenditures	936.93	603.19	695.14	582.81	-241.79	(-1.09)
Transfer Expenditures	2068.46	1955.91	882.75	1142.17	-1185.71	(-1.69)
Interest Expenditures	128.38	117.80	70.85	60.64	-57.53	(-1.37)
Public Investment	636.36	406.41	623.25	378.53	-13.11	(-0.09)
Balance sheet per capita (€)						
Liquidity Loans	650.63	336.26	1510.76	2151.47	860.13**	(3.39)
Investment Loans	1477.19	1303.80	1713.84	1064.81	236.66	(0.50)
Capital Reserves	4432.95	4693.49	4562.09	4391.49	129.14	(0.08)
Population	127128.63	103398.78	52245.05	40718.29	-74883.57	(-2.03)
Observations	8		92		100	

Table 3.C.3: Unit weights of control units in the synthetic control group

Municipality	**Population**	**Mean Weight**	**Min. Weight**	**Max. Weight**
Landau (Pfalz)	46,685	0.053	0.044	0.067
Haßloch	20,195	0.052	0.031	0.112
Remagen	17,156	0.049	0.039	0.074
Germersheim	21,350	0.049	0.030	0.097
Pirmasens	40,176	0.049	0.008	0.071
Alzey	18,820	0.049	0.018	0.080
Bad Dürkheim	18,553	0.047	0.022	0.063
Neuwied	65,918	0.047	0.028	0.060
Speyer	50,741	0.046	0.016	0.071
Mayen	19,284	0.045	0.030	0.057
Kaiserslautern	99,662	0.045	0.002	0.090
Wörth am Rhein	18,217	0.044	0.000	0.102
Neustadt (Weinstraße)	53,306	0.044	0.030	0.052
Frankenthal	49,192	0.043	0.037	0.047
Andernach	30,132	0.043	0.012	0.051
Boppard	15,639	0.043	0.000	0.060
Bad Kreuznach	52,385	0.041	0.030	0.047
Worms	83,459	0.041	0.017	0.055
Ludwigshafen	177,219	0.039	0.008	0.060
Bingen am Rhein	25,736	0.039	0.005	0.054
Trier	110,674	0.039	0.004	0.057
Koblenz	113,388	0.038	0.005	0.071
Ingelheim am Rhein	35,161	0.015	0.000	0.038

This table lists all municipalities in the donor pool in the synthetic control group design and their population in 2020 as well as the average, minimum and maximum estimated unit weight across all estimations.

3.D Expenditure response

Figure 3.D.1: Budgetary response: Annual absolute treatment effects (current spending)

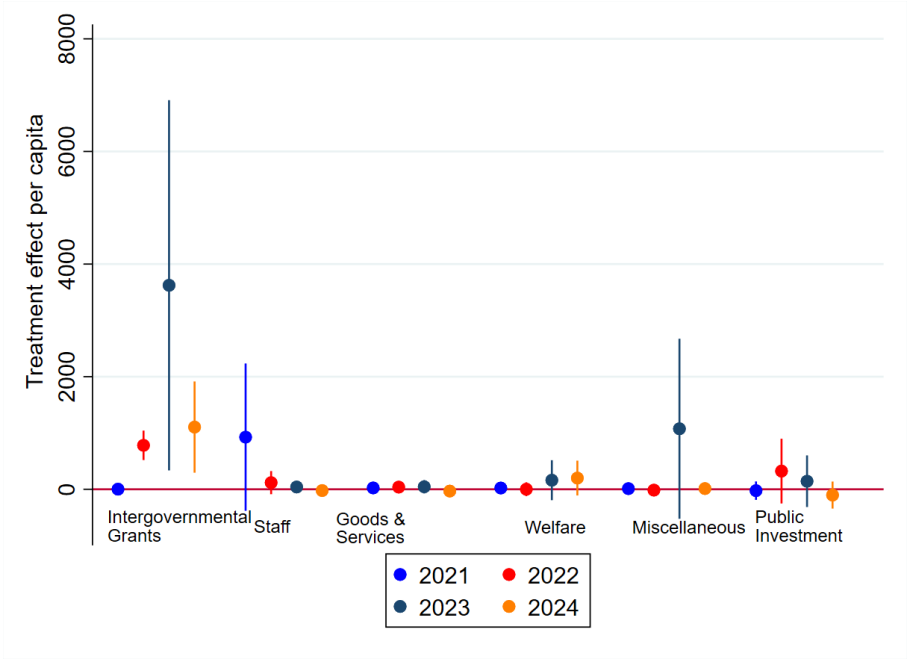


Figure 3.D.2: Budgetary response: Annual absolute treatment effects of debt & reserves

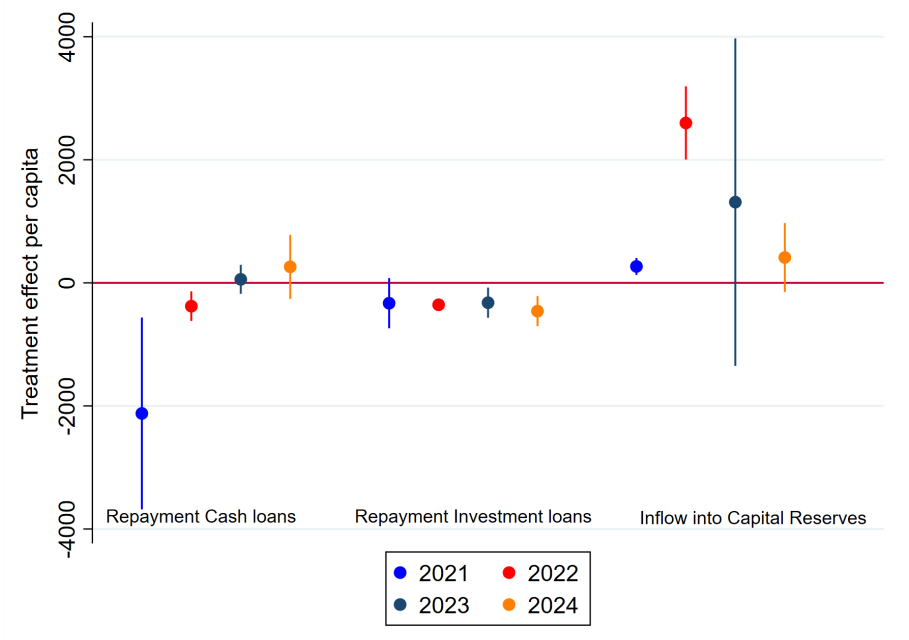
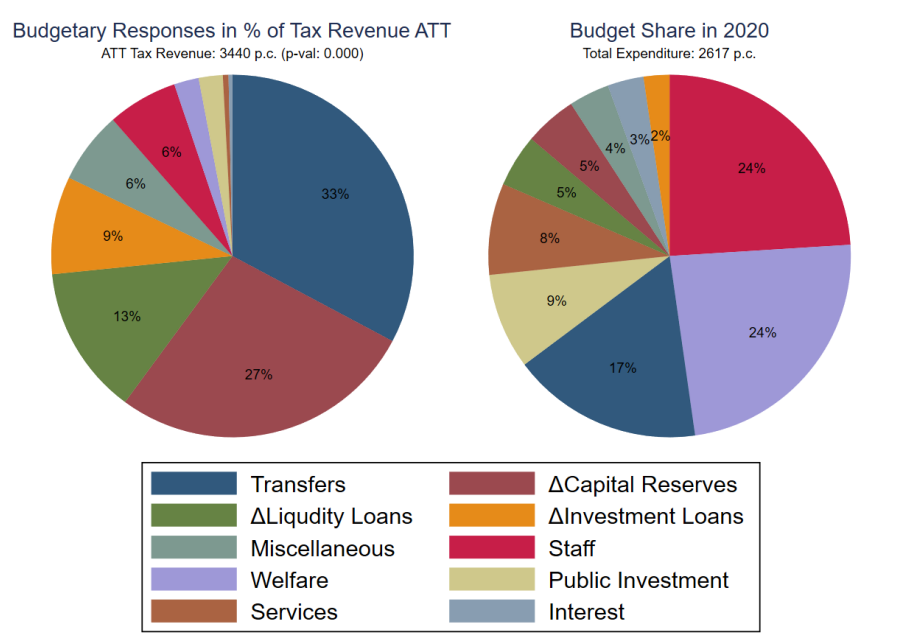


Figure 3.D.3: Budgetary response breakdown



Notes: This figure shows 2020 budget shares (right-hand graph) and estimates (left-hand graph) of the reallocation of municipal expenditures to the windfall tax revenues from the Biontech shock, using a synthetic difference-in-differences (SDID) approach. Results show that approximately 50% of the revenue gains were allocated to debt repayment and capital reserve increases, while discretionary spending, such as public investment, showed insignificant changes. Respective p-values are presented in Tables 3.D.1 and 3.D.2 in the Appendix.

Table 3.D.1: Current spending

	(1) Transfer	(2) Miscel-laneous	(3) Welfare	(4) Service	(5) Staff Expenditure	(6) Public Investment	(7) Interest Expenditure
Share	32.78%	6.43%	2.20%	0.50%	6.26%	2.15%	0.35%
p-value	0.012	0.182	0.38	0.456	0.066	0.518	0.614

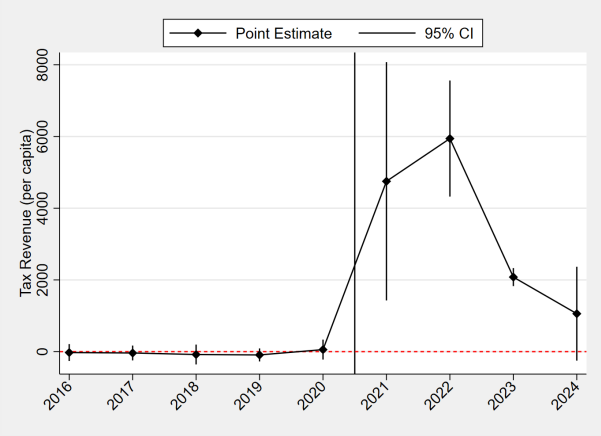
Table 3.D.2: Balance sheet categories

	(1) Inflow Capital Reserves	(2) Liquidity Loan Repayment	(3) Investment Loan Repayment
Share	27.31%	13.30%	8.72%
p-value	0.018	0.000	0.000

3.D.1 Synthetic DiD: Event study plots (both municipalities)

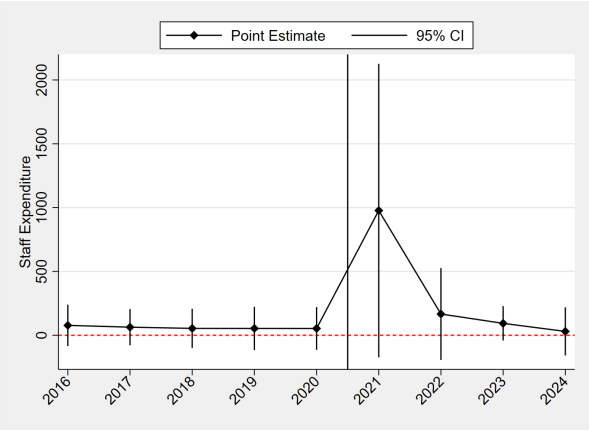
Municipal Revenue:

Figure 3.D.4: Tax revenues

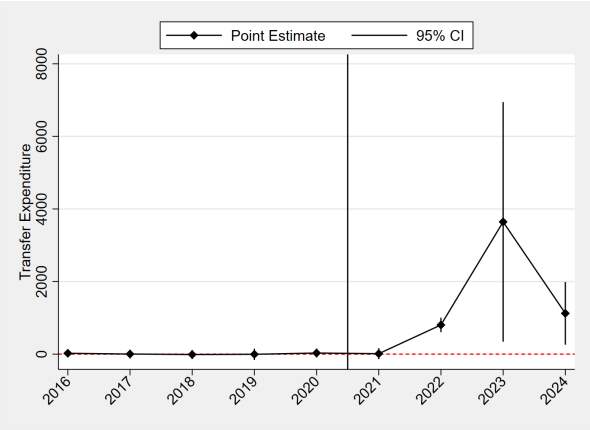


(Current) Expenditure:

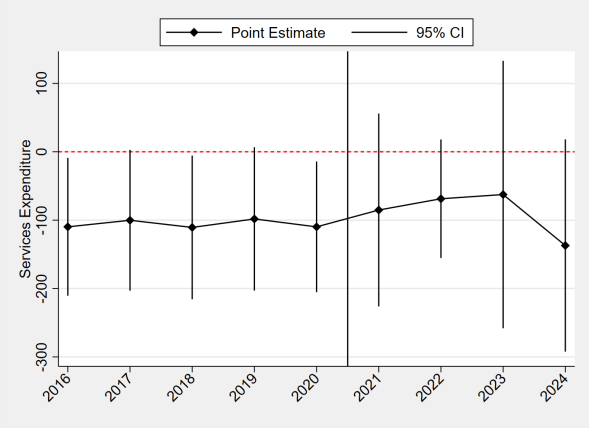
a) Staff expenditure



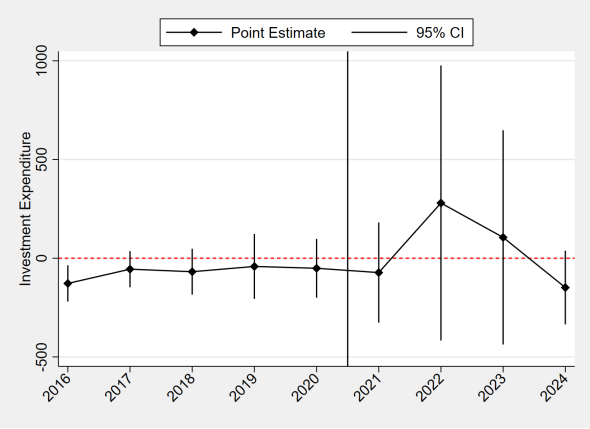
b) Fiscal equalization expenditure



a) Service expenditure

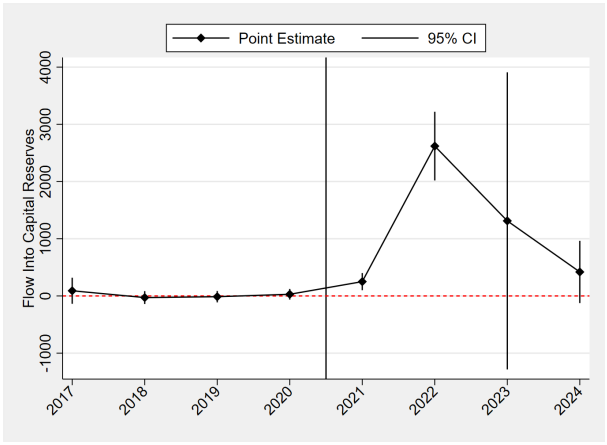


b) Public investment

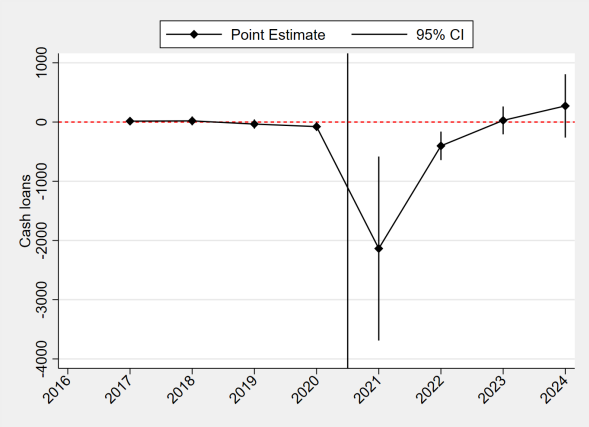


Balance sheet data:

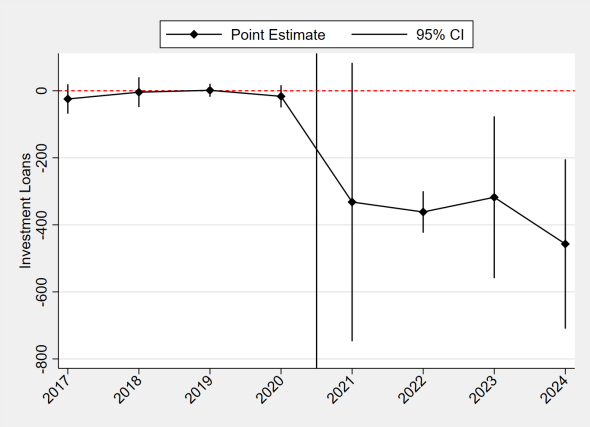
Figure 3.D.7: Inflow into capital reserves



a) Liquidity loans



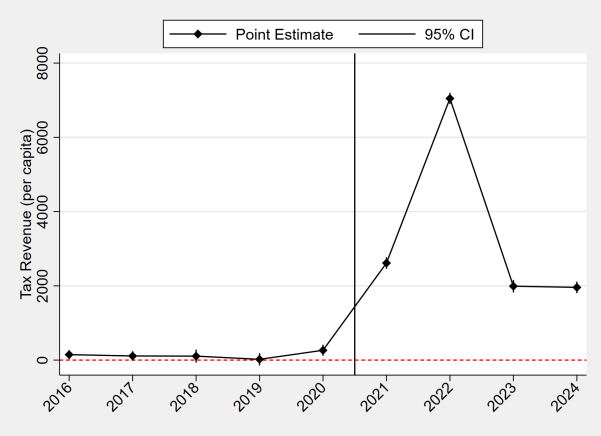
b) Investment loans



3.D.2 Synthetic DiD: Event Study plots (Mainz)

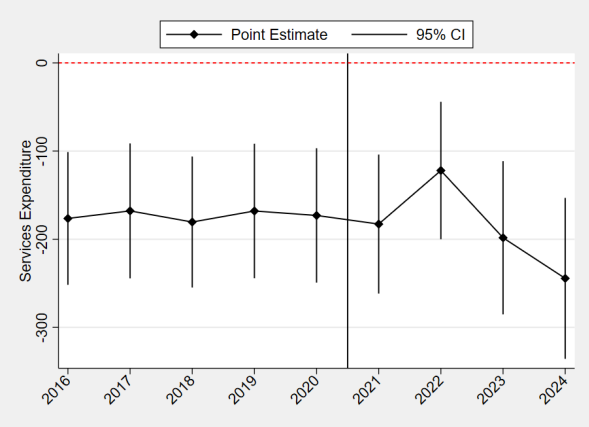
Municipal Revenue:

Figure 3.D.9: Tax revenues

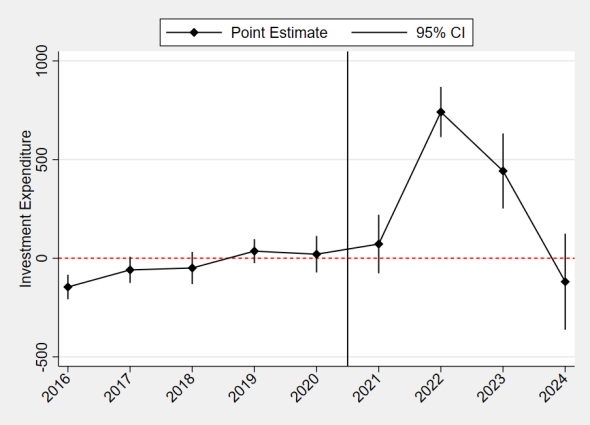


(Current) Expenditure:

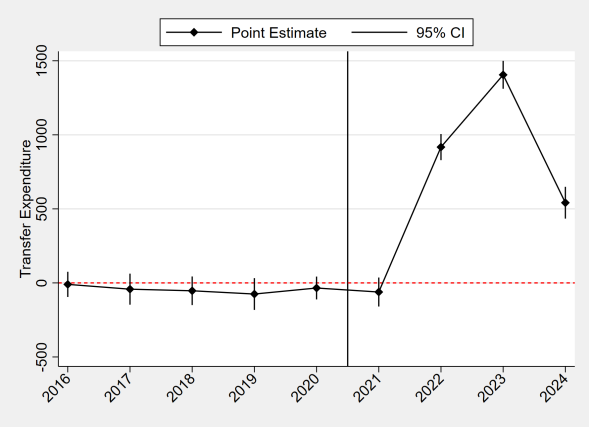
a) Service expenditure



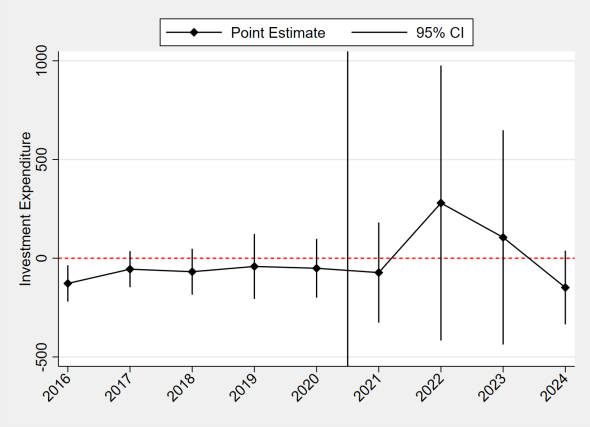
b) Public investment



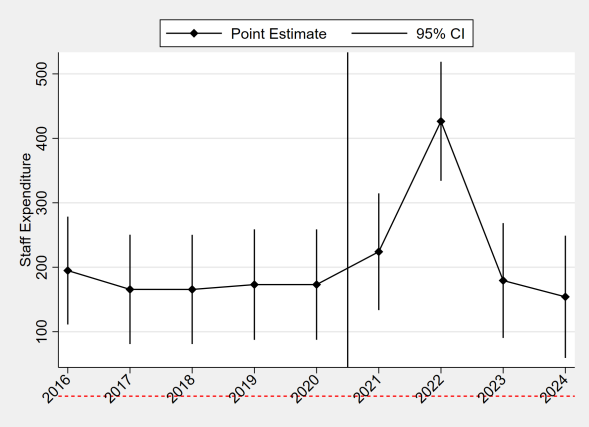
a) Fiscal equalization expenditure



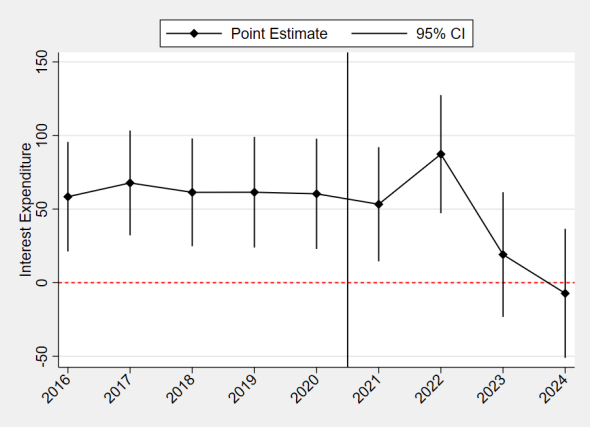
b) Staff expenditure



a) Welfare expenditure

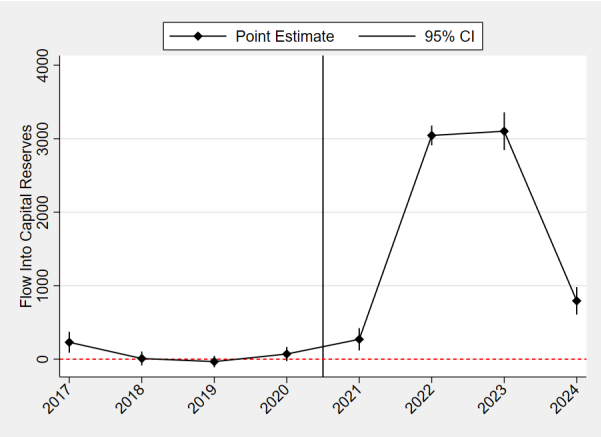


b) Interest expenditure

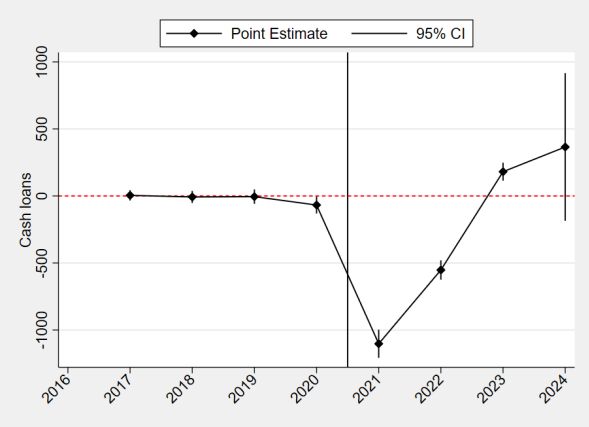


Balance sheet data:

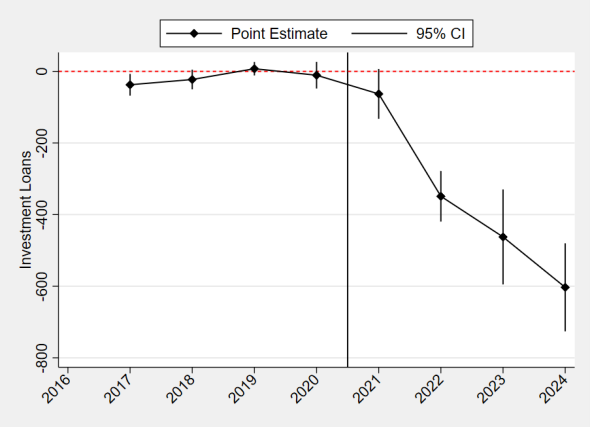
Figure 3.D.13: Inflow into capital reserves



a) Liquidity loans

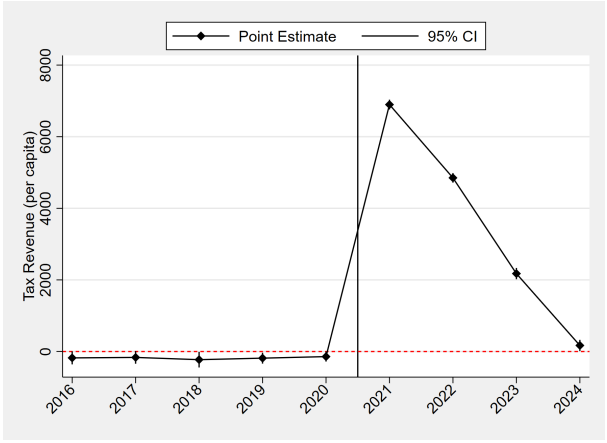


b) Investment loans



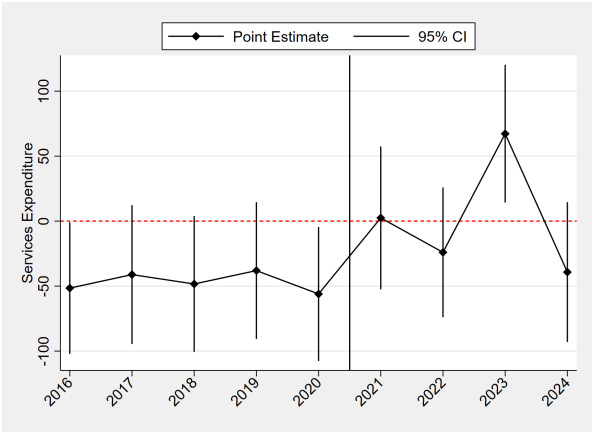
3.D.3 Synthetic DiD: Event study plots (Idar-Oberstein)

Figure 3.D.15: Tax revenues

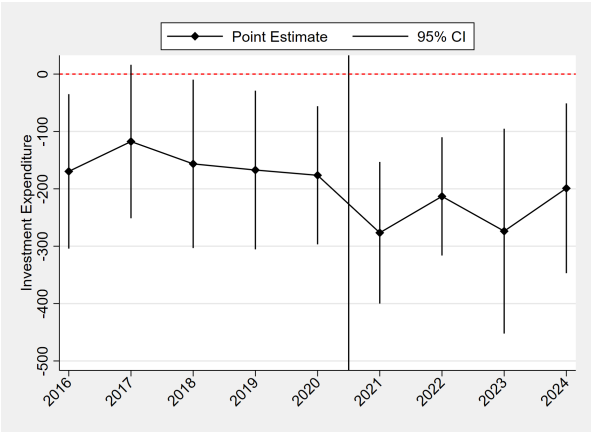


(Current) Expenditure:

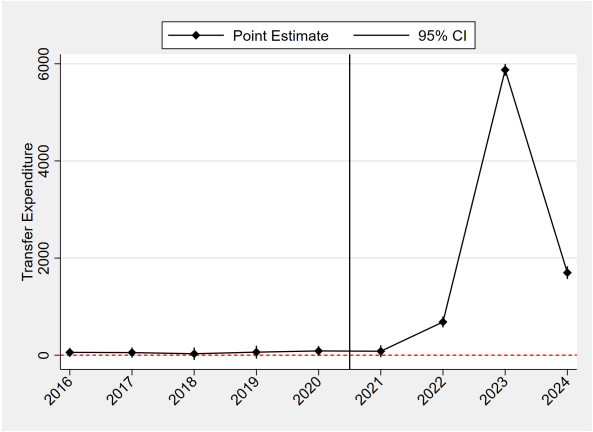
a) Service expenditure



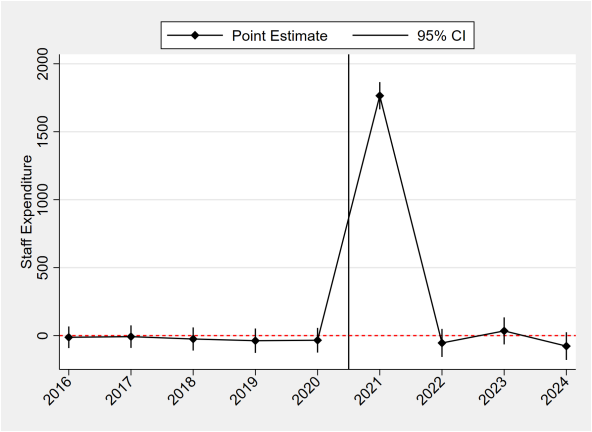
b) Public investment



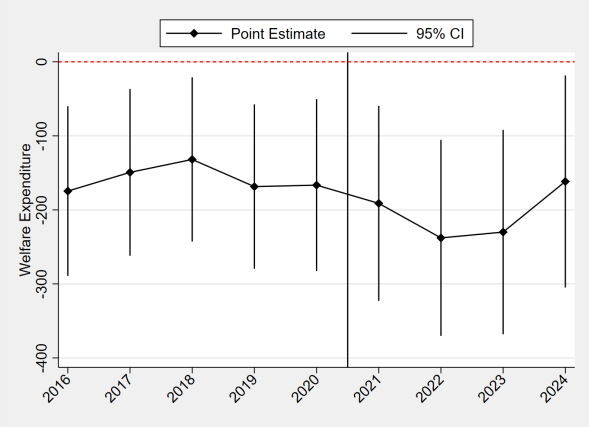
a) Fiscal equalization expenditure



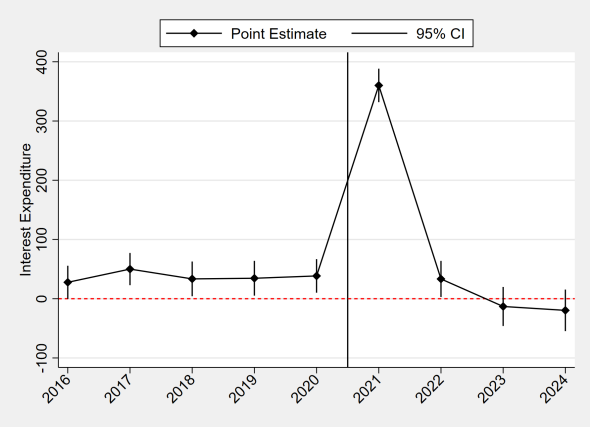
b) Staff expenditure



a) Welfare expenditure

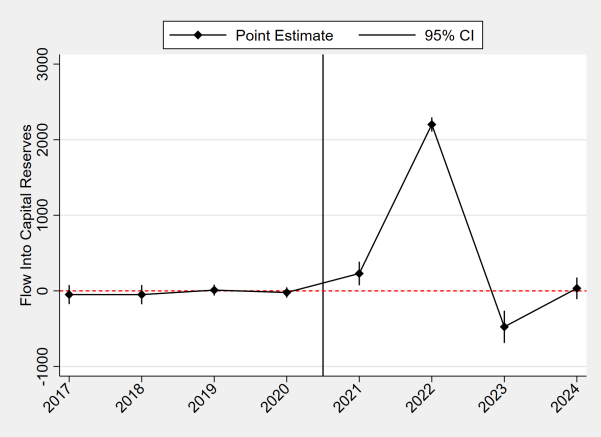


b) Interest expenditure

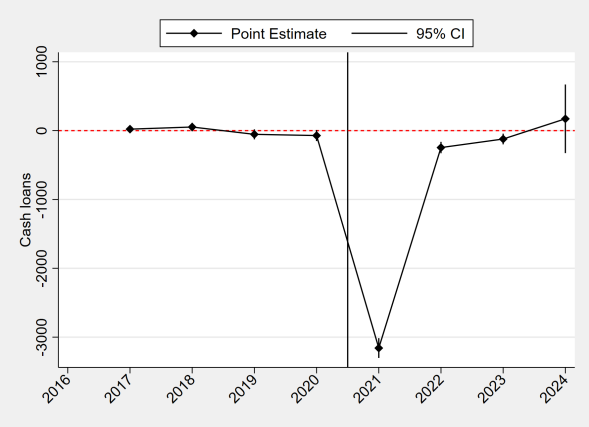


Balance sheet data:

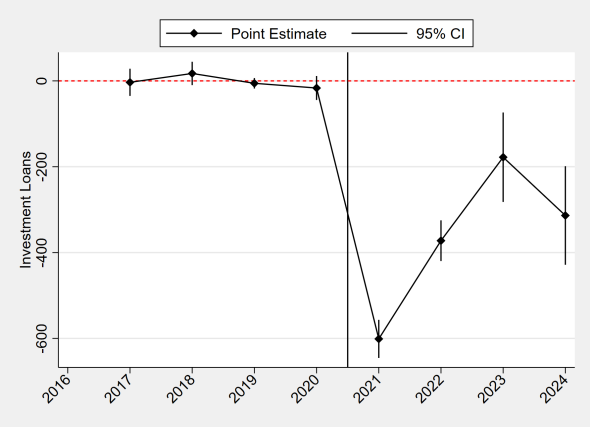
Figure 3.D.19: Inflow into capital reserves



a) Liquidity loans



b) Investment loans



3.D.4 Comparison to Tübingen

Figure 3.D.21: Normalized difference between Mainz and Tübingen of selected budget items

a) Tax revenue

b) Expenditure

c) Public investment

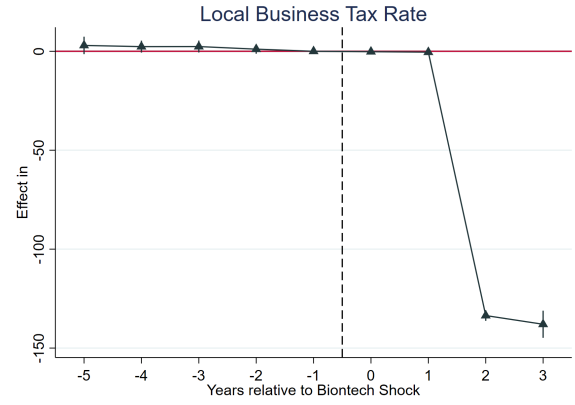
d) Investment loans

3.E Tax responses

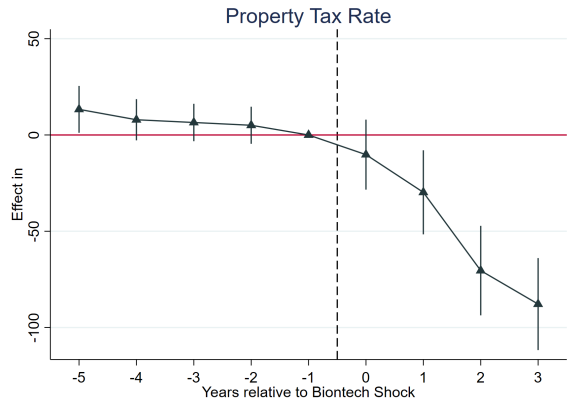
3.E.1 Simple Difference-in-difference

Local tax rates in Mainz:

a) Local business tax rate (Mainz)

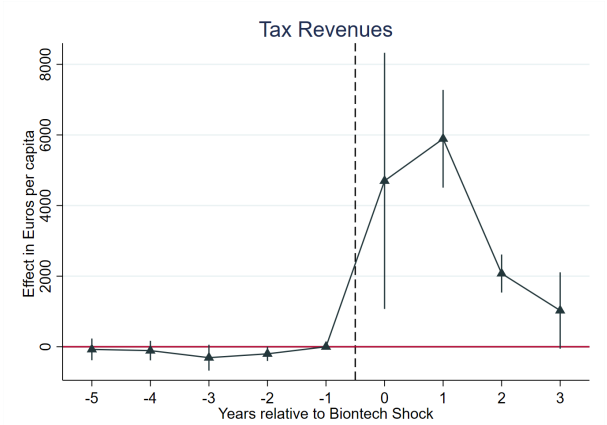


b) Local business tax rate (Idar-Oberstein)



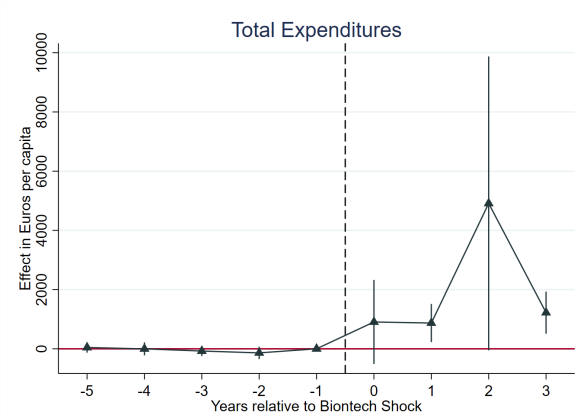
Municipal revenue:

Figure 3.E.2: Tax revenues

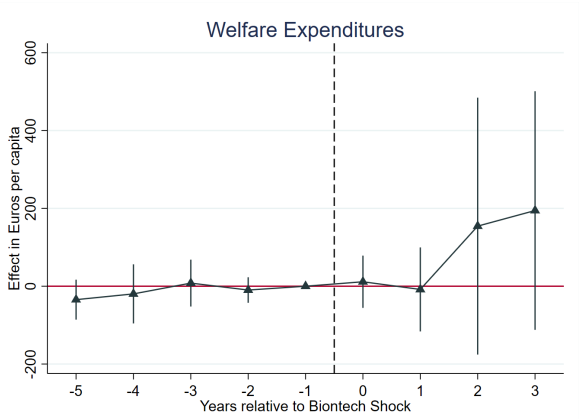


(Current) Expenditure:

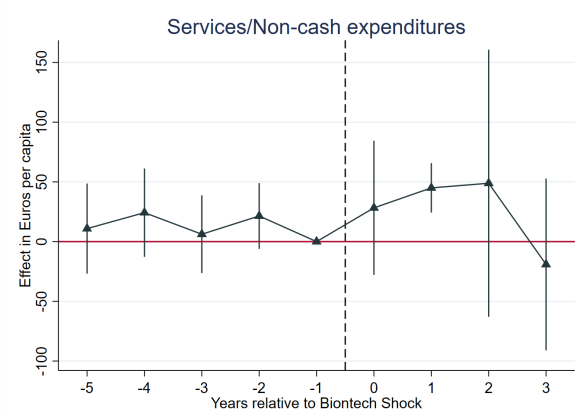
a) Total expenditure



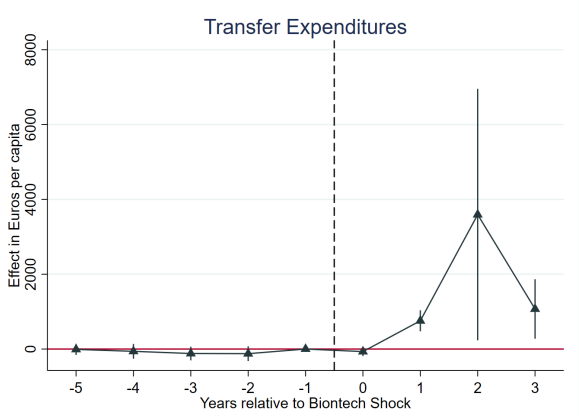
b) Welfare expenditure



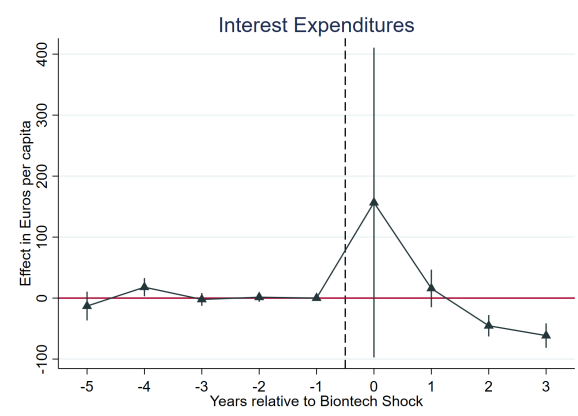
a) Service expenditure



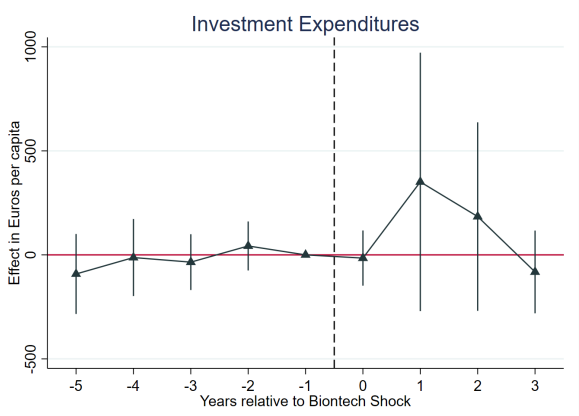
b) Fiscal equalization expenditure



a) Interest expenditure

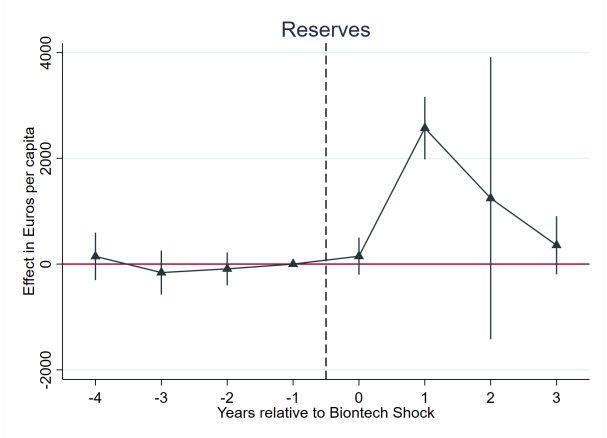


b) Public investment

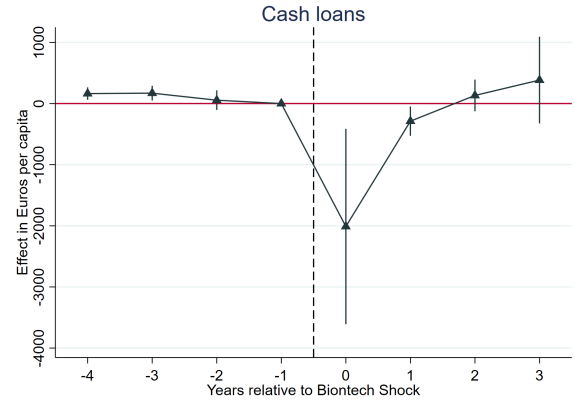


Balance sheet data:

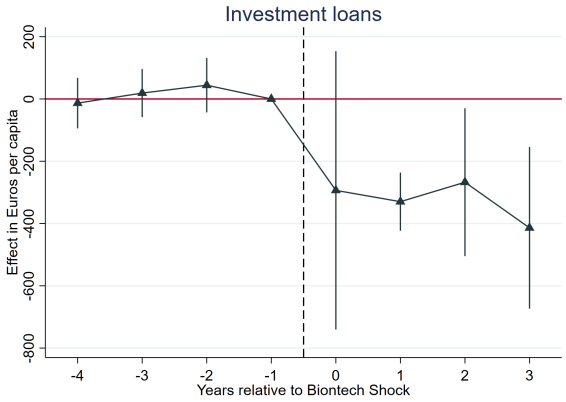
Figure 3.E.6: Inflow into capital reserves



a) Liquidity loans



b) Investment loans



3.F Firm responses

Table 3.F.1: Number of new firms in industry code 72 per year and municipality

Year	Ingelheim	Mainz
2017	1	0
2018	0	4
2019	0	3
2020	0	2
2021	1	4
2022	0	1
2023	0	0
2024	0	0

Table 3.F.2: Mainz: Industry share change among new firms compared to Tübingen

	(1) NACE 72	(2) NACE 721	(3) NACE 7219	(4) NACE 7211
Mainz × Post 2020	0.0006	0.005*	0.008*	-0.003*
	(0.0003)	(0.0001)	(0.0002)	(0.00005)
City FE	Yes	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes	Yes
Mean(Dep Var)	0.011	0.009	0.007	0.002
N	3,716	3,716	3,716	3,716

Regressions estimated according to Equation (3.2). Standard errors in parentheses * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

3.G Tax competition

Figure 3.G.1: Share of municipalities decreasing tax in 2022 by radius

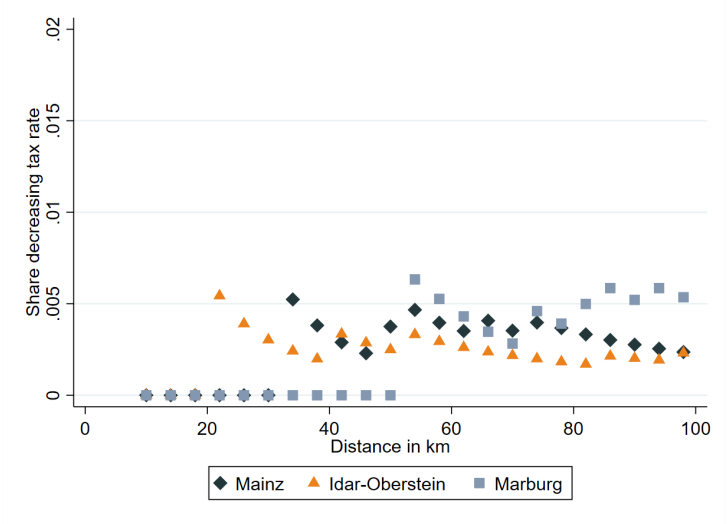
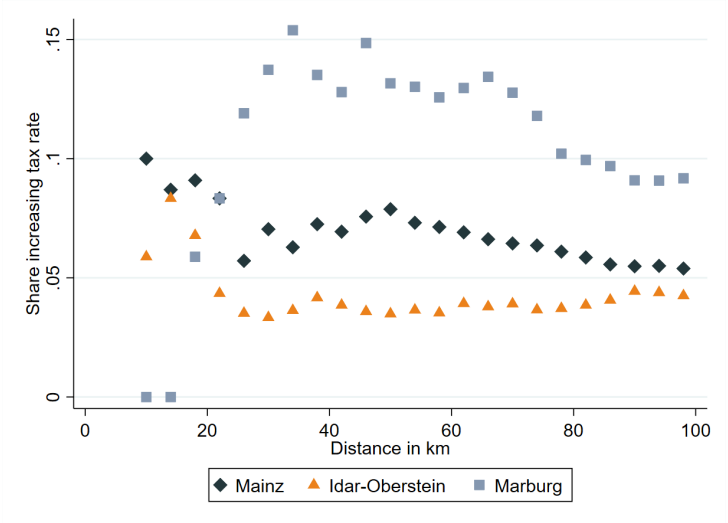


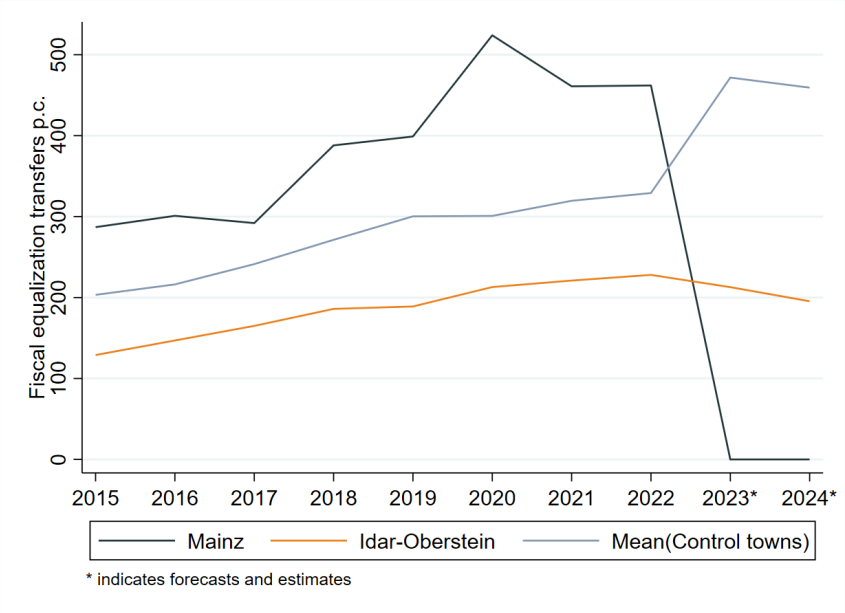
Figure 3.G.2: Share of municipalities increasing tax in 2019 by radius



3.H Internal validity

Figure 3.H.1 plots fiscal equalization payments per capita over time for Mainz, Idar-Oberstein and the mean of municipalities in the control group. Payments develop similarly for treated and control municipalities until 2022. They start to diverge in 2023. Forecast fiscal equalization payments to Mainz collapse to zero and decrease for Idar-Oberstein, but increase substantially on average for control units. The observed delay in divergence is explained by the equalization scheme's provision that fiscal equalization payment are calculated based on data with a lag of up to two years. Thus, a tainted control group is not a major concern for the first two years of the Biontech shock.

Figure 3.H.1: Fiscal equalization transfers per capita



3.I External validity

Table 3.I.1: Prominent cases of firms experiencing sudden spikes in profits

Firms	Location	Circumstances	Timing	Year	Sector
Lilly Elly (Mounjaro)	Minneapolis, US	Development of anti-diabetes medication	Slow	2019+	Health
Novo Nordisk (Ozempic)	Gladsaxe, Denmark	Development of anti-diabetes medication	Slow	2020+	Health
Zoom	San José, US	Videocommunication, Covid-19 Pandemic	Sudden	2020+	IT
Moderna	Cambridge, US	Covid-19 Vaccine	Sudden	2020+	Health
TeamViewer	Göppingen, Germany	Videocommunication, Covid-19 Pandemic	Sudden	2020+	Military
Rheinmetall	Several cities, Germany	Military equipment to Ukraine	Sudden	2022+	Military
General Dynamics	Several cities, US	Military equipment to Ukraine	Sudden	2022+	Military

Table 3.I.2: Studies in meta-analysis

Study	Country	Estimates	Average estimate
Baskaran (2016)	Germany	1	0.92
Berset and Schelker (2020)	Switzerland	5	1.51
Berset et al. (2023)	Switzerland	4	0.22
Brunner et al. (2022)	USA	1	1.27
Cascio et al. (2013)	USA	1	0.50
Dahlberg et al. (2008)	Sweden	1	1.30
Feiveson (2015)	USA	1	0.93
Feler and Senses (2017)	USA	1	0.37
Gadenne (2017)	Brazil	2	1.04
Helm and Stuhler (2024)	Germany	11	1.60
Heyndels and Van Driessche (2002)	Belgium	3	0.48
Litschig and Morrison (2013)	Brazil	1	1.16
Liu and Ma (2016)	China	1	1.11
Lundqvist (2015)	Finland	3	2.20
Lutz (2010)	USA	3	0.16
Martínez (2023)	Colombia	2	-0.01
Monteiro and Ferraz (2010)	Brazil	1	0.53
Rattsø and Tovmo (2002)	Denmark	3	0.29

Table 3.I.3: OLS standardized spending estimate

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Permanent (Transitory omitted)									
Yes		0.54	0.46	0.40	0.69**	0.56**	0.60***	0.85*	-0.13
		(0.35)	(0.30)	(0.22)	(0.18)	(0.17)	(0.15)	(0.29)	(0.25)
Sign (Positive omitted)									
Both			-0.10	-0.24	-0.69	-0.71	-0.54*	-0.52	0.03
			(0.37)	(0.33)	(0.33)	(0.35)	(0.21)	(0.35)	(0.02)
Negative			-0.82*	-0.46	-0.19	-0.23	-0.12	-0.26	0.06
			(0.31)	(0.37)	(0.18)	(0.20)	(0.21)	(0.27)	(0.04)
Shock source (Tax revenues omitted)									
Atypical revenues				-0.35	-0.23	-0.23	-0.33	-0.53**	-1.76**
				(0.24)	(0.27)	(0.28)	(0.31)	(0.17)	(0.34)
Grant				0.60*	0.88***	0.88***	0.84**	0.78	0.00
				(0.25)	(0.20)	(0.19)	(0.26)	(0.40)	(.)
Continuous variables									
Shock as a share of total revenues					-0.05*	-0.04*	-0.02	0.01	-0.01
					(0.02)	(0.02)	(0.01)	(0.01)	(0.04)
Effect time in years						0.04	0.06	0.07	0.05
						(0.05)	(0.04)	(0.05)	(0.06)
Level of govt (Muni omitted)									
County							-0.42	-0.97	
							(0.27)	(0.65)	
School district							-1.06**	-1.77***	0.00
							(0.28)	(0.26)	(.)
Constant	1.01***	0.73**	0.90**	0.57*	0.59*	0.55*	0.45		
	(0.22)	(0.25)	(0.27)	(0.20)	(0.20)	(0.19)	(0.26)		
Country FE	No	No	No	No	No	No	No	Yes	No
Paper FE	No	No	No	No	No	No	No	No	Yes
N	45	45	45	45	42	42	42	40	33

Regressions estimated according to Equation (3.3). Standard errors in parentheses * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 3.I.4: WLS standardized spending estimate

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Permanent (Transitory omitted)									
Yes		0.13	0.13	-0.12	0.14	0.08	0.32**	0.55*	-0.20
		(0.37)	(0.32)	(0.26)	(0.39)	(0.32)	(0.09)	(0.18)	(0.21)
Sign (Positive omitted)									
Both			-0.29	-0.22	-0.37	-0.37	-0.09	-0.02	0.01***
			(0.27)	(0.28)	(0.30)	(0.30)	(0.09)	(0.05)	(0.00)
Negative			-0.52*	-0.11	-0.23	-0.23	-0.01	0.01	0.03***
			(0.23)	(0.17)	(0.20)	(0.22)	(0.05)	(0.03)	(0.00)
Shock source (Tax revenues omitted)									
Atypical revenues				-0.64**	-0.48	-0.44	-0.24	-0.51	8.52
				(0.17)	(0.41)	(0.50)	(0.12)	(0.38)	(3.51)
Grant				0.04	0.18	0.21	0.61***	0.66***	0.00
				(0.27)	(0.38)	(0.44)	(0.07)	(0.12)	(.)
Continuous variables									
Shock as a share of total revenues					-0.03	-0.03	-0.01	0.01	1.35*
					(0.03)	(0.03)	(0.01)	(0.02)	(0.49)
Effect time in years						0.02	0.06	0.05	0.07
						(0.07)	(0.03)	(0.06)	(0.05)
Level of govt (Muni omitted)									
County							-0.10		
							(0.20)		
School district							-1.25***	-1.51***	0.00
							(0.06)	(0.12)	(.)
Constant	0.71**	0.66*	0.82**	1.06**	1.04**	0.99*	0.50**		
	(0.19)	(0.22)	(0.23)	(0.27)	(0.32)	(0.45)	(0.15)		
Country FE	No	No	No	No	No	No	No	Yes	No
Paper FE	No	No	No	No	No	No	No	No	Yes
N	38	38	38	38	36	36	36	34	29

Regressions estimated according to Equation (3.3). Standard errors in parentheses * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

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Curriculum Vitae

2020–2025	University of Mannheim (Germany)
	Ph.D. in Economics
2018–2020	University of Mannheim (Germany)
	M.Sc. in Economics
2014–2018	University of Freiburg (Germany)
	B.Sc. in Economics