# Essays on Empirical Public Economics

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# Preface

This dissertation contributes to current research in public economics and political economy. It consists of three chapters that examine relevant drivers of policy decisions in decentralized countries. A crucial aspect in all three chapters is the factor of competition. Either in the form of political competition at the individual level (in particular re-election concerns of incumbent politicians) or in the form of inter-jurisdictional competition for mobile tax bases, it is shown to have a significant impact on political decisions and policy outcomes.

Starting at the level of the 16 German states, Chapter 1 studies whether re-election and reputation concerns among state parliamentarians prevent more transparency regarding the outcomes of state education policies. Transparency and comparability of outcomes would, in principle, facilitate policy innovation and improvements in student performance through cross-state learning. At the next lower level of government, focusing on Swiss municipalities, Chapter 2 considers the competition for mobile firms between jurisdictions and examines whether local labor markets function as a beneficial location factor that can be taxed by municipalities. The increasing economic relevance of skill shortages may well lead to political considerations to tax firms for a relatively well-functioning local labor market in order to reduce pressure on local public finances. Finally, Chapter 3 goes one step further by looking at state-owned enterprises (SOEs), owned by German municipalities, and analyzes the investment behavior of these SOEs along the electoral cycle of their owners. Given the political influence on SOEs and the fact that they are not captured by typical public finance statistics, individual-level incentives of both politicians and SOE managers may result in higher SOE investment in the run-up to the next local elections in order to secure both parties' positions.

In particular, Chapter 1 – which is co-authored by Sebastian Blesse, Philipp Lergetporer, and Katharina Werner – acknowledges the lack of comparability and transparency regarding student performance in the decentralized German education system. It uses parallel surveys – including survey experiments – of German citizens and state parliamentarians to examine political-economic reasons preventing more transparency and to identify the potential for more policy competition. The results show that especially citizens are misinformed about relative student performance in their home state. Experimentally providing relevant performance information polarizes citizens' satisfaction with state policy between high- and low-performing states, but leaves support for a policy proposal to improve transparency unchanged at a high level. Parliamentarians' support for the reform proposal is opportunistic: it decreases (increases) in states with relatively poor (good) student performance when factual performance information is provided.

Chapter 2 takes advantage of a commuting reform in Switzerland that led to a significant increase in cross-border (CB) workers in Swiss municipalities close to national borders. The exogenous variation across municipalities in the commuting times of CB workers provides an ideal laboratory to study the causal effect of a labor supply shock on local taxes in a setting with significant labor shortages. The results show an increase in *corporate* income taxes in highly-treated border municipalities relative to less-affected hinterland municipalities. The empirical evidence also points to an increase in the municipal component of the *personal* income tax.

Finally, Chapter 3 – which is co-authored by Zareh Asatryan and Désirée Christofzik – uses administrative microdata on the more than 10,500 commercial accounting SOEs owned by German municipalities over the period 2002–2019. Taking advantage of differences in the timing of municipal council elections across the 16 states, event study regressions document a distinct cyclical pattern in SOE investment that is well aligned with the electoral cycle of their owners. The results support the hypothesis that fear of replacement drives the observed increase in investment prior to elections, a pattern that is also found for investment by core budgets as the owners of SOEs.

All three chapters make relevant contributions to the respective literature and improve the basis for scientific, evidence-based policy advice. Chapter 1, on the aspect of policy competition and transparency, makes the methodological contribution of conducting parallel survey experiments with factual information among citizens and politicians. This has the advantage of providing causal evidence on the preferences of both relevant stakeholder groups at the same time. The results provide new evidence for the well-established hypothesis that efforts for more transparency are stuck in a bad equilibrium that may require more room for centralized features in German education policy to reap the benefits of decentralization. Chapter 2 adds to the literature on tax competition an analysis of the relevance of an understudied but increasingly important location factor for firms as a tax determinant: labor. It calls for further research on this aspect to provide more evidence on the underlying mechanisms and the validity of the findings in other settings. Finally, Chapter 3 advances the literature with the first study of political budget cycles that considers the entire public sector rather than just core budgets. The results show a significant election cycle in SOE investment that is complemented by a similarly large cycle in percentage terms in core budget investment. The findings add valuable input to the current academic and political debate on the use of off-budget entities – often referred to as shadow budgets – and their implications for public sector transparency, sustainable public finances, and, as a consequence, effective policy making. Political influence, not just market forces, affects the decisions of outsourced public sector units, creating considerable scope for creative accounting practices and inefficiencies, as the previous literature has shown.

# Chapter 1

# Transparency and Policy Competition: Experimental Evidence from German Citizens and Politicians

**Abstract.** A lack of transparency about policy performance can pose a major obstacle to welfare-enhancing policy competition across jurisdictions. In parallel surveys with German citizens and state parliamentarians, we document that both groups misperceive the performance of their state's education system. Experimentally providing performance information polarizes citizens' political satisfaction between high- and low-performing states. Among citizens, demand for greater transparency of states' educational performance is high for all performance groups. Parliamentarians' support for the transparency policy is opportunistic: Performance information increases (decreases) policy support in high-performing (low-performing) states. We conclude that increasing the salience of educational performance information may incentivize politicians to implement welfare-enhancing reforms.

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This chapter is co-authored by Sebastian Blesse (Ludwig Erhard ifo Center for Social Market Economy and Institutional Economics, CESifo, ZEW Mannheim), Philipp Lergetporer (Technical University of Munich, CESifo), and Katharina Werner (ifo Center for the Economics of Education, CESifo). The surveys were pre-registered in the AEA RCT Registry (population survey: AEARCTR-0005943; parliamentary survey: AEARCTR-0005753).

## 1.1 Introduction

Providing citizens with comparative information on policy outcomes of different jurisdictions is an often-vaunted strategy to reward efficient public service provision and facilitate welfare-improving policy innovations. To evaluate the performance of their own political representatives, citizens can use observable policy outcomes of similar jurisdictions as a benchmark (e.g., Besley and Case, 1995b; Revelli, 2006; Revelli and Tovmo, 2007; Terra and Mattos, 2017). Such horizontal comparisons allow citizens to hold politicians accountable for their policy choices (e.g., Grossman et al., 2020). This not only facilitates the selection of high-quality candidates but also encourages incumbent politicians to enhance their efforts, ultimately leading to improvements in policy outcomes and welfare (e.g., Case, 1993; Besley and Case, 1995a; Congleton, 2007; Banerjee et al., forthcoming). Importantly, a necessary condition for subnational policy competition's ability to improve welfare is transparency over policy outcomes. Transparency implies that citizens have access to information about policy outcomes to form sufficiently accurate beliefs about policy performance and that performance information is salient. This requires comparable and relevant performance information that is regularly and reliably made available (Grossman et al., 2020). There is, however, mounting evidence from various countries that the electorate is largely misinformed about policy outcomes in many areas (e.g., Gilens, 2001; Alesina et al., Alesina et al., 2020; Nyhan, 2020; Haaland et al., 2023). Misperceptions exist not only for policy outcomes at the national level and in comparison to other countries (e.g., Fehr et al., 2022), but also for outcomes at subnational levels, regarding, for example, costs of living across US cities (e.g., Giaccobasso et al., 2022), property tax rates across US school districts (Bottan and Perez-Truglia, 2022), or local tax rates, Covid-19 infection rates, and regional income levels in Spain (Foremny, 2022; Foremny et al., 2020; Balcells et al., 2015). These information frictions limit the extent to which horizontal competition among political representatives can improve welfare.

Similarly, while politicians are often assumed to be well-informed about government performance (e.g., Besley and Case, 1995a; Seabright, 1996), recent evidence shows that they often have misperceptions about policy outcomes, too (e.g., Lee et al., 2021). We argue that such misperceptions are likely rooted in the insufficient availability and salience of information about policy outcomes, which can undermine effective public service provision because uninformed policy makers may vote for suboptimal policies in parliament. Despite the importance of beliefs about jurisdictions' policy performance in reaping the benefits of subnational policy competition, comparative evidence on these beliefs among citizens and politicians is largely lacking. Moreover, nothing is known about whether providing relative performance information affects citizens' satisfaction with their state's policy, and both groups' demand for transparency-enhancing policies. This is the research gap that we address in this paper.

Existing (observational) datasets do not contain information on citizens' and politicians' beliefs about their jurisdictions' policy performance, let alone exogenous variation in these beliefs. To overcome this identification challenge, we conducted parallel surveys with large samples of the German population and parliamentarians from the 16 German federal states. In the surveys, we first elicited respondents' beliefs about their state's relative policy performance in education, one of the key policy areas for which states are responsible. We then implemented experiments to study how factual performance information affects citizens' satisfaction with their state's education policy, and citizens' and parliamentarians' demand for increased transparency of policy outcomes.

We focus on education policy in Germany, which offers an ideal setting to study (the lack of) subnational policy competition. In Germany, legislative and executive power over public education rests with the 16 federal states, and hence both the design of each state's education system and student performance vary widely from one state to the next (e.g., Lergetporer et al., 2018; Mahler and Kölm, 2019). Competition between education systems at the state level in Germany is hampered by a lack of available information to compare student performance across states. For many years, education authorities have hindered systematic research on impacts of state-specific education initiatives, and the comparability of student performance by denying access to existing performance data and limiting possibilities to collect new data (e.g., Riphahn et al., 2016). The few student-achievement tests that enable state-level comparisons are not suitable for targeted analyses of state-specific education reforms (e.g., owing to the large time gaps between tests), and their results are not prominent in public discussions (see Section 1.3 for details). Although most citizens consider education to be a very important policy area (e.g., Henderson et al., 2021) and that transparency can improve student achievement (e.g., Morozumi and Tanaka, 2020; Bergbauer et al., forthcoming), a lack of information on student performance in Germany may prevent citizens from making well-founded assessments of their state's relative educational performance and reduces the accountability of parliamentarians.

We study experimentally how beliefs about states' educational performance affect citizens' and parliamentarians' attitudes towards education policy. We conducted parallel experiments in a large-scale survey with a representative sample of the German population (N>10,000) and a sample of politicians comprising around 30% of all German state parliamentarians (N>500). The population survey was conducted online and first elicited respondents' beliefs about their state's performance rank from 1 (best) to 16 (worst) in the most recent student achievement test on mathematical competencies. We then randomly assigned respondents to one of five experimental groups. In the control group, we elicited respondents' satisfaction with their state's education policy and their support for a proposed transparency policy (i.e., implementing biennial student-achievement tests and publishing their results for state comparisons; details see Section 1.4). Before answering the same outcome questions, respondents in the two information *provision* treatments (*Info provision OWN rank* and *Info provision ALL ranks*) were shown information about their own state's rank or the full ranking of all 16 German federal states, respectively. Reflecting the fact that comparative performance information is often not readily available to citizens, two information *acquisition* treatments were designed to increase the cost of accessing state-ranking information. Instead of providing citizens with the information, we gave respondents in these treatment groups the option of actively retrieving the information by clicking on a link. Treatment *Info acquisition OWN rank* (*Info acquisition ALL ranks*) offered respondents the option to acquire information about their own state's rank (the full ranking of all states). Given that clicking on a link is a low hurdle compared to the efforts needed to obtain comparative state-performance information in practice, any difference between the information *acquisition* treatments and the information *provision* treatments should be interpreted as a lower bound estimate for the effects of costly information acquisition.

Following the same structure, the parliamentary survey first elicited parliamentarians' beliefs about their state's ranking in the most recent student-achievement test. We then informed a randomly selected treatment group about their state's ranking in student performance.[1] Subsequently, we elicited all respondents' preferences for the proposed transparency policy of biennial comparative testing. Finally, we elicited how both citizens and parliamentarians perceive each other's preferences for the transparency policy.

We first establish that citizens and parliamentarians misperceive their state's relative student performance. The beliefs of both groups are biased towards the mean: Citizens and parliamentarians in states with above-average (below-average) student performance estimate the rank of their own state to be worse (better) than it actually is. Misperceptions are somewhat more pronounced among citizens than among parliamentarians, which is consistent with assumptions of voter-politician information gaps in theoretical models of public goods provision, such as models of yardstick competition (Besley and Case, 1995a) or government accountability (Seabright, 1996).[2]

[1]In the parliamentary survey, we implemented only two experimental groups (one control group and one treatment group) because the sample size was smaller compared to the population survey.

[2]Typically, the theoretical political economy literature assumes that politicians are better informed about the production and provision of public goods and services as their true effort is private information and cannot be observed by their respective constituents (e.g.,Biglaiser and Mezzetti, 1997; Raff and Wilson, 1997; Iaryczower et al., 2013). However, there are several reasons why politicians might hold biased beliefs despite potentially having better access to information (e.g., about – as in our context – state performance in education). For example, only few politicians engage in debates about a certain topic (like education policy) on a daily basis, for instance because

Second, we show that the information treatments polarize citizens' satisfaction with their state's education policy. Consistent with theories of politician-electorate interactions (e.g., Lewis-Beck and Stegmaier, 2007), satisfaction depends on the state's educational performance: In the control group, 57.5% of respondents in states with above-average student performance are satisfied with their state's education policy, while satisfaction in average and below-average states is only 42.8% and 39.9%, respectively. Importantly, the information treatments reinforce these differences: Information *provision* increases satisfaction in states with above-average performance by 24.2 (treatment *Info provision OWN rank*) and 27.3 percentage points (treatment *Info provision ALL ranks*). Conversely, the treatments significantly decrease satisfaction by 14.6 to 16.2 percentage points in states with below-average performance. Consistent with information-based updating (e.g., Bleemer and Zafar, 2018), treatment effects are more positive (negative) for those whose prior beliefs were too pessimistic (optimistic) regarding their state's performance. In sum, alleviating citizens' misperceptions regarding their state's relative educational performance further polarizes citizens' satisfaction with their state's education policy. This implies that citizens' ignorance regarding policy outcomes may undermine potential welfare gains in public service provision (due to citizens' difficulties in choosing high-performing political representatives and holding incumbent politicians accountable) that would be possible through subnational policy competition.

Turning to the information *acquisition* treatments, we next study how increasing the cost burden to acquire state-ranking information mitigates the information effects on citizens' satisfaction. Results demonstrate that the information *acquisition* treatments also polarize citizens' satisfaction with their state's education policy. However, treatment effects are smaller compared to the information *provision* treatments, likely because not all respondents (about 60%) retrieve the information on offer. For citizens in states with above-average and average performance, the treatment effects range from +11.5 to +14.6 percentage points. In below-average performing states, treatment effects are negative at -5.2 (treatment *Info acquisition OWN rank*) and -2.6 percentage points (treatment *Info acquisition ALL ranks*). Thus, information on states' relative performance has a much smaller impact on citizens' satisfaction with their state's education policy when access to information is made more costly.

Third, we study the impact of state-ranking information on citizens' demand for a transparency policy (i.e., implementing biennial student-achievement tests used for state comparison). In the control group, support for the proposal is high at 77.6%, regardless of the education performance of the respondents' state. We also find no negative effects of performance information on citizens' support for the policy, again regardless of their state's performance, showing instead that support remains high among respondents who learn that their state is performing below average. In a robustness experiment, we show that these results hold when we attach direct political consequences to survey responses in order to reduce the likelihood that stated preferences are cheap talk (see Section 1.4.1). This additional experiment also suggests that citizens do indeed have a certain preference for expressing their opinions and taking political action.

In the parliamentary survey, preferences for the transparency policy are similar to those of citizens: Support for the policy is high, averaging 75.6% with some evidence that support rates are higher in states with lower student performance. Importantly, and in stark contrast to the citizen results, the impact of the information treatment differs significantly by states' educational performance: Among parliamentarians in the better half of states in terms of student performance, performance information increases support for implementing biennial student-achievement tests by 10.1 percentage points. Most critically, treatment effects are significantly lower by 19.8 percentage points for those in the worse-performing half of German states. While our research design is not intended to identify the ultimate reasons for a lack of transparency in the German education system, these results highlight a plausible information-based impediment to transparency reforms.

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they are members of a committee. For others, performance information may have a similarly low salience as for the average citizen.

Namely, that parliamentarians' support for transparency policies crumbles in poorly-performing states once they are confronted with their state's low performance.[3]

Finally, we show that beliefs among citizens and parliamentarians about each other's transparency-policy preferences are asymmetrically misaligned. Parliamentarians have accurate beliefs about citizens' support for increased transparency. This finding is difficult to reconcile with the hypothesis that transparency of states' educational performance is low because politicians misperceive citizens' preferences for transparency. In contrast, citizens in the control group greatly underestimate politicians' stated support and believe that only 46.0% of their state's parliamentarians support the transparency policy, while the actual average share is 75.6%. Moreover, citizens expect their parliamentarians to be opportunistic (which aligns with our findings from the parliamentary experiment): State-ranking information in above-average performing states significantly increases citizens' beliefs about the share of parliamentarians who support the transparency policy.

The rest of the paper is structured as follows: Section 1.2 describes our literature contributions. Section 1.3 gives a brief summary of education policy in Germany. Section 1.4 introduces the data and the experimental design. Section 1.5 presents our results and Section 1.6 concludes.

## 1.2 Literature Contributions

Our paper contributes to several strands of research. It adds to the political economy literature on yardstick competition (see, e.g., Salmon (2019) for an overview and Bordignon et al. (2003), Revelli (2006), Buettner and von Schwerin (2016), and Terra and Mattos (2017) for empirical analyses of yardstick competition). At the most basic level, we advance this literature by studying citizens' beliefs about their state's policy performance – an important but experimentally understudied aspect of yardstick-competition theory. Our results highlight how providing citizens with accurate state-performance information might be a means to holding politicians accountable, and thus capturing the welfare gains from yardstick competition.

Relatedly, several papers study how the availability of information affects politicians' decisions. For instance, Nielsen (2014) and Avis et al. (2018) show that bureaucrats and politicians are responsive to performance information. Akhmedov and Zhuravskaya (2004), Ferraz and Finan (2008), and Repetto (2018) find that the extent of policy transparency is strongly linked to political outcomes like public spending, budget deficits, or prospects of re-election for incumbent politicians. Hjort et al. (2021) show that informing Brazilian mayors about the effectiveness of policies to enhance tax compliance increases the probability of such policies being implemented. Geys and Sørensen (2018) use a survey experiment to study how politicians change their preferences for school reforms when confronted with local school-performance data. Similarly, Banerjee et al. (forthcoming) experimentally show that local politicians are more responsive to their electorate's preferences when they expect their individual performance data to be disclosed. In sum, while the literature on the role of information and transparency in influencing politicians' behavior is relatively extensive, evidence on the determinants of transparency-enhancing reforms is scarce. A small number of descriptive studies examine various correlates with public sector transparency (see, e.g., Alt et al., 2006; Alt and Lassen, 2006; Rodríguez Bolívar et al., 2013; Wehner and De Renzio, 2013). We provide first causal evidence on the determinants of politicians' preferences for increased transparency and show that their transparency preferences for policy outcomes strongly depend on information about their own state's performance.

Our study also adds to the growing economics literature that uses survey experiments to study how information affects public policy preferences in various policy areas (for a review, see Haaland et al., 2023). Examples include Cruces et al. (2013), Alesina et al. (2018), and Fehr

[3]Unfortunately, we cannot test how more transparency of comparative performance information would discipline poorly performing politicians toward more effort and better policies (e.g., Banerjee et al., forthcoming). However, we show that increased transparency, and thus the exposure of poor policy performance, may not occur in the first place because parliamentarians from worse-performing states may block reform efforts to increase transparency and the availability of comparative information.

et al. (2022) on preferences towards redistribution, Bursztyn (2016) and Lergetporer et al. (2018) on education policy preferences, Haaland and Roth (2020) on attitudes towards immigration, or Blesse and Heinemann (2020) and Roth et al. (2022) on preferences towards state mergers and public debt. There is also a small but growing literature that uses survey- and field experiments to study political decision making. These papers find that politicians are interested to learn about scientific evidence on policy effectiveness Hjort et al. (2021), and use the experimentally-provided information to update their beliefs (Hjort et al., 2021; Nakajima, 2021; Vivalt and Coville, 2023). In some cases, politicians also change their policy positions (Geys and Sørensen, 2018; Lee, 2022) and policy choices in the field in response to the provided information (Zelizer, 2018; Hjort et al., 2021; Jablonski and Seim, 2023; see, Camp et al., 2023 for null effects of informational lobbying on politicians' choices).[4] Toma and Bell (2022) show that politicians often encounter cognitive difficulties in incorporating information about a policy's impact into their valuation of that policy. Relatedly, Mehmood et al. (2023) find that training politicians in causal inference increases their demand for causal policy evaluation. We extend this literature by conducting parallel factual-information experiments with citizens *and* politicians and compare how both groups respond to information about educational performance.[5] In particular, we study how *relative* performance information (as opposed to absolute information) affects policy satisfaction and transparency preferences, which are crucial in the context of yardstick competition.

Finally, by comparing beliefs among citizens and parliamentarians about each other's support for a proposed transparency policy, we contribute to an emerging literature that studies citizens' and politicians' beliefs about one another. In line with the notion that people often hold biased beliefs about others (e.g., Bursztyn and Yang, 2022), past research has shown that politicians tend to misperceive citizens' policy preferences across several countries and policy domains (e.g., Broockman and Skovron, 2018; Liaqat, 2019; Rosenzweig, 2021; Pilet et al., 2023; Walgrave et al., 2023; with the exception of local politicians in Switzerland, see Pereira, 2021), and citizens tend to misperceive politicians' policy stances (e.g., Samuels and Zucco, 2014; Grewenig et al., 2020). We document that German state parliamentarians correctly estimate citizens' transparency preferences in the context of German education policy, whereas citizens misperceive parliamentarians' stated preferences. Importantly, we show experimentally that information about policy outcomes affects citizens' beliefs about politicians' preferences. In this regard, our comparative analysis of citizens and parliamentarians contributes to the literature on gaps in attitudes, preferences, and behavior between the public and political elites such as parliamentarians (for a review, see Kertzer, 2020).

## 1.3 Institutional Background

### 1.3.1 Federalism in the German Education System

In Germany, the autonomy of the 16 federal states over their education policies is enshrined in the constitution (*Grundgesetz*). Changes to the *Grundgesetz* require a two thirds majority in both legislative chambers, and are therefore much more rarely attempted than changes in other laws that can be made by majority vote. German states are directly represented in the legislative process as the second chamber of the legislative (*Bundesrat*) and can reject any proposals that

[4]There are also several pieces of evidence that politicians change their policy position towards the majority when being confronted with such information (Butler et al., 2011; Liaqat, 2019; Hager and Hilbig, 2020; Pereira, 2021; Pereira et al., forthcoming; Dur et al., 2023), although Kalla and Porter (2021) show that this does not need to be the case necessarily. In their set-up, US legislators are neither interested in nor do they update on public opinion.

[5]In the political-science literature, several studies have conducted parallel experiments with the general public and elites to investigate determinants of elite-public gaps in political behavior (see Kertzer, 2020, for a review). Importantly, only a small subset of these studies leverage parallel information experiments (e.g., Baekgaard et al., 2019; Christensen and Moynihan, 2020; Arnesen et al., 2021). These few parallel information experiments do not, however, provide *factual* information as we do, but rather rely on fictitious information or hypothetical effects of the studied policies.

threaten their federalist competencies. The federalist structure of education is therefore deeply embedded and well protected in the German context.

As a result, laws to enact education initiatives on the federal level are rarely attempted, rarely successful, and generally fraught with difficulty. This is despite the fact that voluntary cooperation between federal and state level is explicitly permitted in this context based on Articles 91b and 104c of the *Grundgesetz*. A recent example of such a cooperation is the attempt to accelerate the digital transformation of the German education system through the *Digitalpakt*, which was designed as an instrument to make federal funds available for schools to purchase digital equipment, and has suffered from political delay and low take-up (Federal Ministry of Education and Research, 2021). Similarly, laws that are seen as infringing upon states' autonomy to legislate on education policy are heavily contested, and often subject to legal disputes.

In the absence of decision-making competencies at the federal level, the education ministers of the 16 states form the *Kultusministerkonferenz* (KMK), a framework that allows for coordination of education policies across Germany. As the KMK does not have legislative powers of its own, any decisions of the group are non-binding until they have been implemented in state law through the appropriate legislative process in each state. Therefore, the responsibility of education policy lies with the state parliamentarians who we have surveyed for this paper.

As decisions by the KMK rely on the voluntary participation of states, its effectiveness in standardizing education policy across Germany has been limited. One salient example is the initiative to standardize the university entrance qualification (*Abitur*) across states. While there is general agreement that the divergence of standards across states creates inequalities in access to higher education, and recommendations for education standards in key subjects were published as early as 2004, no agreement on fully standardized *Abitur* examinations has been reached to date (KMK, 2022).

As a result, the federal nature of education policy is reflected in the wide differences in the design of education systems across states. States vary in the types of schools that exist and how students are assigned to a particular school track. States also design their own curricula and decide, for example, which subjects they teach, how many lessons they teach in each grade, and how many years of schooling it takes to complete the *Abitur*. In addition, they also have different standards regarding the training and hiring of teachers, with some states offering civil servant status to large shares of their teachers, while others do not. This heterogeneity in education systems gives rise to large differences in students' outcomes. For instance, the share of school graduates that have obtained a university entrance qualification varied between 60.5% and 38.4% across states in 2019 (Federal Statistical Office, 2020).

### 1.3.2 The Debate on State Comparisons of Student Performance

The wide variety of state-specific education policies would, in principle, lend itself to studying the effects of a decentralized education system on student achievement and to reaping the benefits of policy competition in the sense of yardstick competition. Such competition encourages states to implement policies observed to be successful in other states and could lead to a productive policy environment raising student achievement throughout Germany. However, such competition is largely undermined by the lack of comparative student-performance data across states.

Student achievement tests are a standard instrument to monitor the performance of education systems in many countries (Hanushek and Wößmann, 2015). Such tests are not only a necessary prerequisite for evaluating the performance of education systems, they can also have direct positive effects on student performance (Bergbauer et al., forthcoming). In recent decades, large-scale student performance tests like the *Programme for International Student Assessment* (PISA), which tests 15-year olds in mathematics, science, and reading every three years, have been increasingly used to compare educational quality across countries.

The results of the first PISA test in 2000 showed that – contrary to the expectations of many politicians, education practitioners, and journalists – the achievements of German 15-year-olds are only mediocre at an international level. The impact of the first PISA test results on the public debate and education policy in Germany (often called the "PISA-Schock") was substantial and initiated a set of major education reforms (e.g., Davoli and Entorf, 2018; Sancassani, 2022).

As well as the regular PISA test intended for international comparisons, Germany conducted a supplementary study (PISA-E) to analyze educational performance of individual German states. PISA-E was representative at state level and revealed large differences in student performance between states: Comparing German states to other countries, the highest-performing state was just below the international top ten at the level of Sweden, while the lowest-performing state ranked at the bottom of the 31-country list at the performance level of the Russian Federation (FOCUS, 2002; Baumert et al., 2013).[6] These wide performance disparities played an important role in the ensuing policy debates.

Strong political pressure forced low-performing states in particular to act and reform their education system. This effort was not without success (Riphahn et al., 2016). By 2003, the three worst-performing states (Brandenburg, Saxony-Anhalt, and Bremen) all managed to achieve significantly better results in the area of reading competencies, which was the focus of the 2000 PISA test. In contrast, among the top-three performance states, only Saxony (ranked 3rd in 2000) was able to achieve a small and significant increase in test results (Prenzel et al., 2005, p. 13). By 2006, the discrepancy between the best- and worst-performing states continued to decrease – a result that is particularly due to improvements in all three worst-performing states from 2000 (Prenzel et al., 2008). These absolute and relative improvements in low-performance states strongly suggest that there is significant room for state policy to improve educational outcomes, even within a relatively short time horizon.

While Germany as a country still regularly participates in PISA tests which take place every three years, the state-level study PISA-E was discontinued in 2006 after just three waves and was replaced by the newly-developed student achievement test *IQB Bildungstrend* since 2008/09. The new test was designed to measure whether students reach adequate competence levels defined by the states through the KMK. They are conducted every five years at primary-school level and every three years at lower secondary level; but with alternating emphases in the latter case (languages or mathematics/science). The *IQB Bildungstrend* has been severely criticized by education researchers since the structural break with the previous PISA-E tests and the large intervals between comparable tests of five to six years make targeted analyses of state-specific education reforms impossible (Riphahn et al., 2016).[7]

In addition, the *IQB Bildungstrend* is no longer comparable to other international student achievement tests and is much less salient in the German public debate compared to the PISA test. Appendix Figure A1.1 documents the relative frequency of Google search requests for the two tests from January to December 2019. The figure shows that the relative number of search requests for "PISA" are much larger than the ones for "IQB" or "Bildungstrend". In addition, research requests for "PISA" spiked around the release of the PISA results on December 3, 2019, whereas there was no spike in search requests for "IQB" or "Bildungstrend" around the release date of the *IQB Bildungstrend* on October 18, 2019. The KMK has a long history of actively undermining state comparisons of student achievements by denying data access (Wößmann, 2013) or deleting state identifiers from existing datasets (Riphahn et al., 2016). It is therefore not surprising that the official *IQB Bildungstrend* reports explicitly discourage state comparisons of student achievements (e.g., Stanat et al., 2019). In this sense, the replacement of PISA-E by the *IQB Bildungstrend* has led to a substantial reduction in the availability and salience of comparable state-specific performance data.

[6]In fact, variation in student performance was higher in Germany than in most other OECD countries (KMK, 2002).

[7]Our experiment focuses on students' math competencies elicited in the 2018 *IQB Bildungstrend* study (see Section 1.4). The first of these assessments of students' math competencies took place in 2012 – six years after the last PISA-E study. This time gap makes it difficult to assess dynamics in states' performance differences from 2006 (when PISA-E ended) to 2012. For the following period from 2012 to 2018, differences in math performance between the best and the worst performing state increased in the *IQB Bildungstrend*, whereas the German average decreased slightly. Similarly, average PISA test scores in mathematics for Germany as a whole decreased from 2012 to 2018 (and from 2006 to 2018). This may be taken as evidence that differences between states continue to be substantial and also economically significant.

## 1.4 Data, Experimental Design, and Econometric Model

This section describes the survey- and experimental design of the population survey (Section 1.4.1) and the parliamentary survey (Section 1.4.2), and introduces our econometric model (Section 1.4.3).

### 1.4.1 The Population Survey

Our survey of the general population was fielded as part of the ifo Education Survey, an annual opinion survey on education policy. It covers 10,325 respondents aged between 18 and 69 years, surveyed between June 3 and July 1, 2020 via an online access panel.[8] The sample was drawn to be representative of the German population with quotas in regard to gender, age, education, region of residence, and employment status. Respondents answered the survey on a computer or other digital device. Item non-response for the outcome variables is low, well below 1%. Appendix Table A1.1 presents descriptive statistics on the population sample's sociodemographic characteristics, and political and economic preferences.

To study the effect of performance information on citizens' satisfaction and trans-parency-policy preferences, we first elicited respondents' prior beliefs about their state's relative education performance. Specifically, we asked: "A recent educational study compared the mathematics performance of 9th grade students in the 16 German federal states. What is your best guess on how the students in your state ranked?". Respondents were encouraged to report a number between 1 and 16, 1 implying their state was the best-performing state in Germany. Moreover, we asked respondents how sure they are that their beliefs are correct. Appendix Figure A1.2 presents the wording of the population survey questions.[9] We focus on math competency scores because they are (i) easily comparable across different education systems and are (ii) strongly linked to later labor-market success (e.g., Hanushek and Wößmann, 2015).

Second, we randomly assigned respondents to one of four different information treatment groups or a control group. The treatments used information about the relative performance of 9th graders in respondents' states on the 2018 *IQB Bildungstrend* assessment in mathematics (Stanat et al., 2019, p. 203). The experiment comprised two information *provision* treatments: Treatment *Info provision OWN rank* provided respondents with information about their own state's rank. Treatment *Info provision ALL ranks* informed them about the full ranking of all 16 states. The difference between the two information *provision* treatments enables us to ascertain to what degree respondents' attitudes are influenced not only by the rank of the own state, but also by a comparison to ranks of other (e.g., neighboring) states.

The two remaining treatments featured information *acquisitions* (e.g., Capozza et al., 2021). Respondents in treatments *Info acquisition OWN rank* and *Info acquisition ALL ranks* had the option of actively retrieving the same information provided in the information *provision* treatments by clicking on a link. This design reflects the fact that comparative performance information is often not readily available to citizens, especially in the case of the *IQB Bildungstrend*. Respondents in the control group did not receive any information about state-level student performance, nor did they receive the option to retrieve such information.

Third, we measured citizen's satisfaction with their state's education policy as well as individual preferences for increasing the transparency of states' educational performance.[10] Specifically,

[8]Data from the ifo Education Survey 2014–2021 are available free of charge for scientific use (Freundl et al., 2022).

[9]The survey questions used in this study were embedded as numbers six to nine in the ifo Education Survey 2020, which comprised a total of 29 substantive questions on education policy. Importantly, no preceding questions pertained to satisfaction with education policy or student achievement tests. While spillover effects from earlier questions are unlikely, any such effects would uniformly impact all experimental groups, thus not altering the reported treatment effects. For a non-technical summary of the ifo Education Survey 2020 in German, see Wößmann et al. (2020).

[10]Naturally, the survey question on the perceived rank of the own state might prime respondents and affect their stated satisfaction levels. Importantly, respondents in all experimental groups – including the control group

we asked respondents how satisfied they are with the education policy in their state on a 5-point Likert scale from “very satisfied”, “rather satisfied”, “neither satisfied nor unsatisfied”, and “rather unsatisfied” to “very dissatisfied”.[11] To elicit respondents’ preferences for increased performance transparency, we asked whether they favor or oppose the following concrete reform proposal: Introducing nationwide student achievement tests in all school types, which take place regularly every two years from the 5th grade onwards. The average results by state are published to compare student achievements across states. This policy proposal is based on a suggestion by the Advisory Council of the Federal Ministry for Economic Affairs and Energy, which has criticized the lack of transparency and policy competition in the German education system (see Riphahn et al., 2016, p. 12). Respondents reported their preference on a 5-point Likert scale, ranging from “very in favor”, “rather in favor”, “neither in favor nor against”, and “rather against” to “very against”.

A common criticism against eliciting policy preferences using surveys is that stated preferences may be susceptible to reporting bias and experimenter demand effects because they are “cheap talk” in the sense that they have no immediate political consequences (e.g., Carson, 2012; Kling et al., 2012). To test the robustness of our results to attaching political consequences to individual survey responses, we randomly assigned half of all respondents to a treatment group (*Consequential*). Respondents in this treatment group were informed that aggregate survey answers would be passed on to politicians in their state parliament. Respondents then indicated their preferences for the transparency policy. Following completion of the survey, we informed each of the 126 parliamentarians serving as education-policy spokespersons in the respective state parliament about average public support for the policy proposal. Randomization was carried out independent of the randomization into the information treatments, which allows us to study information-effect heterogeneities by responses’ consequentiality (see Lergetporer and Wößmann, 2022, for a similar application).

Finally, we elicited respondents’ beliefs about the share of state parliamentarians who would support the policy proposal. Respondents were asked to state the share of state parliamentarians who they think “strongly support” or “rather support” the transparency-policy proposal.

### 1.4.2 The Parliamentary Survey

To complement the population survey, we conducted a parallel survey among all elected members in the 16 German state parliaments. This survey constituted a joint project between ZEW Mannheim and University of Mannheim and was in the field between May 25 and July 31, 2020.[12] Parliamentarians could either participate using a pen and paper questionnaire that was sent to them by postal mail or online via an individualized survey link. Importantly, parliamentarians were assured that their answers would be anonymized and that the data would not enable any conclusions to be drawn about their identity.[13] We contacted all 1,862 parliamentarians and received responses from 557, which yielded a response rate of approximately 30%.[14] Appendix Table A1.2 documents selection into our sample. We find no selection based on party affiliation, parliamentarians’ educational background, or whether they work in the field of education. Parliamentarians from below-average performing states in education are significantly less likely to

– answered this question and where thus exposed to potential priming, so priming should not affect the difference in outcomes between the control group and the treatment groups.

[11] The neutral option was presented as the last option to minimize error of central tendency.

[12] The study was conducted as part of the Collaborative Research Center SFB 884 “Political Economy of Reforms”. For more details on the survey see Blesse et al. (2021).

[13] To underline the credibility of this statement, reference was made to previous surveys of state parliamentarians concerning different policy areas such as budgetary issues and fiscal rules, conducted by the same institutions and using the same procedure. Results of these surveys are, among others, published in Heinemann et al. (2016) and Heinemann et al. (2021).

[14] For the questions used in this paper, we have at most 520 observations due to item non-response.

participate in the survey (column 1), but this relationship becomes smaller and insignificant after accounting for other observable characteristics (column 4). Female parliamentarians and those with longer tenure in parliament are, however, somewhat less likely to participate (see columns 3 and 4 of Appendix Table A1.2).

The parliamentary survey followed the same structure as the population survey.[15] First, we elicited parliamentarians' beliefs about their state's relative student performance. Second, we randomly assigned parliamentarians to an information treatment, providing them with information about relative student performance. Third, we measured respondents' preferences for the reform proposal to increase transparency of states' educational performance. Finally, we elicited parliamentarians' beliefs regarding the share of respondents in their electorate who favor the reform proposal (see Appendix Figure A1.3 for the question wordings). Furthermore, we hand collected publicly available background information of all parliamentarians. Appendix Table A1.3 presents descriptive statistics of our sample of parliamentarians.

We applied the same survey design to the population- and parliamentary survey. However, given the smaller sample size in the parliamentary survey, we only implemented one information *provision* treatment which informed parliamentarians whether their state is in the better or worse half of states in terms of student performance in the *IQB Bildungstrend* 2018. This is in contrast to the population survey in which the information *provision* and *acquisition* treatments included information about the actual rank of the own state. As a result, the information treatment in the parliamentary survey is somewhat less informative than in the population survey.

Given that the experimental design entails eliciting prior beliefs before providing the factual information, we only administered the information treatment in the online version of the parliamentary survey (where we were able to prevent respondents from going back and revising their answer to the prior-belief question after receiving the information treatment). About two thirds of the participating parliamentarians were surveyed online, forming our experimental parliamentarian sample.

### 1.4.3 The Econometric Model

Owing to the random assignment of respondents to experimental groups in both surveys, the treatment effects can be estimated with the following regression model:

$$y_i = \alpha + \beta_k' \mathbf{Treatment}_{k,i} + \theta' \mathbf{X}_i + \mu_s + \varepsilon_i, \tag{1.1}$$

where $y_i$ is the outcome of interest for individual $i$ and $\mathbf{Treatment}_{k,i}$ is an indicator of whether respondent $i$ received treatment $k$. In the population survey, $k \in \{$*Information provision OWN rank, Information provision ALL ranks, Information acquisition OWN rank, Information acquisition ALL ranks*$\}$; in the parliamentary survey $k =$ *Information provision worse/better half*. Vector $\mathbf{X}_i$ contains relevant control variables and $\mu_s$ represent state fixed effects. We discuss estimation results with and without covariates and state fixed effects. Providing information about state performance should influence political satisfaction and transparency-policy preferences in opposite directions, depending on whether the information shows that the relative performance of a respondent's state is high or low. Therefore, for our main analysis of the population survey, we estimate Equation (1.1) separately for three groups of respondents who currently live in states with above-average, average, or below-average student performance using sample splits. This categorization is based on whether student performance in the *IQB Bildungstrend* 2018 of the respective state was statistically significant above or below the German average and is taken from

[15]Similar to the population survey, the survey items relevant for this paper were preceded by questions on other topics, like the German debt brake, or expectations about public deficit and economic growth (Blesse et al., 2021). Crucially, there were no prior questions about satisfaction with education policy (see Blesse and Nover, 2020, for a non-technical summary in German).

the official report (Stanat et al., 2019). The ranking, scores, standard errors, and classification into the three performance groups is also documented in Appendix Table A1.4.[16]

Throughout our regression analysis of the population survey, we employ survey weights calibrated to match cells defined by combinations of administrative statistics with respect to age, gender, state, educational attainment, municipality size classes, and employment status.[17]

We also estimate interaction models to analyze whether treatment effects differ by specific subgroups of the sample such as respondents whose prior beliefs are too optimistic or too pessimistic regarding the education performance of their own state. For these analyses, we extend the regression model in Equation (1.1) to:

$$y_i = \alpha + \beta_k' \mathbf{Treatment}_{k,i} + \gamma' \mathbf{Subgroup}_i + \delta_k' \mathbf{Treatment}_{k,i} \times \mathbf{Subgroup}_i + \theta' \mathbf{X}_i + \mu_s + \varepsilon_i, \quad (1.2)$$

where $\mathbf{Subgroup}_i$ is equal to 1 if respondent $i$ belongs to the respective subgroup. The treatment effect for non-members of the subgroup is given by $\beta_k$, whereas $\delta_k$ measures the additional treatment effect on the subgroup.

To test whether our randomization was successful, we estimate regressions of different sociodemographic characteristics on the treatment dummies. For the population survey (parliamentary survey) Appendix Table A1.1 (Appendix Table A1.3) confirms that random assignment balanced respondents' characteristics across experimental groups.

## 1.5 Results

This section presents our empirical results. We start by documenting misperceptions about state's educational performance (Section 1.5.1). We then show how providing citizens with factual performance information affects their satisfaction with their state's education policy (Section 1.5.2). Section 1.5.3 studies how performance information affects citizens' and parliamentarians' support for a transparency-enhancing policy proposal, and Section 1.5.4 studies citizens' and parliamentarians' beliefs about each other's preferences for the transparency policy.

### 1.5.1 Citizens' and Parliamentarians' Misperceptions about Educational Performance

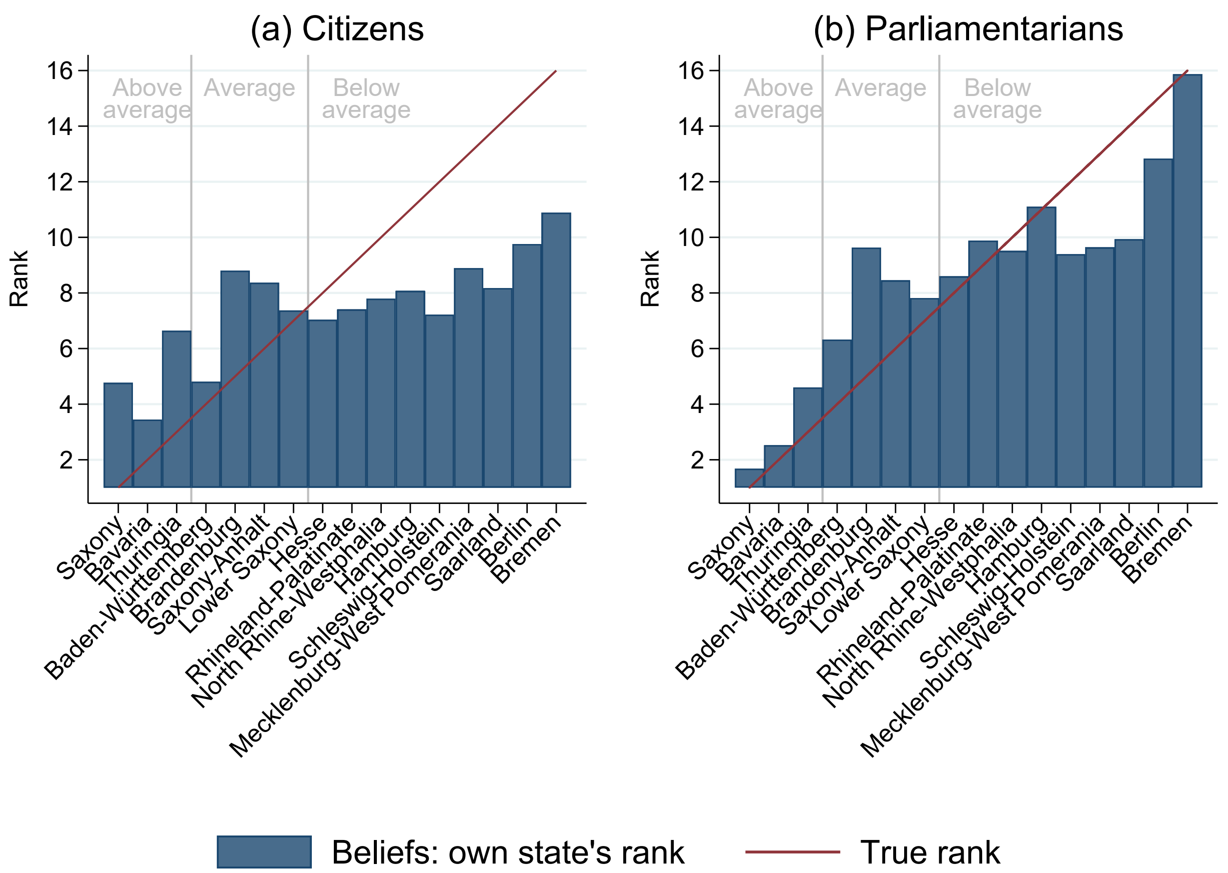
Panel (a) of Figure 1.1 shows the distribution of citizens' beliefs about their state's performance rank from 1 (best) to 16 (worst). The vertical axis depicts average rank beliefs, while the horizontal axis ranks the individual states according to their actual educational performance of 9th graders in mathematics (in the *IQB Bildungstrend* 2018). Thus, the perfect information case is represented by the 45-degree line. As it turns out, participants from above-average performing states are overly pessimistic, i.e., they tend to estimate their state's rank on average 2.2 ranks worse than it actually is. Similarly, respondents from average performing states estimate their state's rank on average 1.1 ranks worse than it actually is. In contrast, citizens from below-average performance states are overly optimistic and estimate their state's rank on average 2.6 ranks better than it actually is. Additional regression analyses show that the relationship between respondents' own state's rank and their optimism/pessimism about their own state's rank holds when accounting for respondents' characteristics.[18]

[16] Note that survey respondents in both surveys were at no point confronted with this performance-based categorization of states, which merely serves us as a simplification for the cleaner presentation of our findings.

[17] The findings are robust to using unweighted regressions. Results are available upon request.

[18] The results also show that respondents who are older, were born in Germany, are employed, and have children are less optimistic about their state's rank, whereas more risk-tolerant respondents are more optimistic. Importantly, the strongest predictor for respondents' optimism and pessimism is their own state's actual rank. Results are available upon request.

**Figure 1.1:** Mean beliefs about the own state's rank by state



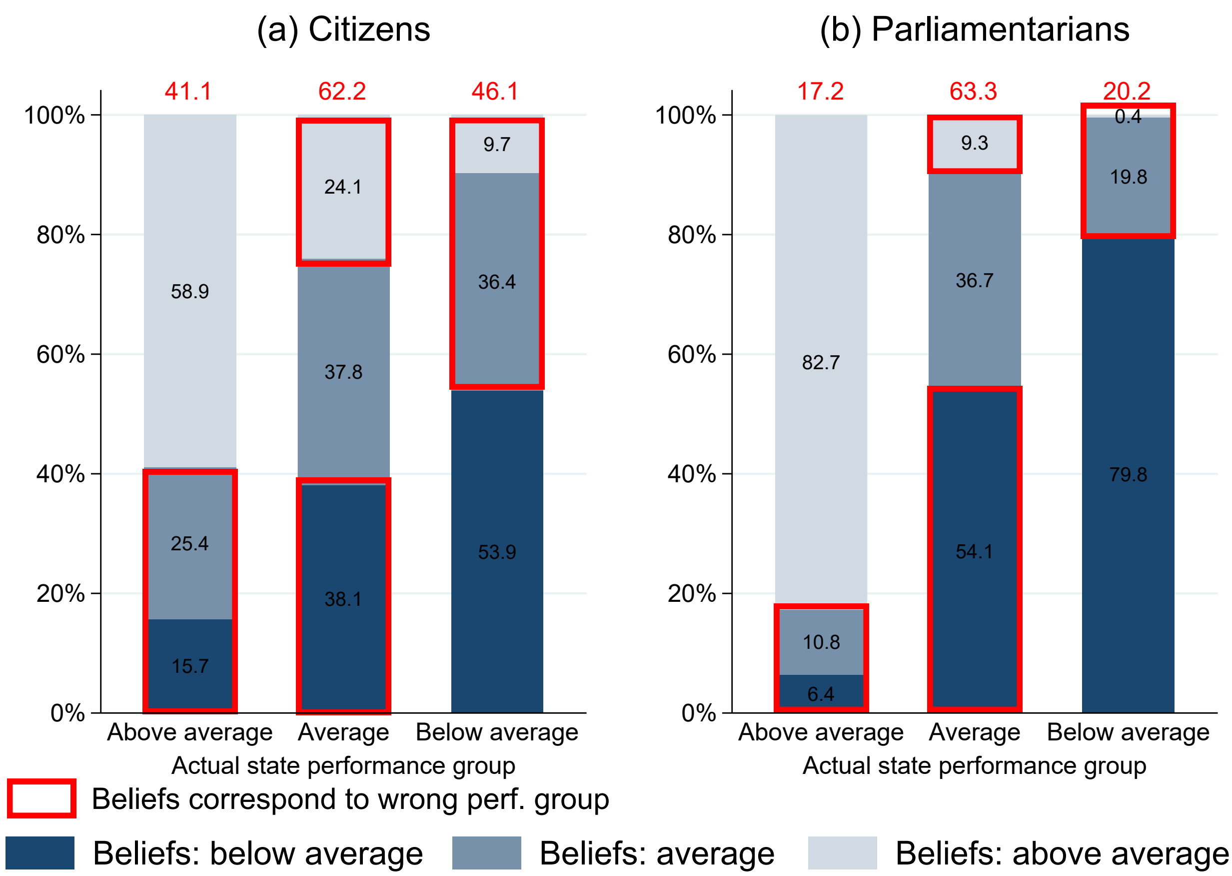

Notes: States are grouped into three categories according to the results of a recent cross-state comparative student test among 9th graders in the subject of math (Stanat et al., 2019, p. 203). The categorization into the three performance groups is indicated by the vertical gray lines in both subfigures. Panel (a): N=10,313. Panel (b): N=515. Data source: ifo Education Survey 2020, ZEW/University Mannheim parliamentary survey 2020.

To illustrate the extent of citizens' misperceptions, we also calculate the shares of citizens whose beliefs about their state's performance rank would imply a substantial deviation from its true rank position among German states according to these three performance groups. This takes into account the fact that overly optimistic and overly pessimistic rank perceptions cancel each other out when looking at average misperceptions by state. In the above-average performing states, 41.1% of respondents are overly pessimistic and believe that student performance in their state is only at the rank-level of average or below-average performance states (see Panel (a) of Figure 1.2). Similarly, 46.1% of respondents from below-average performance states are overly optimistic and mistakenly think that their state's performance rank is at the rank-level of average or above-average states. Misperceptions are even higher among respondents from states in the average-performance group, where 62.2% believe that their own state's performance is equivalent to the rank level of above- or below-average states.

Thus, citizens are misinformed about their state's educational performance, especially in poorly-performing states. These information gaps limit the extent to which citizens can hold their state politicians accountable: If voters are not sufficiently well-informed about policy outcomes, the performance of other jurisdictions cannot serve as a yardstick for evaluating their own politicians' performance.

Turning to the parliamentary survey, comparing Panel (b) of Figure 1.1 to the citizen results shows that parliamentarians are on average better informed about their state's relative educational performance than citizens. Misperceptions among parliamentarians follow a similar pattern as misperceptions among citizens: Those from above-average performance states estimate their true rank on average 0.8 ranks worse than the actual rank (compared to 2.2 in the population

**Figure 1.2:** Share of respondents with beliefs about own state performance in education that correspond to the wrong performance group




Notes: Figure shows the share of respondents whose guess for their state's performance (measured by respondents' rank beliefs) corresponds to each of the three performance groups. States are grouped into three categories according to the results of a recent cross-state comparative student test among 9th graders in the subject of math (Stanat et al., 2019, p. 203). The categorization into the three performance groups is indicated by the vertical gray lines in Figure 1. Panel (a): N=10,313. Panel (b): N=515. Data source: ifo Education Survey 2020, ZEW/University Mannheim parliamentary survey 2020.

survey), while those from below-average performing states estimate their state's rank 0.9 ranks better (compared to 2.6 in the population survey). Misperceptions are relatively large among parliamentarians in average performing states: They estimate their rank to be 2.2 ranks worse than it actually is (compared to 1.1 in the population survey). Again, the relationship between the own state's actual rank and parliamentarians' (mis)perceptions holds in a regression analysis which accounts for parliamentarians' observable characteristics.[19] Instead of the mean rank misperceptions, we can again look at the shares of parliamentarians from above-average, average, and below-average performance states whose answers would incorrectly imply that their state is in another performance category. In the parliamentary survey, these shares are 17.2%, 63.3%, and 20.2%, respectively (see Panel (b) of Figure 1.2). Overall, perceptions among parliamentarians are more accurate than those of citizens. However, parliamentarians still show relevant misperceptions, especially those from average performing states. This is not inconsistent with previous evidence which finds mixed effects. In particular, some studies find that politicians are rather well-informed about policy outcomes and policy effectiveness in education (e.g., Hjort et al., 2021;

[19]The regressions show that the own state's performance is the most important predictor of parliamentarians' (mis)perceptions. Moreover, members of an opposition party or a fringe political part ("other") are less optimistic about their state's educational performance rank. Results are available upon request.

Geys and Sørensen, 2018), while others document substantial misperceptions among politicians (e.g., Nakajima, 2021).

In the experiment implemented in the parliamentary survey, we consider only two groups of states in terms of their educational performance due to limited sample size: Those in the better half of all states, and those in the worse half. Using these performance categories, 25.2% of parliamentarians from the better half, and 27.2% of parliamentarians from the worse half, have performance beliefs that place their state in the wrong group.

In sum, we document misperceptions among respondents about the educational performance of their own state that are larger among citizens than parliamentarians.[20] Figure 1.2 also documents that performance beliefs are substantially off for a large share of citizens. In the next section, we study how correcting these misperceptions through information interventions affects political satisfaction and preferences for increased transparency.

### 1.5.2 Treatment Effects on Citizens' Satisfaction

Table 1.1 reports effects of our information treatments on citizens' satisfaction with their state's education policy based on Equation (1.1).[21] Columns 1, 2 and 3 report results for citizens in states with above-average, average, and below-average performance, respectively. The dependent variable is a binary variable coded 1 if respondents are "very" or "rather" satisfied, 0 otherwise. The control means reveal that respondents' satisfaction with their state's education policy is strongly correlated with a state's actual educational performance: A majority of 57.5% of respondents in states with above-average performance is satisfied with their state's education policy, whereas this only holds for 42.8% of respondents in states with average performance, and for 39.9% with below-average performance. More importantly, our treatments significantly amplify these gaps: The information *provision* treatments *Info provision OWN rank* and *Info provision ALL ranks* significantly increase satisfaction in states with above-average performance by 24.2 and 27.3 percentage points, respectively. The treatments also increase satisfaction in average-performing states, which is not surprising given that respondents in these federal states tend to be overly pessimistic about their state's rank (see Figure 1.1). Intriguingly, the treatments significantly decrease satisfaction in states with below-average performance by 14.6 to 16.2 percentage points. Thus, citizens' satisfaction with their state's education policy further diverges when provided with factual information about their state's performance in educating students. The magnitude of effects is similar across information treatments, suggesting that citizens mostly care about their states' relative performance overall, and not the comparison with specific states (e.g., neighboring states).

Table 1.1 also presents the results of the information *acquisition* treatments on citizens' satisfaction with the education policy in the own state, which gave participants the option to retrieve the ranking information by clicking on a link. We find that treatment effects largely remain statistically and economically significant. However, the magnitude of the treatment effects is roughly halved, which is consistent with the finding that about 60% of respondents chose to retrieve the information before stating their satisfaction.[22] This information-acquisition rate is relatively high compared to other studies (see, e.g., Capozza et al., 2021, for a review), suggesting

[20] It is worth noting that the absolute difference in averages of standardized student achievement between states on adjacent ranks might be small in the middle of the distribution (see Table A1.4). However, as highlighted in Section 1.3.2, the performance differences across states are meaningfully large in an international context (KMK, 2002).

[21] Results of Table 1.1 are robust to excluding individual controls and state fixed effects (see Appendix Table A1.6).

[22] Calculating treatment-effects-on-the-treated (TOT) in the information *acquisition* treatments by dividing the intention-to-treat effects (ITT) reported in Table 1.1 by the information-acquisition rate shows that those who retrieve the information have similar information effects as the general-population subsample assigned to the information *provision* treatments. Thus, there does not appear to be a strong selection into information retrieval based on potential information effects. The exception is the subset of respondents in below-average performing states, where the TOT is less than half the size of the ITT. In these states, it appears to be the case that those who retrieve

**Table 1.1:** Treatment effects of performance information on citizen satisfaction with state education policy

| | Dependent variable: Satisfaction with education policy | | |
|---|---|---|---|
| State's student performance: | Above average (1) | Average (2) | Below average (3) |
| Info provision OWN rank | 0.242*** | 0.182*** | -0.162*** |
| | (0.027) | (0.029) | (0.020) |
| Info provision ALL ranks | 0.273*** | 0.265*** | -0.146*** |
| | (0.027) | (0.028) | (0.020) |
| Info acquisition OWN rank | 0.115*** | 0.125*** | -0.052** |
| | (0.030) | (0.029) | (0.021) |
| Info acquisition ALL ranks | 0.146*** | 0.128*** | -0.026 |
| | (0.029) | (0.029) | (0.022) |
| Control mean | 0.575 | 0.428 | 0.399 |
| Individual controls | yes | yes | yes |
| State fixed effects | yes | yes | yes |
| Number of observations | 2,421 | 2,982 | 4,871 |
| R-squared | 0.126 | 0.090 | 0.081 |

Notes: *** p<0.01, ** p<0.05, * p<0.1. OLS regressions. *Info provision OWN rank*, *Info provision ALL ranks*, *Info acquisition OWN rank*, and *Info acquisition ALL ranks* are experimental treatments in the survey. Dependent variable: Dummy variable 1 = respondents are "very satisfied" or "rather satisfied" with their state's education policy, 0 otherwise. Control mean: mean of the outcome variable in the control group in a regression without covariates (see Table A1.6). Weighted regressions. Survey weights are constructed to match administrative data for the German population regarding age, gender, state, educational attainment, municipality size classes, and employment status. Robust standard errors in parentheses. Data source: ifo Education Survey 2020.

that respondents consider performance information important for their satisfaction with state education policies.[23] At the same time, the fact that treatment effects halve when switching from information *provision* to the option of information *acquisition* highlights that even low barriers to performance information can have major effects on the extent to which citizens take this information into account. Adjusting standard errors for multiple hypotheses testing (three performance groups and four information treatments) using the method of Steinmayr (2020) based on List et al. (2019) does not change the statistical significance of the effects (not shown).

Next, we study whether information treatment effects differ by respondents' prior beliefs. Specifically, we categorize respondents into those who (i) estimate their state's rank worse than it is (too pessimistic), (ii) correctly estimate it, and (iii) estimate it to be better than the correct rank (too optimistic), and run interacted models based on Equation (1.2). To ease exposition, we combine the two information *provision* treatments and information *acquisition* treatments, respectively.[24] Appendix Table A1.7 reveals two key findings: First, treatment effects are significantly more positive for those who are overly pessimistic about their own state's rank, and more negative for those who are overly optimistic. This suggests that treatment effects are driven by genuine belief-updating. At the same time, we also find significant treatment effects among those who correctly estimate their state's rank. The latter finding suggests that salience-based updating

the information are less responsive to the information than the general-population subsample in the information *provision* treatments.

[23]Analyzing information acquisition behavior, Appendix Table A1.5 shows that click rates are higher among females, those with higher income and education levels, frequent voters, less patient and more risk tolerant respondents, and those working outside the education sector. Interestingly, click rates hardly vary by state performance.

[24]Wald tests of group equivalence confirm that differences between these treatments are statistically insignificant.

plays a role in our setting, too (e.g., Bleemer and Zafar, 2018).[25] This is hardly surprising given the lack of transparency and salience of student-performance information available to citizens.[26]

In sum, we find that citizens misperceive their state's educational performance, and that correcting these misperceptions through information provision strongly affects how satisfied citizens are with their state's education policies. These findings provide first evidence that citizens' misinformation could undermine efforts to hold politicians accountable for their policy performance; thus, preventing the full benefits of subnational policy competition from being realized.[27]

### 1.5.3 Treatment Effects on Citizens' and Parliamentarians' Policy Preferences

Next, we analyze how alleviating misperceptions about relative student performance affects citizens' and parliamentarians' support for a reform proposal to increase transparency regarding the performance of the education system.

**Results for citizens.** Table 1.2 illustrates the information treatment effects on a binary indicator of citizens' support for the transparency policy proposal (coded 1 if respondents are "very" or "rather" in favor, 0 otherwise). The control means reveal that support for the policy proposal is high at 77.6% in the control group, and that support does not differ by states' student performance. The information treatments, if anything, tend to have a small positive effect on support for the reform proposal in all performance groups. This finding is in contrast with the polarizing effects of the information treatments on satisfaction with state policy (see Table 1.1). The results imply that respondents favor the publication of performance data irrespective of their own state's position in the ranking, although their motives may well differ (e.g., utility from proving that ones' state is top ranked versus holding politicians accountable for poor educational performance of their own state). In particular, treatment *Info provision OWN rank* increases support for the transparency policy among respondents in above- and below-average performing states by 7.1 and 4.8 percentage points, respectively. The magnitude of these effects must, however, be interpreted carefully as coefficients become statistically insignificant when we correct for multiple hypotheses testing. The coefficients on treatment *Info provision ALL ranks* are smaller and not statistically significant. While the effect of treatment *Information acquisition OWN rank* is positive and marginally significant for respondents in below-average performance states (in line with the effect of treatment *Information provision OWN rank*) the other coefficients on the information-*acquisition* indicators are also small and statistically insignificant. These effects remain mostly insignificant when studying heterogeneities by prior beliefs about the own state's relative performance (see Appendix Table A1.8).[28] In addition, Appendix Table A1.9 reveals that

[25]The information-updating effects of Table A1.7 are also qualitatively unchanged when we do not interact our treatment indicators with binary variables for optimists or pessimists, but rather with a continuous variable measuring the deviation of the true rank of one's own state from the respective rank beliefs of our respondents (results are available upon request).

[26]In additional analyses we study how treatment effects differ by how sure respondents are that their prior beliefs are correct to distinguish misinformation from uninformedness (see Kuklinski et al., 2000; Lergetporer et al., 2020). Interaction effects between the treatment indicators and respondents' beliefs do not vary systematically by the certainty with which respondents hold their beliefs (results are available upon request).

[27]Note that more information on policy performance does not necessarily imply that voters change their voting behavior (Dunning et al., 2019). For example, voters might face multifaceted barriers for translating their policy satisfaction into political action. However, the consequentiality treatment (described in more detail below; see Table A1.11) shows that voter support for the transparency policy is independent of whether voter opinion is shared with state parliamentarians. This indicates that voters in the present context may indeed have a certain willingness for political action.

[28]We also pre-registered a secondary analysis of heterogeneities based on (i) citizens' stated baseline importance of comparability and (ii) citizens' stated political ideology. With respect to (i), the share of respondents who do not support the policy goal of comparability is too small (11.2%) for meaningful comparisons. With respect to (ii), we see some evidence for a reduced treatment effect of the *Info provision* treatments on satisfaction with policy in above-average performance states for right-leaning citizens (supporters of the CDU/CSU, FDP, or AfD) compared to respondents with no party preference, and for a reduced treatment effect of the *Info acquisition* treatments on

**Table 1.2:** Treatment effects of performance information on citizen support for the transparency policy

| | Dependent variable: Support for comparative tests | | |
|---|---|---|---|
| State's student performance: | Above average (1) | Average (2) | Below average (3) |
| Info provision OWN rank | 0.071*** | 0.032 | 0.048*** |
| | (0.025) | (0.024) | (0.018) |
| Info provision ALL ranks | 0.030 | -0.007 | -0.015 |
| | (0.028) | (0.026) | (0.019) |
| Info acquisition OWN rank | -0.003 | -0.004 | 0.031* |
| | (0.027) | (0.025) | (0.018) |
| Info acquisition ALL ranks | 0.010 | -0.019 | -0.030 |
| | (0.027) | (0.025) | (0.019) |
| Control mean | 0.776 | 0.779 | 0.775 |
| Individual controls | yes | yes | yes |
| State fixed effects | yes | yes | yes |
| Number of observations | 2,421 | 2,983 | 4,872 |
| R-squared | 0.053 | 0.068 | 0.075 |

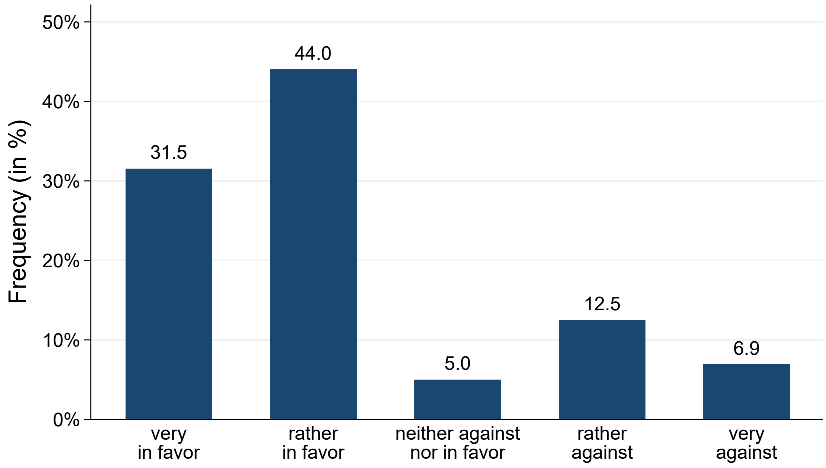
Notes: *** p<0.01, ** p<0.05, * p<0.1. OLS regressions. *Info provision OWN rank*, *Info provision ALL ranks*, *Info acquisition OWN rank*, and *Info acquisition ALL ranks* are experimental treatments in the survey. Dependent variable: Dummy variable 1 = respondents are "very in favor" or "rather in favor" of introducing regular comparative student tests, 0 otherwise. Control mean: mean of the outcome variable in the control group in a regression without covariates (see Table A1.9). Weighted regressions. Survey weights are constructed to match administrative data for the German population regarding age, gender, state, educational attainment, municipality size classes, and employment status. Robust standard errors in parentheses. Data source: ifo Education Survey 2020.

the identified treatment effects of Table 1.2 are robust to the exclusion of control variables and state fixed effects. Finally, note that the absence of a more systematic increase in policy support in response to the information experiments is unlikely to be due to a potential ceiling effect (because average support is already high). This is evident from ordered probit models using the full variation of the categorical policy support variable (see Appendix Table A1.10). The results are qualitatively similar to the binary-outcome regressions in Table 2, although only 34.9% of respondents state that they are "very in favor" of the reform proposal, leaving considerable room for higher policy support on the intensive margin. Overall, these results show that support for the transparency-enhancing reform among the general public is high, and does not decrease when performance information is provided.

To address a common concern about policy preferences elicited in surveys, namely that they are susceptible to reporting bias or experimenter demand effects, we randomly informed respondents that their aggregate answers to the question about support for the transparency policy will be passed on to their state politicians (treatment *Consequential*). Appendix Table A1.11 presents results of an interaction model based on Equation (1.2). If anything, treatment *Consequential* increases support for the transparency policy, implying that high support levels reported in Table 1.2 are not due to a lack of political consequences of the survey answers. At the same time,

reform preferences in average performance states for left-leaning citizens (supporters of the SPD, DIE LINKE, or the Greens) compared to respondents with no party preference. We refrain from interpretation of these results due to the number of estimated coefficients. Details are available upon request.

**Figure 1.3:** Parliamentarian support for the transparency policy




Notes: The question asked parliamentarians whether they are in favor or against the policy proposal to introduce regular comparative student tests (details see Figure A1.3 in the Appendix). N=520. Data source: ZEW/University Mannheim parliamentary survey 2020.

the small and insignificant coefficients on the interaction terms reveal that information treatment effects do not vary systematically by responses' consequentiality.[29]

**Results for parliamentarians.** We now turn to our parliamentary survey and analyze to what extent parliamentarians' preferences for the transparency policy are consistent with citizens' preferences, and how they are affected by performance information. Although politicians should theoretically represent the policy preferences of their constituents, they may have different attitudes towards transparency.[30] However, as Figure 1.3 shows, a large share of 75.6% of all survey respondents state that they are "very" or "rather" in favor of the transparency policy, a share almost as high as in the population sample (77.6%).[31] Similarly, 87.3% of parliamentarians state that they think comparability of student achievement across states is "very important" or "rather important",[32] which is closely in line with the share of citizens with that view (88.8%). These results suggest that transparency preferences of parliamentarians and citizens are well aligned, which speaks against the hypothesis that the lack of performance transparency in the German education system is due to parliamentarians not representing citizens' preferences. However, it is important to keep in mind that stated average support rates among parliamentarians may be subject to social desirability bias in the context of our survey.

We then analyze the effect of providing relative performance information to parliamentarians on their preferences for the transparency policy. In Table 1.3, we regress policy support on (i) the treatment dummy *Information provision worse/better half* (coded 1 if the parliamentarian

[29]Importantly, Lergetporer and Wößmann (2022) verify that informing respondents that their aggregate answers will be passed on to state politicians successfully alters their perceptions about the political consequences of their survey answers.

[30]In theories of political representation, voters delegate decisions to citizens who present themselves as candidates to run for public office and become elected to represent their constituents (i.e., the idea of citizen candidates as developed by Besley and Coate, 1997; Osborne and Slivinski, 1996). However, politicians are often misinformed about their citizens' preferences, which undermines the ability of politicians to represent the electorate's preferences (Broockman and Skovron, 2018).

[31]Support rates are similar among members of government parties (72.7%) and opposition parties (79.6%), though the share of those "very in favor" is significantly lower in the former group. The survey mode (pen and paper versus online), has no statistically significant effect on policy support (results are available upon request).

[32]Support for general comparability among parliamentarians is not affected by whether or not they were asked to consider support rates among voters (this was an additional treatment in the parliamentary survey, details see Figure A1.3).

**Table 1.3:** Treatment effects of performance information on parliamentarian support for the transparency policy

| Dependent variable: | Dependent variable: Support for comparative tests | |
|---|---|---|
| | Dummy | 5-point scale |
| | (1) | (2) |
| Info provision worse/better half | 0.101* | 0.367** |
| | (0.058) | (0.150) |
| Worse half (ranks 9-16) | 0.135** | 0.508*** |
| | (0.066) | (0.168) |
| Info provision*Worse half | -0.198** | -0.785*** |
| | (0.093) | (0.242) |
| Constant | 0.508 | 3.088 |
| Individual controls | yes | yes |
| Party fixed effects | yes | yes |
| Number of observations | 353 | 353 |
| R-squared | 0.156 | 0.245 |

Notes: *** p<0.01, ** p<0.05, * p<0.1. OLS regressions. *Info provision worse/better half* is an experimental treatment in the survey. Dependent variable model (1): Dummy variable 1 = respondents are "very in favor" or "rather in favor" of introducing regular comparative student tests, 0 otherwise. Dependent variable model (2): Categorical variable 1 = respondents are "very against" to 5 = "very in favor" of introducing regular comparative student tests. Missing data for three control variables (*Education profession*, *Abitur*, *University degree*) has been imputed based on sample means. Imputation dummies included in both models. Robust standard errors in parentheses. Data source: ZEW/University Mannheim parliamentary survey 2020.

received information about whether his/her state is in the worse/better half of German states, 0 otherwise), (ii) an indicator whether the parliamentarian's state is actually in the worse half, and (iii) the interaction of the two. As dependent variables, we use the binary indicator for policy support (column 1) as well as the 5-point scale (column 2). Among parliamentarians in the better half of states, the information treatment increases support for the transparency policy by 10.1 percentage points ($p < 0.1$). The positive effect is also sizable and statistically significant at the 5% level in model (2), which exploits the full variation in the outcome variable. While parliamentarians in worse-performing states tend to be *more* supportive of comparative tests when not provided with performance information (see coefficients on the worse-half indicator), more importantly, their information-treatment effect is *negative* and statistically significantly lower than that of parliamentarians in better-performing states (see the respective interaction terms). These rather strong and heterogeneous treatment effects which amount to 21.7% or 22.6% of a standard deviation for the worse or better half of German states, respectively suggest that parliamentarians are (i) poorly informed about their state's educational performance (in line with our descriptive findings in Section 1.5.1) and (ii) opportunistic in how they change their support for the transparency policy in response to performance information: They increase (decrease) their support for the transparency policy when learning that their state's education policy outcomes are in the better (worse) half of all states.[33] Given the space constraints in our parliamentarian survey, we could not ask further questions on the perception of the policy post-treatment to study the mechanisms of these findings (such as its usefulness from the perspective of parliamentarians).

[33]The regression results for parliamentarians need to be interpreted with some caution as the number of observations is relatively low (N=353). Yet, effect sizes are somewhat larger but comparable to the effects of other information interventions among politicians on their policy choices in surveys or in the field (Geys and Sørensen, 2018; Jablonski and Seim, 2023; Lee, 2022). Findings from Table 1.3 are robust to using ordered probit regressions instead of OLS (see Appendix Table A1.12). Table A1.12 also reports results from OLS models excluding covariates.

However, we observe a high correlation of policy support and support for general comparability of education in Germany (elicited before the experiment; 42.5%) in the control group which hints at the fact that parliamentarians see comparative student testing as a useful tool to foster comparability in education.

In sum, while the population strongly supports greater transparency with support rates, if anything, increasing with the provision of educational performance information, the direction in which performance information affects parliamentarians' transparency preferences depends on whether the information disclosed is favorable to them or not. Parliamentarians' heterogeneous reactions to performance information are consistent with social-image or re-election concerns, and may constitute an impediment to implementing policies to foster comparability of educational performance across states. This is particularly true since parliamentarians are likely to inform themselves about their state's performance prior to voting on the introduction of regular student performance assessments.[34] [35]

### 1.5.4 Citizens' and Parliamentarians' Beliefs about Each Other's Policy Preferences

This section investigates citizens' and parliamentarians' beliefs about each other's transparency preferences. In addition to citizens' and parliamentarians' preferences for transparency, their beliefs about each other's preferences may also determine whether transparency reforms are actually implemented or not. For instance, if parliamentarians (mistakenly) believe that citizens do not support the transparency policy, they could be reluctant to implement it even if they support it themselves.

First, we analyze to what extent parliamentarians are aware of citizens' preferences. To do so, we asked parliamentarians to guess what share of the public in their state supports the proposed transparency policy. Figure 1.4 presents the distribution of beliefs and reveals that parliamentarians are rather well informed of citizens' transparency preferences. On average, parliamentarians believe that 64.9% of citizens support the policy (compared to an actual support rate of 77.6%). Put differently, well over two-thirds of parliamentarians believe that a majority of citizens in their state supports the policy proposal.

Next, we analyze citizens' beliefs about parliamentarians' transparency preferences. These beliefs were elicited after the information treatments in the population survey. Thus, we cannot only report average beliefs, but can also estimate performance-information effects on these beliefs.

Interestingly, the control means of Table 1.4 reveal that across respondents in different states only minorities of 45.6% to 46.6% believe that their state parliamentarians support the reform proposal. This lies in stark contrast to parliamentarians' stated support of 75.6%. The coefficients on the treatment indicators shows that citizens update their beliefs about parliamentarians' policy support after receiving information about their state's relative educational performance. In particular, both information *provision* and *acquisition* treatments significantly increase citizen beliefs by 2.7 to 7.2 percentage points in states with above-average student performance. In average performing states, we find positive effects of both information *provision* treatments

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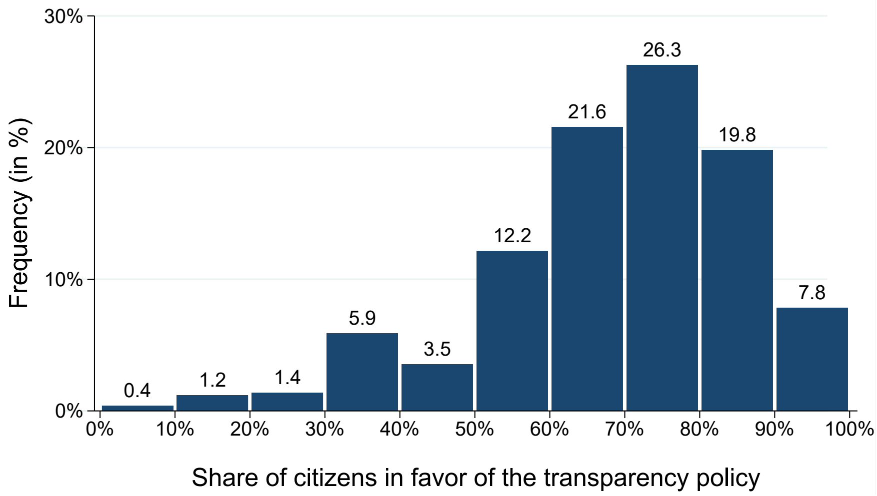
[34]Note that the political hurdles for implementing more stringent nationwide student-performance tests are relatively high (see Section 1.3.1). A comprehensive reform would require the governments of all 16 states to reach a consensus on the issue. This process can be easily undermined by individual states unwilling to implement the transparency policy (e.g., since it would increase the visibility of low performance).

[35]As a final question in the parliamentary survey (online version), respondents had the option to request information on population preferences regarding the reform proposal of comparative student tests elicited in the population survey (as pre-registered). Following completion of the survey, we sent each respondent an email with the requested support rates among the respective group of citizens. We find that parliamentarians in the treatment group (i.e., those who received performance information) are not more likely to request information than those in the control group. However, conditional on requesting information, parliamentarians in the treatment group are more interested in information on the opinion of the German population overall rather than that of supporters of their own party or residents in their own state. For the detailed wording of the respective question, see Figure A1.4. Results are available upon request.

**Figure 1.4:** Parliamentarian beliefs about citizen support for the transparency policy




Notes: The question elicited parliamentarians' beliefs about the support among citizens for the introduction of regular comparative tests. Parliamentarians were asked to state the share of citizens in their state that they think is "very in favor" or "rather in favor" of the reform proposal (details see Figure A1.3 in the Appendix). N=510. Data source: ZEW/University Mannheim parliamentary survey 2020.

(of 2.9 and 4.5 percentage points). Among citizens living in below-average performing states, beliefs about parliamentarian support for the transparency policy remain low regardless of the information treatments.[36] These treatment effects reveal that citizens (correctly) estimate parliamentarians' policy support as opportunistic in the sense that parliamentarians are expected to support performance transparency only if the disclosed performance information is favorable to them. This view among the population may ultimately undermine public pressure to improve transparency of states' educational performance if citizens believe that politicians will only be willing to implement such policies if they generate positive reputation signals for themselves (e.g., to increase re-election probabilities).[37]

## 1.6 Conclusion

The key argument for policy competition between subnational entities is that allowing citizens to compare policy outcomes of their and similar jurisdictions encourages politicians to deliver public services effectively and efficiently. We argue that for that to be the case, policy outcomes of different jurisdictions need to be observable and comparable, which is often not the case and can lead to misinformation about policy outcomes. Indeed, information frictions about policy outcomes are ubiquitous across many policy areas and countries and exist both at the national and subnational level. A case in point is the German education system, where the legislative and executive power over public education is vested in the 16 federal states. While this structure would, in principle, lend itself to reaping the benefits of yardstick competition, this is undermined by the lack of regular student achievement tests that would enable comparisons of educational performance across states. In this context, we implemented parallel surveys with German citizens (N>10,000) and state parliamentarians (N>500) to study (i) the degree of misperceptions about state's educational performance in both groups, and (ii) how factual information about states'

[36] The findings are robust to the exclusion of individual controls and state fixed effects (see Appendix Table A1.13).

[37] In additional robustness analyses, our regression results hold when using interaction models instead of sample splits to analyze the citizen survey, and when using probit regressions instead of OLS (results are available upon request).

**Table 1.4:** Treatment effects of performance information on citizen beliefs about parliamentarian support for the transparency policy

| | Dependent variable: Citizen belief about parliamentarian support | | |
|---|---|---|---|
| State's student performance: | Above average (1) | Average (2) | Below average (3) |
| Info provision OWN rank | 7.159*** | 2.911** | -1.264 |
| | (1.482) | (1.256) | (0.982) |
| Info provision ALL ranks | 2.671* | 4.454*** | -0.875 |
| | (1.442) | (1.209) | (0.989) |
| Info acquisition OWN rank | 4.845*** | 1.469 | -0.734 |
| | (1.386) | (1.269) | (0.969) |
| Info acquisition ALL ranks | 7.175*** | 1.729 | -1.014 |
| | (1.421) | (1.219) | (0.973) |
| Control mean | 45.629 | 46.584 | 45.868 |
| Individual controls | yes | yes | yes |
| State fixed effects | yes | yes | yes |
| Number of observations | 2,418 | 2,981 | 4,866 |
| R-squared | 0.042 | 0.036 | 0.032 |

Notes: *** p<0.01, ** p<0.05, * p<0.1. OLS regressions. *Info provision OWN rank*, *Info provision ALL ranks*, *Info acquisition OWN rank*, and *Info acquisition ALL ranks* are experimental treatments in the survey. Dependent variable: Continuous variable ranging from 0 to 100%, capturing the share of parliamentarians "very in favor" or "rather in favor" of the policy proposal to introduce regular comparative student tests from the citizen perspective. Control mean: mean of the outcome variable in the control group in a regression without covariates (see Table A1.13). Weighted regressions. Survey weights are constructed to match administrative data for the German population regarding age, gender, state, educational attainment, municipality size classes, and employment status. Robust standard errors in parentheses. Data source: ifo Education Survey 2020.

relative educational performance affects citizens' satisfaction with education policy, and both citizens' and parliamentarians' support for increasing transparency in the education system.

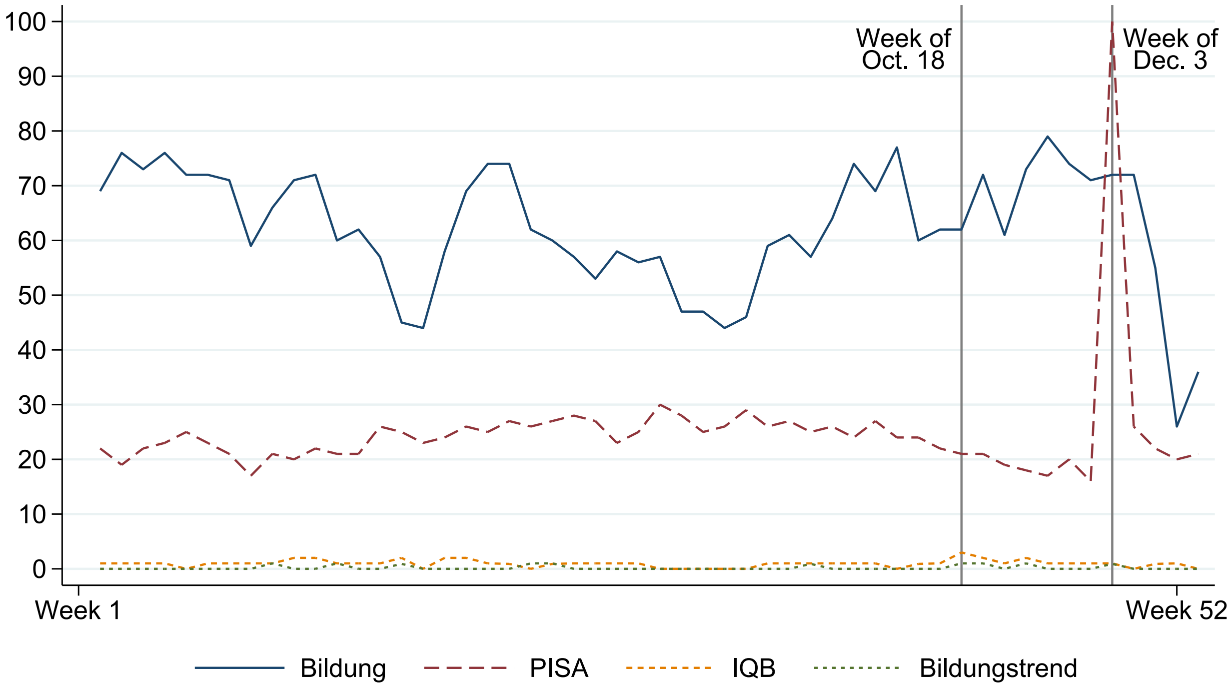
We first document that beliefs about states' educational performance are biased. In particular, citizens and parliamentarians from low-performing states are too optimistic about their state's relative performance. Second, we show that citizens' satisfaction with their own state's education policy is strongly correlated with the state's educational performance, and that information about actual student performance further polarizes the satisfaction levels between better- and worse-performing states. Third, citizens strongly support the proposal to improve transparency in the education system by introducing regular comparative student tests for state comparisons. Providing information about actual student performance further increases preferences for transparency among citizens in above- and below-average performing states. Fourth, parliamentarians' support for increased transparency (which is comparable to the support of citizens in the control group) responds strongly and opportunistically to relative performance information: Parliamentarians in above-average (below-average) performing states increase (decrease) their support for increased transparency upon learning about their own state's performance rank. Fifth, citizens underestimate their parliamentarians' support for increased transparency, but correctly anticipate that parliamentarians increase (decrease) their stated policy support when provided with favorable (unfavorable) information about their state's educational performance.

In sum, we have identified two plausible obstacles to citizens holding their state parliamentarians accountable for low policy performance in the spirit of yardstick competition. First, citizens are poorly informed about their state's educational performance, implying that a prerequisite for subnational policy competition is not met in the German education system. Second, and relatedly, our results are consistent with opportunistic behavior by parliamentarians in the sense that they only support increased performance transparency if information disclosure is favorable to them (e.g., with respect to their public image which will eventually translate into their likelihood for re-election). At the most basic level, our results call for an incorporation of insufficient and non-comparable information on policy outcomes into models of yardstick competition, since ensuing informational frictions can undermine potential welfare gains from subnational policy competition.

From a policy perspective, our findings suggest that the discussions around increased student-performance testing in Germany are stuck in a "bad equilibrium": Citizens are misinformed about their state's educational performance, and parliamentarians in low-performing states have incentives to block initiatives to increase transparency so as to not to be held accountable for low performance. Our strong information treatment effects on citizens' satisfaction with their own state's education policy demonstrate that providing them with more state performance information (possibly not only regarding education performance) may increase pressure on state parliamentarians not only to improve transparency but also public service performance more generally.

# Appendix

**Figure A1.1:** Google search requests for “PISA”, “IQB”, “Bildungstrend”, and “Bildung”, in 2019




Notes: Google search requests from January to December 2019 in Germany. Frequencies depicted relative to the highest number of search requests. The spike in the search request for PISA coincides with the release of the PISA test results on December 3, 2019. The results of the IQB Bildungstrend were published on October 18, 2019 (no spike visible). Source: Google Trends (www.google.com/trends [accessed: November 18, 2022]), own figure.

**Figure A1.2:** Questions from the population survey

| Please state your spontaneous opinion. | |
|---|---|
| How important do you consider the comparability of student performance across states? | ○ Very important<br>○ Rather important<br>○ Rather unimportant<br>○ Very unimportant<br>○ Neither important nor unimportant |

| Please state your spontaneous opinion. | |
|---|---|
| A recent educational study compared the mathematics performance of 9th grade students in the 16 German federal states. What is your best guess on how the students in your state ranked? (1 is the best, 16 is the worst) | Rank ... |
| How certain are you about your answer being roughly correct? | Very uncertain<br>1 2 3 4 5 6 7<br>Very certain |

| Randomization:<br>5 groups in total (a-e). Probabilities of being allocated into one of the groups are indicated below. | |
|---|---|
| Please state your spontaneous opinion. | |
| [a] [Probability=1/3; N≈3,333] | |
| How satisfied are you with the education policy of your state? | ○ Very satisfied<br>○ Rather satisfied<br>○ Rather unsatisfied<br>○ Very unsatisfied<br>○ Neither satisfied nor unsatisfied |
| [b] [Probability=1/6; N≈1,666] | |
| The students in **your state ranked X among all 16 states** in the educational study mentioned in the previous question. | |
| How satisfied are you with the education policy of your state? | ○ Very satisfied<br>○ Rather satisfied<br>○ Rather unsatisfied<br>○ Very unsatisfied<br>○ Neither satisfied nor unsatisfied |

Figure continues on next page.

Figure A2 continued

| | |
|---|---|
| [c] [Probability=1/6; N≈1,666. Example here: Lower Saxony] | |
| Students from the **different states** ranked as shown below in the educational study mentioned in the previous question.<br>1. Saxony<br>2. Bavaria<br>3. Thuringia<br>4. Baden-Württemberg<br>5. Brandenburg<br>6. Saxony-Anhalt<br>7. Lower Saxony<br>8. Hesse<br>9. Rhineland-Palatinate<br>10. North Rhine-Westphalia<br>11. Hamburg<br>12. Schleswig-Holstein<br>13. Mecklenburg-West Pomerania<br>14. Saarland<br>15. Berlin<br>16. Bremen | |
| How satisfied are you with the education policy of your state? | ○ Very satisfied<br>○ Rather satisfied<br>○ Rather unsatisfied<br>○ Very unsatisfied<br>○ Neither satisfied nor unsatisfied |
| Please state your spontaneous opinion. | |
| [d] [The information of randomization [b] is shown if participants click. Probability=1/6; N≈1,666] | |
| *Click here* if you want to learn how students from **your state** ranked in the educational study mentioned in the previous question. | |
| How satisfied are you with the education policy of your state? | ○ Very satisfied<br>○ Rather satisfied<br>○ Rather unsatisfied<br>○ Very unsatisfied<br>○ Neither satisfied nor unsatisfied |
| [e] [The information of randomization [c] is shown if participants click. Probability=1/6; N≈1,666] | |
| *Click here* if you want to learn how students from **different states** ranked in the educational study mentioned in the previous question. | |
| How satisfied are you with the education policy of your state? | ○ Very satisfied<br>○ Rather satisfied<br>○ Rather unsatisfied<br>○ Very unsatisfied<br>○ Neither satisfied nor unsatisfied |

Figure continues on next page.

Figure A2 continued

| |
|---|
| Randomization: Half of the participants saw the question below (i.e., [a]). The other half received the information as shown in the text box below (see [b]). |

Please state your spontaneous opinion.

[a] [Probability=1/2; N≈5,000]

| | |
|---|---|
| Are you in favor or opposed to the proposal to introduce uniform nationwide tests in Mathe- matics and German in all school types that, starting in grade 5, are conducted every two years and whose average results by state would be published to allow a comparison of student achievement across states? | I am ...<br>○ Very in favor<br>○ Rather in favor<br>○ Rather against<br>○ Very against<br>○ Neither in favor nor against |

[b] [Probability=1/2; N≈5,000]

| |
|---|
| The average answers to the next question will be send to the parliamentarians of your state after the survey is finished. Your answer is therefore particularly important. |

| | |
|---|---|
| Are you in favor or opposed to the proposal to introduce uniform nationwide tests in Mathe- matics and German in all school types that, starting in grade 5, are conducted every two years and whose average results by state would be published to allow a comparison of student achievement across states? | I am ...<br>○ Very in favor<br>○ Rather in favor<br>○ Rather against<br>○ Very against<br>○ Neither in favor nor against |

Please state your spontaneous opinion.

| | |
|---|---|
| What is your best guess, which share of parliamentarians in your state is “very in favor” or “rather in favor” of the reform proposal mentioned in the previous question about introducing nationwide uniform tests? | ... percent |
| How certain are you about your answer being roughly correct? | Very uncertain<br>1 2 3 4 5 6 7<br>Very certain |

Notes: Randomization procedures are described by the gray-shaded text. Information treatments are indicated by a black box around the provided information text. Source: ifo Education Survey 2020.

**Figure A1.3:** Questions from the parliamentary survey (pen & paper)

| Please state your spontaneous opinion. | |
|---|---|
| How important do you consider the comparability of student performance across states? | ○ Very important<br>○ Rather important<br>○ Rather unimportant<br>○ Very unimportant<br>○ Neither important nor unimportant |

| | |
|---|---|
| Randomization:<br>Half of the participants received the question below (i.e., [a]), whereas the other half did not receive the question and continued with the subsequent question. | |
| [a] [Probability=1/2]<br>What is your best guess, what share of citizens in your state considers the comparability of student performance across states “very important” or “rather important”? | ... percent |
| [a] [Probability=1/2]<br>Did not receive the above question (see [a]) and continued with the subsequent question. | |

| | |
|---|---|
| A recent educational study compared the mathematics performance of 9th grade students in the 16 German federal states. What is your best guess on how the students in your state ranked? (1 is the best, 16 is the worst.) | Rank ... |

| | |
|---|---|
| What is your best guess, what do citizens in your state think about how the students in your state ranked in the educational study? (1 is the best, 16 is the worst.) | Rank ... |

| | |
|---|---|
| Are you in favor or opposed to the proposal to introduce uniform nationwide tests in Mathe- matics and German in all school types that, starting in grade 5, are conducted every two years and whose average results by state would be published to allow a comparison of student achievement across states? | I am ...<br>○ Very in favor<br>○ Rather in favor<br>○ Rather against<br>○ Very against<br>○ Neither in favor nor against |

| | |
|---|---|
| What is your best guess, which share of citizens in your state is “very in favor” or “rather in favor” of the reform proposal mentioned in the previous question about introducing nationwide uniform student tests? | ... percent |

Notes: The pen & paper survey was sent to all parliamentarians. Participants could choose whether they wanted to use the pen & paper or the online version of the survey (see Section 4.2). Source: ZEW/University Mannheim parliamentary survey 2020.

**Figure A1.4:** Additional questions from the online version of the parliamentary survey

| | |
|---|---|
| Randomization:<br>Half of the participants received the question below without any additional information (i.e., [a]). The other half received the information as shown in the text box below (see [b]). | |
| [a] [Probability=1/2]<br>Now we would like to once more learn about your opinion on regular comparative tests. | |
| Are you in favor or opposed to the afore-mentioned reform proposal about introducing nationwide uniform student comparison tests? | I am ...<br>○ Very in favor<br>○ Rather in favor<br>○ Rather against<br>○ Very against<br>○ Neither in favor nor against |
| [b] [Probability=1/2]<br>Now we would like to once more learn about your opinion on regular comparative tests. | |
| In a recent educational study on student performance in the subject of mathematics, students from your state ranked in the **better/worse** half among all states. | |
| Are you in favor or opposed to the afore-mentioned reform proposal about introducing nationwide uniform student comparison tests? | I am ...<br>○ Very in favor<br>○ Rather in favor<br>○ Rather against<br>○ Very against<br>○ Neither in favor nor against |

| | |
|---|---|
| Randomization:<br>Half of the participants received the question below with the order of the answer categories as shown in [a]. The other half received the same question and answer categories but with the reversed order of the answer categories (see [b]). | |
| [a] [Probability=1/2]<br>Thank you very much for taking part in the survey. Parallel to this survey among parliamentarians, we also surveyed the German population about the same topic. We now offer you the opportunity to receive information about to what extent **respondents in the population survey** support the reform proposal about introducing **nationwide uniform student comparison tests**. We will send you the chosen information by email. Which of the following information would you like to receive?<br>(Please chose **one** of the options) | Average support rates among ...<br>○ ... all surveyed citizens in Germany<br>○ ... all surveyed cizizens in your state<br>○ ... all supporters of your party in Germany<br>○ ... all supporters of your party in your state<br>○ I do not want to receive any information |

Figure continues on next page.

Figure A4 continued

[b] [Probability=1/2]

| | |
|---|---|
| Thank you very much for taking part in the survey.<br>Parallel to this survey among parliamentarians, we also surveyed the German population about the same topic. We now offer you the opportunity to receive information about to what extent **respondents in the population survey** support the reform proposal about introducing **nationwide uniform student comparison tests**. We will send you the chosen information by email. Which of the following information would you like to receive?<br>(Please chose **one** of the options) | Average support rates among ...<br>○ ... all supporters of your party in Germany<br>○ ... all supporters of your party in your state<br>○ ... all surveyed citizens in Germany<br>○ ... all surveyed cizizens in your state<br>○ I do not want to receive any information |

The pen & paper survey was sent to all parliamentarians. Participants could choose whether they wanted to use the pen & paper or the online version of the survey (see Section 4.2). For questions that were asked to all parliamentarians, see Appendix Figure A1.3. Parliamentarians who chose to participate online received additional questions that made use of information treatments as described in this figure. Randomization procedures are described by the gray-shaded text. Information treatments are indicated by a black box around the provided information text. Source: ZEW/University Mannheim parliamentary survey 2020.

**Table A1.1:** Summary statistics and balancing tests – population survey

| | Control group | Info provision OWN rank | | Info provision ALL ranks | | Info acquisition OWN rank | | Info acquisition ALL ranks | |
|---|---|---|---|---|---|---|---|---|---|
| | mean | mean | difference | mean | difference | mean | difference | mean | difference |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| Age | 45.48 | 44.68 | -0.81* | 44.01 | -1.48*** | 44.68 | -0.81* | 44.39 | -1.10** |
| Female | 0.510 | 0.488 | -0.023 | 0.523 | 0.012 | 0.474 | -0.036** | 0.481 | -0.030* |
| Born in Germany | 0.944 | 0.935 | -0.009 | 0.947 | 0.003 | 0.949 | 0.005 | 0.948 | 0.004 |
| Monthly household income (€) | 2,757 | 2,716 | -42.0 | 2,767 | 9.5 | 2,707 | -50.0 | 2,716 | -42.0 |
| Partner in household | 0.615 | 0.602 | -0.013 | 0.618 | 0.003 | 0.583 | -0.032** | 0.601 | -0.014 |
| Parent(s) with university degree | 0.323 | 0.343 | 0.020 | 0.328 | 0.005 | 0.324 | 0.001 | 0.328 | 0.005 |
| Works in education sector | 0.114 | 0.130 | 0.016 | 0.138 | 0.024** | 0.124 | 0.010 | 0.130 | 0.016 |
| *Highest educational attainment* | | | | | | | | | |
| No degree/basic degree | 0.308 | 0.303 | -0.005 | 0.305 | -0.003 | 0.318 | 0.010 | 0.325 | 0.017 |
| Middle school degree | 0.296 | 0.280 | -0.016 | 0.283 | -0.013 | 0.267 | -0.029** | 0.280 | -0.016 |
| Abitur | 0.396 | 0.417 | 0.021 | 0.412 | 0.016 | 0.416 | 0.020 | 0.395 | -0.001 |
| *Highest professional degree* | | | | | | | | | |
| Vocational degree | 0.696 | 0.655 | -0.041*** | 0.664 | -0.033** | 0.650 | -0.046*** | 0.670 | -0.027* |
| University degree | 0.220 | 0.244 | 0.024* | 0.218 | -0.002 | 0.235 | 0.015 | 0.221 | 0.001 |
| No degree | 0.082 | 0.086 | 0.003 | 0.109 | 0.027*** | 0.096 | 0.014 | 0.100 | 0.018* |
| In training | 0.046 | 0.064 | 0.017** | 0.060 | 0.014* | 0.061 | 0.015* | 0.059 | 0.012 |
| University student | 0.027 | 0.038 | 0.011* | 0.032 | 0.005 | 0.034 | 0.007 | 0.031 | 0.004 |
| *Employment status* | | | | | | | | | |
| Full-time employed | 0.497 | 0.465 | -0.032** | 0.493 | -0.003 | 0.496 | -0.001 | 0.492 | -0.005 |
| Part-time employed | 0.156 | 0.166 | 0.009 | 0.158 | 0.002 | 0.155 | -0.001 | 0.162 | 0.006 |
| Self-employed | 0.053 | 0.047 | -0.006 | 0.048 | -0.005 | 0.050 | -0.003 | 0.054 | 0.001 |
| Unemployed | 0.040 | 0.057 | 0.017** | 0.054 | 0.015** | 0.033 | -0.006 | 0.041 | 0.002 |
| *Parental status* | | | | | | | | | |
| No children | 0.451 | 0.453 | 0.002 | 0.456 | 0.005 | 0.450 | 0.000 | 0.449 | -0.002 |
| At least one child $< 18$ | 0.234 | 0.244 | 0.010 | 0.256 | 0.022 | 0.231 | -0.004 | 0.230 | -0.004 |
| All children $\geq 18$ | 0.315 | 0.303 | -0.012 | 0.288 | -0.027* | 0.319 | 0.004 | 0.321 | 0.006 |
| *Political party preference* | | | | | | | | | |
| CDU/CSU | 0.223 | 0.209 | -0.014 | 0.219 | -0.004 | 0.202 | -0.022* | 0.204 | -0.020 |
| SPD | 0.120 | 0.111 | -0.008 | 0.135 | 0.015 | 0.132 | 0.012 | 0.131 | 0.011 |
| Linke | 0.088 | 0.073 | -0.015* | 0.088 | 0.000 | 0.086 | -0.002 | 0.081 | -0.007 |
| Grüne | 0.134 | 0.131 | -0.002 | 0.135 | 0.001 | 0.161 | 0.028** | 0.140 | 0.007 |
| Other | 0.162 | 0.181 | 0.018 | 0.161 | -0.001 | 0.162 | 0.000 | 0.160 | -0.003 |
| None | 0.273 | 0.295 | 0.022 | 0.263 | -0.010 | 0.257 | -0.016 | 0.284 | 0.011 |
| Frequent voter | 0.823 | 0.817 | -0.006 | 0.831 | 0.008 | 0.819 | -0.004 | 0.811 | -0.011 |
| Patience (11-point scale) | 7.434 | 7.400 | -0.035 | 7.429 | -0.005 | 7.409 | -0.025 | 7.316 | -0.118 |
| Risk tolerance (11-point scale) | 5.320 | 5.387 | 0.066 | 5.309 | -0.011 | 5.339 | 0.018 | 5.420 | 0.100 |
| Number of observations | 3,404 | 1,756 | | 1,713 | | 1,731 | | 1,721 | |

Notes: *** p<0.01, ** p<0.05, * p<0.1. Columns 3, 5, 7, and 9 show the difference to the control group (i.e., column 1). Weighted summary statistics. Survey weights are constructed to match administrative data for the German population regarding age, gender, state, educational attainment, municipality size classes, and employment status. Data source: ifo Education Survey 2020.

**Table A1.2:** Survey participation analysis – parliamentary survey

| | Dependent variable: Survey participation | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Average | -0.014 | | | -0.002 |
| | (0.032) | | | (0.033) |
| Below average | -0.077*** | | | -0.042 |
| | (0.027) | | | (0.029) |
| *Political party affiliation* | | | | |
| SPD | | -0.054* | | -0.025 |
| | | (0.028) | | (0.030) |
| Grüne | | 0.007 | | -0.002 |
| | | (0.035) | | (0.037) |
| AfD | | -0.025 | | -0.058 |
| | | (0.035) | | (0.044) |
| Linke | | 0.011 | | 0.020 |
| | | (0.043) | | (0.046) |
| FDP | | 0.071 | | 0.085 |
| | | (0.050) | | (0.054) |
| Other | | -0.029 | | -0.090 |
| | | (0.060) | | (0.061) |
| Age | | | 0.002 | 0.002* |
| | | | (0.001) | (0.001) |
| Female | | | -0.076*** | -0.077*** |
| | | | (0.023) | (0.025) |
| Education profession | | | 0.060 | 0.065 |
| | | | (0.049) | (0.048) |
| Abitur | | | 0.040 | 0.042 |
| | | | (0.038) | (0.038) |
| University degree | | | -0.008 | -0.012 |
| | | | (0.032) | (0.032) |
| Opposition | | | -0.031 | -0.019 |
| | | | (0.022) | (0.026) |
| Years in parliament | | | -0.007*** | -0.008*** |
| | | | (0.002) | (0.002) |
| State education committee | | | 0.006 | 0.001 |
| | | | (0.028) | (0.028) |
| Constant | 0.344*** | 0.311*** | 0.232*** | 0.250*** |
| | (0.023) | (0.019) | (0.063) | (0.066) |
| Number of observations | 1,862 | 1,862 | 1,604 | 1,604 |
| R-squared | 0.006 | 0.005 | 0.016 | 0.023 |

Notes: *** p<0.01, ** p<0.05, * p<0.1. OLS regressions. Robust standard errors in parentheses. Data source: ZEW/University Mannheim parliamentary survey 2020.

**Table A1.3:** Summary statistics and balancing tests – parliamentary survey

| | Control group | Treatment: Information provision | |
|---|---|---|---|
| | mean | mean | difference |
| | (1) | (2) | (3) |
| *Political party affiliation* | | | |
| CDU/CSU | 0.305 | 0.240 | -0.065 |
| SPD | 0.226 | 0.209 | -0.017 |
| Grüne | 0.168 | 0.168 | -0.000 |
| AfD | 0.121 | 0.117 | -0.004 |
| Linke | 0.089 | 0.112 | 0.023 |
| FDP | 0.074 | 0.102 | 0.028 |
| Other | 0.016 | 0.051 | 0.035* |
| Age | 50.71 | 50.26 | -0.450 |
| Female | 0.253 | 0.250 | -0.003 |
| Education profession | 0.109 | 0.069 | -0.040 |
| Abitur | 0.878 | 0.950 | 0.072 |
| University degree | 0.814 | 0.764 | -0.050 |
| Opposition | 0.38.9 | 0.429 | 0.039 |
| Years in parliament | 6.47 | 6.22 | -0.249 |
| State education committee | 0.279 | 0.224 | -0.054 |
| Number of observations | 190[1] | 196[2] | |

Notes: *** p<0.01, ** p<0.05, * p<0.1. Column 3 shows the difference to the control group (i.e., column 1). [1]Reduced number of observations for *Abitur* (180), *University degree* (188), *Education profession* (156). [2]Reduced number of observations for *Abitur* (179), *University degree* (195), *Education profession* (159). Data source: ZEW/University Mannheim parliamentary survey 2020.

**Table A1.4:** State results for the cross-state student test in mathematics including the categorization into three performance groups

| Rank | State | Mean | Standard error | Performance group |
|---|---|---|---|---|
| 1 | Saxony | 530 | 3.1 | |
| 2 | Bavaria | 524 | 3.3 | Above average |
| 3 | Thuringia | 507 | 3.7 | |
| 4 | Baden-Württemberg | 503 | 2.7 | |
| | **Germany** | **499** | **1.2** | |
| 5 | Brandenburg | 493 | 3.0 | Average |
| 6 | Saxony-Anhalt | 493 | 3.6 | |
| 7 | Lower Saxony | 491 | 4.2 | |
| 8 | Hesse | 491 | 2.8 | |
| 9 | Rhineland-Palatinate | 490 | 3.4 | |
| 10 | North Rhine-Westphalia | 490 | 3.4 | |
| 11 | Hamburg | 488 | 2.8 | |
| 12 | Schleswig Holstein | 486 | 3.8 | Below average |
| 13 | Mecklenburg West-Pomerania | 482 | 3.5 | |
| 14 | Saarland | 481 | 4.0 | |
| 15 | Berlin | 479 | 4.3 | |
| 16 | Bremen | 460 | 4.0 | |

Notes: States are grouped into three categories according to the results of a recent cross-state comparative student test among 9th graders in the subject of math (*IQB Bildungstrend* 2018). Categorization into the three performance groups is based on statistically significant differences (p<0.05) to the German average. Source: Stanat et al. (2019, p. 203).

**Table A1.5:** Individual characteristics and information acquisition in the population survey

| | Dependent variable: Information acquisition via link | | | | | |
|---|---|---|---|---|---|---|
| | All | Above average | Average | Below average | Univ. regressions | Multiv. regression |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Info acquisition ALL ranks | -0.037** (0.018) | 0.016 (0.037) | -0.041 (0.034) | -0.060** (0.026) | -0.037** (0.018) | -0.028 (0.017) |
| Optimist (regarding own state's rank) | | | | | -0.019 (0.018) | -0.006 (0.026) |
| Pessimist (regarding own state's rank) | | | | | 0.011 (0.018) | 0.004 (0.026) |
| Sure about beliefs | | | | | -0.025 (0.018) | -0.014 (0.018) |
| Age | | | | | 0.003*** (0.001) | 0.002** (0.001) |
| Female | | | | | 0.085*** (0.018) | 0.065*** (0.019) |
| Born in Germany | | | | | 0.089** (0.041) | 0.038 (0.039) |
| Monthly household income (€) | | | | | 0.022*** (0.005) | 0.019*** (0.006) |
| Partner in household | | | | | 0.003 (0.018) | -0.033 (0.021) |
| Parent(s) with univ. degree | | | | | -0.031* (0.019) | -0.011 (0.021) |
| Middle school degree | | | | | 0.045*** (0.020) | 0.106*** (0.028) |
| Abitur | | | | | 0.080*** (0.018) | 0.137*** (0.028) |
| University degree | | | | | 0.028 (0.020) | -0.021 (0.026) |
| University student | | | | | 0.172*** (0.043) | 0.142** (0.051) |
| Full-time employed | | | | | -0.062*** (0.018) | -0.096*** (0.025) |
| Part-time employed | | | | | -0.001 (0.024) | -0.077** (0.029) |
| Self-employed | | | | | 0.040 (0.041) | -0.029 (0.044) |
| Unemployed | | | | | -0.144*** (0.050) | -0.109** (0.054) |
| Works in education sector | | | | | -0.130*** (0.028) | -0.106*** (0.028) |
| At least one child $< 18$ | | | | | -0.029* (0.021) | 0.004 (0.024) |
| All children $\geq 18$ | | | | | 0.056*** (0.019) | 0.004 (0.025) |
| CDU supporter | | | | | 0.020 (0.022) | 0.000 (0.021) |
| Frequent voter | | | | | 0.150*** (0.024) | 0.085*** (0.026) |
| Patience (11-point scale) | | | | | -0.017*** (0.004) | -0.020*** (0.004) |
| Risk tolerance (11-point scale) | | | | | 0.025*** (0.004) | 0.025*** (0.004) |
| Constant | 0.615 | 0.604 | 0.602 | 0.627 | | 0.280 |
| Number of observations | 3,451 | 823 | 999 | 1,629 | | 3,433 |
| R-squared | 0.001 | 0.000 | 0.002 | 0.004 | | 0.080 |

Notes: *** p<0.01, ** p<0.05, * p<0.1. OLS regressions. Only including respondents that were randomly allocated to the information *acquisition* experiments. *Info provision ALL ranks* is an experimental treatment in the survey. Dependent variable: Dummy variable 1 = respondents acquired information on their state's student performance rank by clicking on a link, 0 otherwise. Column (5) shows the coefficient estimates of separate univariate regressions for each explanatory variable. Column (6) shows the coefficient estimates of a multivariate regression. Weighted regressions. Survey weights are constructed to match administrative data for the German population regarding age, gender, state, educational attainment, municipality size classes, and employment status. Robust standard errors in parentheses. Data source: ifo Education Survey 2020.

**Table A1.6:** Treatment effects of performance information on citizen satisfaction with state education policy – excluding covariates and state fixed effects

| | Dependent variable: Satisfaction with education policy | | |
|---|---|---|---|
| State's student performance: | Above average | Average | Below average |
| | (1) | (2) | (3) |
| Info provision OWN rank | 0.245*** | 0.173*** | -0.162*** |
| | (0.028) | (0.029) | (0.021) |
| Info provision ALL ranks | 0.267*** | 0.262*** | -0.147*** |
| | (0.028) | (0.028) | (0.021) |
| Info acquisition OWN rank | 0.111*** | 0.122*** | -0.054** |
| | (0.031) | (0.030) | (0.022) |
| Info acquisition ALL ranks | 0.148*** | 0.116*** | -0.024 |
| | (0.030) | (0.029) | (0.022) |
| Control mean | 0.575 | 0.428 | 0.399 |
| Individual controls | no | no | no |
| State fixed effects | no | no | no |
| Number of observations | 2,434 | 3,000 | 4,888 |
| R-squared | 0.052 | 0.035 | 0.020 |

Notes: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. OLS regressions. *Info provision OWN rank*, *Info provision ALL ranks*, *Info acquisition OWN rank*, and *Info acquisition ALL ranks* are experimental treatments in the survey. Dependent variable: Dummy variable 1 = respondents are "very satisfied" or "rather satisfied" with their state's education policy, 0 otherwise. Control mean: mean of the outcome variable in the control group. Weighted regressions. Survey weights are constructed to match administrative data for the German population regarding age, gender, state, educational attainment, municipality size classes, and employment status. Robust standard errors in parentheses. Data source: ifo Education Survey 2020.

**Table A1.7:** Treatment effects of performance information on citizen satisfaction with state education policy – by prior beliefs

| | Dependent variable: Satisfaction with education policy | | | |
|---|---|---|---|---|
| State's student performance: | All | Above average | Average | Below average |
| | (1) | (2) | (3) | (4) |
| Info provision (OWN & ALL ranks) | 0.038 | 0.196*** | 0.121* | -0.172*** |
| | (0.030) | (0.045) | (0.066) | (0.048) |
| Info acquisition (OWN & ALL ranks) | 0.034 | 0.042 | 0.129** | -0.048 |
| | (0.032) | (0.049) | (0.064) | (0.051) |
| Optimist (regarding own state's rank) | 0.114*** | 0.061 | 0.009 | 0.070* |
| | (0.027) | (0.060) | (0.055) | (0.041) |
| Info provision*Optimist | -0.138*** | -0.069 | 0.006 | -0.016 |
| | (0.035) | (0.077) | (0.076) | (0.052) |
| Info acquisition*Optimist | -0.061* | -0.036 | -0.052 | -0.012 |
| | (0.037) | (0.081) | (0.075) | (0.055) |
| Pessimist (regarding own state's rank) | -0.160*** | -0.122*** | -0.158*** | -0.145*** |
| | (0.027) | (0.043) | (0.052) | (0.047) |
| Info provision*Pessimist | 0.192*** | 0.108** | 0.188** | 0.163*** |
| | (0.036) | (0.054) | (0.073) | (0.060) |
| Info acquisition*Pessimist | 0.107*** | 0.142** | 0.026 | 0.097 |
| | (0.037) | (0.058) | (0.072) | (0.064) |
| Control mean | 0.520 | 0.661 | 0.528 | 0.368 |
| Individual controls | yes | yes | yes | yes |
| State fixed effects | yes | yes | yes | yes |
| Number of observations | 10,265 | 2,419 | 2,980 | 4,866 |
| R-squared | 0.164 | 0.133 | 0.100 | 0.093 |

Notes: *** p<0.01, ** p<0.05, * p<0.1. OLS regressions. *Info provision (OWN & ALL ranks)* and *Info acquisition (OWN & ALL ranks)* are (combinations of) experimental treatments in the survey. Dependent variable: Dummy variable 1 = respondents are "very satisfied" or "rather satisfied" with their state's education policy, 0 otherwise. Control mean: mean of the outcome variable in the control group in a regression without covariates. Weighted regressions. Survey weights are constructed to match administrative data for the German population regarding age, gender, state, educational attainment, municipality size classes, and employment status. Robust standard errors in parentheses. Data source: ifo Education Survey 2020.

**Table A1.8:** Treatment effects of performance information on citizen support for the transparency policy – by prior beliefs

| | Dependent variable: Support for comparative tests | | | |
|---|---|---|---|---|
| State's student performance: | All | Above average | Average | Below average |
| | (1) | (2) | (3) | (4) |
| Info provision (OWN & ALL ranks) | 0.056** | 0.054 | 0.121** | 0.029 |
| | (0.027) | (0.044) | (0.054) | (0.044) |
| Info acquisition (OWN & ALL ranks) | -0.022 | 0.010 | -0.036 | -0.031 |
| | (0.029) | (0.045) | (0.060) | (0.047) |
| Optimist (regarding own state's rank) | 0.006 | -0.000 | 0.080* | -0.019 |
| | (0.023) | (0.056) | (0.047) | (0.035) |
| Info provision*Optimist | -0.051 | 0.002 | -0.159** | -0.015 |
| | (0.031) | (0.074) | (0.063) | (0.048) |
| Info acquisition*Optimist | 0.014 | -0.029 | -0.036 | 0.045 |
| | (0.033) | (0.076) | (0.069) | (0.051) |
| Pessimist (regarding own state's rank) | -0.008 | -0.020 | 0.029 | 0.005 |
| | (0.024) | (0.039) | (0.047) | (0.041) |
| Info provision*Pessimist | -0.022 | -0.003 | -0.101* | -0.009 |
| | (0.032) | (0.052) | (0.061) | (0.056) |
| Info acquisition*Pessimist | 0.034 | -0.006 | 0.071 | 0.001 |
| | (0.034) | (0.053) | (0.067) | (0.060) |
| Control mean | 0.779 | 0.781 | 0.743 | 0.800 |
| Individual controls | yes | yes | yes | yes |
| State fixed effects | yes | yes | yes | yes |
| Number of observations | 10,267 | 2,419 | 2,981 | 4,867 |
| R-squared | 0.060 | 0.052 | 0.072 | 0.072 |

Notes: *** p<0.01, ** p<0.05, * p<0.1. OLS regressions. *Info provision (OWN & ALL ranks)* and *Info acquisition (OWN & ALL ranks)* are (combinations of) experimental treatments in the survey. Dependent variable: Dummy variable 1 = respondents are "very in favor" or "rather in favor" of introducing regular comparative student tests, 0 otherwise. Control mean: mean of the outcome variable in the control group in a regression without covariates. Weighted regressions. Survey weights are constructed to match administrative data for the German population regarding age, gender, state, educational attainment, municipality size classes, and employment status. Robust standard errors in parentheses. Data source: ifo Education Survey 2020.

**Table A1.9:** Treatment effects of performance information on citizen support for the transparency policy – excluding covariates and state fixed effects

| | Dependent variable: Support for comparative tests | | |
|---|---|---|---|
| State's student performance: | Above average (1) | Average (2) | Below average (3) |
| Info provision OWN rank | 0.064** | 0.019 | 0.046** |
| | (0.025) | (0.025) | (0.018) |
| Info provision ALL ranks | 0.021 | -0.017 | -0.023 |
| | (0.029) | (0.027) | (0.020) |
| Info acquisition OWN rank | -0.002 | -0.017 | 0.029 |
| | (0.027) | (0.026) | (0.019) |
| Info acquisition ALL ranks | 0.009 | -0.035 | -0.040* |
| | (0.028) | (0.026) | (0.020) |
| Control mean | 0.776 | 0.779 | 0.775 |
| Individual controls | no | no | no |
| State fixed effects | no | no | no |
| Number of observations | 2,434 | 3,001 | 4,889 |
| R-squared | 0.003 | 0.002 | 0.005 |

Notes: *** p<0.01, ** p<0.05, * p<0.1. OLS regressions. *Info provision OWN rank*, *Info provision ALL ranks*, *Info acquisition OWN rank*, and *Info acquisition ALL ranks* are experimental treatments in the survey. Dependent variable: Dummy variable 1 = respondents are "very in favor" or "rather in favor" of introducing regular comparative student tests, 0 otherwise. Control mean: mean of the outcome variable in the control group. Weighted regressions. Survey weights are constructed to match administrative data for the German population regarding age, gender, state, educational attainment, municipality size classes, and employment status. Robust standard errors in parentheses. Data source: ifo Education Survey 2020.

**Table A1.10:** Treatment effects of performance information on citizen support for the transparency policy – ordered probit models

| | Dependent variable: Support for comparative tests (categorical variable) | | |
|---|---|---|---|
| State's student performance: | Above average (1) | Average (2) | Below average (3) |
| Info provision OWN rank | 0.142* | 0.076 | 0.121** |
| | (0.073) | (0.066) | (0.051) |
| Info provision ALL ranks | -0.008 | 0.018 | -0.025 |
| | (0.072) | (0.069) | (0.051) |
| Info acquisition OWN rank | 0.052 | 0.034 | 0.050 |
| | (0.074) | (0.068) | (0.050) |
| Info acquisition ALL ranks | 0.051 | -0.056 | -0.048 |
| | (0.073) | (0.066) | (0.051) |
| Individual controls | yes | yes | yes |
| State fixed effects | yes | yes | yes |
| Number of observations | 2,421 | 2,983 | 4,872 |

Notes: *** p<0.01, ** p<0.05, * p<0.1. Ordered probit regressions. *Info provision OWN rank*, *Info provision ALL ranks*, *Info acquisition OWN rank*, and *Info acquisition ALL ranks* are experimental treatments in the survey. Dependent variable: Categorical variable 1 = "very against", 2 = "rather against", 3 = "neither in favor nor against", 4 = "rather in favor", 5 = "very in favor" of introducing regular comparative student tests. Weighted regressions. Survey weights are constructed to match administrative data for the German population regarding age, gender, state, educational attainment, municipality size classes, and employment status. Robust standard errors in parentheses. Data source: ifo Education Survey 2020.

**Table A1.11:** Treatment effects of performance information on citizen support for the transparency policy – by consequentiality of answer

| | Dependent variable: Support for comparative tests | | | |
|---|---|---|---|---|
| State's student performance: | All | Above average | Average | Below average |
| | (1) | (2) | (3) | (4) |
| Info provision (OWN & ALL ranks) | 0.037** | 0.057* | 0.034 | 0.027 |
| | (0.015) | (0.032) | (0.029) | (0.022) |
| Info acquisition (OWN & ALL ranks) | 0.008 | 0.018 | -0.015 | 0.015 |
| | (0.016) | (0.033) | (0.030) | (0.022) |
| Consequential | 0.044*** | 0.038 | 0.055* | 0.041* |
| | (0.015) | (0.031) | (0.028) | (0.022) |
| Info provision*Consequential | -0.028 | -0.015 | -0.044 | -0.020 |
| | (0.021) | (0.044) | (0.040) | (0.030) |
| Info acquisition*Consequential | -0.021 | -0.032 | 0.005 | -0.027 |
| | (0.022) | (0.045) | (0.041) | (0.031) |
| Control mean | 0.752 | 0.757 | 0.748 | 0.752 |
| Individual controls | yes | yes | yes | yes |
| State fixed effects | yes | yes | yes | yes |
| Number of observations | 10,276 | 2,421 | 2,983 | 4,872 |
| R-squared | 0.061 | 0.053 | 0.070 | 0.072 |

Notes: *** p<0.01, ** p<0.05, * p<0.1. OLS regressions. *Info provision (OWN & ALL ranks)*, *Info acquisition (OWN & ALL ranks)*, and *Consequential* are (combinations of) experimental treatments in the survey. *Consequential* captures whether the respondent was informed about average support rates for the policy by state being communicated to the parliamentarians of the own state or not. Dependent variable: Dummy variable 1 = respondents are "very in favor" or "rather in favor" of introducing regular comparative student tests, 0 otherwise. Control mean: mean of the outcome variable in the control group in a regression without covariates. Weighted regressions. Survey weights are constructed to match administrative data for the German population regarding age, gender, state, educational attainment, municipality size classes, and employment status. Robust standard errors in parentheses. Data source: ifo Education Survey 2020.

**Table A1.12:** Treatment effects of performance information on parliamentarian support for the transparency policy – probit regressions and excluding covariates

| | Dependent variable: Support for comparative tests | | | |
|---|---|---|---|---|
| | (Ordered) probit | | OLS | |
| Dependent variable: | Dummy | 5-point scale | Dummy | 5-point scale |
| | (1) | (2) | (3) | (4) |
| Info provision worse/better half | 0.327 | 0.379** | 0.092 | 0.314* |
| | (0.200) | (0.157) | (0.061) | (0.161) |
| Worse half (ranks 9-16) | 0.498** | 0.578*** | 0.075 | 0.302 |
| | (0.231) | (0.178) | (0.069) | (0.184) |
| Info provision*Worse half | -0.689** | -0.843*** | -0.182* | -0.685*** |
| | (0.317) | (0.251) | (0.097) | (0.264) |
| Individual controls | yes | yes | no | no |
| Party fixed effects | yes | yes | no | no |
| Number of observations | 353 | 353 | 353 | 353 |

Notes: *** p<0.01, ** p<0.05, * p<0.1. (Ordered) probit regressions (columns 1 and 2) and OLS regressions (columns 3 and 4). *Info provision worse/better half* is an experimental treatment in the survey. Dependent variable models (1) and (3): Dummy variable 1 = respondents are "very in favor" or "rather in favor" of introducing regular comparative student tests, 0 otherwise. Dependent variable models (2) and (4): Categorical variable 1 = respondents are "very against" to 5 = "very in favor" of introducing regular comparative student tests. Missing data for three control variables (*Education profession*, *Abitur*, *University degree*) has been imputed based on sample means. Imputation dummies included in models (1) and (2). Robust standard errors in parentheses. Data source: ZEW/University Mannheim parliamentary survey 2020.

**Table A1.13:** Treatment effects of performance information on citizen beliefs about parliamentarians support for the transparency policy – excluding covariates and state fixed effects

| | Dependent variable: Citizen belief about parliamentarian support | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| State's student performance: | Above average | Average | Below average |
| Info provision OWN rank | 6.374*** | 2.604** | -1.065 |
| | (1.528) | (1.257) | (0.998) |
| Info provision ALL ranks | 2.428* | 4.857*** | -0.411 |
| | (1.469) | (1.215) | (0.995) |
| Info acquisition OWN rank | 4.772*** | 1.023 | -0.736 |
| | (1.407) | (1.290) | (0.988) |
| Info acquisition ALL ranks | 7.229*** | 1.537 | -1.008 |
| | (1.442) | (1.246) | (0.983) |
| Control mean | 45.629 | 46.584 | 45.868 |
| Individual controls | no | no | no |
| State fixed effects | no | no | no |
| Number of observations | 2,430 | 2,999 | 4,883 |
| R-squared | 0.017 | 0.006 | 0.000 |

Notes: *** p<0.01, ** p<0.05, * p<0.1. OLS regressions. *Info provision OWN rank*, *Info provision ALL ranks*, *Info acquisition OWN rank*, and *Info acquisition ALL ranks* are experimental treatments in the survey. Dependent variable: Continuous variables ranging from 0 to 100%, capturing the share of parliamentarians "very in favor" or "rather in favor" of the policy proposal to introduce regular comparative student tests from the citizen perspective. Control mean: mean of the outcome variable in the control group. Weighted regressions. Survey weights are constructed to match administrative data for the German population regarding age, gender, state, educational attainment, municipality size classes, and employment status. Robust standard errors in parentheses. Data source: ifo Education Survey 2020.

# Chapter 2

# Local Labor Markets as a Taxable Location Factor? Evidence From a Shock to Foreign Labor Supply

**Abstract.** This paper examines how municipal taxes respond to the local impact of a labor market shock. The analysis exploits a commuting policy that liberalized cross-border labor markets between Switzerland and the EU. The reform was implemented at a time of skilled labor shortages and led to a substantial inflow of cross-border workers into Swiss border municipalities. Identification rests on exogenous regional variation in treatment intensities based on commuting times. The results show that *corporate* tax changes are significantly larger than zero in highly-treated border municipalities after the reform and when compared to less-affected regions. This is consistent with the theory according to which governments can tax rents that arise from productive location factors – an interpretation supported by several model extensions and robustness tests. The results on *personal* income taxation indicate a similar yet smaller and lagged response.

## 2.1 Introduction

Location factors such as public infrastructure, natural resources, or agglomeration economies are critical for firms' performance and location choices (e.g., Devereux et al., 2007; Fisher-Vanden et al., 2015). Simultaneously, contributions from tax competition literature have shown how the presence of such factors of production leads to higher taxes, as they generate taxable corporate rents (e.g., Brülhart et al., 2015). More recently, due to the increased frequency of labor shortages, skilled labor has grown in importance relative to other factors of production (OECD, 2019), and the competitiveness of firms often hinges on their success in the race for skill and talent (e.g., Doms et al., 2010; Crook et al., 2011). Building on these insights from tax and labor market research, I examine whether and, if so, how local tax policies also incorporate changes in the labor supply that benefit firms. To this end, I exploit a regionally-confined labor market reform in the Swiss setting, where skilled labor shortages persist since the 2000s (Kägi et al., 2009).

The study exploits a commuting policy that permanently increased the labor supply in Swiss municipalities close to national borders in order to analyze whether the affected jurisdictions capitalized on the benefits of the reform for local firms. The main hypothesis argues that the labor supply shock boosts the attractiveness of such locales in the spirit of a productive amenity due to the sudden availability of this factor (see Section 2.2). As such, it allows Swiss border municipalities – generally disadvantaged due to their remote location (cf., Redding and Sturm, 2008) – to set higher *corporate* taxes after the reform and relative to more centrally-located municipalities with little access to foreign commuters. In Section 2.2, I also discuss potential adjustments to the municipal component of the *personal* income tax. Due to the presence of arguments for both an increase (e.g., positive spillover effects for domestic workers) and a decrease (e.g., competitive pressure on wages), the *personal* income tax response likely hinges on local perceptions and is essentially an empirical question for which heterogeneous local conditions are studied.

The reform considered in this paper is the *Agreement on the Free Movement of Persons* (AFMP) between Switzerland and the EU from 1999. It eventually led to the liberalization of cross-border labor markets between both economies and, most importantly for this analysis, led to a substantial inflow of cross-border (CB) workers in Switzerland. In particular, the Swiss CB worker share in local 1998 employment increased from about 20% to 30% as a result of the reform in border locations most heavily affected (see Section 2.3).

As a result of this dramatic change in regional labor markets as well as the interaction between location characteristics, firm performance, and local taxes (as suggested by the literature), two questions arise: (i) what are the economic effects of the reform in the affected regions and (ii) how did fiscal policy respond. The first question is addressed in Beerli et al. (2021), who show that, in highly-treated border locations, the share of foreign workers increased by up to 10 percentage points through 2010 (previous restrictions on CB employment were fully abandoned by 2004). The authors also show that about two-thirds of new workers were high-skill, high-wage earners. In a second step, they identify a stimulating effect of the labor supply shock on the size, innovation performance, and productivity of skill-intensive incumbent firms, and even firm creation. Finally, the authors refute arguments about potential negative effects on domestic workers whose employment conditions remained largely unaffected. Furthermore, the real wages of highly-educated nationals even increased by 4.5% in response to the reform as some of them moved to managerial jobs at a higher level.[1]

[1] Few other studies relate to the research on the AFMP: Siegenthaler et al. (2016, p. 53) seek to explain the Swiss "Job Miracle" since the early 2000s which "correlates with a substantial increase in the labor intensity of economic activity", and identify immigration as the key driver. Others descriptively analyze the AFMP's effects on the number and composition of immigrants, their integration into labor markets, and Swiss wage effects (Aeppli et al., 2008; Cueni and Sheldon, 2011; Henneberger and Ziegler, 2011; Abberger et al., 2015). Ramel and Sheldon (2012) study the fiscal incidence and find a positive balance for immigration from the EU-17/EFTA area and a zero balance overall. Von Stokar et al. (2015) summarize that the increase in foreign workers matches a demand by firms and led to almost exclusively positive outcomes for the Swiss economy.

Building on this evidence regarding the direct effects of the AFMP, I focus on the second question and study a more indirect outcome, namely on local taxes. In particular, I track changes in *corporate* and *personal* income tax rates at the municipal level and argue that they are motivated by effects of the commuting policy. In so doing, this study provides first empirical evidence on how politicians internalize the characteristics of local labor markets as a productive force in their policy decisions. The Swiss setting is particularly well-suited, as Swiss municipalities exhibit significant tax autonomy. In addition, the country's political institutions demonstrate exceptionally high levels of direct democracy (e.g., Brülhart and Jametti, 2019) such that population attitudes (e.g., fears about negative wage effects) are often reflected in local policies.

Similar to Dustmann et al. (2017) and Beerli et al. (2021), I use the fact that CB worker employment patterns vary with a location's distance to the border (measured by driving time to the nearest border crossing) due to longer commuting times for more distant regions. Beyond facilitating the identification of causal effects, studying a rise in foreign commuters rather than migration flows has the advantage that results are not driven by foreigners' integration into the political system, the unemployment scheme, or the local society, but instead can be ascribed predominantly to the labor market dimension. This supports a cleaner identification of the labor market effects as compared to a setting where foreigners actually migrate to another country and inevitably alter the political and social fabric of a society (compare, e.g., Burchardi et al., 2020).

Exploiting the as-good-as-random regional variation in treatment intensities in difference-in-differences and event study models supports the identification of three findings: First, in line with expectations, *corporate* income is taxed at a relatively higher rate in highly-treated border municipalities when compared to less affected hinterland jurisdictions. An extensive set of robustness tests and model extensions supports the interpretation of a more attractive economic environment as the crucial driver of this policy response due to the increase in local labor supply and the expansion of the skill mix. Second, I find some evidence of a relative increase, yet smaller and lagged, also for the *personal* income tax. The lagged response is in line with evidence from, for example, Haaland and Roth (2020), who show that labor market concerns in the context of migration are reduced when people learn about actual impact. This relates to the Swiss setting as the commuting policy's impact on local residents was unclear and turned out to be predominantly positive only later. Third, as hypothesized in Section 2.2, local population attitudes play an important role in tax responses: Municipalities where a majority of citizens favored the labor market reform show a particularly strong and robust tax response. This may be taken as evidence for the presence of both local perceptions of negative effects of the AFMP for domestic workers (in places with no significant *personal* income tax response) and optimism about positive spillover effects on Swiss nationals (in locations with a positive tax response). The results on underlying mechanisms must, however, be treated with some caution: Given data restrictions, they are limited to a subsample of Swiss municipalities. For this subsample, the institutional framework prevents a conclusive judgment about whether the response is targeted at firms, households, or both, as the relevant tax parameters are linked in these cases.[2]

The paper is structured as follows: Section 2.2 develops the hypotheses and summarizes the contributions to the extant literature. Section 2.3 details the design of the commuting policy and local taxation in Switzerland, followed by a description of the data and estimation strategy in Section 2.4. The main results, analyses into underlying mechanisms, and robustness tests are discussed in Section 2.5. Section 2.6 concludes.

## 2.2 Theoretical Framework and Contributions to the Literature

In this section, I summarize the relevant findings of the literature to develop my hypotheses about *corporate* and *personal* income tax responses to the commuting policy, and highlight the contributions to the related strands of the literature.

[2]This is due to differences in cantonal (i.e., state) tax laws. The details are discussed in Section 2.3 below.

**Corporate income tax.** Studies from traditional tax competition and New Economic Geography (NEG) literature have explored extensively the determinants that affect local tax levels (see Brülhart et al., 2015, for an overview). These determinants can be categorized into factors that are under the direct influence of politicians such as publicly-provided goods (e.g., infrastructure, public R&D, or capital, labor, and environmental regulations; Pieretti and Zanaj, 2011) and factors that politicians can influence only indirectly (e.g., agglomeration). These studies establish that (endogenous) location characteristics can reduce competitive pressure to set low taxes in order to attract mobile capital or agents.

The way papers model the moderating effect of location factors on tax competition differs somewhat across studies. Zissimos and Wooders (2008), Hindriks et al. (2008), and Pieretti and Zanaj (2011) all develop models where two regions choose a level of public investment to improve firms' productivity and subsequently compete over tax rates in a second stage. Thereafter, Zissimos and Wooders (2008) assume heterogeneous requirements for public goods amongst firms. Sufficient differentiation in this set of goods is further posited to limit firms' incentives to relocate when confronted with high taxes. On the other hand, Pieretti and Zanaj (2011) focus on the case of two unevenly-sized regions and show that, in contrast to the basic tax competition model, the smaller region does not have to set the lower tax to attract investments but can opt instead for higher public investments as long as capital is not perfectly mobile.

Turning to the NEG literature, numerous papers stress the relevance of agglomeration economies, a phenomenon that makes economies "lumpy" as firms benefit from geographic concentration (see, e.g., Duranton and Puga, 2004, for an overview). While agglomeration processes can have various drivers (knowledge spillovers, shared input or output markets, labor matching), Baldwin and Krugman (2004) model them by means of imperfect competition and increasing returns in the production of differentiated varieties of an industrial good. Their model shows that industry will cluster in one region (often called the "core-periphery outcome") and is willing to accept a higher tax due to higher profits in the presence of agglomeration economies. Ludema and Wooton (2000) reach a similar conclusion but rely on a model with a homogeneous good and also consider imperfectly mobile manufacturing labor. Ottaviano and Van Ypersele (2005) focus on the size of the output market as a driver for capital concentration. In a model with mobile capital and *ex ante* asymmetric regions, firms agglomerate in the larger jurisdiction to save on trade costs. This larger region will end up with a more than proportionate capital share and trade costs create a taxable agglomeration rent. Borck and Pflüger (2006) generalize the finding of higher taxes in higher-agglomeration regions to less extreme cases where the economy is only partially-agglomerated. Charlot and Paty (2007), Koh et al. (2013), Jofre-Monseny (2013), Luthi and Schmidheiny (2014), and Brülhart and Simpson (2018) identify empirical evidence for these theoretical predictions in France, Germany, Spain, Switzerland, and the UK.[3]

This analysis contributes to and extends this literature, studying changes in labor endowments as a crucial input factor which may equally limit firms' responsiveness to marginal tax changes. This argument relates particularly to the second strand of literature focusing on location characteristics largely exogenous to policy makers as well as other agglomeration literature identifying labor as a factor which greatly facilitates agglomeration (e.g., as an input factor or a determinant of knowledge spillovers and consumption). In fact, empirical studies commonly rely on some measure of employment (density) to proxy agglomeration (see, e.g., Nakamura and Paul, 2019). Both Rosenthal and Strange (2004) and Combes and Gobillon (2015) clarify in their handbook chapters that empirical estimates relying on employment measures constitute an upper bound for agglomeration economies. This can be attributed to the fact that the concentration of taxable economic factors results not only from agglomeration forces, but also due to exogenous advantages in natural endowments.

In the Swiss setting, the commuting policy gives rise to an exogenous increase in labor supply in all affected border regions. Pursuant to the discussion in the literature above, a positive impact

[3]Empirical evidence to support the argument that agglomeration economies benefit firms is, for example, identified in Brülhart et al. (2012) in the case of Switzerland.

on the *corporate* income tax rate in these highly-treated municipalities is to be expected relative to hinterland regions without access to commuters. Aside from factors stressed by the agglomeration literature,[4] the shortage of (high-)skilled labor among Swiss businesses (see Kägi et al., 2009) and potentially lower wage costs due to increased competition on the labor market (see, e.g., Borjas, 2003; Dustmann et al., 2013) may further increase firms' preferences for a border location and, in turn, lead to higher *corporate* income tax rates in these jurisdictions. Importantly, there is anecdotal evidence to support the hypothesis that the increase in labor supply is considered to be an asset to border regions, both from the perspective of businesses[5] and local politicians[6].

Finally, the assumption of government appetite to tax firms' rents is in line with many tax competition models that assume governments to be revenue or rent (i.e., tax revenue minus public input costs) maximizers (e.g., Zissimos and Wooders, 2008). Borck and Pflüger (2006, p. 651) serve as an emblematic example from the NEG literature, modeling the government objective function as "representing either a government that acts benevolently (i.e. it cares for the tax proceeds in order to provide public goods that raise consumer welfare) or a "Leviathan" government (i.e. one which maximizes the size of the state or its own utility)."[7] What is important to understand is that the empirical analysis identifies *relative* tax changes between regions with different treatment intensities. Thus, a positive tax change can be (and indeed is in some cases) associated with a decline of local tax rates in absolute value. Put differently, taxing parts of the rents from the commuting policy may lead neither to an absolute increase in tax rates nor tax revenue (and, subsequently, public goods provided) but might simply prevent as strong a decline in these parameters relative to municipalities which do not benefit from the additional labor supply.

**Personal income tax.** With respect to the impact of the reform on labor income and in particular *personal* income taxes, conflicting arguments exist which complicate the derivation of a clear hypothesis. Importantly, CB workers' income is *not* taxed at the tax rates set by municipalities, nor do CB workers add significant extra revenue for Swiss municipalities (details see Section 2.3.2). Taking this into account, in the presence of fears and perceptions about (potential) adverse effects on employment conditions or prospects,[8] citizens are likely to oppose a higher tax burden on their part, regardless of benefits from improved public finances, and may even demand a redistribution of rents in the form of a lower *personal* and higher *corporate* income tax. In contrast, if the expected or perceived effects of the commuting policy on domestic workers

[4]For example, a more efficient matching of firms and workers due to the quantitative and qualitative increase in labor supply (e.g., Orefice and Peri, 2020) or performance enhancing intra-firm adjustments in factor intensities (e.g., Dustmann and Glitz, 2015).

[5]Aside from the observed real effects on firms' performance and entrepreneurial behavior identified in Beerli et al. (2021), a survey that was conducted among Swiss businesses during the implementation phase of the reform confirms the high relevance of an access to skilled labor for the Swiss economy (see FEW-HSG, 2008). According to this survey, there is no other location factor more important, but also none that is less under-performing than the availability of skilled labor. The fiscal environment, in contrast, was rated as important but sufficiently attractive.

[6]Among politicians, there is evidence that CB workers are considered a valuable asset for Swiss border regions (Kreis, 2007) and that a withdrawal of the reform would be expected to seriously hamper the climate for investment (see, e.g., Hofmann, 2014; Brutschin, 2014).

[7]Taxing firms' rents is arguably not the only possible course of action for politicians. In case they follow a revenue-targeting strategy, higher firm productivity could already increase tax revenues and create incentives to decrease the *corporate* tax. However, a number of arguments might render this the less likely response: Politicians may have an incentive to raise taxes, e.g., (i) to compensate for their border location that put them at a relative disadvantage in terms of tax base size before the reform when compared with non-border locations with similar public good preferences, (ii) to move closer towards a situation of revenue sufficiency, or (iii) to satisfy the alleged preference of bureaucrats for a larger government along the lines of Niskanen (1975).

[8]Hainmueller and Hiscox (2010) find that people often oppose an increase in foreign workers, independent of the foreigners' skill level which is high among CB workers in Switzerland.

are positive, largely immobile workers might also face, and be willing to accept a relatively higher *personal* income tax. A similar line of reasoning as for *corporate* taxation might therefore apply.[9]

Following Schumpeter's (1918) proposition about the investigation of fiscal policy as being the best way to understand a society and its priorities, I take a more exploratory approach regarding *personal* income tax responses and study local policy responses to learn about how the impact on domestic workers was perceived. The Swiss setting is particularly well-suited as the country's institutions demonstrate an exceptionally high level of direct democracy and residents have substantial power over local policies (Brülhart and Jametti, 2019). To address potentially differing expectations and perceptions surrounding the impact of the reform across regions, I study heterogeneous policy responses with the expectation that relative tax changes are quantitatively more positive in municipalities with higher political support for the policy.

The paper most closely resembling the analysis here is Chevalier et al. (2018). In their working paper, the authors link the inflow of eight million poor migrants to West Germany after WWII – often called *expellees* – to higher taxes on farm and business owners, with no effects on labor income or residential property taxes. However, the setting differs significantly from the commuting scenario studied in this paper as migrants in Chevalier et al. (2018) had full voting rights and benefited from the welfare state. As a result, voting behavior is a critical driver for their results as migrants, who were rarely business owners, increased local support for a more generous welfare state. In addition, migration patterns were likely not exogenous. To the best of my knowledge, the analysis presented here is therefore the first to examine how exogenous changes in local labor supply affect fiscal policy, while being able to largely disregard confounding effects that inevitably accompany migration flows.

## 2.3 Institutional Setting

### 2.3.1 Agreement on the Free Movement of Persons

The reform that is exploited here to study local policy makers' response to an exogenous change in local labor supply is the *Agreement on the Free Movement of Persons* (AFMP) between Switzerland and the EU. It was signed in 1999 as part of a broader bilateral agreement (summarized under the term *Bilaterale I*) with the goal to link the Swiss economy closer to EU markets.

The major innovations of the AFMP are summarized in Table 2.1. Its ultimate outcome was the free mobility of workers amongst signatory countries. Following Beerli et al. (2021), the table differentiates between three phases: (i) pre-reform, (ii) transition, and (iii) post-reform. In addition to the differentiation along the time dimension, Table 2.1 distinguishes between legal innovations for CB worker employment, the focus of this study, and changes for resident immigrants. For the former, a further subdivision is required as the legal innovations differed for CB workers employed in the border region (BR) and the non-border region (NBR). Details of this regional component of the policy are discussed below.

The focus in this paper is on CB workers; people residing in one of the neighboring countries (France, Italy, Austria, Liechtenstein, Germany) and who commute to their workplace in Switzerland on a regular basis. Before the reform, employment of such workers was heavily regulated by law and limited to the officially defined border region as depicted in Figure 2.1. This circumstance led to a region-dependent implementation, in addition to the step-wise implementation as discussed next.

As illustrated in Table 2.1, firms with a location in the non-border region were subject to an employment ban for CB workers that was only lifted in 2007. In the border region, however, employment restrictions for these workers were gradually relaxed after the AFMP was enacted

[9] Another possible channel for a relative increase in *personal* income taxes is modeled in the theoretical contribution of Andersson and Forslid (2003). They consider an NEG model with mobile skilled workers and immobile unskilled workers. In the presence of trade costs, firms in the growing (i.e., agglomerating) region experience a cut in trade costs such that prices shrink and real wages rise, increasing the attractiveness of the region for additional mobile workers.

**Table 2.1:** Legal innovations for the employment of foreigners in Switzerland

| | | | Cross-border (CB) workers | | Immigrants |
|---|---|---|---|---|---|
| Phase | Year | Event | Border region (BR) | NBR | Entire country |
| Pre-reform | 1995 | | Admission process (priority rule) and further restrictions | Employment ban by federal law | Admission process, annual quotas, and further restrictions |
| | 1996 | | | | |
| | 1997 | | | | |
| | 1998 | Announcement | | | |
| Transition | 1999 | AFMP signed | Anticipation & early effects | | |
| | 2000 | Referendum | | | |
| | 2001 | | | | |
| | 2002 | AFMP enacted | Abolition of some restrictions | | Higher quotas, further changes[1] |
| | 2003 | | | | |
| Post-reform (Free movement) | 2004 | Liberalization in the BR | Free | | Abolition of admission process |
| | 2005 | | | | |
| | 2006 | | | | |
| | 2007 | Full liberalization | | Free | Free |
| | 2008 | | | | |

Notes: Based on Beerli et al. (2021). Columns BR and NBR document changes in employment regulations in the border region and non-border region (details see text). A darker gray denotes periods of more restrictive legislation.
[1] Extended duration of some residency permits and admittance of family reunion for the majority of permit holders.

in June 2002. Beerli et al. (2021) describe this as two-step process. During the transition phase, starting from 1999, cantonal offices which handled the application process of CB workers, could do so largely at their discretion. Then, in 2002, some of the restrictions were lifted.[10] Full liberalization was achieved in 2004 when the lengthy and costly admission process was abandoned. Most importantly, Swiss firms no longer had to prove that they did not find an equally-qualified domestic worker for a vacancy (the so-called *priority rule*). From an economic perspective, this converted CB workers from complements to Swiss workers into potential substitutes.[11]

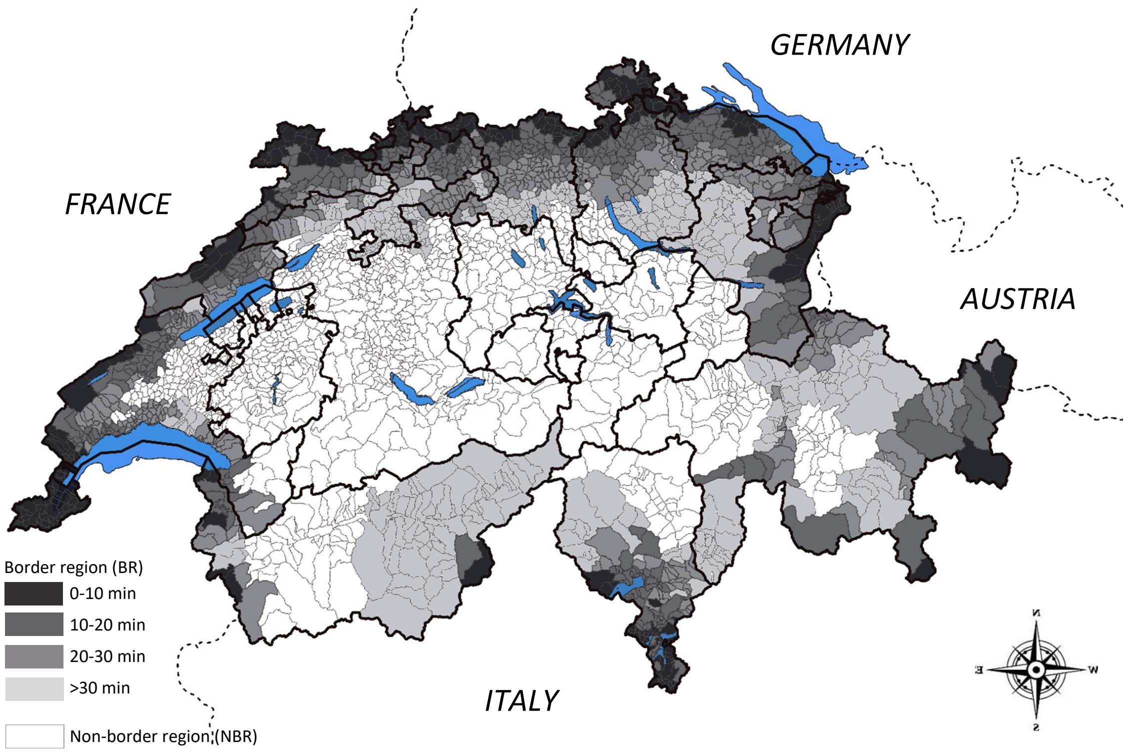
These changes remained a privilege for firms located in the official border region.[12] Figure 2.1 maps all municipalities in the border region in gray. The different gray scales indicate the driving time to the nearest border crossing and define the treatment groups for the empirical analysis. Municipalities close to the border are most significantly affected by the commuting policy as

[10] First, firms could now also hire CB workers from countries other than the Swiss neighbors and living farther away from the border. Second, CB workers no longer had to prove that they had lived in one of the neighboring countries for at least six months. Third, CB worker permits were now valid for more than a year and were not automatically invalidated if an employment contract ended. Fourth, CB workers were only required to commute to their workplace on a weekly instead of a daily basis and could therefore rent an apartment in Switzerland.

[11] Note that restrictions were also lifted for Swiss CB workers seeking employment in EU member states. Thus, the reform could also lead to a reduction in labor supply on the Swiss side if Swiss residents were to seek employment abroad as a result of the reform. However, "the change in employment of CBW [CB workers] in Switzerland was about nine times larger than the change of CBW from Switzerland working in neighboring countries" (Beerli et al., 2021, p. 984), so that the access of foreign workers to the Swiss labor market was the main outcome of the reform.

[12] These regions were defined in four bilateral agreements between Switzerland and Austria (1973), France (1946), Germany (1970), and Italy (1928). The geographical definition of the border region was unchanged by the reform. Importantly, frontiers of the border region do not always coincide with cantonal borders, nor do they follow religious or cultural patterns.

**Figure 2.1:** Boundaries of Swiss border regions according to bilateral agreements




Notes: Categorization of municipalities in the border region into treatment groups by bins of 10 minutes driving time. Driving time measures workforce-weighted distances of all establishments in a municipality to the nearest border crossing. Data comes from Beerli et al. (2021) and relates to the road infrastructure in 2010 (Henneberger and Ziegler, 2011). Thicker black lines denote cantonal borders. National borders from neighboring countries are depicted as dashed lines.

proximity to their location of residence heavily determines CB workers' willingness to commute (see Figure 2.2 below). The sample consisting of all municipalities located in the border region therefore lends itself to the analysis of reform effects and is the focus of this study. Identification rests on the differentiation between municipalities with varying treatment intensities, based on driving time to national borders.

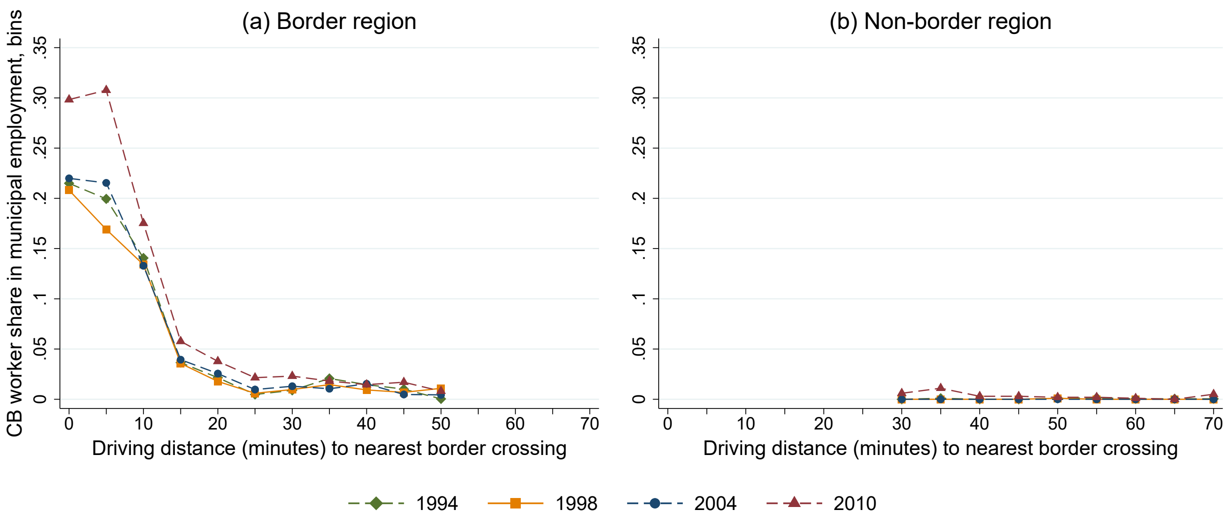
Finally, as shown by the last column of Table 2.1, restrictions for resident immigrants were also loosened step-by-step. Unlike CB workers, migrants were not bound to certain regions and distance to the border has no direct effect on immigrants' choice of location. In addition, the timing of the new rules was different for CB workers and immigrants, implying that a potential effect of new immigrant workers would become visible in a year-by-year analysis.[13] Lastly, as detailed in Section 2.3.2, newly-arriving immigrants are not subject to the normal tax schedule in the first years and therefore have no direct impact on the tax bases relevant for the analysis of this paper. I am thus confident that legal changes for immigrants have no relevant confounding effect on the results.

### 2.3.2 Municipal Taxation in Switzerland

Switzerland is a highly-decentralized country with three major government layers: the federal state, 26 cantons (states), and roughly 2,865 municipalities (as of 2002). The cantons have full authority over the legal framework for cantonal and municipal taxes. They grant municipalities

[13]Beerli et al. (2021) show that, somewhat contrary to expectations, the increase in CB workers led to some crowding-in of immigrants in Swiss border municipalities that started in 2008. This timing is in line with the repeal of restrictions for this group that took place in 2007. Importantly, results of the main analysis, which ends in 2008, do not seem to be affected by this circumstance.

**Figure 2.2:** Increase of CB worker employment by distance to the border




Notes: Number of CB workers relative to total employment in 1998 at the municipal level, shown separately for the border region (Panel a) and non-border region (Panel b). Municipalities grouped into bins of 5min driving distance to the nearest border crossing. Bins with very few workers are omitted. Source: based on Beerli et al. (2021).

substantial freedom to tax local residents and economic activity such that own taxes constitute about 60% of total municipal revenue. The most important taxes in terms of revenue are those on *personal* and *corporate* income.

With a few exceptions, municipalities do not decide on the whole tax scheme but instead set a multiplier which shifts the (progressive) cantonal tax scheme. The amount of taxes to be paid to a municipality is thus determined as a multiple of the basic statutory tax rate. This is particularly convenient, as municipal tax policies are therefore reduced to a single instrument: the tax multiplier.[14]

Importantly, the definitions for various tax bases are not only identical within but also across cantons. This is the outcome of a federal law implemented in 1993. It harmonized the procedural details as the information required to determine the national tax base is drawn from cantonal records (Brülhart and Jametti, 2006).

**Corporate income tax.** In 12 of the 18 border-region cantons, municipalities can levy their own *corporate* tax (see columns 4 and 5 of Table 2.2). These include a tax on corporate profit/income and (equity) capital. The latter is, however, largely superfluous, as it is small and firms can in most cases deduct it from the income tax. The decisive local tax parameter for legal persons, including the most common legal forms like stock companies, limited liability companies, and cooperatives, is therefore the *corporate* income tax multiplier (c.f., Krapf and Staubli, 2020).[15] It is determined on a yearly basis by local policymakers.

**Personal income tax.** Municipalities can set a tax on *personal* income of those residents registered in the jurisdiction. The respective multipliers also apply to taxation of private wealth, but revenue from this tax base is small.[16] Personal income is taxed by all Swiss municipalities.

[14] An exception is the canton of Basel-Stadt, where the tax rate of the three municipalities is included in the cantonal tax rate. I therefore drop this canton for the analysis. Due to data availability issues, I also drop all municipalities from the canton Neuchâtel. Finally, I exclude one municipality which changed affiliation from the canton of Bern to Jura in 1996.

[15] The canton of Basel-Landschaft is an exception in this regard as municipalities in this canton levy a simple tax in the range of 2-5% uniformly on corporate income (without any progressivity) instead of specifying a multiplier.

[16] I ignore church taxes for both *private* and *corporate* income, as the focus is on politicians' response, who do not decide on church taxes.

**Table 2.2:** Municipal taxes in Swiss border-region cantons

| Canton | Munici-palities (in BR) | Tax on personal income | Tax on personal & corporate income: same multiplier | Tax on personal & corporate income: separate multipliers |
|---|---|---|---|---|
| **type 1** (35 % of BR observations) | | | | |
| Aargau | 197 | x | | |
| Graubünden | 106 | x | | |
| St. Gallen | 81 | x | | |
| Valais | 81 | x | | |
| Appenzell-Ausserrhoden | 20 | x | until 2000 | |
| **type 2** (49% of BR observations) | | | | |
| Appenzell-Innerrhoden | 6 | x | until 2006 | |
| Bern | 65 | x | x | |
| Genève | 45 | x | x | |
| Jura | 59 | x | x | |
| Thurgau | 63 | x | x | |
| Ticino | 118 | x | x | |
| Vaud | 182 | x | x | |
| Zürich | 145 | x | x | |
| **type 3** (16% of BR observations) | | | | |
| Schaffhausen | 25 | x | until 2003 | since 2004 |
| Basel-Landschaft | 73 | x | | x |
| Solothurn | 119 | x | | x |
| Total | 1,385 | | | |

Notes: 18 of 26 Swiss cantons officially belong to the border region. Most (eleven) are entirely in the border region. The territory of the remaining seven belongs only partly to the border region. Information displayed in the table is taken from cantonal tax laws. The number of municipalities corresponds to the number of observations in the border-region sample. The cantons Basel-Stadt and Neuchâtel are excluded due to data issues (see Footnote 14).

However, as the last three columns of Table 2.2 show, differences in cantonal laws led to the emergence of three municipality/canton types. Municipalities of *type 1* can only tax *personal* income,[17] whereas *type 2* and *type 3* municipalities levy a local tax on both *personal* and *corporate* income. These latter municipalities differ in the flexibility granted to them to set different rates for *personal* and *corporate* income, with the latter able to set different rates for each. The empirical analysis exploits the existence of these three tax regimes to shed some light on considerations regarding local tax policies in this two tax instrument environment.[18]

**Tax on foreigners.** Foreigners – including individuals who do not hold a permanent residence permit or who are not married to a Swiss national – are not subject to the aforementioned *ordinary tax scheme* but are taxed instead *at the source* (Quellensteuer). Only after living in Switzerland for at least five years can foreigners can be granted a permanent residence permit, with which they are taken up into the *ordinary tax scheme*. Contrary to the *ordinary tax scheme*, there is no

[17]In these cases, corporate taxes are either collected at the canton level and municipalities are allocated a share of the revenue or the tax is collected by municipalities but a uniform rate applies that is set at the canton level. Thus, there is no variation in taxes across municipalities.

[18]The two small cantons Appenzell-Ausserrhoden and Appenzell-Innerrhoden changed their tax regime during the considered period (see Table 2.2). I therefore exclude both in the main analyses and only consider them in robustness checks. This also applies to Schaffhausen.

variation in *source taxes* across municipalities, as it is based on a weighted average of the previous year's municipal tax multipliers (details see Schmidheiny and Slotwinski, 2018). The municipal *personal* income tax therefore only applies to local residents.

**Tax on CB workers.** For CB workers, a *special taxation scheme* exists, based on double taxation agreements between Switzerland and its neighbors. These treaties remained unchanged by the commuting policy. Depending on where they are registered, CB workers are taxed in their country of residence, Switzerland, or both. The crucial commonality is that when taxes in Switzerland apply, no heterogeneity in terms of tax rates exists among municipalities. Hence, they cannot directly incentivize CB workers to (not) seek employment in their jurisdiction.

Despite having no say in the taxation of CB workers, municipalities do benefit to some extent from the CB worker tax as they receive a share of the fiscal revenue. The allocated amount is significantly smaller compared to the revenue from a wage earner taxed in the *ordinary tax scheme*. The details of the double taxation agreements are summarized in Appendix Table A2.1.[19]

## 2.4 Data & Estimation Strategy

The main variables of interest are the municipal tax multipliers for legal and private persons between 1995 and 2008. Data on *personal* income tax multipliers comes from Parchet (2019). To collect information on *corporate* tax multipliers, I relied on both official cantonal websites and statistical yearbooks, or, where necessary, I contacted the cantonal tax authorities.

For municipality background characteristics I also rely on data from Parchet (2019). This data is available for jurisdictions that did not dissolve due to municipal mergers during the period considered (i.e., 1995–2008). The final sample is therefore balanced and includes 1,385 municipalities located in the Swiss border region, which results in 19,390 municipality-year observations.

### 2.4.1 Descriptive Statistics

Intra-national variation in tax rates is exceptionally large across Swiss municipalities. In 2005, combined cantonal and municipal tax rates for the average firm in cantonal capitals differed by a factor of almost four with a maximum tax rate of 23.5% in Genève and the smallest tax rate in Zug (6.4%) (Bacher and Brülhart, 2013). Variation in the combined *personal* income tax rate is similarly high.[20] As shown in Figure 2.3, there is also considerable variation over time in the tax multipliers, which is crucial to identify the fiscal response to the labor market reform studied here.

Figure 2.3 plots yearly averages of tax multipliers by distance group and by tax base. The numbers are residualized based on a canton-year interaction to take into account canton-specific regulations and changes thereof. According to the figure, in the pre-reform phase (before 1999), municipalities with a shorter distance to the border exhibit lower taxes but followed a similar trend as jurisdictions in greater distance of national borders. These lower tax levels are in line with results from the literature that show how the remote location of border regions negatively affects their economic performance and fiscal capacity as they are partially cut off from neighboring markets (e.g., Niebuhr and Stiller, 2004; Redding and Sturm, 2008).[21] Already the descriptive figures suggest a convergence of local tax multipliers in the course of the transition and post-reform phase.

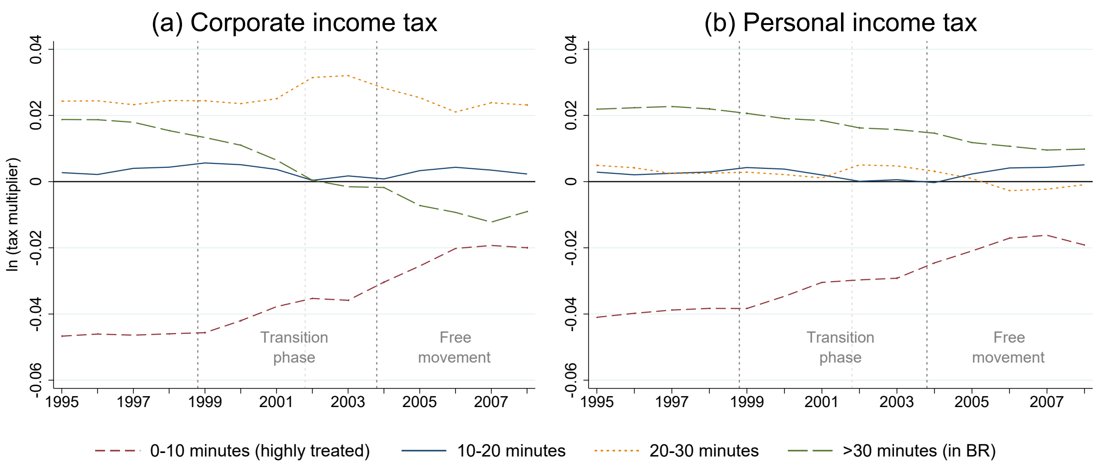
---

[19] In the context of the commuting policy, an additional CB worker category was created. These are foreigners who commute only on a weekly rather than a daily basis (this was not permitted before the reform). As this does not match the definition of a CB worker as set out in the double taxation agreements, these (very few) workers are taxed *at the source* like any other resident immigrant.

[20] Cantonal taxes account for a somewhat larger share of the combined tax rate. From the respective *corporate* (*personal*) income tax revenue, cantons received 64% (60%) in 2010.

[21] Border regions are believed to be at a disadvantage due to smaller markets, longer distances, less diversity, and a limited labor supply – all due to their regional remoteness.

**Figure 2.3:** Average of residualized tax multipliers, by distance group




Notes: Yearly average of residualized tax multipliers (in logs), separately for both tax bases. Residualization: Canton-specific trends as well as details of the different legal tax systems are taken into account by regressing tax multipliers on canton-year fixed effects.

A development not visible in Figure 2.3 but documented in the results section, is the general downward trend observable for Swiss tax multipliers over the considered period.[22] This decline is due to at least two factors. The first is a mechanical effect and relates to the fact that municipalities set a multiplier that shifts the respective canton's tax scheme rather than directly setting a tax rate (see Section 2.3.2). In those cases where a canton adjusts its tax scheme, municipalities also need to adjust their tax multiplier should they want to keep taxes constant. A second factor is the economic upsurge in Switzerland during the considered period, especially in the 2000s, which, on average, even led to slightly increasing tax revenues despite the decline in tax rates (e.g., Pittet, 2014). As already noted above, with this downward trend, it is particularly important that any identified tax effect is interpreted in *relative* terms as a difference in policy responses between municipalities with varying treatment intensities, and does not necessarily correspond to an actual, for example, tax increase.

A crucial requirement for the analysis is the comparability of municipalities in the specified distance groups with respect to factors other than exposure to the labor market reform (i.e., distance to the border). For this purpose, the main analysis excludes locations with a driving time of more than 30 minutes to the border. This assures that I focus on locations that exhibit more similar background characteristics. As a robustness check, I also run regressions including more distant locations. Hence, the control group consists either of municipalities in the distance group *20-30 minutes* (in the main analysis) or *>30 minutes (in BR)* (in a robustness check). In both cases, only municipalities in the border region are included in the analysis to ensure that the same legal rules apply (see Section 2.3.1). The direct effects of the commuting policy are very limited for both distance groups, as they experienced almost no change in commuter flows in response to the labor market reform (see also Figure 2.2) and both are likely to be affected only indirectly by spillover effects.

To evaluate the comparability of the main distance groups, Table 2.3 reports summary statistics for a number of municipality background characteristics and migration-related variables by distance category. The last four columns document differences between the subgroups. Where statistically significant, most differences either reflect characteristics of the local geography (i.e., more lakes but lower altitudes closer to the borders) or are relatively small in terms of absolute

[22]The methodology of residualizing the tax multipliers to control for canton-specific trends and institutional differences that would otherwise bias the cross-canton perspective taken in Figure 2.3 eliminates such general trends.

and mean deviations such that I do not expect them to affect my conclusions. Yet, some differences are larger (in particular employment in the primary sector) and need to be controlled for by including municipality fixed effects as well as all time-varying background characteristics from Table 2.3 as covariates in the regressions.

### 2.4.2 Estimation & Identification

As the number of CB workers employed in a given location is likely endogenous to local tax rates,[23] any coefficient estimate based on this explanatory variable will be biased (e.g., Koethenbuerger, 2014). The categorization of municipalities based on their driving distance to the border and, hence, treatment intensity, supports the estimation of causal effects by using event study models and a difference-in-differences (DiD) design. A crucial requirement for this approach is that municipalities closer to the border are more heavily exposed to the reform than more centrally-located ones. This pattern, which emerges due to a limited willingness of CB workers to commute long distances, is clearly confirmed by the results of Beerli et al. (2021) and is descriptively documented in Figure 2.2 above.[24]

The outlined identification strategy is operationalized by comparing tax policy changes in municipalities close to the border with those of more distant jurisdictions. Specifically, I estimate the following event study model:

$$\begin{aligned} ln(mult_{i,t}) = & \sum_{t=1995}^{2008} \delta_{d_1,t} \times \mathbb{1}\{year = t\} \times \mathbb{1}\{0 \leq d_i \leq 10\} + \alpha_i + \alpha_{c,t} + \\ & \sum_{t=1995}^{2008} \delta_{d_2,t} \times \mathbb{1}\{year = t\} \times \mathbb{1}\{10 < d_i \leq 20\} + \theta' \mathbf{X}_{i,t} + \varepsilon_{i,t}, \end{aligned} \tag{2.1}$$

where the dependent variable is the municipal tax multiplier, either for *corporate* or *personal* income. The coefficients $\delta_{d_1,t}$ and $\delta_{d_2,t}$ capture the reform effects for highly-treated (0-10min) and slightly-treated (10-20min) municipalities for $t \geq 1999$, and should be zero for the pre-treatment years. Throughout the analysis, the year 1998 is used as a baseline by excluding it in the sum operator.

Unobserved determinants of local tax policies are controlled for by the municipality fixed effects $\alpha_i$, whereas $\alpha_{c,t}$ captures differences in cantonal institutions as well as canton-specific trends. Finally, $\mathbf{X}$ includes municipal-level variables to control for compositional changes. These include all time-varying background characteristics listed in Table 2.3 above. The migration-related characteristics from the table are excluded from the regressions as they are likely to be endogenous. Standard errors are clustered at the level of municipalities.[25]

The crucial identifying assumption is the standard parallel trends assumption in DiD models: within municipalities and conditional on controls, one would have observed the same average change in tax policies in the three specified distance groups, absent the commuting policy. The event study model presents an important way to assess this assumption as it supports a comparison of the relative changes in tax multipliers in the distance groups *before* treatment.

To get a better idea of the overall magnitude of the policy response, I estimate a second model with a DiD design to determine the average change in taxes for the transition phase (1999–2003) and post-reform phase (≥2004) rather than estimating yearly changes. For this purpose, the following model specification is used, where the pre-reform years (<1999) represent the baseline:

[23] For example by means of firms' location decisions and their demand for labor.

[24] Note that the distance variable only captures CB workers' driving time on the Swiss side of the border and therefore does not document their total commuting time.

[25] The results are robust to using two-way clustering, by municipality and by year, which allows for arbitrary correlations between observations of the same municipality or year. Results are available upon request.

**Table 2.3:** Summary statistics by distance group

| Variable | 0-10 min | 10-20 min | 20-30 min | (0-10) − (20-30) | | (10-20) − (20-30) | |
|---|---|---|---|---|---|---|---|
| | | | | absolute deviation | mean deviation | absolute deviation | mean deviation |
| **Background characteristics** | | | | | | | |
| Population (in 1,000) | 3.835 | 2.320 | 4.240 | -0.404 | -9.95% | -1.920* | -58.29% |
| % Young ($\leq$ 20) | 18.66 | 19.61 | 19.16 | -0.503* | -2.66% | 0.449* | 2.32% |
| % Old ($\geq$ 80) | 3.086 | 3.153 | 3.010 | 0.075 | 2.46% | 0.143 | 4.64% |
| % Primary sector | 6.309 | 10.58 | 8.506 | -2.197*** | -29.13% | 2.073*** | 21.76% |
| % Secondary sector | 29.70 | 31.09 | 31.82 | -2.116** | -6.85% | -0.723 | -2.30% |
| % Tertiary sector | 3.990 | 58.33 | 59.68 | 4.313*** | 7.01% | -1.350 | -2.29% |
| Unemployment rate | 2.317 | 1.830 | 1.791 | 0.525*** | 25.96% | 0.038 | 2.10% |
| Total employment (per capita) | 0.317 | 0.273 | 0.310 | 0.007 | 2.24% | -0.037* | -12.67% |
| % Votes for left-of-center parties (nat. elections) | 22.56 | 20.72 | 21.65 | 0.913 | 4.14% | -0.930 | -4.39% |
| Urban area | 0.622 | 0.445 | 0.518 | 0.104*** | 18.44% | -0.073* | -15.11% |
| Center of urban area | 0.027 | 0.028 | 0.042 | -0.015 | -42.86% | -0.014 | -40.00% |
| Tourist destination | 0.031 | 0.040 | 0.036 | -0.005 | -15.15% | 0.004 | 10.53% |
| No. of movie theaters within 10km | 6.332 | 3.071 | 4.473 | 1.859** | 35.16% | -1.403*** | -37.10% |
| Lake shore | 0.198 | 0.117 | 0.089 | 0.109*** | 79.56% | 0.027 | 26.21% |
| Altitude (m.a.s.l.) | 441.47 | 520.83 | 556.66 | -115.19*** | -22.76% | -35.83** | -6.65% |
| Productive area ($km^2$) | 430.65 | 460.59 | 470.05 | -39.39 | -8.70% | -9.454 | -2.03% |
| **Migration-related characteristics** | | | | | | | |
| % Foreign nationals | 16.46 | 12.47 | 13.37 | 3.089*** | 20.98% | -0.897 | -6.94% |
| Per capita income in top 10 percentile | 0.174 | 0.092 | 0.147 | 0.027 | 16.98% | -0.055** | -45.45% |
| Per capita income in bottom 50 percentile | 0.330 | 0.492 | 0.396 | -0.066* | -17.98% | 0.096** | 21.72% |
| Gini index | 0.363 | 0.338 | 0.329 | 0.034*** | 9.88% | 0.009** | 2.69% |
| % High education | 0.270 | 16.748 | 16.49 | 3.783*** | 20.85% | 0.261 | 1.48% |
| % Intermediate education | 75.85 | 79.21 | 79.21 | -3.364*** | -4.33% | -0.006 | -0.01% |
| % No education | 3.881 | 4.045 | 4.300 | -0.420*** | -10.20% | -0.255* | -6.11% |
| Number of municipalities | 262 | 326 | 336 | | | | |

Notes: *** p<0.01, ** p<0.05, * p<0.1. Summary statistics correspond to the pre-reform period (1995–1998). The distance group 20-30 minutes functions as the comparison group in the last four columns. Cantons that switched between tax regimes (see Table 2.2) are excluded. Standard errors clustered by municipality.

$$
\begin{aligned}
ln(mult_{i,t}) = {} & \beta^T_{d_1}\left[Transition_t \times \mathbb{1}\{0 \leq d_i \leq 10\}\right] + \beta^F_{d_1}\left[Post_t \times \mathbb{1}\{0 \leq d_i \leq 10\}\right] + \\
& \beta^T_{d_2}\left[Transition_t \times \mathbb{1}\{10 < d_i \leq 20\}\right] + \beta^F_{d_2}\left[Post_t \times \mathbb{1}\{10 < d_i \leq 20\}\right] \\
& + \theta' \mathbf{X}_{i,t} + \alpha_i + \alpha_{c,t} + \varepsilon_{i,t}.
\end{aligned} \tag{2.2}
$$

Before turning to the results, a clarifying note concerning the interpretation of the estimation results for both models shall be reiterated here. As discussed in Section 2.2, municipalities set their taxes in an environment where they compete for mobile factors by offering attractive bundles of location characteristics, public goods, and taxes. Any positive or negative deviation in local taxes from the general (downward) trend should therefore not be interpreted as a unilateral change in tax policies of municipalities close to the border. Instead, the effects might partly also be driven by changes in tax setting behavior of more centrally-located municipalities which forgo the opportunity to benefit from the improvements in labor supply and could lower their taxes to remain attractive. The outlined estimation approach simply focuses on *relative* differences between municipalities' tax parameters as the outcome of this interactive tax setting process.

## 2.5 Results

Section 2.5.1 presents the main results on how municipalities respond to the free mobility of CB workers and analyzes changes in their *corporate* and *personal* income tax. Section 2.5.2 covers four extensions to the analysis to shed light on the underlying mechanisms. The robustness of the results is discussed in Section 2.5.3.

### 2.5.1 Main Results

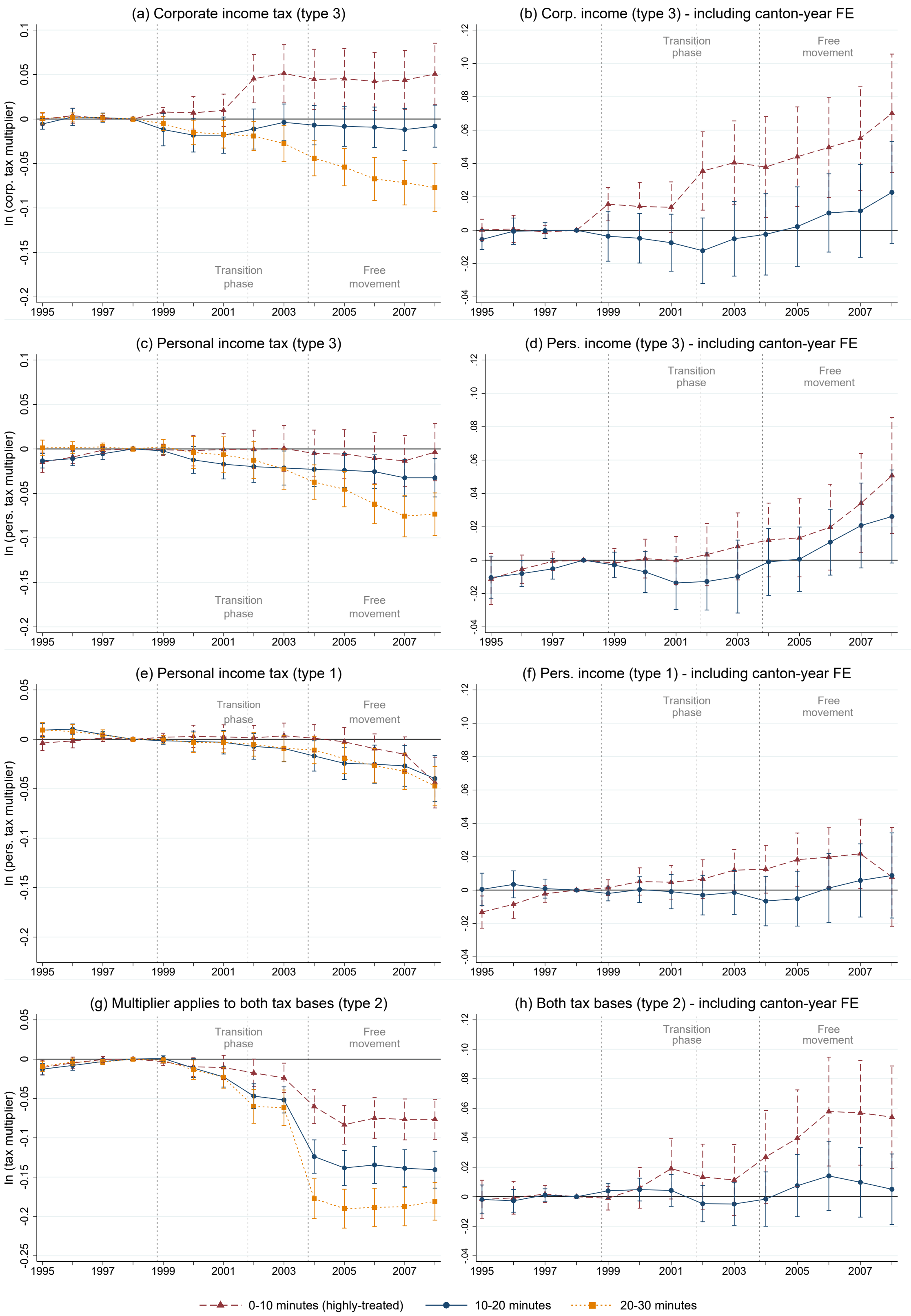
The main results cover the heterogeneous response of municipalities, distinguishing amongst them by their tax base and legal setup due to differing discretion afforded to municipalities in their setting of *corporate* and *personal* income taxes (details see Section 2.3.2). I therefore follow a sample split strategy and separately discuss policy responses in: (i) the *corporate* income tax (*type 3*), (ii) the *personal* income tax (*types 1* and *3*), and (iii) municipalities where the tax applies to both income groups simultaneously (*type 2*).

Figure 2.4 shows the yearly estimates of the event study models and presents results with both excluded (left) and included (right) year fixed effects to also illustrate the downward trend of tax multipliers (see discussion in Section 2.4.1). In the second case (right), municipalities in the 20-30 minutes distance group represent the control group, whereas in the first case (left) there is no control group. In light of the overall tax decline, the research question is essentially whether policymakers in Swiss border regions follow this declining trend or adopt a less aggressive tax policy where local conditions permit.

**Corporate income tax.** Panels (a) and (b) of Figure 2.4 present the response in *corporate* tax multipliers. The underlying sample includes only the most flexible municipalities that can set different multipliers for both tax bases (*type 3*). Independent of excluding (Panel a) or including (Panel b) year fixed effects, the graphs show a constant co-movement in the tax during the pre-reform phase across distance groups. This supports the identifying assumption of parallel trends. The transition phase then shows first differences in the evolution of tax multipliers between the distance groups with coefficients turning statistically significant from 2002 onward when the first employment restrictions were lifted. In line with expectations, this relative increase in *corporate* taxes is positive, largest for municipalities closest to the border, and largest after 2004 when cross-border employment was completely liberalized. This is evidence in favor of the hypothesis that the labor supply shock constitutes a favorable and taxable location factor.

**Personal income tax.** The results on policy responses in the *personal* income tax initially appear mixed. The analysis looks at two different samples: Panels (c) and (d) focus on the same sample of municipalities as above (i.e., *type 3*) but concentrate on their *personal* income tax multi-

**Figure 2.4:** Tax policy response by tax base




Notes: Coefficient estimates and 95% confidence intervals for municipalities in the distance groups with 0-10min, 10-20min, and 20-30min driving time to the nearest border crossing. Results correspond to Equation (2.1). Municipality controls as well as municipality and canton-year (right) fixed effects (FE) included. Standard errors clustered by municipality.

plier, whereas panels (e) and (f) focus on the group of least flexible municipalities (i.e., *type 1*) which can only tax *personal* income at their individual rate.

For the former sample of *type 3* municipalities, no significant pre-trends are observed and the results show a similar policy response in the *personal* income tax as identified for the *corporate* tax above. However, the response is lagged by about five years and smaller in magnitude. The lagged response is in line with evidence from, for example, Haaland and Roth (2020) who show that labor market concerns in the context of migration are reduced when people learn about actual impact. This relates to the Swiss setting as the commuting policy's impact on local residents was unclear and turned out to be predominantly positive only later. The estimates for the latter sample (i.e., *type 1*) in panels (e) and (f) cannot confirm this finding and are problematic due to the presence of significant pre-trends. Putting this fact aside for a moment, the observed patterns suggest that taxes in highly-treated border municipalities have not evolved differently than in less-affected municipalities. To make sense of this result, Figure A2.1 in the Appendix offers additional evidence and shows that the sample of municipalities considered in panels (e) and (f) of Figure 2.4 happens to be almost entirely unaffected by the commuting policy insofar as municipalities close to the border did not experience an increase in CB worker numbers.[26] This is line with official statistics which document that CB worker employment is largely concentrated along the French and German border (as well as the canton of Ticino at the Italian border, see BFS, 2023) where few *type 1* municipalities are located.[27] It also explains why Figure 2.4 shows no meaningful pattern for this small subsample and I abstain from including it in the further analysis.[28]

**Combined tax multiplier.** Policymakers in the final sample of municipalities (i.e., *type 2*) face a complex task as they can set only one multiplier that applies to both tax bases. Thus, a balance must be struck that takes into account anticipated effects of a change in the tax parameter on revenue from both sources. Panels (g) and (h) of Figure 2.4 confirm that all distance groups in this sample followed a common trend in the pre-reform phase. Starting in the transition phase, and even more so in the post-reform phase, municipalities closer to the border set higher taxes than less-affected municipalities farther away from the border. Compared to the evidence on *corporate* income taxation discussed above, the policy response from this sample of municipalities is quite similar. However, the first significant differences occurred about three years later and are slightly smaller – a pattern also observed for the *personal* income tax (see Panels (c) and (d) in Figure 2.4), where it is even more pronounced. In this sense, *type 2* municipalities that set a combined tax multiplier show an intermediate tax response.

In sum, the event study models in Figure 2.4 support the hypothesis about a relative increase in *corporate* income taxes in locations where CB worker inflows benefit firms. In addition, Figure 2.4 documents relative increases also for *personal* income taxes in highly-treated border municipalities. This may be taken as evidence that, on average, CB workers' impact on domestic workers is perceived as positive, as *personal* income in municipalities close to national borders is apparently expected to be able to cope with a tax that declines less than in more centrally-located regions with fewer CB workers. The fact that the response in the *personal* income tax was lagged (and

[26]Naturally, Swiss border regions did not experience an equally large inflow of CB workers in response to the commuting policy. It seems that municipalities of *type 1* are an extreme example for this. In some cases it is their geography with high mountains (e.g., Valais) or large lakes (e.g., Appenzell-Ausserrhoden, St. Gallen) at the border that prevents an increase in CB workers even with liberalized CB labor markets (c.f., Appendix Figure A2.2). In other cases, it is likely that demand and supply side effects on the local labor markets in Switzerland and its neighboring countries prevented a relevant change in the number of CB workers.

[27]One of the two *type 1* cantons located at the French or German border that did experience significant inflows of CB workers is Neuchâtel. Unfortunately, this canton does not provide sufficient data on local taxes to be included in the analysis.

[28]Various robustness checks that use alternative model specifications or definitions of the underlying sample, as deployed in Section 2.5.3 for the other main results, support the conclusion of no differential tax response for this group of municipalities. Results are available upon request.

smaller) may be natural as the effect on domestic workers was less clear *a priori* compared to the impact on firms and potential positive effects likely not as pronounced.

In addition to the yearly estimates, Table 2.4 presents average effects for the transition and post-reform period with the pre-reform period as the baseline. The previous results are confirmed: (i) a statistically significant upward deviation in taxes is only observed for the distance group closest to the border, (ii) the response is strongest in the post-reform phase, (iii) it is particularly strong in municipalities where the multiplier (also) applies to corporate income, and (iv) it is largest in *type 3* municipalities. The existence of three different types of municipalities with different mandates to set local taxes complicates the identification of local fiscal policy responses to the labor supply shock, but the results are all consistent with the conclusion that both *corporate* and *personal* income taxes increased, although the latter response is lagged and smaller in magnitude. The results also suggest that *type 2* municipalities, where the multiplier applies to both *corporate* and *personal* income, adopted an intermediate strategy in the sense that *corporate* taxes were raised (in relative terms) but not as much and later than in *type 3* municipalities, but still more and earlier than in *type 1* municipalities.

Table 2.4 also shows that these findings are robust when extending the sample to include (i) three smaller cantons that switched between tax regimes (see Table 2.2 for details) and (ii) observations from the entire border region in the control group. The latter modification redefines the baseline to include municipalities even more distant to the border, yet still located within the border region to assure that the same legal standards apply. Including these observations increases the identified tax response in the first distance group across all models which could suggest that the impact of the commuting policy declines further with the distance to the border and beyond the 30-minutes boundary.

### 2.5.2 Mechanisms Behind the Tax Responses

The data analyses presented in Section 2.5.1 suggest that observed differences in tax changes among Swiss municipalities are due to the exogenous inflow of foreign workers and their impact on the local economy. According to this interpretation, the increase in local labor supply and the skill-mix improves the affected municipalities' attractiveness for firms (and potentially local residents). In turn, this reduces incentives to relocate by increasing relocation costs, which allows politicians to maintain higher taxes than nearby municipalities that are not directly affected by the reform.

This section provides further evidence in support of this hypothesis and shows that the effects are largest in municipalities with (i) an *a priori* higher expected exposure to the reform and (ii) higher electoral support for the treaty to capture voter sentiments (see second hypothesis in Section 2.2). Moreover, two further extensions are presented which suggest that the results are not driven by alternative explanations. All four heterogeneity analyses focus on only the largest subsample of municipalities where the multiplier applies to both tax bases (i.e., *type 2*) in order to achieve sufficient power for the heterogeneity analyses.[29]

First, I use a sample split based on whether the local employment share of CB workers was below or above the median in 1998 (the last pre-reform year). The hypothesis is that in municipalities with an *ex ante* higher CB worker employment share, the conditions for the employment of CB workers are better than in below-median municipalities (e.g., more firms within commuting distance or more supply of CB workers from across the border). Theoretically, a higher share of CB workers before the reform could of course also lead to a lower inflow of additional CB workers due to market saturation. However, this is unlikely in the present case, given the strict entry regulations and the prevalence of skill shortages over the period considered, especially in sectors where CB workers often find employment. Accordingly, the inflow of CB workers should be higher

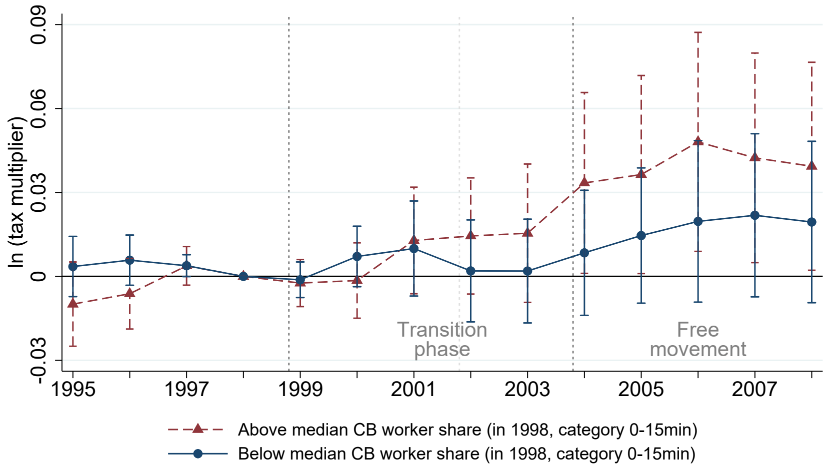
[29]To have sufficient observations in each subsample, I also rely on a slightly more crude distance categorization with bins of 15 minutes driving time (instead of 10 minutes) in this section. The conclusions from Section 2.5.1 also hold with this less fine-grained categorization approach (see Section 2.5.3 below).

**Table 2.4:** Tax policy response by tax base and treatment phase

| | **Corporate income tax** (type 3) | | | **Personal income tax** (type 3) | | | **Multiplier applies to both tax bases** (type 2) | | |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| $\mathbb{1}\{0 \leq d_i \leq 10\}$*Transition | 0.024*** | 0.022*** | 0.031*** | 0.006 | 0.007 | 0.006 | 0.010 | 0.009 | 0.015* |
| | (0.007) | (0.007) | (0.008) | (0.007) | (0.007) | (0.007) | (0.009) | (0.009) | (0.008) |
| $\mathbb{1}\{0 \leq d_i \leq 10\}$*Post-reform | 0.051*** | 0.050*** | 0.067*** | 0.030** | 0.028** | 0.035** | 0.047*** | 0.047*** | 0.053*** |
| | (0.015) | (0.015) | (0.017) | (0.013) | (0.013) | (0.014) | (0.017) | (0.017) | (0.016) |
| $\mathbb{1}\{10 < d_i \leq 20\}$*Transition | -0.005 | -0.003 | 0.005 | -0.003 | -0.0003 | -0.0003 | 0.002 | 0.000 | 0.007 |
| | (0.007) | (0.006) | (0.007) | (0.007) | (0.007) | (0.007) | (0.005) | (0.005) | (0.005) |
| $\mathbb{1}\{10 < d_i \leq 20\}$*Post-reform | 0.011 | 0.012 | 0.032** | 0.017* | 0.018* | 0.033*** | 0.008 | 0.007 | 0.015 |
| | (0.012) | (0.012) | (0.014) | (0.010) | (0.010) | (0.011) | (0.011) | (0.011) | (0.010) |
| **Post-reform effect (0-10min)** | **(5.0 – 6.7%)** | | | **(2.8 – 3.5%)** | | | **(4.7 – 5.3%)** | | |
| Number of observations | 1,750, | 2,100 | 2,688 | 1,750, | 2,100 | 2,688 | 7,770 | 7,854 | 9,478 |
| Municipality controls | × | × | × | × | × | × | × | × | × |
| Municipality fixed effects | × | × | × | × | × | × | × | × | × |
| Canton-year fixed effects | × | × | × | × | × | × | × | × | × |
| Including switching states | | × | | | × | | | × | |
| Contr. group: >20min (in BR) | | | × | | | × | | | × |

Notes: *** p<0.01, ** p<0.05, * p<0.1. The pre-treatment period from 1995 to 1998 represents the baseline in all models. *Transition* corresponds to the years 1999–2003 during which CB worker employment restrictions were partially lifted. The *Post-reform* period starts in 2004, when CB worker employment was completely liberalized in the border region. Results correspond to Equation 2.2. Standard errors clustered by municipality.

**Figure 2.5:** Tax policy response by pre-reform CB worker employment share




Notes: Coefficient estimates and 95% confidence intervals for highly-treated municipalities in the distance group with 0-15min driving time to the nearest border crossing relative to observations in the distance category 15-30min. Coefficient estimates based on separate regressions. The comparison group includes all municipalities, irrespective of their CB worker share. Results correspond to Equation (2.1). Municipality controls as well as municipality and canton-year fixed effects included. Standard errors clustered by municipality.

which is confirmed by the numbers, and tax changes should be larger in municipalities with a pre-reform higher CB worker share.

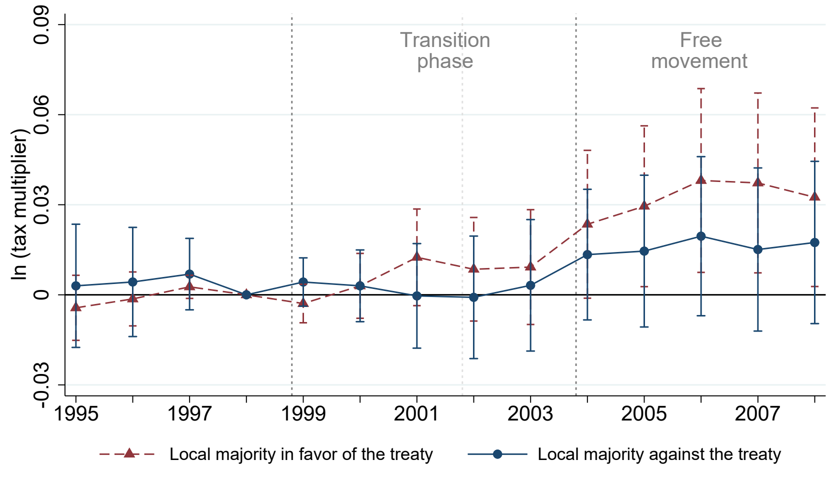
Figure 2.5 shows the results which support the claim of reform-induced inflows of CB workers constituting a crucial driver for the identified changes in municipal taxes. The results document a positive, statistically significant, and robust effect for locations where the pre-reform CB worker share was above the median – an indication of higher exposure to new CB worker inflows.[30] The converse argument then says that, under the assumption that local conditions were suboptimal for the employment of CB workers in municipalities with below-median CB worker employment shares in 1998, these jurisdictions would not experience a noticeable impact of the commuting policy, irrespective of their driving time to the nearest border crossing. The absence of a statistically significant tax policy response in these municipalities substantiates this interpretation.

The second extension exploits the electoral support for the AFMP. In 2000, there was a national referendum and all Swiss citizens were asked to vote on the treaty between Switzerland and the EU. The referendum outcomes are available at the municipal level and can help to better understand the local expectations towards the reform, its economic impact, and, thus, the drivers for the observed tax responses. Figure 2.6 shows the policy response, separately for municipalities where the majority of citizens voted in favor or against the reform. In line with expectations, a relative tax increase is only identified for jurisdictions close to the border and with a local majority in favor of the AFMP. Wherever citizens opposed the treaty, no significant policy response is identified. This evidence supports the hypothesis that more optimistic views about the local economic impact of the reform were a key driver behind the policy response.[31] This is expected to be particularly true for the *type 2* municipalities considered in the heterogeneity analysis, where the tax rates for *personal* and *corporate* income are linked. If the local population is opposed to the migration reform, perhaps because they see no economic advantage in it, citizens should be less willing to accept a relative increase in the local tax multiplier as this affects their own tax burden.

[30] Appendix Table A2.2 reports the results as an average across the reform stages based on Equation 2.2 and shows their robustness to alternative definitions of the control groups.

[31] This conclusion is supported by the results of Appendix Table A2.3. It shows the average response for each reform stage based on Equation 2.2, tests the robustness with respect to an extension of the control group to the entire border region, and reports the results of interaction models.

**Figure 2.6:** Tax policy response by municipal-level support for the treaty




Notes: Coefficient estimates and 95% confidence intervals for highly-treated municipalities in the distance group with 0-15min driving time to the nearest border crossing relative to observations in the distance category 15-30min. Coefficient estimates based on separate regressions. Local support for the treaty measured by support rates in the 2000 national referendum. Results correspond to Equation (2.1). Municipality controls as well as municipality and canton-year fixed effects included. Standard errors clustered by municipality.

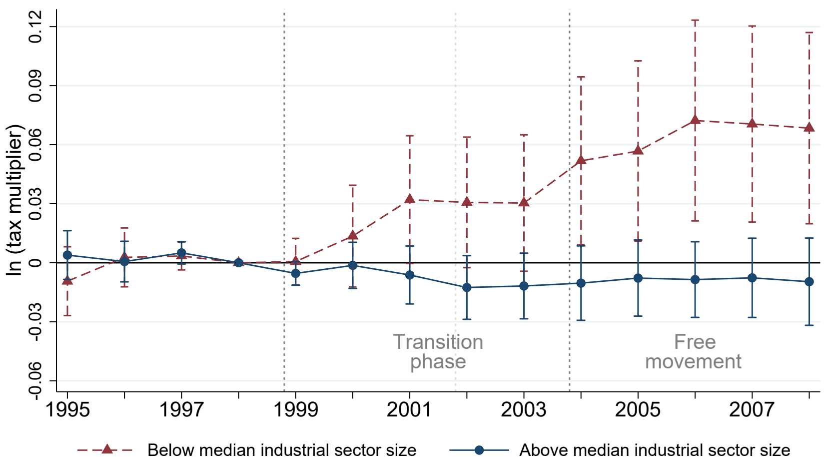
Next, I discuss two extensions to address concerns about alternative explanations potentially driving the observed tax adjustments. A first concern might be that some of the other legal adjustments that were part of the treaty between Switzerland and the EU could have initiated an adjustment of tax policies by Swiss border municipalities. In particular, the agreement to cut back technical barriers to trade between the economies might affect strong exporting regions more heavily. The overall argument – that tax adjustments are a response to an (expected) improvement of local economic conditions – would then still be the same. Yet, the channel would be different and policy adjustments might be due to a stronger performance of exporting firms rather than firms in need of the newly incoming foreign workers. I therefore analyze the policy response separately for municipalities with an above versus below median share of industrial sector size, as firms in this sector are the strongest exporters. Beerli et al. (2021) use a similar approach to show that the implementation of the treaty did not have a stronger effect on firms with higher exports. In fact, their results suggest that the positive effects on firms "are similar, if anything even stronger, if we exclude all two-digit industries that were directly affected by the other bilateral agreements" (Beerli et al., 2021, p. 1008)

Figure 2.7 shows the results which clearly speak against the hypothesis of stronger exports as the key determinant for the observed policy responses. Instead, they corroborate the argument that the sharp increase in the inflow of foreign workers against the backdrop of a shortage of skilled labor – a particularly important factor for the tertiary sector – are the decisive drivers behind the tax adjustments.[32]

Finally, a second concern might be that policy changes and in particular tax competition with neighboring jurisdictions across national borders could explain the results. However, the observation of a downward trend in international corporate taxation over the period in question, including in the countries neighboring Switzerland (see, e.g., Spengel et al., 2020), renders this a rather unlikely scenario. In a classical tax competition model, declining tax rates across the border result in downward pressure on taxes in Swiss border municipalities when compared to more centrally-located municipalities. Yet, the results show the opposite which suggests that

[32] I also report the average response for each reform stage, test the robustness with respect to an extension of the control group to the entire border region, and report the results of interaction models. These results are documented Table A2.4 and confirm the above-drawn conclusions.

**Figure 2.7:** Tax policy response by local importance of the secondary sector




Notes: Coefficient estimates and 95% confidence intervals for highly-treated municipalities in the distance group with 0-15min driving time to the nearest border crossing relative to observations in the distance category 15-30min. Coefficient estimates based on separate regressions. Local importance of industrial production measured by employment in the secondary sector at the municipal level. Results correspond to Equation (2.1). Municipality controls as well as municipality and canton-year fixed effects included. Standard errors clustered by municipality.

the presented analysis rather underestimates the true effects of the commuting policy on local taxation in Switzerland.

To test the possibility of tax competition with jurisdictions across the border empirically, Figure 2.8 presents results when including country-border times year fixed effects in the regression to capture average adjustments across the border for each national border separately. The results are almost identical to the main specification and suggests that the observed local policy responses are not due to developments across the border.

Overall, the presented model extensions all lend support to the conclusion that the inflow of foreign workers allows policy makers to maintain higher levels of *corporate* (and to some extent also *personal* income) taxes compared to less-affected municipalities, as the reform effects increase the attractiveness of municipalities close to the border and relocation costs of mobile factors and agents increase.

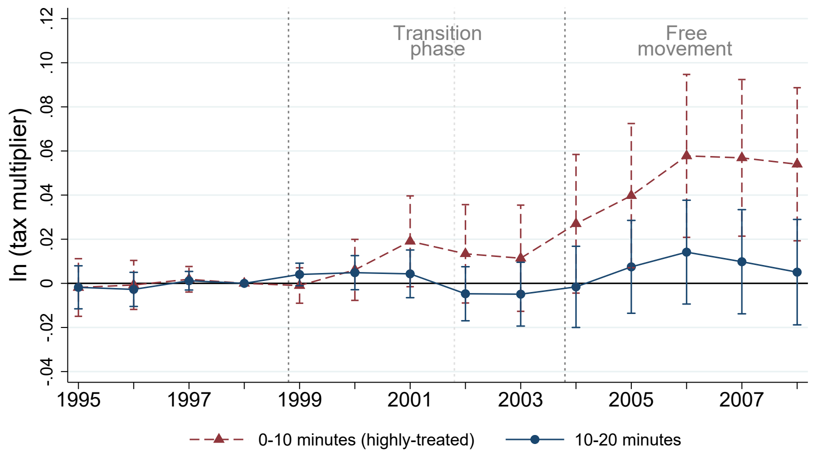
### 2.5.3 Robustness Tests

The sensitivity of the results is tested by applying a number of adjustments to the model specification and the definition of the underlying data sample. First, Appendix Table A2.5 evaluates the results' robustness to using a cruder categorization into bins of 15-minutes driving time (rather than 10-minute bins) while extending the sample to the entire border region. The results confirm the previous findings and show that municipalities closer to the border deviate the most from the overall downward trend in local taxes.

Second, the specification behind the results in Appendix Figure A2.3 abstains from a categorization into distance groups altogether. Instead, a continuous driving time variable is considered. These results again support the previous findings: the response is statistically significant and robust for the *corporate* income tax and most pronounced for the period after 2004 when labor markets were completely liberalized. The effect on the *personal* income tax is smaller and statistically significant only in some specifications.

Third, Table A2.6 in the Appendix investigates whether a restriction in one of the *type 3* cantons' tax laws affects the results in the respective municipalities. According to tax laws in the canton of Solothurn, the municipal tax multipliers for *corporate* and *personal* income must differ by at most 30% of the cantonal tax. Only a small share of municipalities is restricted by this law, but the legal requirement might either exert upward pressure on *personal* income taxes or depress *corporate* income taxes in the respective municipalities. To test this possibility, Appendix Table

**Figure 2.8:** Tax policy response by tax base – accounting for changes at the country-border*year level




Notes: Coefficient estimates and 95% confidence intervals for municipalities in the distance groups with 0-10min and 10-20min driving time to the nearest border crossing, relative to the group 20-30min. Results correspond to Equation (2.1) but in addition include country-border*year fixed effects. Standard errors clustered by municipality.

A2.6 compares the previous results with an adjusted sample in which all municipalities affected by this restriction were excluded. As the results show, this adjustment has almost no effect on the coefficient estimates.

Fourth, Figure A2.4 in the Appendix extends the sample period until 2012 to analyze the persistence of relative differences in tax multipliers. For the main results, the sample ended in 2008 to avoid that the global financial crisis (GFC) could confound the results. The extended sample results suggest that the differences in tax multipliers persist also ten years after the implementation of the commuting policy, even though differences seem to decrease to some extent for *corporate* income taxes in *type 3* municipalities with the advent of the GFC.

## 2.6 Concluding Remarks

The analysis in this paper establishes a link between changes in local labor supply and local taxes. Building on, and extending, established lines of reasoning from the traditional tax competition and NEG literature, it is argued that a quantitatively larger or qualitatively better labor supply pool constitutes a favorable location factor which can, in principle, be taxed. For identification, I exploit a commuting policy that liberalized cross-border labor markets between Switzerland and the EU. This facilitates the estimation of event study and DiD models that compare highly-treated municipalities close to the national border with more centrally-located locations where firms miss out on the benefits of CB workers.

The empirical results show that highly-treated municipalities close to the border set higher *corporate* taxes after the reform compared to less-affected jurisdictions. Various model extensions and robustness tests speak for the interpretation that municipalities used the favorable effects of the commuting policy for their local economies to maintain higher tax levels. Among other things, I provide suggestive evidence that policy responses are not driven by tax competition with jurisdictions across the national border or by exporting firms which benefited from a cut back of technical barriers to trade that occurred around the same time as the liberalization of cross-border labor markets. The results on the local *personal* income tax indicate a similar yet smaller and lagged response. In particular the lagged response might not come as a surprise, as the impact of the commuting policy on domestic workers was less clear *a priori* but also turned out to be predominantly positive. Finally, local electoral support for the reform constitutes a critical factor for the differential tax changes, a finding that is likely particularly relevant for the adjustments in the *personal* income tax.

In conclusion, the findings of this paper call for paying careful attention to potential (policy) responses at lower government levels as well as to local attitudes, as they will affect the overall impact of any market-integration agreement like the AFMP reached at the national level. Importantly, the argumentation in this paper and the findings regarding local tax responses build on the positive effects of the reform for Swiss firms (and workers) that have been identified in a recent paper (see Beerli et al., 2021). Yet, similar conclusions regarding the economic effects of (highly-skilled) migration have been reached for other settings (see, e.g., Peri, 2012, on US states; Hornung, 2014, on historical Prussia; Mitaritonna et al., 2017, on France) which suggests a broader validity of the conclusions that are to be confirmed by future research.

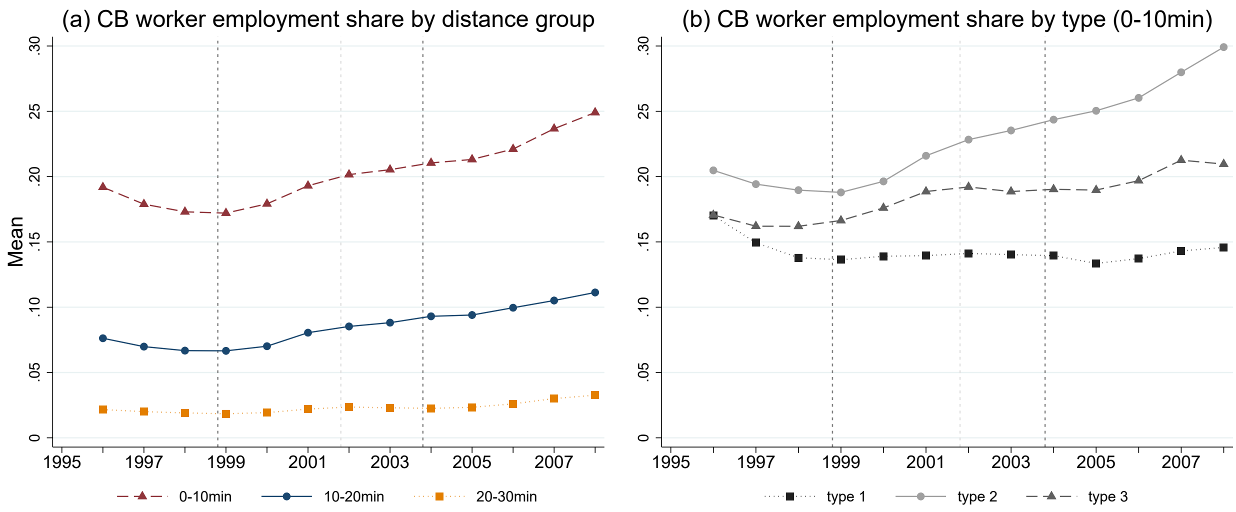
# Appendix

**Table A2.1:** Taxation of CB workers according to double taxation agreements

| **Country of residence** | **Regulation** |
| --- | --- |
| Germany | • A max. of 4.5% of gross income has to be paid to cantonal tax authorities (taxation at the source).<br>• Usual income tax to be paid in Germany (the amount due to Swiss tax authorities is accounted for). |
| France | • CB workers' income is only taxed in France<br>• The usual income tax applies. French tax authorities transfer 4.5% of gross income to the respective Swiss canton (if the CB worker is employed in the border cantons Basel-Stadt, Basel-Landschaft, Jura, Neuchâtel, Solothurn, Vaud, Valais, or Bern).<br>• Exception 1: when employed in the canton of Genève, CB workers are taxed at the source. The French border Departments Ain or Haute-Savoie receive 3.5% of gross income as compensation.<br>• Exception 2: when employed in another canton, French CB workers are taxed at the source. French tax authorities grant a tax break on the amount taxable in France. |
| Italy | • CB workers' income is only taxed in Switzerland (taxation at the source).<br>• The Swiss border cantons (Valais, Ticino, Graubünden) then transfer 40% (38.5% for Ticino) of the tax amount to the home municipality in Italy as compensation. |
| Austria | • CB workers' are taxed in Switzerland and Austria.<br>• Austrian CB workers are taxed at the source in Switzerland. Swiss tax authorities transfer 12.5% of the tax amount to Austria as compensation.<br>• The amount paid to Swiss authorities is deducted when calculating the Austrian income tax.<br>• Before 2006, CB workers only had to pay a tax of 3% to the respective canton which was then deducted when calculating their income tax due to Austrian tax authorities. |

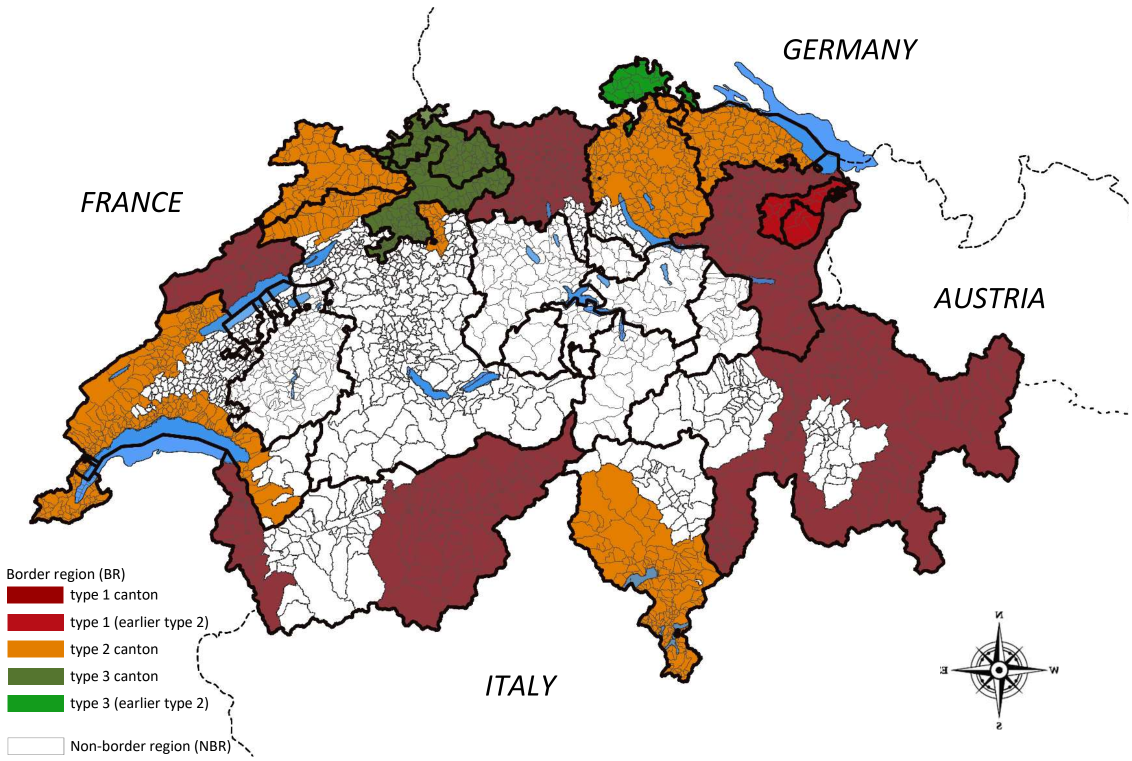
Notes: When not returning to Germany (France) for more than 60 (45) working days, the CB worker status is lost and the individual gets fully taxed at the source. The information is based on the official double taxation agreements between Switzerland and its neighboring countries.

**Figure A2.1:** Evolution of CB worker employment shares by distance group and municipality type




Notes: Panel (a) plots the yearly CB worker employment shares relative to total municipal employment in 1998 across municipality types (see Table 2.2). Panel (b) focuses only on the highly-treated group right at the border (0-10min) and plots the employment shares by municipality type.

**Figure A2.2:** Border region cantons by municipality type




Notes: Thicker black lines correspond to cantonal borders. Thinner lines highlight municipal borders. National borders from neighboring countries are depicted as dashed lines.

**Table A2.2:** Tax policy response by treatment phase – pre-reform CB worker share

| | Total | CB worker share below median | CB worker share above median | Total | CB worker share below median | CB worker share above median |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| $\mathbb{1}\{0 \leq d_i \leq 15\}$*Transition | 0.004 | 0.001 | 0.011 | 0.022** | 0.020** | 0.028*** |
| | (0.006) | (0.007) | (0.009) | (0.007) | (0.008) | (0.010) |
| $\mathbb{1}\{0 \leq d_i \leq 15\}$*Post-reform | 0.026** | 0.013 | 0.043** | 0.053*** | 0.045*** | 0.069*** |
| | (0.012) | (0.013) | (0.018) | (0.012) | (0.013) | (0.017) |
| Number of observations | 7,770 | 5,824 | 5,936 | 9,478 | 7,532 | 7,644 |
| Municipality controls | × | × | × | × | × | × |
| Municipality fixed effects | × | × | × | × | × | × |
| Canton-year fixed effects | × | × | × | × | × | × |
| Contr. group: 15-30min | × | × | × | | | |
| Contr. group: >30min (in BR) | | | | × | × | × |

Notes: *** p<0.01, ** p<0.05, * p<0.1. The *pre-reform* period 1995–1998 represents the baseline in all models. *Transition* corresponds to the years 1999-2003 during which CB worker employment restrictions were partially lifted. The *Post-reform* period starts in 2004, when CB worker employment was completely liberalized in the border region. The table focuses on the response by highly-treated municipalities (i.e., in the first distance category). CB worker shares relative to 1998 employment. The comparison groups include all municipalities in the respective category, irrespective of their CB worker share. Standard errors clustered by municipality.

**Table A2.3:** Tax policy response by treatment phase – municipality-level support for the treaty in the 2000 referendum

| | In favor of treaty | Against treaty | In favor of treaty | Against treaty | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| **A: Sample splits** | | | | | | |
| $\mathbb{1}\{0 \leq d_i \leq 15\}$*Transition | 0.006 | -0.008 | 0.024*** | 0.011 | | |
| | (0.008) | (0.012) | (0.009) | (0.012) | | |
| $\mathbb{1}\{0 \leq d_i \leq 15\}$*Post-reform | 0.032** | 0.005 | 0.070*** | 0.019 | | |
| | (0.016) | (0.018) | (0.016) | (0.017) | | |
| **B: Interaction models** | | | | | | |
| $\mathbb{1}\{0 \leq d_i \leq 15\}$*Transition | | | | | 0.005 | 0.022** |
| | | | | | (0.008) | (0.009) |
| $\mathbb{1}\{0 \leq d_i \leq 15\}$*Post-reform | | | | | 0.031* | 0.068*** |
| | | | | | (0.016) | (0.016) |
| $\mathbb{1}\{0 \leq d_i \leq 15\}$*Transition*Against | | | | | -0.018 | -0.016 |
| | | | | | (0.013) | (0.013) |
| $\mathbb{1}\{0 \leq d_i \leq 15\}$*Post-reform*Against | | | | | -0.031 | -0.061*** |
| | | | | | (0.023) | (0.021) |
| Number of observations | 5,796 | 1,358 | 7,014 | 1,554 | 7,182 | 8,582 |
| Municipality controls | × | × | × | × | × | × |
| Municipality fixed effects | × | × | × | × | × | × |
| Canton-year fixed effects | × | × | × | × | × | × |
| Contr. group: 15-30min | × | × | | | × | × |
| Contr. group: >30min (in BR) | | | × | × | | × |

Notes: *** p<0.01, ** p<0.05, * p<0.1. The *pre-reform* period 1995–1998 represents the baseline in all models. *Transition* corresponds to the years 1999-2003 during which CB worker employment restrictions were partially lifted. The *post-reform* period starts in 2004, when CB worker employment was completely liberalized in the border region. The table focuses on the response by highly-treated municipalities (i.e., in the first distance category). Standard errors clustered by municipality.

**Table A2.4:** Tax policy response by treatment phase – share of secondary sector employment at the municipality level

| | Industry presence below median | Industry presence above median | Industry presence below median | Industry presence above median | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| **A: Sample splits** | | | | | | |
| $\mathbb{1}\{0 \leq d_i \leq 15\}$*Transition | 0.022 | -0.009 | 0.051*** | 0.002 | | |
| | (0.014) | (0.006) | (0.015) | (0.006) | | |
| $\mathbb{1}\{0 \leq d_i \leq 15\}$*Post-reform | 0.063*** | -0.001 | 0.104*** | 0.006 | | |
| | (0.023) | (0.009) | (0.023) | (0.011) | | |
| **B: Interaction models** | | | | | | |
| $\mathbb{1}\{0 \leq d_i \leq 15\}$*Transition | | | | | 0.016 | 0.057*** |
| | | | | | (0.013) | (0.015) |
| $\mathbb{1}\{0 \leq d_i \leq 15\}$*Post-reform | | | | | 0.052** | 0.115*** |
| | | | | | (0.022) | (0.023) |
| $\mathbb{1}\{0 \leq d_i \leq 15\}$*Transition*Industrial | | | | | -0.024* | -0.053*** |
| | | | | | (0.014) | (0.016) |
| $\mathbb{1}\{0 \leq d_i \leq 15\}$*Post-reform*Industrial | | | | | -0.059** | -0.115*** |
| | | | | | (0.024) | (0.026) |
| Number of observations | 4,174 | 3,591 | 4,739 | 4,734 | 7,770 | 9,478 |
| Municipality controls | × | × | × | × | × | × |
| Municipality fixed effects | × | × | × | × | × | × |
| Canton-year fixed effects | × | × | × | × | × | × |
| Contr. group: 15-30min | × | × | | | × | × |
| Contr. group: >30min (in BR) | | | × | × | | × |

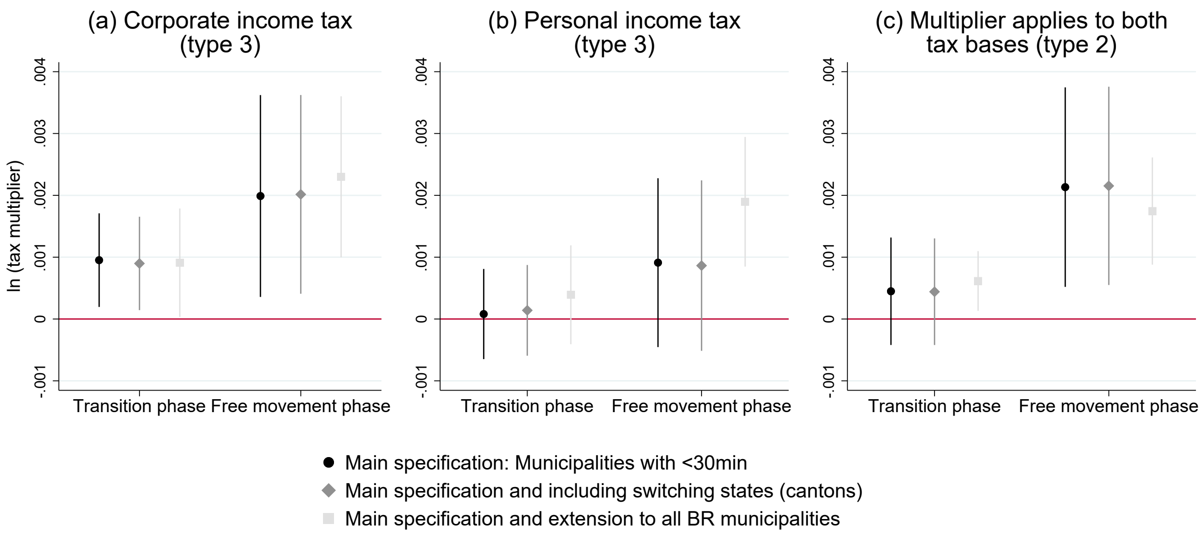
Notes: *** p<0.01, ** p<0.05, * p<0.1. The *pre-reform* period 1995–1998 represents the baseline in all models. *Transition* corresponds to the years 1999-2003 during which CB worker employment restrictions were partially lifted. The *post-reform* period starts in 2004, when CB worker employment was completely liberalized in the border region. The table focuses on the response by highly-treated municipalities (i.e., in the first distance category). Standard errors clustered by municipality.

**Table A2.5:** Tax policy response by tax base and treatment phase – alternative categorization with bins of 15 minutes driving time

| | **Corporate income tax** (type 3) | | **Personal income tax** (type 3) | | **Multiplier applies to both tax bases** (type 2) | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| $\mathbb{1}\{0 \leq d_i \leq 15\}$*Transition | 0.022* | 0.026** | 0.006 | 0.011 | 0.022*** | 0.022*** |
| | (0.013) | (0.013) | (0.011) | (0.010) | (0.007) | (0.007) |
| $\mathbb{1}\{0 \leq d_i \leq 15\}$*Post-reform | 0.068*** | 0.068*** | 0.034** | 0.036** | 0.053*** | 0.053*** |
| | (0.020) | (0.020) | (0.015) | (0.015) | (0.012) | (0.012) |
| $\mathbb{1}\{15 < d_i \leq 30\}$*Transition | 0.021** | 0.023** | 0.014 | 0.016* | 0.019*** | 0.018*** |
| | (0.010) | (0.010) | (0.009) | (0.009) | (0.006) | (0.006) |
| $\mathbb{1}\{15 < d_i \leq 30\}$*Post-reform | 0.051*** | 0.055*** | 0.041*** | 0.044*** | 0.029*** | 0.029*** |
| | (0.016) | (0.016) | (0.012) | (0.013) | (0.010) | (0.010) |
| **Post-reform effect (0-15min)** | **(6.8%)** | | **(3.4 – 3.6%)** | | **(5.3%)** | |
| Number of observations | 2,688 | 3,038 | 2,688 | 3,038 | 9,478 | 9,562 |
| Municipality controls | × | × | × | × | × | × |
| Municipality fixed effects | × | × | × | × | × | × |
| Canton-year fixed effects | × | × | × | × | × | × |
| Including switching states | | × | | × | | × |

Notes: *** p<0.01, ** p<0.05, * p<0.1. The pre-treatment period from 1995 to 1998 represents the baseline in all models. *Transition* corresponds to the years 1999-2003 during which CB worker employment restrictions were partially lifted. The *Post-reform* period starts in 2004 when CB worker employment was completely liberalized in the border region. Results correspond to Equation 2.2 but are based on a cruder categorization into distance groups. Standard errors clustered by municipality.

**Figure A2.3:** Tax policy response by tax base and treatment phase – continuous driving time specification




Notes: Coefficient estimates and 95% confidence intervals for the interaction of the *transition* phase (1999-2003) and *free movement* phase ($t \geq$2004) with the driving time variable. For an easier interpretation of the graph, driving time has been multiplied by (-1). Coefficients ($\delta_1$ and $\delta_2$) estimated based on the following main specification:

$$\begin{aligned} ln(mult_{i,t}) = {} & \beta_1 Transition + \delta_1(Transition \times driving\_time) + \\ & \beta_2 Free\_move + \delta_2(Free\_move \times driving\_time) + \theta' \mathbf{X}_{i,t} + \alpha_i + \alpha_{c,t} + \varepsilon_{i,t}. \end{aligned}$$

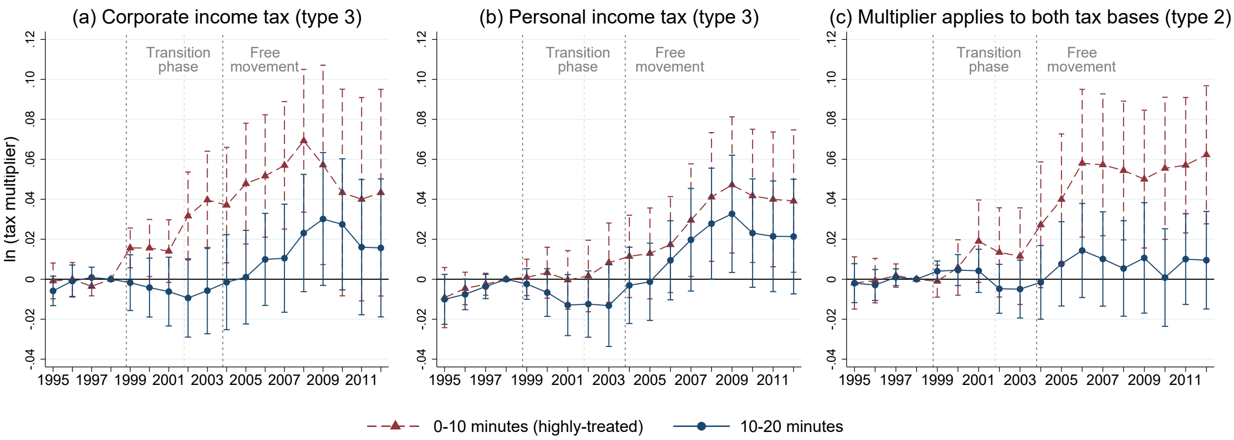
The pre-treatment period from 1995 to 1998 represents the baseline in all models. Standard errors clustered by municipality.

**Table A2.6:** Tax policy response by tax base and treatment phase – exclusion of observations that are constrained in their tax policy

| | Corporate income tax (type 3) | | | | Personal income tax (type 3) | | | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| $\mathbb{1}\{0 \leq d_i \leq 10\}$*Transition | 0.024*** | 0.023*** | | | 0.006 | 0.005 | | |
| | (0.007) | (0.007) | | | (0.007) | (0.007) | | |
| $\mathbb{1}\{0 \leq d_i \leq 10\}$*Post-reform | 0.051*** | 0.050*** | | | 0.030** | 0.032** | | |
| | (0.015) | (0.015) | | | (0.013) | (0.013) | | |
| $\mathbb{1}\{10 < d_i \leq 20\}$*Transition | -0.005 | -0.005 | | | -0.004 | -0.003 | | |
| | (0.008) | (0.008) | | | (0.007) | (0.007) | | |
| $\mathbb{1}\{10 < d_i \leq 20\}$*Post-reform | 0.011 | 0.012 | | | 0.017* | 0.019* | | |
| | (0.012) | (0.012) | | | (0.010) | (0.010) | | |
| $\mathbb{1}\{0 \leq d_i \leq 15\}$*Transition | | | 0.022* | 0.022 | | | 0.006 | 0.006 |
| | | | (0.013) | (0.014) | | | (0.011) | (0.011) |
| $\mathbb{1}\{0 \leq d_i \leq 15\}$*Post-reform | | | 0.068*** | 0.066*** | | | 0.034** | 0.034** |
| | | | (0.020) | (0.022) | | | (0.015) | (0.016) |
| $\mathbb{1}\{15 < d_i \leq 30\}$*Transition | | | 0.021** | 0.021** | | | 0.014 | 0.014 |
| | | | (0.010) | (0.010) | | | (0.009) | (0.009) |
| $\mathbb{1}\{15 < d_i \leq 30\}$*Post-reform | | | 0.051*** | 0.049*** | | | 0.041*** | 0.040*** |
| | | | (0.016) | (0.017) | | | (0.012) | (0.013) |
| Number of observations | 1,750 | 1,718 | 2,688 | 2,627 | 1,750 | 1,718 | 2,688 | 2,627 |
| Constrained obs. excluded | | × | | × | | × | | × |
| Municipality controls | × | × | × | × | × | × | × | × |
| Municipality fixed effects | × | × | × | × | × | × | × | × |
| Canton-year fixed effects | × | × | × | × | × | × | × | × |

Notes: *** p<0.01, ** p<0.05, * p<0.1. The pre-treatment period from 1995 to 1998 represents the baseline in all models. *Transition* corresponds to the years 1999-2003 during which CB worker employment restrictions were partially lifted. The *Post-reform* period starts in 2004 when CB worker employment was completely liberalized in the BR. Results correspond to Equation 2.2. Standard errors clustered by municipality.

**Figure A2.4:** Tax policy response by tax base – extension of the post-reform period until 2012




Notes: Coefficient estimates and 95% confidence intervals for municipalities in the distance groups with 0-10min and 10-20min driving time to the nearest border crossing, relative to the group 20-30min. Results correspond to Equation (2.1) with an extended sample period until 2012. Municipality controls as well as municipality and canton-year fixed effects are included. Standard errors clustered by municipality.

# Chapter 3

# Political Budget Cycles in Local Public Investment: The Contribution of State-Owned Enterprises

**Abstract.** We study the degree and nature of political budget cycles in public investment when two instruments are available: Investment by core governments and, more indirectly, by state-owned enterprises (SOEs). While fiscal and democratic pressures on core budgets may induce politicians to shift election-induced investment to SOEs, voters' uncertainties in clearly attributing the benefits of SOE investment to incumbent politicians may encourage the opposite. Our empirical analysis of public investment at the municipal level in Germany, by both SOEs and their public owners (i.e., municipal core budgets), documents substantial election cycles. The percentage changes relative to the election year are similar in the two sectors. These results are consistent with the interpretation that German municipal counselors use public investment, financed and implemented through both instruments, in order to improve their re-election chances. Our results imply that the total effect of elections on municipal investment in our sample is over EUR 1 billion for the pre-election year in each electoral cycle, while the past literature focusing only on core budgets would miss about a third of this effect.

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This chapter is co-authored by Zareh Asatryan (ZEW Mannheim) and Désirée Christofzik (University of Speyer).

## 3.1 Introduction

The concept of a political budget cycle (PBC) describes the idea that the electoral cycle in democracies creates incentives for incumbent politicians to adopt voter-friendly policies just before the next election, thereby increasing the politicians' re-election prospects. Existing research has almost exclusively focused on core public budgets to examine the question of whether, for example, total public spending, investment, or employment are higher on the eve of an election. We build on the theoretical and empirical evidence from this PBC literature and extend its arguments to the economic activities of state-owned enterprises (SOEs) in Germany. These comprise all institutions, funds, or firms, where governments hold a majority of capital or voting rights (details see Section 3.3.2).

By focusing on SOEs, we address two research questions: First, we discuss theoretically and test empirically whether the electoral cycle gives rise to a PBC in public investment by SOEs. This question has received little attention in the existing literature. We argue that the incentives that lead to PBCs in core budgets also apply to SOEs, although there are differences (see discussion below). Our results confirm the existence of a PBC in SOE investment. Second, we investigate the role of SOEs for politically-motivated spending in the public sector more generally and examine their role in the electoral process vis-à-vis core budgets. To this end, we also examine the PBC in municipal core budgets and compare it with that of SOEs. We ask whether the *total* PBC is larger when SOEs are included. This is crucial to understand because previous evidence that ignores SOEs and focuses only on core budgets may underestimate the true extent of election-driven policy decisions. Our results confirm this for the sample of German municipalities that own at least one SOE.

Our motivation for the focus on SOEs is twofold: First, they represent a sizable sector with significant economic impact, employing as many people as the core budgets of local governments, but their finances are rarely the subject of academic or political debate.[1] This makes them attractive instruments for electioneering. Second, in theory, SOEs are held for efficiency reasons and to be able to provide public services more flexibly. However, given their special position from a statistical perspective (they are not captured by typical public finance statistics) and from a legal perspective (e.g., they are mostly not captured by fiscal rules such as the German debt brake), there is a risk that SOEs are politicized, reducing the efficiency gains they might offer.[2] Against this backdrop, we examine the extent to which SOEs are subject to political rather than market forces, using electoral cycles for identification.

Regarding the theoretical underpinning of the PBC literature and our analysis, research on this phenomenon began in the mid-1970s with seminal papers such as Nordhaus (1975) and Hibbs (1977). Tufte (1978) provides a particularly simple and useful theoretical framework for conceptualizing the idea of a PBC, arguing that in order for policy variables to follow a cyclical pattern that is driven by the electoral cycle, three ingredients must be present: (1) a motive, (2) an opportunity, and (3) an instrument. The standard motive for politicians is their interest in being re-elected. This creates incentives to engage in political behavior that makes a good impression on voters. It is highest just before an election, when voter interest and attention are particularly high. Next, an opportunity exists when election outcomes depend, at least to some extent, on the actual policy performance or on policy signals from incumbents observed during the previous term. In addition, the opportunity for electioneering may also depend on voter characteristics or the information available to voters. While the very first PBC papers

[1] Despite recent efforts to implement commercial accounting standards at the municipal level in Germany (e.g., Christofzik, 2019), consolidated financial statements that integrate the financial situation of SOEs into municipal accounts and thus facilitate full transparency of local public finances are still the absolute exception.

[2] Heinemann and Nover (2023) examine the implementation of the German debt brake at the state level in 2020 and document a decrease in state and municipal SOE equity and reserves and an increase in SOE debt in fiscally constrained states when compared to SOEs in less fiscally constrained states and the shorter the distance to the 2020 deadline. This points to circumvention and creative accounting practices that make use of SOEs (see also Potrafke, 2023, on this aspect).

assume voters to be myopic, allowing politicians to debt-finance additional expenditure just before elections, second-generation models emphasize the aspect of information asymmetries between voters and politicians holding office, which can lead to election cycles despite rational expectations among voters (for an overview, see Veiga et al., 2019). Finally, any fiscal or economic policy instrument at the disposal of incumbent politicians can serve as an instrument to convince voters of their ability and suitability for the next government.[3] We argue that SOEs are yet another, previously understudied, instrument that incumbent politicians might exploit for electioneering purposes. However, Tufte's conditions one (i.e., motive) and two (i.e., opportunity) may differ for SOEs, with implications for the existence and the magnitude of a PBC in SOE investment.

With respect to the motive, the standard re-election incentive of incumbent politicians that applies to core budget PBCs also extends to a potential election cycle in SOEs. Governments, as the majority owners of SOEs, can force SOEs to pursue a particular investment plan that increases their re-election chances (c.f., Shleifer and Vishny, 1994; Alok and Ayyagari, 2020; Li et al., 2020). From this perspective, SOEs simply add to the list of strategic manipulation tools available to incumbent politicians. In addition to this *external* motive, there may be *internal* motives of SOE managers that can lead to a cyclical pattern of SOE investment, independent of the *external* motive of SOE owners. First, managers may fear being replaced when a new government with new politicians is elected, and may therefore choose to invest more heavily prior to elections in order to increase the likelihood that incumbents and current majorities will be retained.[4] Second, a common approach with SOEs is to nominate a member of the municipal council to serve on the board of directors. If this person does not win a seat on the local council for the next term, he or she will lose this prestigious position. This may create additional incentives for such council members to work in favor of a PBC in SOE investment from within the firm.

With respect to opportunities, there are arguments both for and against the existence of a strong election cycle in SOEs' investment behavior. In terms of amplifying factors, it is important to note that German municipalities operate under fiscal constraints and have only a limited set of policy instruments at their disposal to strategically manipulate policy instruments for re-election purposes or to react to unexpected negative changes in their budget. This could induce a substitution towards unrestricted financing instruments such as SOEs (Von Hagen, 1991; Von Hagen and Wolff, 2006; Inoue, 2020). This would not only lead to a PBC in SOE activities, but also implies a potential contingent liability and potential risks to public finances (Baum et al., 2021). In a similar vein, there are stronger checks and balances in core budgets than in SOEs. For example, typical public finance statistics discussed in the media and presented by politicians only capture core budget figures, while SOE figures are less frequently discussed, leading to a significantly biased picture of public finances (Asatryan et al., 2022). Voter scrutiny and oversight of public finances is therefore stronger for core budgets, and most likely strongest in highly-indebted municipalities, creating additional incentives to outsource public investment associated with electioneering motives to SOEs. In terms of moderating factors, one might expect a lower salience of SOE investment activity compared to core budgets, or expect voters not to attribute SOE investment to incumbent politicians. This would reduce the attractiveness of SOEs for electioneering, despite their advantages from a financing perspective. It should be noted, however, that most SOEs provide goods and services that have been outsourced from the

[3]The literature has considered a wide variety of instruments in the context of PBCs, ranging from classical fiscal instruments such as total public spending, capital expenditure, taxation, or transfers (e.g., Foremny et al., 2018; Repetto, 2018; Foremny and Riedel, 2014; Corvalan et al., 2018) over public employment (e.g., Cahan, 2019), public procurement (e.g., Havlik et al., 2021), or politicians extra-parliamentary activities (Geys, 2013) to economic forecasts (e.g., Cipullo and Reslow, 2022; Bohn and Veiga, 2021).

[4](Anecdotal) evidence on this possibility for German SOEs is lacking, so more research in this direction is needed. De Meo and Ferrari (2018) find suggestive evidence for a 16% increase in the probability of observing the appointment of a new board member in Italian SOEs due to party turnover at the level of the municipal owners. A related study by Shen and Lin (2012) exploits such board member turnover in state-owned banks (SOBs) associated with national elections in 63 countries. The authors do not provide information on the frequency with which CEOs or chairpersons of SOBs are replaced after an election.

core budget in order to manage these activities more efficiently. They are frequently involved in sectors like energy utilities, transportation, water supply, waste disposal services, and local real estate ventures. As such, they often do not differ significantly from the public activities of core budgets, reducing the likelihood that they will not be considered public. In addition, many German municipalities choose not to outsource such activities and continue to organize and finance them in the core budget.[5] In these cases, such services are still provided by core budgets. Whether voters are aware of the circumstance that such tasks are sometimes institutionalized by SOEs and in other cases by core budgets and then choose to attribute such activities to incumbent politicians or not is not clear and remains essentially an empirical question.[6]

In summary, our theoretical discussion highlights the attractiveness of SOEs as an instrument for electoral campaigning alongside core budget (investment) spending. We therefore expect to find an election cycle in SOE investment and empirically examine the relative role of public investment through SOEs compared to investment through municipal core budgets in the context of local elections. Regarding our outcome variable of interest, it should be noted that investment projects often take time to be completed and become visible. In addition, there is often some uncertainty about the time to completion, so politicians may not want to risk making investments too late. Both characteristics of investment lead us to expect an increase in investment spending with some distance to the next election. This would also be consistent with the previous PBC literature focusing on public investment, which finds the highest or significant investment spending in the pre-election year (e.g., Veiga and Veiga, 2007; Repetto, 2018), while studies that examine more flexible spending categories in terms of timing tend to identify the peak in the election year itself (see, e.g., Akhmedov and Zhuravskaya, 2004, on direct monetary transfers to citizens in Russian regions).

For identification, we take advantage of the fact that municipal council elections in Germany are held on the same day within each state, but the election years differ across the 16 states. This allows us to disentangle the effect of the electoral cycle on SOE investment from general time trends in investment activity. Data on SOEs and the identity of their owners come from the federal statistical offices and cover the universe of commercially accounting SOEs in Germany. They allow us to track investment dynamics at the firm level while controlling for relevant firm- and owner-level characteristics. We complement this focus on SOEs with data on public investment activity through core budgets – the municipal owners of SOEs. This provides a first insight into the interplay between public investment through SOEs and core budgets and the extent to which they are used for electioneering purposes. Since the municipal and state levels are not well distinguishable in the three city-states of Berlin, Bremen, and Hamburg, our final sample is limited to observations from the 13 area states in order to have a comparable institutional setting. This leaves us with data on more than 10,500 SOEs and over 4,400 municipal owners over the period 2002 to 2019.

The main part of the paper examines the existence of an election cycle in SOE investment and provides evidence on the underlying mechanisms. Using event study regressions that model the full electoral cycle at the municipal level, we show that per capita SOE investment follows the cyclical pattern that is well known from the PBC literature: Investment starts to increase about two years before an election, is highest in the pre-election year, and then declines to a lower level in the post-election year and years with a greater distance to the next election. This cyclical effect is robust to different specifications and is lower for SOEs without a clear majority

[5] In about 69.5% of German municipalities, there is not a single local SOE in 2019, according to our data.

[6] Departing from the PBC literature, there are papers that study the existence and effects of political *business* cycles. Dating back to Nordhaus (1975), the idea regarding motive and goal is the same as for PBCs, but instead of fiscal or policy parameters acting as instruments to increase voter satisfaction and thus re-election chances, this literature argues that generally good economic conditions in the relevant jurisdiction – influenced by policy decisions – increase the re-election odds of incumbent politicians (for an overview, see Dubois, 2016). Higher pre-election SOE investment may therefore be an attractive instrument for incumbent politicians to boost their re-election chances, even if voters do not attribute this investment activity to politicians. This argument is also stressed in the literature on PBCs in the lending activity of state-owned banks (SOBs), as discussed below.

owner who can dictate an investment strategy. Our main specification documents a EUR 6.96 increase in per capita SOE investment at the municipal level in the pre-election year relative to the election year. Further heterogeneity analyses support the hypothesis that the observed investment dynamics are election-driven by exploiting the different timing of elections within an election year. An election earlier (later) in the year leads to a larger (smaller) increase in the pre-election year, as less (more) investment activity takes place in the election year itself as the baseline. Further analyses suggest that the PBC is stronger in municipalities where the council is dominated by a right-wing majority and is particularly pronounced in more visible sectors such as public transport, pre-primary education, or sports, cultural, and recreational education.

Our complementary results on public investment activity through the core budgets themselves show a very similar pattern to that observed for SOEs. In relative terms, per capita investment is 4.1% higher in the pre-election year than in the election year (compared to 3.4% to 4.1% for SOEs). In sum, *total* public investment in per capita terms is about EUR 18.01 higher in pre-election years relative to an election year, with SOEs accounting for about a third of the effect. We conclude that SOE activity is a relevant but understudied part of the public sector that is subject to significant political influence, most likely distorting the efficient application of funds in SOEs (see, e.g., Alok and Ayyagari, 2020, on the distortive effect of political influence on SOEs). In comparison to the established PBC literature on core budgets, this total effect still represents only a small to medium amount. For example, Repetto (2018) finds a pre-election year effect of EUR 81.80 to EUR 139.00 per capita for municipal core budgets in Italy over the period 1999 to 2012. One likely reason for this difference is the generally higher baseline investment of Italian municipalities compared to German municipalities (+55.4%). Another explanation may be provided by other PBC papers that seek to identify factors responsible for strong heterogeneities in the existence and magnitude of election cycles across countries and identify institutional factors such as long-established and well-functioning public institutions or corruption and rent-extraction opportunities as relevant determinants to explain cross-sectional differences (for an overview, see De Haan and Klomp, 2013).

The rest of the paper is structured as follows: Section3.2 summarizes our literature contributions. Section 3.3 describes the institutional setting of SOEs and municipal elections. Section 3.4 introduces the data and estimation approach. Section 3.5 presents our results on SOEs and core budgets and Section 3.6 concludes.

## 3.2 Literature Contributions

In this section, we briefly summarize the relevant literature and discuss our contributions. Early studies on core budget PBCs have mostly considered the national level (see, e.g., Brender and Drazen, 2005; Alt and Lassen, 2006, or De Haan and Klomp, 2013 for an overview). More recently, there has been a shift in focus to subnational levels, which provide rich variation in terms of election timing and election outcomes, while also allowing for more compelling identification strategies, as the institutional setting is the same for all observations. This has led to the emergence of numerous papers investigating the mechanisms behind PBCs and identifying amplifying and moderating factors. For example, Akhmedov and Zhuravskaya (2004) and Repetto (2018) argue that voter information is a crucial determinant of PBCs. The latter exploit an exogenous reform that required the publication of municipal balance sheets before, and no longer after elections, and find that this significantly reduced the otherwise strong cycle in municipal investment in Italy. Bonfatti and Forni (2019) identify the implementation of a fiscal rule as a limiting factor for PBCs in Italian municipal investment. Drazen and Eslava (2010) present a model of PBCs that argues for a cycle in the composition of public spending rather than in the overall budget, and find empirical evidence supporting their hypothesis for Colombian municipalities. Klein and Sakurai (2015), Ferraresi (2020), and Bohn and Veiga (2021) all find evidence suggesting that term limits are another moderating factor for the existence or size of a PBC. We contribute to this literature by conducting an analysis into election cycles of SOEs and argue that these represent an amplifying factor in the sense that electioneering is potentially underestimated when the activity of SOEs is ignored.

Turning to the PBC literature that studies the German municipal setting, the focus of this paper, Foremny and Riedel (2014) exploit the same identifying variation as we do and show that changes in the local business tax follow the electoral cycle, with growth rates of the business tax being significantly lower in the pre-election and election year as compared to other years of the electoral cycle. Foremny et al. (2018) use the different timing of council and mayoral elections in the two German states of Baden-Württemberg and Bavaria and identify a sizable election cycle in total spending of municipal core budgets for council elections. Hessami (2018) exploits a reform in the German state of Hesse, creating quasi-experimental variation across municipalities, after which mayors were no longer appointed by the council, but rather directly elected by voters. She finds that in election years, elected mayors attract 7.4% more investment transfers (in per capita euro values) from the state than appointed mayors – an outcome for which they can clearly claim credit, given their institutional responsibility, and which is consistent with the hypothesis that re-election incentives affect policy choices.[7] We extend this literature by conducting the first large-sample analysis of a PBC in total public investment by German municipalities (as the owners of SOEs).

With respect to PBCs in SOE activities, we are aware of only three directly related studies: First, Inoue (2020) examines SOEs in the Brazilian water sector and finds higher employment by these entities in state election years over the period 2004–2014, an effect that is stronger in economically disadvantaged regions and lower when the SOE is partially privately owned. Second, with a similar focus on SOE investment as our analysis, Alok and Ayyagari (2020) study large project-level investments by Indian SOEs and private firms, exploiting variation in state and national elections over the period 1995–2009. They find an increase in project announcements and project size for SOEs during elections. No effects are found for the placebo group of private investment. They show that the PBC is stronger in districts with highly-contested elections, establish that additional investment increases the margin of victory of incumbent parties, and provide suggestive evidence of a negative effect on shareholder value, given indication for a negative net present value of the additional projects. SOE investment is also the outcome variable of interest in the third study by Li et al. (2020). The authors examine the investment dynamics of SOEs in most EU countries over the period 2001–2015. They rely on Bureau van Dijk's Amadeus database, which covers only a subset of all SOEs, and relate SOE investment to national elections, regardless of whether SOEs are owned by the central government or by subnational governments. They find large increases in SOE investment around elections, an effect that is stronger in close elections, in countries with weaker institutions, and in countries with a state-dominated banking system. A fourth paper differs slightly from the previous ones in that it tests Benford's Law for the financial statements of SOEs owned by Italian municipalities. This law considers the distribution of numbers in large data samples and is commonly used in the auditing and fraud detection literature. Capalbo et al. (2023) show that in non-election years, the financial statements of SOEs satisfy Benford's Law, while during election seasons, the authors find suggestive evidence of manipulation in the form of diffuse data anomalies.[8] We add to this small but growing literature a first analysis of SOE investment in relation to local elections that focuses on (i) all SOEs in (ii) a well-defined institutional setting that is the same for all observations, rather than considering only a potentially selective subsample of SOEs or a cross-country sample for which a comprehensive list of relevant control variables to account for critical institutional differences is usually not available.

[7]For the state level, Tepe and Vanhuysse (2009) and Tepe and Vanhuysse (2013) examine the timing of hiring decisions for teachers and police officers and find more intensive hiring activity in election periods.

[8]Relatedly, De Meo and Ferrari (2018) apply a regression discontinuity design to close elections in Italy and find that party turnover disrupts investment, productivity, and financial stability of municipally-owned SOEs, while SOE profitability and employment activity are unaffected. Consistent with Capalbo et al. (2023), Capalbo et al. (2021) provide evidence of election-driven earnings management in a subsample of Italian SOEs. Mang and Schmidt (2023) examine the impact of an economic stimulus package in Germany in 2009 on a cross-section of public and private hospitals. In one specification, they control for the effect of local elections and provide preliminary evidence in line with the interpretation that prior to elections, public hospitals receive funding for fewer projects but more funding for individual, potentially more visible projects.

While there is surprisingly little research on election cycles in SOEs, a related and growing literature examines lending cycles of state-owned banks (SOBs, for an overview, see Weill, 2022). These are typically not considered as SOEs, nor are they part of our empirical analysis, but like SOEs they are subject to political influence. A seminal paper in this literature is Dinç (2005), who provides cross-country evidence of higher lending by SOBs in election years relative to private banks. Taken together, these papers present two key findings relevant to our analysis: First, political influence around elections is strong and leads to increased lending, and second, this impact of the electoral cycle negatively affects loan quality, repayment rates, and bank performance (e.g., Cole, 2009 on India; Carvalho, 2014 on Brazil; Lavezzolo, 2015 on Spanish regions; Englmaier and Stowasser, 2017 on German local savings banks; Faraz and Rockmore, 2020 on Pakistan). In contrast, Baum et al. (2010) find that the behavior of Turkish SOBs is not significantly different from that of Turkish private banks during national parliamentary election seasons, while Bircan and Saka (2021) confirm the existence of a lending cycle for Turkish local elections.

Finally, although there is a paucity of work on the relevance of SOEs to election cycles, there is a large literature that considers SOE activity more generally.[9] With a similar focus on political influence as our analysis, Menozzi et al. (2012) find evidence for the hypothesis that politically-connected board members in Italian SOEs increase firm employment while reducing profitability when compared to non-connected board members. Asatryan et al. (2022) provide a descriptive overview of SOEs in Germany and argue that they represent a substantial but often ignored part of the overall public sector. The authors refer to SOEs as "other governments" because of their importance to the public sector but separate statistical and legal status. Boll and Sidki (2021) study the effect of politically-fragmented councils in German municipalities on SOE investment and find some evidence of a negative impact.

## 3.3 Institutional Background

### 3.3.1 Municipal Governments and Elections

Compared to many other industrialized countries, the German state is rather decentralized, consisting of the central government, 16 federal states and about eleven thousand municipalities. Municipalities are either organized in districts, where several (smaller) municipalities together form a district and the district government takes on overarching tasks such as the provision and maintenance of county roads, or municipalities form a district on their own. The latter case typically describes larger municipalities, which are called urban districts (as opposed to rural districts) and they are responsible for both municipal-level and district-level public tasks. While municipalities generate significant revenues of their own, mainly by taxing local businesses' profit and private property within their jurisdiction (e.g., Foremny and Riedel, 2014), rural districts do not have any tax-setting autonomy of their own and are mostly financed by a fee that they levy on their member municipalities (the so called *Kreisumlage*). As rural districts are thus quite different from the municipalities, we focus our analysis on the effect of the electoral cycle on SOEs owned by municipalities and urban districts only.

On the political side, municipalities are governed by a local council and a mayor. Both types of politicians are not subject to a term limit. Together, they form the local executive and are elected

[9] Most of this economic research focuses on the policy question of whether or not to privatize SOEs (for reviews, see Megginson and Netter, 2001; Sheshinski and López-Calva, 2003; Barkley, 2021). The evidence that SOEs are inefficient compared to private firms is generally well established (e.g., Atkinson and Halvorsen, 1986; Ehrlich et al., 1994; Hausman and Neufeld, 1991; La Porta and López-de Silanes, 1999). This particular aspect has fueled a wave of privatization of SOEs in the 1980s, which was further reinforced by the market transition processes of the 1990s following the dissolution of the Soviet Union (IMF, 2020). The literature of this period tends to focus on the political-economy reasons behind the question of why governments struggle to manage SOEs effectively, for example, highlighting the role of rent-seeking (Djankov and Murrell, 2002). More recently, the special case of China, which combines extraordinary growth with the large presence of SOEs in its economy, has generated renewed interest in SOEs (e.g., Fan et al., 2007; Storesletten and Zilibotti, 2014; Berkowitz et al., 2017; Lin et al., 2020).

by the local population (details see below).[10] Majorities in the council change, depending on the issue under discussion, so there is no well-defined ruling party or coalition and no opposition. Municipal tasks are either mandatory (e.g., public administration, school buildings, local roads, sewerage management, social housing, or local police and fire protection) or voluntary (e.g., recreation, culture, and sports or business development). Municipalities may choose to outsource some of these activities to SOEs (details see Section 3.3.2 below). In total, municipalities are responsible for about 40% of public spending in the federal states (Federal Statistical Office, 2017).

The details of municipal duties, elections, and the decision-making powers are regulated by the so-called municipal code (*Gemeindeordnung*) of the respective state. These differ in the timing and frequency of elections, but are otherwise similar in the institutional details relevant to a PBC analysis (e.g., Foremny and Riedel, 2014). Municipalities in all states are subject to a fiscal rule: Debt may only be taken on in order to finance investment projects or to bridge short-term liquidity shortfalls. Elections for the local council take place every five or six years, while the mayor is elected in a separate election and remains in power for five to eight years (in the small state of Saarland, she is elected for ten years).[11] The mayor has a special position in that she is the head of the municipal administration and typically represents the municipality. Otherwise, the mayor usually has the same voting rights as the council members. Since important decisions that shape municipal policy are voted on by the council, typically by a simple majority, we focus our analysis on the electoral cycles of council elections.[12] While elections are held on the same day within states, the different timing of these elections across states allows us to disentangle the effects of the electoral cycle from general trends in public investment over time. Figure 3.1 shows the distribution of council elections for our sample, which spans from the year 2002 to 2019.

### 3.3.2 State-Owned Enterprises (SOEs) in Germany

The mainstream economic rationale for the establishment of state ownership includes the need to address market failures, for example in natural monopoly situations, to provide public goods, or because of significant externalities from private goods, for example in health and education services. In practice, however, SOEs have been established and continue to flourish for many other reasons, as evidenced by their existence in a wide range of sectors. In Germany, about half of all economic sectors have at least one SOE (OECD, 2020). In addition, SOEs also function as an instrument to facilitate economic activity in general (Federal Statistical Office, 2023b). The list of more than 20,000 German-based SOEs is therefore highly diverse and includes airports and railway companies, universities, financial vehicles such as the "Sondervermögen Bundeswehr", nursing homes and hospitals, energy utilities, water supply and waste disposal services, special purpose associations for organizing inter-municipal cooperation, real estate firms in charge of managing public property, and even breweries, among many others.

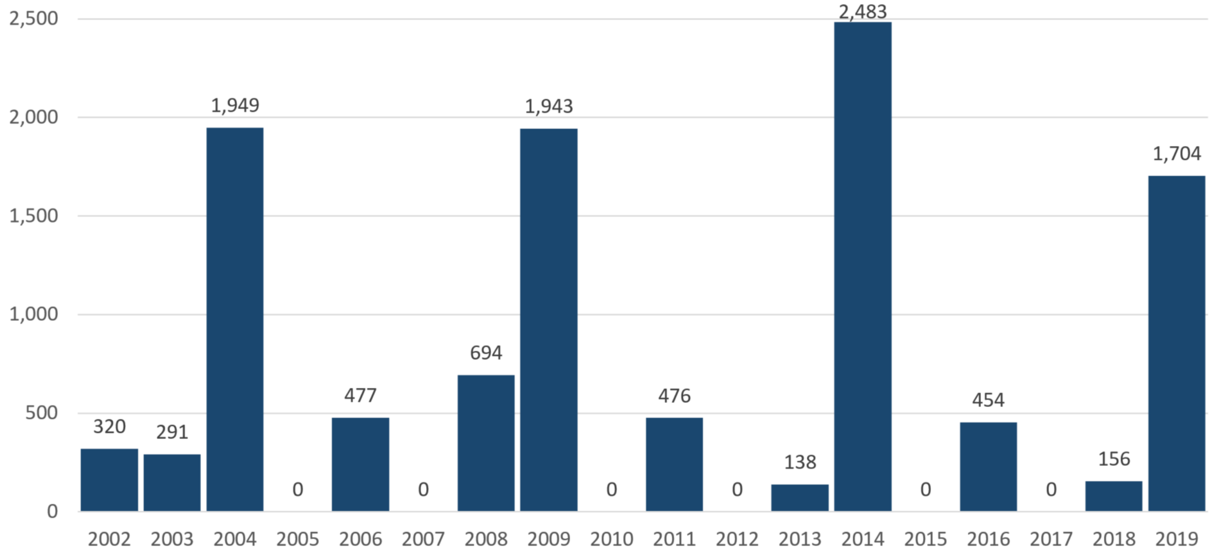
Institutional details regarding German SOEs have, for example, been described and discussed in Asatryan et al. (2022) with a focus on the structure of the SOE sector and its economic relevance, Hesse et al. (2017) with a focus on SOE investment in the context of a public investment

[10]Note that there is no legislative at the municipal level, as the local council governs only in the form of statutes and legal norms, while laws can only be enacted or amended by state governments or the federal government.

[11]Compared to state and federal elections, municipal elections are much more focused on individual candidates than on political parties. As a result, municipal counselors have particular incentives to signal high competence and good performance before elections, as they often cannot rely on strong and stable party preferences among the electorate. While individual counselors may not be particularly visible to the public in normal times, it is common to see them campaigning in the run-up to an election on campaign posters on the streets, in person at public places such as farmers' markets or other campaign events, and in recent years on social media platforms.

[12]In five of the thirteen area states, council elections and mayoral elections are often held on the same day, though not in all elections considered. Foremny et al. (2018) examine the existence of a PBC in both council and mayoral elections in Germany. They find robust evidence of a cycle in total municipal expenditure associated with council elections, but not with mayoral elections. However, the effect in the year prior to a council election is slightly larger when a mayoral election is held in the same year as a council election, but only when the mayor re-runs for office.

**Figure 3.1:** Number of municipal elections by year




Notes: The figure shows the number of municipal elections (at the level of municipalities as the owners of SOEs) per year for the observations covered in this paper.

shortage in recent years, or Schmidt (2011), Schmidt et al. (2017), and Wägner (2017) who look at the SOE sector from the perspective of a statistician.

By definition, and according to the European System of Accounts (ESA 2010), SOEs describe all units in which governments hold a majority share of more than 50% in terms of capital or voting rights (Eurostat, 2013; Schmidt, 2011). The implication of this property is that policymakers, not just markets, shape the decisions of SOEs. At the same time, SOEs operate outside of the core public sector, which often means that commonly used financial statistics do not capture their activities and that SOEs receive far less attention in public and policy debates than core budgets.[13]

Among SOEs, there is a distinction between non-market and market producers, based on a categorization scheme in financial statistics that has been harmonized across EU countries in order to ensure a consistent classification of, for example, government spending or debt. As shown in Appendix Figure A3.1, the total public sector thus consists of three layers: (1) core budgets, (2) non-market producer SOEs, and (3) market-producer SOEs. The latter distinction is particularly important for the consistent calculation of public debt figures – and thus for issues related to fiscal rule compliance – and is mostly based on whether or not SOEs' own revenues cover more than 50% of their production costs.[14]

[13] In German, SOEs are called "Öffentliche Fonds, Einrichtungen und Unternehmen", and include not only companies but also public funds or institutions with either a private or public legal form.

[14] The criteria for the classification of public institutional units according to the government sector accounts are illustrated in Appendix Figure A3.2. Examples of non-market producers are most public universities (university hospitals are an exception), or tourism, city marketing, and public administration associations owned by municipalities. In contrast, Deutsche Bahn or many local SOEs that provide energy or water – and thus generate significant revenues of their own – are market producers. Complete lists of all SOEs are available online for the more recent years (Federal Statistical Office, 2021; 2023a).

## 3.4 Data and Econometric Model

### 3.4.1 Data: Annual Accounts of SOEs

Our analysis of SOE investment dynamics is based on an administrative microdata set that covers the annual financial statements of all commercial accounting SOEs in Germany.[15] Asatryan et al. (2022) provide an overview of the size, structure, and economic relevance of the German SOE sector. With respect to the representativeness of the data, the authors compare the microdata with aggregate figures for the universe of all SOEs, regardless of their accounting system. This comparison suggests that the annual accounts data set covers 82.3% of all SOEs owned by local governments which is the sample on which our analysis is based (details see Asatryan et al., 2022).[16]

Focusing on the municipal level and the time period from 2002 to 2019 leaves us with over 160,000 firm-year observations relevant for our analysis. However, since we also rely on information on the owners of SOEs, we process all available information on the identity and individual shares of owners to define our final sample. Unfortunately, the information on SOE owners available to researchers is not of the highest quality, is captured by different variables, and is somewhat unstructured in the annual accounts dataset (c.f., Wägner, 2017). As a consequence, we only have sufficiently detailed information on the identity of the owners (in particular their official municipality key) for a reduced but still substantial share of 73.0% of all relevant observations.

Our outcome variable of interest is annual real investment by SOEs, which we divide by the number of inhabitants in the owner's jurisdiction to obtain a comparable per capita measure of investment.[17] Depending on the model specification and the underlying sample, per capita SOE investment aggregated to the municipal level is EUR 150 to 170. Thus, it accounts for about one third (32.3–35.1%) of total municipal investment, with core budget investment amounting to EUR 314 per capita.[18] However, the aggregate figures for total public investment show that we significantly underestimate the relative importance of SOEs for total per capita investment at the municipal level. Unfortunately, the limitations of the annual accounts dataset with respect to information on ownership of SOEs imply that we capture SOEs with per capita investment figures below the population average. While the figures for core budgets (i.e., EUR 314) are quite accurate, per capita investment of SOEs is actually at EUR 360, and thus accounts for more than half of total municipal investment (see also Hesse et al., 2017). Whether this implies that we are also underestimating the relative importance of SOEs to the total PBC in municipal investment may be hypothesized, but cannot be assessed without additional data.

In addition to SOE investment, we use information on the legal form of SOEs, their ownership structure, or the main economic sector in which they operate to control for important firm characteristics. Summary statistics for these variables are documented in Table 3.1.

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[15] Source: Research data center of the Statistical Offices of the Federation and the Länder. The data set includes information from firms' balance sheets, income statements, and asset records. Missing information is not an issue in this data set, as SOEs are subject to disclosure requirements and must provide all information requested by statistical offices. For more details, see `https://www.forschungsdatenzentrum.de/de/finanzen/jahresabschluss`.

[16] More precisely, we restrict our analysis to SOEs owned by local governments from the 13 area states, as the municipal and state levels are not clearly distinguishable in the three city states of Berlin, Bremen, and Hamburg. Thus, SOEs in the city states often perform systematically different tasks and are not well comparable with SOEs owned by municipalities in the area states.

[17] This definition is consistent with the approach of Boll and Sidki (2021), the only other academic paper that examines SOE investment based on the microdata set. In addition to the per capita regressions, we also examine the dynamics of SOE investment in absolute terms. These results also confirm our election cycle hypothesis and are available upon request.

[18] Figures correspond to the sample considered in this analysis and not to the whole of Germany.

### 3.4.2 Data: Municipalities

For the SOE owners – over 4,400 municipalities – we collect a number of variables for two reasons: First, since the size, structure, and political landscape of a municipality affect SOE's investment activities, we want to control for such municipal characteristics in the regressions. The second reason relates to the dual objective of this paper. In addition to investigating a potential PBC in SOE investment, we also seek to get a more complete picture of the overall election cycle in public investment by comparing the dynamics of SOE investment behavior with that of core budgets. Therefore, we also rely on data that capture local government investment through core budgets.

Data to control for owner characteristics in the SOE analysis come from several sources. First, basic characteristics on municipalities such as population size are taken from the official portal for data at the subnational level in Germany (Statistical Offices of the Federation and the Länder, 2023). For information on political aspects, such as voter turnout and the distribution of seats in the local councils by party, there is no centralized database. Some states make these data publicly available for municipalities within the state. For those states and years where no data are publicly available, we contacted the relevant statistical offices and often received the data by e-mail. For the remaining observations, we collected the data by hand through an extensive online search of municipal websites and online archives of regional newspapers.

The final control variable at the municipal level for our SOE analysis is a dummy controlling for the accounting system of the owner. During the period under consideration, municipalities in most German states had to switch or voluntarily switched from cash to accrual accounting for their core budgets. As this could have an impact on the activities of SOEs in general and SOE investments in particular (see, e.g., Christofzik, 2019), the accounting system is a crucial control variable. It is even more important for the analysis of a potential PBC in investment through core budgets, as investment figures are not comparable across accounting systems. To obtain this information, we contacted all German states. Data on the switching date of municipalities were provided either by the statistical offices or by the ministries of the interior. In one state (Rhineland-Palatinate) the authorities had no information on the switching date of their municipalities. Here, we reconstructed the information by checking whether the municipalities reported whether they provided cash-based or accrual-based figures to the statistical offices. To fill in some missing data points, we conducted online searches on the municipal websites and online archives of regional newspapers. Summary statistics for both firm-level and owner-level control variables are shown in Table 3.1.

Finally, data on local government investment through core budgets come from a database called "Wegweiser Kommune" from the Bertelsmann foundation that obtained and processed the data from the statistical offices of the states (Bertelsmann Stiftung, 2023). As for SOEs, the variable captures real investment and, thus, represents the counterpart to SOEs' investment (Hesse et al., 2017). Measured in euro per capita, we employ this variables as our regressand in the municipal-level analysis for core budgets. As the Bertelsmann data cover a slightly shorter time period (i.e., 2006–2019 instead of 2002–2019 as for SOEs) and only contain information on municipalities with a population of around 5,000 or more, this part of the analysis is limited to a smaller sample than the main SOE analysis. Summary statistics for this reduced-sample analysis at the municipal level are documented in Appendix Table A3.3.

### 3.4.3 The Econometric Model

To identify a potential cycle in SOE investment, we follow the established literature (e.g.,Repetto, 2018; Inoue, 2020) and estimate event study regressions that model the entire electoral cycle of German municipalities, the owners of SOEs. The identifying variation stems from the fact that electoral cycles differ in their timing across German states. This allows us to disentangle the effect

**Table 3.1:** Summary statistics

| Variable | N | Mean | Std. dev. | 1st perc. | Median | 99th perc. |
|---|---|---|---|---|---|---|
| **Firm-level characteristics** | | | | | | |
| Single-owner SOE | 104,016 | 0.828 | 0.377 | 0 | 1 | 1 |
| Market producer | 104,016 | 0.820 | 0.384 | 0 | 1 | 1 |
| *Legal form* | | | | | | |
| GmbH | 104,016 | 0.381 | 0.486 | 0 | 0 | 1 |
| OHG, KG, and similar | 104,016 | 0.025 | 0.158 | 0 | 0 | 1 |
| Other private legal forms | 104,016 | 0.001 | 0.038 | 0 | 0 | 1 |
| Eigenbetriebe | 104,016 | 0.505 | 0.500 | 0 | 1 | 1 |
| Zweckverbände | 104,016 | 0.038 | 0.191 | 0 | 0 | 1 |
| Other public legal forms | 104,016 | 0.049 | 0.217 | 0 | 0 | 1 |
| *Economic sector (NACE)* | | | | | | |
| A: Agriculture, forestry | 104,016 | 0.002 | 0.046 | 0 | 0 | 0 |
| D: Electricity, gas | 104,016 | 0.103 | 0.304 | 0 | 0 | 1 |
| E: Water, sewerage, waste | 104,016 | 0.407 | 0.491 | 0 | 0 | 1 |
| F: Construction | 104,016 | 0.019 | 0.138 | 0 | 0 | 1 |
| H: Transportation, storage | 104,016 | 0.019 | 0.135 | 0 | 0 | 1 |
| I: Accommodation | 104,016 | 0.005 | 0.073 | 0 | 0 | 0 |
| J: Information/communication | 104,016 | 0.006 | 0.080 | 0 | 0 | 0 |
| K: Financing, insurance | 104,016 | 0.002 | 0.045 | 0 | 0 | 0 |
| L: Real estate activities | 104,016 | 0.144 | 0.351 | 0 | 0 | 1 |
| M: Professional/techn. activ. | 104,016 | 0.023 | 0.150 | 0 | 0 | 1 |
| N: Admin./support services | 104,016 | 0.025 | 0.157 | 0 | 0 | 1 |
| O: Public administration | 104,016 | 0.063 | 0.243 | 0 | 0 | 1 |
| P: Education | 104,016 | 0.012 | 0.107 | 0 | 0 | 1 |
| Q: Human health, soc. work | 104,016 | 0.061 | 0.239 | 0 | 0 | 1 |
| R: Arts, entertainm., recreation | 104,016 | 0.079 | 0.269 | 0 | 0 | 1 |
| S: Other service activities | 104,016 | 0.023 | 0.150 | 0 | 0 | 1 |
| Other sectors | 104,016 | 0.008 | 0.097 | 0 | 0 | 0 |
| **Owner-level characteristics** | | | | | | |
| Population | 104,016 | 63,754 | 150,943 | 1,110 | 17,290 | 691,518 |
| Share population >65 years | 104,016 | 0.213 | 0.036 | 0.141 | 0.209 | 0.317 |
| Share working age population | 104,016 | 0.620 | 0.027 | 0.548 | 0.620 | 0.688 |
| Population density | 104,016 | 613.2 | 657.8 | 33.9 | 377.0 | 2,864.4 |
| Turnout (in previous election) | 104,016 | 0.523 | 0.083 | 0.351 | 0.517 | 0.740 |
| *Share of seats in local council by political party* | | | | | | |
| CDU/CSU | 104,016 | 0.355 | 0.135 | 0.00 | 0.358 | 0.656 |
| SPD | 104,016 | 0.243 | 0.129 | 0.00 | 0.246 | 0.545 |
| FDP | 104,016 | 0.042 | 0.049 | 0.00 | 0.033 | 0.200 |
| B90/GRÜNE | 104,016 | 0.061 | 0.065 | 0.00 | 0.053 | 0.250 |
| PDS/DIE LINKE | 104,016 | 0.048 | 0.086 | 0.00 | 0.000 | 0.335 |
| AfD | 104,016 | 0.005 | 0.022 | 0.00 | 0.000 | 0.111 |
| Others/indep. counselors | 104,016 | 0.247 | 0.221 | 0.00 | 0.183 | 1.000 |
| Accrual accounting system | 104,016 | 0.428 | 0.495 | 0 | 0 | 1 |

Notes: Confidentiality policies of the German statistical offices do not permit the reporting of minima and maxima. Instead, we report the 1st and 99th percentiles. Sample period: 2002–2019. Summary statistics correspond to the sample used in our main specification.

of the PBC from general time trends in SOE investment.[19] In particular, our main specification is based on the following regression model:

$$ln(y_{i,m,t}) = \alpha + \beta' \mathbf{d_{m,t}} + \gamma' \mathbf{X}_{i,t} + \delta' \mathbf{Z}_{m,t} + \alpha_i + \alpha_t + \varepsilon_i, \tag{3.1}$$

where $y_{i,m,t}$ captures per capita investment of SOE $i$ ($i = 1, ..., 9263$) owned by municipality $m$ ($m = 1, ..., 3696$) in year $t$ ($t = 2002, ...., 2019$).[20] Similar to Alok and Ayyagari (2020) and Lin et al. (2020), our focus on investment is driven by the fact that it has been shown to be a key determinant of economic growth and incumbents' likelihood of getting re-elected by stimulating the local economy and local employment in the present and future. Alternative outcome variables at the SOE level, such as consumer fees or transfers to the public owners, are highly heterogeneous in terms of whether they apply to individual SOEs and how SOEs record them in their accounts, so focusing on investment facilitates a more comprehensive analysis of the role of SOEs in the electoral process.

The election cycle is modeled by a matrix $\mathbf{d_{m,t}}$ containing five dummies equal to 1 for the years before an election as well as the post-election year, with the election year itself serving as the baseline (in line with, e.g., Repetto, 2018). More specifically:

$$\mathbf{d_{m,t}} = \begin{cases} d_{m,t}^{e-4} = 1 & \text{four years before an election} \\ d_{m,t}^{e-3} = 1 & \text{three years before an election} \\ d_{m,t}^{e-2} = 1 & \text{two years before an election} \\ d_{m,t}^{e-1} = 1 & \text{one year before an election} \\ d_{m,t}^{e+1} = 1 & \text{one year after an election} \end{cases} \tag{3.2}$$

Since most states have local electoral cycles of only five years, the identifying variation for $d_{m,t}^{e-4}$ comes entirely from the state of Bavaria, where the duration of the electoral cycle is six years, as well as from the two states of Brandenburg and North Rhine-Westphalia, which moved their elections from 2013 to 2014 and 2019 to 2020, respectively.[21]

To control for firm-level characteristics, **X** includes a dummy for market-producer SOEs (as opposed to non-market producers/extra budgets), a dummy for single-owner SOEs, legal form fixed effects, and NACE code fixed effects. Time-invariant SOE characteristics are captured by firm fixed effects $\alpha_i$. At the level of SOE owners, **Z** includes several control variables for municipalities. In particular, we control for population size, the share of the population aged 65 and over, the share of the population of working age (18–64), the population density, voter turnout in the last council election, the share of seats held by the main political parties (i.e., CDU/CSU, SPD, FPD, B90/GRÜNE, PDS/DIE LINKE, AfD), and a dummy to capture the accounting system of the owner to account for the fact that municipalities switched from cash to accrual accounting standards during the period considered (see discussion in Section 3.4.2).

[19]This identification approach is consistent with the established PBC literature on Germany (e.g., Foremny and Riedel, 2014; Tepe and Vanhuysse, 2009). It identifies the effects of interest as long as SOE investment does not follow state-specific trends that are aligned with the electoral cycle of the state and are not captured by the included control variables. Although there are differences in the magnitude and length of business cycles across German states that may affect public investment, these cycles follow the same trend and are usually not state-specific but are rather common to larger regions or groups of states (with different electoral cycles), so we are confident that our identification approach provides reliable estimates (see, e.g., Lehmann and Wikman, 2023, for an analysis of state business cycles). In a robustness test, we also take into account state-level elections as another potential confounding factor to rule out that they drive our results. Moreover, additional results that exploit the different timing of elections within an election year further substantiate the validity of our identification approach (see Section 3.5.2).

[20]The figures correspond to our main specification and are smaller or larger for other models.

[21]In the case of Brandenburg, the reason for the change in the election year was to align the local electoral cycle with the EU election cycle. In North Rhine-Westphalia, the aim was that council elections were again to be held on the same day as the mayoral elections.

Finally, $\alpha_t$ represents year fixed effects. Standard errors are clustered at the municipal level. Table 3.1 provides summary statistics for all variables.

In order to examine heterogeneities in investment dynamics, we extend our regression model in Equation (3.1) to include interaction terms for subgroups of the sample. The model then reads as:

$$\begin{aligned} ln(y_{i,m,t}) = \alpha + \beta' \mathbf{d_{m,t}} + \theta Subgroup_{i,m,t} + \lambda' \mathbf{d_{m,t}} \times Subgroup_{i,m,t} + \\ \gamma' \mathbf{X}_{i,t} + \delta' \mathbf{Z}_{m,t} + \alpha_i + \alpha_t + \varepsilon_i, \end{aligned} \tag{3.3}$$

where *Subgroup* denotes, for example, SOEs where the owner's council is dominated by left-wing parties, as opposed to councils dominated by right-wing parties. The effect of the electoral cycle for such a subgroup thus includes both the baseline estimates $\beta'$ and the estimates of the interaction term (i.e., $\lambda'$).

A final conceptual comment relates to a common concern in papers examining PBCs in core budgets: After an election, some share of politicians are newly elected to office and are in a position to familiarize themselves with public administration procedures, agree on an agenda for the term, and possibly cancel previous investment projects of their predecessors. This in itself could lead to a cyclical pattern in core budget investment that is aligned with the electoral cycle but not driven by re-election motives, complicating the identification of PBCs in core budgets (e.g., Repetto, 2018). Since there is no direct effect of an election on the composition of SOE boards, and election outcomes may only have an indirect impact if new politicians and political majorities decide to replace individual board members, we expect this restructuring effect of the (political) leadership to be weaker for SOEs than for core budgets. Empirically, this means that core budget estimates might overestimate the PBC, while this should be less of an issue for SOE estimates.

## 3.5 Results on PBCs in Public Investment

### 3.5.1 Main SOE Results

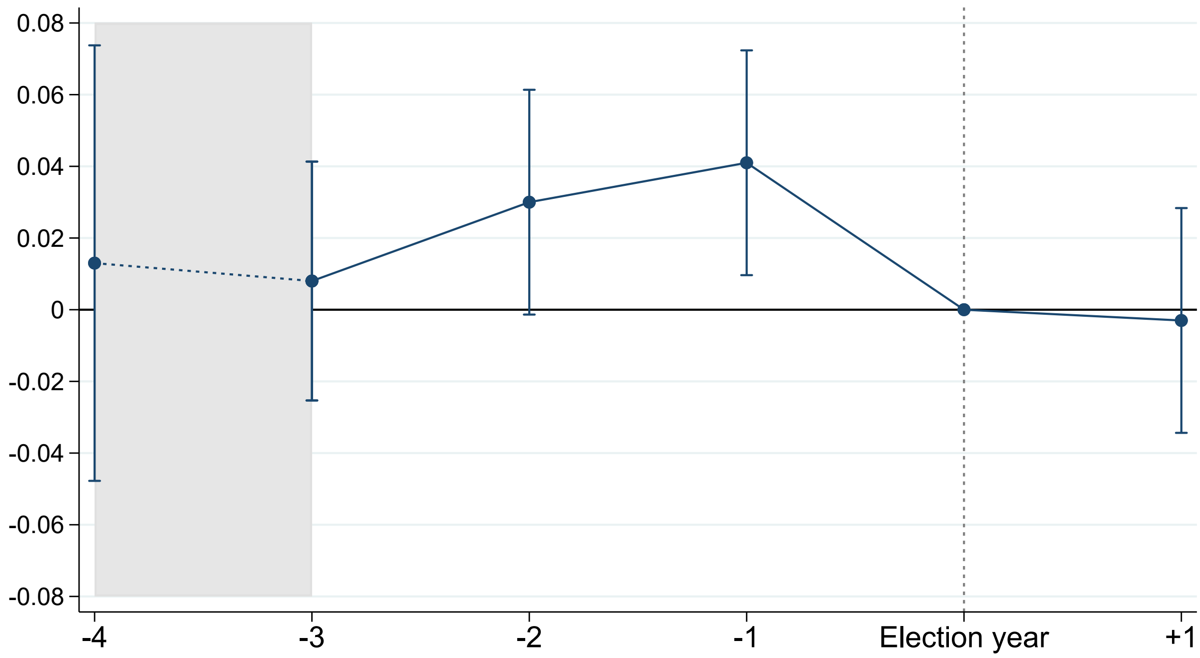
Figure 3.2 presents our main results on the dynamics of SOE investment along the electoral cycle. The figure shows a clear cyclical pattern: Two years before an election, and even more so one year before an election, SOE investment in per capita terms is 3.0% and 4.1% higher than in the election year, respectively. In the other years of the electoral cycle, investment figures are not significantly different from the election year that serves as the baseline.[22] This evidence speaks in favor of the interpretation that SOEs take an active role in the campaigning efforts of incumbent politicians and strategically decide on investments so that they are higher in the run-up to an election.

In terms of magnitude, our results suggest that SOE per capita investment at the municipal level is EUR 6.96 (EUR 3.12 at the firm level) higher in the pre-election year than in the baseline year. Taking into account also the effect two years before an election, which just falls short to be statistically significant at the 5%-level, this leads to per capita investment at the municipal level being higher by EUR 12.05 compared to the election year. The fact that SOE investment does not decline again in years with a greater distance to the next election may by taken as preliminary evidence that SOEs actually increase investments in the pre-election years, rather than shifting investment across the electoral cycle to invest in strategically advantageous years. Yet, further analyses of SOEs' financing is needed to substantiate this hypothesis.

Table 3.2 shows the robustness of the election cycle pattern for SOE investment across various model specifications and sample definitions, with model (3) representing the main specification also depicted in Figure 3.2. In particular, the pre-election year effect is robust as to the inclusion of minority interests (see column 4). These observations do not have a single owner holding more

[22]The effect four years before an election is estimated relatively imprecisely, as the underlying identifying variation is much lower than for the other years of the electoral cycle (see discussion in Section 3.4.3.)

**Figure 3.2:** Election cycle in public investment – SOEs




Notes: Coefficient estimates and 95% confidence intervals based on Equation 3.1 (see also model (3) in Table 3.2). Standard errors clustered by municipality. The shaded area on the left indicates that the typical election cycle lasts only five years (i.e., from -3 to +1). The identifying variation for the coefficient four years before an election comes only from municipalities in the state of Bavaria as well as from one extended electoral cycle in the states of North Rhine-Westphalia and Brandenburg, respectively (details see Section 3.4.3).

than 50% of the firm, but are instead owned by multiple minority owners. When these SOEs are counted as owned by the largest minority shareholder, the pre-election year effect remains statistically significant at the 5% level, but is reduced by almost 20%. This is to be expected as the absence of a single controlling shareholder prevents the easy use of an SOE for creative financing activities.

Models (5) and (6) in Table 3.2 examine the impact of changes in the identity of the majority owner. In most cases, this will not be a situation where an SOE is sold to another municipality, but rather a change in the structure of the majority owner due to a merger or dissolution of municipalities. Controlling for changes in the identity of the owner could have opposing effects. On the one hand, it could necessitate new investments to meet the requirements of the new (structure of the) owner (e.g., because the SOE now provides its services to more people and areas). From a political economy perspective, there is an incentive to time such investments to take place predominantly just before an election. Changes in majority ownership would then increase the election cycle effect. On the other hand, taking over an SOE that was previously owned by another or differently structured local government may require the establishment of effective monitoring and governance of the firm before it can be used for strategic investment decisions. A change in majority ownership would then reduce the PBC. Econometrically, we consider changes in ownership in two different ways: Model (5) simply includes owner fixed effects, while model (6) examines the dynamics of SOE investment over the electoral cycle for firms with stable ownership only. The results suggest that the first channel dominates, as the pre-election year effect is 0.5 to 0.8 percentage points lower when controlling for major changes in the ownership structure.

Finally, model (7) in Table 3.2 documents the results for a reduced sample that only considers firms that are present in the dataset every year over the entire sample period from 2002 to 2019. This addresses the concern that SOEs newly entering the annual accounts dataset (see Section 3.4.1) might affect the estimation of an election cycle in SOE investment. As model (7) shows,

**Table 3.2:** Election cycle in public investment – SOEs

| | Unbalanced panel | | | | | Stable owner | Balanced panel |
|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| Election year −4 | 0.007 | 0.008 | 0.013 | 0.013 | 0.010 | 0.019 | 0.035 |
| | (0.031) | (0.031) | (0.031) | (0.029) | (0.032) | (0.031) | (0.034) |
| Election year −3 | 0.001 | 0.002 | 0.008 | 0.008 | 0.005 | 0.007 | 0.016 |
| | (0.017) | (0.017) | (0.017) | (0.016) | (0.017) | (0.017) | (0.021) |
| Election year −2 | 0.015 | 0.016 | 0.030* | 0.029* | 0.025 | 0.024 | 0.033* |
| | (0.015) | (0.015) | (0.016) | (0.015) | (0.016) | (0.016) | (0.019) |
| Election year −1 | 0.032** | 0.031** | 0.041** | 0.034** | 0.036** | 0.033** | 0.042** |
| | (0.014) | (0.014) | (0.016) | (0.015) | (0.016) | (0.017) | (0.019) |
| Election year +1 | -0.012 | -0.013 | -0.003 | -0.007 | -0.007 | -0.003 | -0.010 |
| | (0.016) | (0.016) | (0.016) | (0.015) | (0.016) | (0.016) | (0.019) |
| Baseline investment in EUR per capita | 76.37 | 76.37 | 75.83 | 95.99 | 75.83 | 71.06 | 79.92 |
| SOE fixed effects | x | x | x | x | x | x | x |
| Year fixed effects | x | x | x | x | x | x | x |
| SOE controls | | x | x | x | x | x | x |
| Municipality controls | | | x | x | x | x | x |
| Incl. minority interests | | | | x | | | |
| Municipality fixed effects | | | | | x | | |
| Number of observations | 104,051 | 104,051 | 103,412 | 118,096 | 103,367 | 86,542 | 69,707 |
| R-squared | 0.765 | 0.765 | 0.766 | 0.774 | 0.772 | 0.773 | 0.746 |

Notes: *** p<0.01, ** p<0.05, * p<0.1. Results correspond to Equation 3.1. Standard errors clustered by municipality. Model (5) is limited to SOE observations that are part of the dataset every year over the entire sample period from 2002 to 2019. For model (6), all observations are dropped that experienced a change in the owner during the considered period.

the dynamics in SOE investment two years and one year before an election are almost unchanged by this sample adjustment.

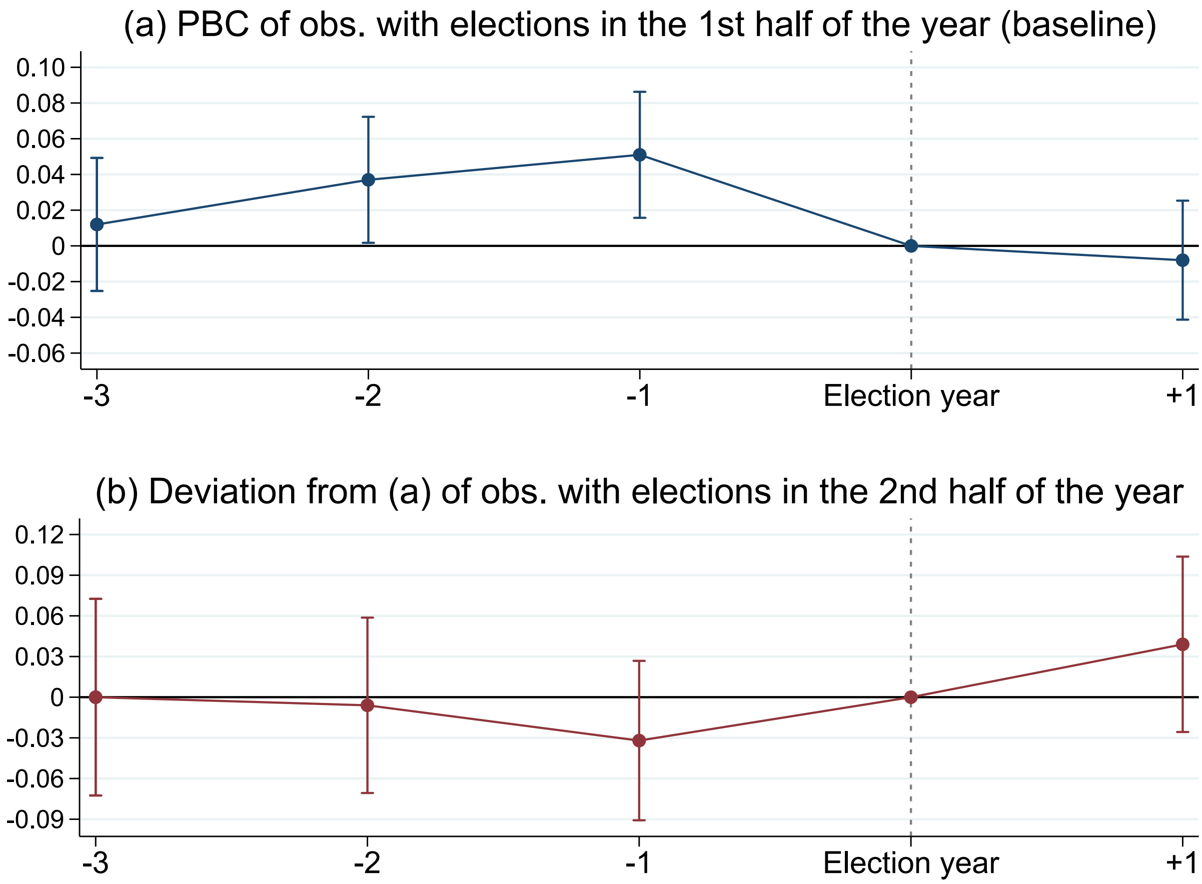
In addition to Table 3.2, we test whether the identified effects are driven by observations from a single state by dropping all SOEs that belong to municipalities from the same state one by one. The results are presented in Appendix Table A3.1 and reject this hypothesis. The effect in the pre-election year is quite stable in magnitude and varies from 3.7% to 4.8% (compared to 4.1% for the full sample). The effect two years before an election ranges from 2.5% to 4.1% (compared to 3.0% for the full sample), but remains statistically insignificant at conventional levels in all but two of the 13 regressions.[23]

### 3.5.2 Heterogeneities in the SOE Election Cycle

In this section, we consider heterogeneities among SOEs along three dimensions in order to learn more about the drivers and mechanisms behind our results. In particular, we examine differential dynamics of SOE investment by (1) the timing of local elections within the election year, (2) the distribution of seats in local councils, and (3) the economic sector associated with an SOE. To this end, we augment our main econometric model to include an interaction of the electoral cycle

[23]Some PBC papers also consider vertical interactions between municipal elections and higher-level elections for regional or state parliaments (e.g., Foremny and Riedel, 2014; Cahan, 2019). Some of them find an effect of higher-level elections on municipal policy or a stronger local PBC for politically aligned jurisdictions. We test for the influence of state-level elections once by including a dummy for state election years and once by modeling the full electoral cycle for the states as we do for municipalities. In both cases, we find no increase in investment of municipal SOEs around state elections, while the PBC around municipal elections is robust and larger than in models where we do not control for state elections (results are available upon request).

**Figure 3.3:** Election cycle in SOE investment – heterogeneity by election date within the election year




Notes: Coefficient estimates and 95% confidence intervals based on Equation 3.1, but including an interaction term that separately models the election cycle for SOEs where local elections at the owner level were held during the second half of the year. Observations where elections took place during the first half of the year represent the baseline (see Panel (a)). Panel (b) shows the interaction terms. Standard errors clustered by municipality.

dummies with dummies for the respective subgroups of the sample (see Equation 3.3 in Section 3.4.3).[24]

Figure 3.3 starts with an analysis that looks at the different timing of municipal elections within an election year. For about a fifth of the observations (20.1%), elections are held in the second half of the year, leaving more time to invest in the early months of the election year to signal good performance or to please specific voter groups and constituencies. As a result, this increases the baseline (i.e., investment in the election year) in our main specification and should therefore reduce the cyclical pattern in the years preceding an election (see Hessami, 2018, for a similar test). Figure 3.3 confirms this hypothesis. Panel (a) depicts the dynamics of SOE investment for observations with elections early in the year and shows that the cyclical pattern is more pronounced compared to our main specification, which looks at average effects (increase of 5.1% compared to 4.1% in the pre-election year). Panel (b) of Figure 3.3 documents the interaction terms, confirming our expectation that the effect in the pre-election year should be smaller for observations with elections late in the year. The difference is not statistically significant at conventional levels.

[24]To avoid problems in interpreting estimates with small sample sizes, we do not report estimates for the year four years prior to an election in all of the analyses in this section, as it can only be estimated for at most three of the 13 area states considered (details see Section 3.4.3).

Figure 3.4 continues our heterogeneity analysis by looking into the relevance of political majorities in the municipal councils for SOE investment. It differentiates between SOEs where the owner's governing body is dominated by either right-wing or left-wing parties, or where there is no clear left-right majority. This relates to the large literature examining the role of party ideology for policymaking and public spending (see, e.g., Potrafke, 2018, who reviews the literature for the US and finds mixed evidence for the hypothesis that left-wing governments tend to adopt expansionary policies more often and have higher spending). Following the common argument in this literature, one might expect a stronger PBC in SOEs owned by a municipality with a left-dominated council. On the contrary, Veiga et al. (2019) and Lopes da Fonseca (2020) discuss the possibility that politicians mostly act in line with their ideological preferences only when risks are low (e.g., little competition or term limits), so that right-wing politicians may also increase spending before elections.

Figure 3.4 shows the evidence for German SOEs, where right-wing dominated councils (including members of the CDU/CSU, FDP, and AfD) serve as the baseline.[25] They exhibit an investment cycle similar to the average cycle shown in Figure 3.2 above, but the effect size of the pre-election year is much larger at 6.4% compared to 4.1% for the full sample (see Panel (a) of Figure 3.4). Panel (b) shows the interaction terms and, thus, the deviation from the pattern depicted in Panel (a). The solid red line suggests that left-wing dominated councils (including members of the SPD, PDS/DIE LINKE, and B90/GRÜNE) show, if anything, a less pronounced cyclical pattern in SOE per capita investment than SOEs in municipalities with a right-wing dominated council. However, the effect is not statistically significant at conventional levels. The same conclusion is reached for the majority of SOEs, where there is no clear left-right majority in the owner's council.[26] These results tend to support a stronger SOE investment cycle in municipalities with a right-wing dominated council. In the specific case of SOEs, this could also be explained by the possibility that right-wing parties face a stronger backlash from their voter base if they fail to balance the core budget or if they visibly spend significant additional amounts in the core budget before elections and, as a result, use SOEs. Similarly, they may focus more on investment for electioneering purposes, whereas a left-wing council may exhibit a stronger cycle in social spending or public employment.[27]

The final heterogeneity analysis considers SOE investment separately by economic sector to learn more about which types of SOEs invest more heavily in the months before an election. From a political economy perspective, one would expect increases in SOE investment in the pre-election year to be concentrated in sectors that are particularly visible to voters or that are considered particularly important by (subgroups of) the electorate. We operationalize the analysis into economic sectors by differentiating SOEs in terms of their classification according to the internationally standardized NACE system.

Figure 3.5 presents the results, with confidence intervals omitted for better readability.[28] A particularly distinct cycle is observed for SOEs in the transportation and storage sector. These are almost exclusively SOEs that provide road passenger transport services or support activities

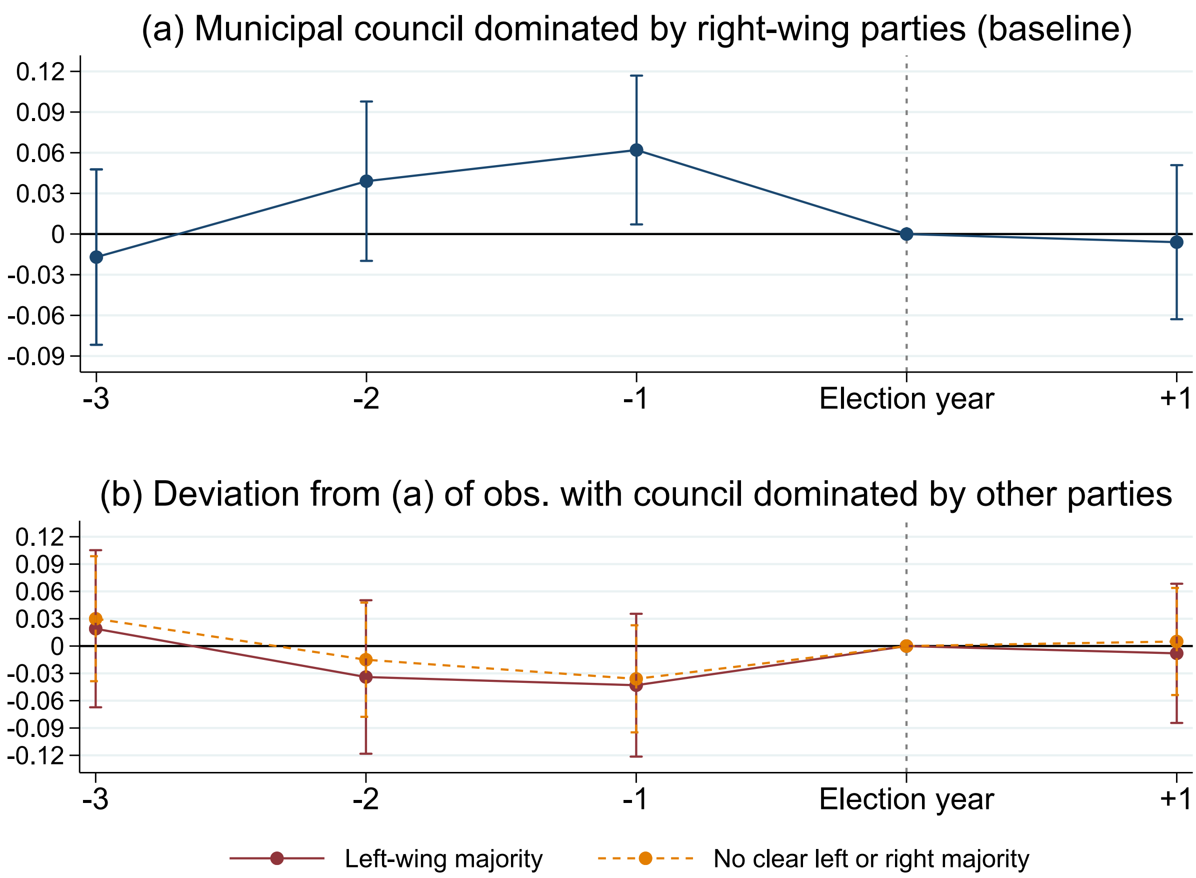
[25] On average, municipalities are more often governed by right-wing dominated councils (21.4%) than by left-wing dominated councils (16.4%). In most cases, however, there is no clear left-right majority, as smaller regional parties and independent counselors play an important role in German local politics. The results are similar when only the majorities of the two largest parties (right: CDU/CSU, left: SPD) are considered, although observed differences are somewhat larger. Results are available upon request.

[26] With a similar focus on political differences between owners, we test for a differential PBC in SOEs owned by municipalities from former East Germany, where state ownership of firms was more the norm rather than the exception. We find no economically or statistically significant differences between SOEs from former East and West Germany with respect to cyclical investment patterns. Results are available upon request.

[27] This relates to an ongoing research project of the authors in which we are investigating a potential PBC in SOE employment and its distributional implications for employees.

[28] Complete results, including information on the statistical precision of the estimates, are documented in Appendix Table A3.2. Note that for ease of interpretation and to reduce the complexity of the econometric model, we rely on sample splits for the sectoral analysis.

**Figure 3.4:** Election cycle in SOE investment – heterogeneity by political majorities in the municipal council



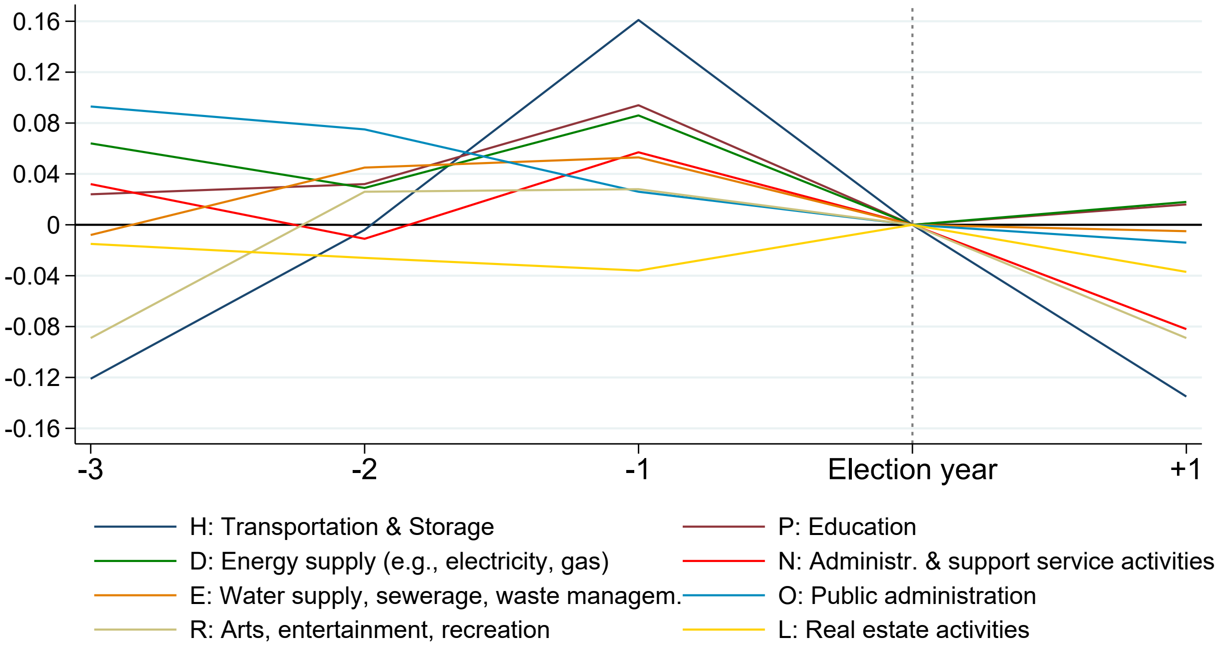

Notes: Coefficient estimates and 95% confidence intervals based on Equation 3.1, but including interaction terms that separately model the election cycle for SOEs where the owner's council is dominated by (1) left-wing parties or (2) has no clear left or right majority. Observations where the council is dominated by right-wing parties represent the baseline (see Panel (a)). Panel (b) shows the interaction terms. Standard errors clustered by municipality.

for transportation.[29] The cyclical pattern for these observations is quite pronounced, with per capita investment first falling by -12.1% after an election and then rising sharply by 16.1% in the pre-election year. Other sectors that show a strong cyclical pattern are education, energy, and sector N, which at the municipal level mainly includes landscape service and cleaning activities, the organization of conventions and trade shows, and the activities of employment placement agencies. The SOEs classified in the education sector predominantly provide services in the areas of sports, cultural, and recreation education as well as pre-primary education (e.g., day-care centers). Overall, these results suggest a shift in the portfolio of SOE investments towards more visible categories prior to elections.

Sectors E (water supply, sewerage, and waste management) and R (dominated by sports, amusement, and recreation activities) show a distinct cycle where there is a sharp increase in investment figures already two years before an election. While it may be reasonable to increase investment at this early stage in order to spread it out a little, it is not clear why it is concentrated in these two sectors. Finally, one might also expect a cyclical pattern of investment in sector L, which includes SOEs that rent and operate public real estate. Yet, SOE investment in this sector is fairly stable along the electoral cycle. Reasons for this could be that such investments only benefit individual households rather than a larger electoral base and are therefore less attractive

[29]This finding relates well to the results of Veiga and Veiga (2007), who identify a strong PBC for investment in overpasses, roads, and ancillary works for municipal core budgets in Portugal.

**Figure 3.5:** Election cycle in SOE investment – heterogeneity by economic sectors (selection)




Notes: Coefficient estimates based on Equation 3.1. Separate regressions for each economic sector (measured by the first digit of the NACE classification system). Confidence intervals omitted for better readability (detailed regression results are documented in Appendix Table A3.2).

for strategic manipulation. Similar to previous papers, we find no pre-election increase for the general public administration sector (e.g., Khemani, 2004).

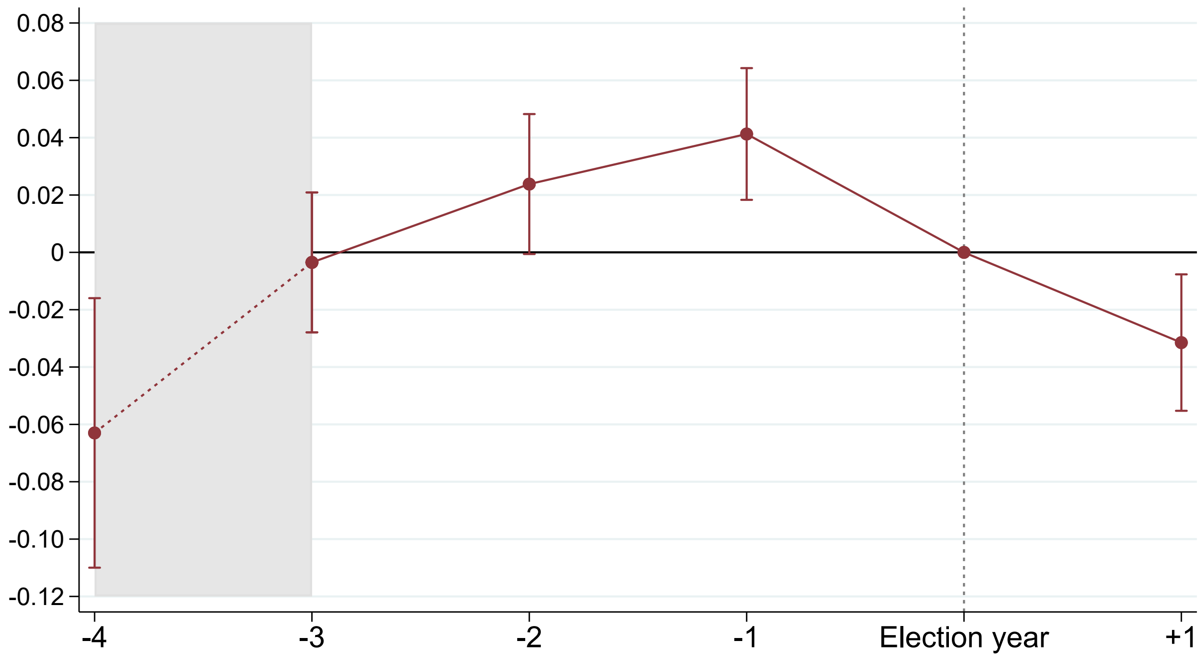
In terms of absolute magnitude, the electoral cycle has the largest effect for sectors D (energy) and E (water, sewerage, and waste management), as these are the sectors with the highest levels of per capita investment and where the majority of SOEs are active (see Table A3.2 in the Appendix). Hence, while these two sectors dominate the election cycle effect in terms of per capita euro values, it is the transportation and education sectors, which are highly visible to voters and often considered as particularly important in political debates, that show the most pronounced cyclical pattern in relative terms.

### 3.5.3 PBC in Core Budget Investment

In this section, we present additional results that support the comparison of the dynamics of investment decisions along the electoral cycle of SOEs with those of their owners; the core budgets of local governments. The latter sample is the focus of the numerous published articles that study a PBC in public investment. The combination of both perspectives, that of SOEs and that of core budgets, allows to obtain insights on the election cycle in public investment of the public sector as a whole.

In the context of this comparison, two different perspectives are of interest: First, we seek to understand the relative importance of SOEs for the PBC in public investment in German municipalities that have both instruments at their disposal (i.e., investment by SOEs and core budgets). For this first perspective, the appropriate core budget sample consists of all municipalities that manage at least one SOE. This sample is the focus of the subsequent analysis. Second, we are interested in the contribution of SOEs to the *total* election cycle of the municipal public sector. The relevant core budget sample in this case consists of all municipalities in Germany, regardless of whether they own an SOE or not. An analysis of this sample allows conclusions to be drawn about the importance of SOEs for the PBC in the entire public sector at the municipal level. In addition, this second perspective allows to study whether investment by SOEs complements or substitutes for investment by core budgets. Substitution in this context would imply that some

**Figure 3.6:** Election cycle in public investment – core budgets




Notes: Coefficient estimates and 95% confidence intervals (see also model (2) in Table 3.3). Standard errors clustered by municipality. The shaded area on the left indicates that the typical election cycle lasts only five years (i.e., from -3 to +1). The identifying variation for the coefficient four years before an election comes only from municipalities in the state of Bavaria as well as from one extended electoral cycle in the states of North Rhine-Westphalia and Brandenburg, respectively (details see Section 3.4.3). Reduced sample period from 2006 to 2019 due to data availability issues for municipal core budgets.

of the election-induced investment spending in the core budgets of municipalities is outsourced to SOEs. As a result, municipalities that own at least one SOE would exhibit a smaller PBC in their core budgets than municipalities that do not own an SOE. Complementarity, on the other hand, would imply that the PBC in SOE investment serves a different purpose than investment by core budgets, so that municipalities that manage an SOE would have a higher *overall* PBC in public investment than municipalities that do not own an SOE. The sample required for investigating this second perspective requires considerable additional data collection effort, so at this stage we can only hypothesize about this second comparison.

Figure 3.6 shows the election cycle in public investment for the municipal owners of SOEs. Already two years before an election year, investment is 2.4% higher than in the election year. However, in line with expectations and previous empirical evidence, investments of core budgets are even higher in the pre-election year, in our case by 4.1%, which amounts to EUR 12.90 per capita. After an election year, investments decrease by 3.1%. Table 3.3 documents the robustness of these results. Model (2) is the main specification, also shown in Figure 3.6. Model (3) shows that the results are similar when excluding observations that merged, were newly created, or did not own an SOE in *all* years during the considered sample period from 2006 to 2019.

In the next step, we are interested in comparing the magnitude of the PBC in SOEs with that in core budgets to learn about the big picture. Since for the core budgets, we consider all municipalities who are the main owner of at least one SOE, the most comparable model from the SOE analysis is column (4) of Table 3.2, where we also include minority interests. This comparison suggests that per capita SOE investment (at the municipal level) is EUR 5.77 higher in the pre-election year than in the baseline year, compared to EUR 12.90 for core budgets.[30] This

[30]This calculation compares only the figures for the pre-election year, for which we find robust effects for both samples.

**Table 3.3:** Election cycle in public investment – core budgets

| | Unbalanced panel | | Balanced panel |
|---|---|---|---|
| | (1) | (2) | (3) |
| Election year −4 | -0.032 | -0.063*** | -0.061** |
| | (0.025) | (0.024) | (0.025) |
| Election year −3 | 0.003 | -0.004 | 0.004 |
| | (0.012) | (0.013) | (0.013) |
| Election year −2 | 0.031*** | 0.024* | 0.031** |
| | (0.012) | (0.012) | (0.013) |
| Election year −1 | 0.052*** | 0.041*** | 0.043*** |
| | (0.010) | (0.012) | (0.012) |
| Election year +1 | -0.023* | -0.031*** | -0.028** |
| | (0.012) | (0.012) | (0.013) |
| Baseline investment, EUR p.c. | 314.59 | 314.59 | 312.92 |
| Municipality fixed effects | x | x | x |
| Year fixed effects | x | x | x |
| Municipality controls | | x | x |
| Number of observations | 24,696 | 24,696 | 22,140 |
| R-squared | 0.630 | 0.633 | 0.626 |

Notes: *** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered by municipality. The dummy to capture whether a municipality applies cash or accrual accounting is included in all regressions as investment figures are comparable over time only within the two accounting systems (details see 3.4.2). Model (3) is limited to local governments that are part of the dataset every year over the considered period. Reduced sample: restricted to the period 2006–2019 and municipalities with at least five thousand inhabitants due to data availability issues for municipal core budgets.

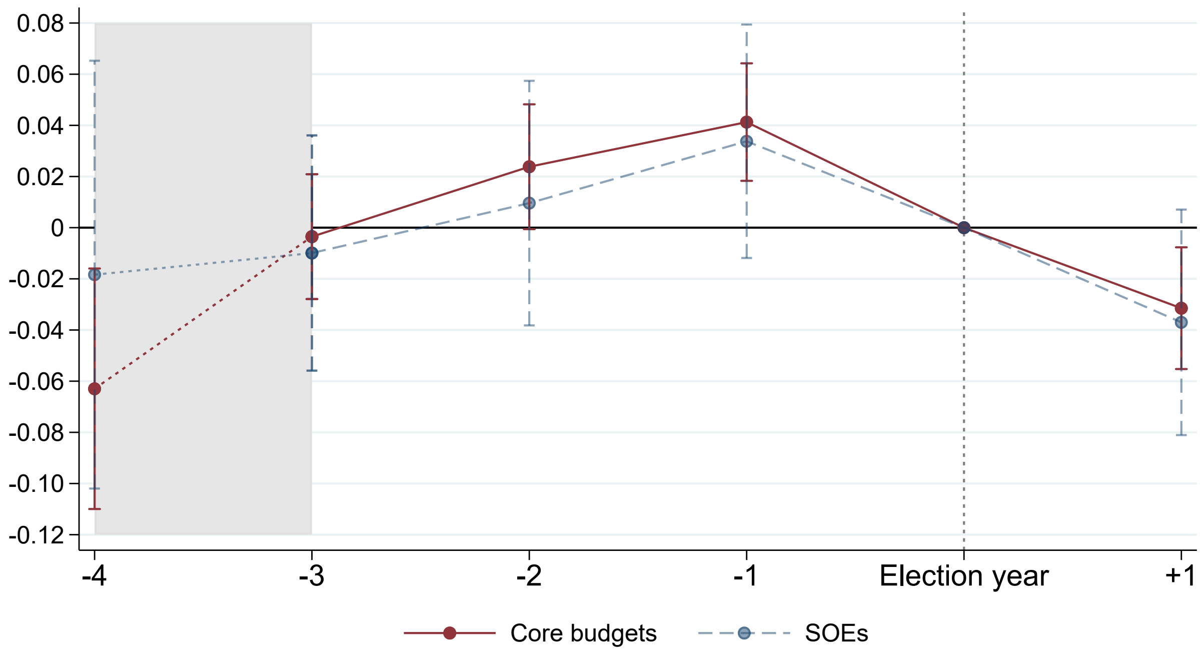
would imply that the total effect for the pre-election year is EUR 18.67, of which SOEs account for 30.9%.

There are a number of issues that limit the comparability of the figures discussed above, some of which we can address, and others for which we can only get a rough idea about their impact. First, data on core budget investment are only available for a reduced period from 2006 to 2019 and for municipalities with at least 5,000 inhabitants. The underlying sample for SOEs is therefore somewhat larger. To address this issue, we re-estimate the relevant SOE model, restricting the sample to the years and owners included in the core budget analysis. Since municipalities manage on average 2.5 SOEs, we also aggregate SOE investment to the municipal level to improve comparability.[31] A second issue is already briefly discussed in Section 3.4.1, namely that average per capita investment by SOEs in our sample is significantly below the population average (EUR 150 versus EUR 360 at the municipal level). Assuming that the relative investment dynamics along the electoral cycle are the same for our sample and the full sample, we are significantly underestimating the election cycle of SOEs in per capita terms. As a simple, albeit unsatisfactory, solution, we can multiply our effects by per capita investment in the full sample to get an idea of what the true effect might be for SOEs.[32]

[31] In particular the latter adjustment significantly reduces the precision of the SOE estimates as we can no longer control for firm-specific characteristics.

[32] To get an idea of the validity of such a solution, we compare the results of a regression that we can run on the full sample with the results using our sample and the same specification (i.e., absolute instead of per capita investment and excluding all owner-level control variables). This analysis shows a pre-election year effect that is almost identical for the full sample and our sample. Moreover, the effects for the earlier years in the electoral cycle are negative for the full sample, making the cyclical pattern even more pronounced than in our main analysis. Results are available upon request.

**Figure 3.7:** Election cycle in public investment – core budgets and SOEs




Notes: Figure based on Equation 3.1 but at the municipality level (i.e., excluding firm-level controls for the SOE regression and using aggregated investment figures for SOEs). The shaded area indicates that the typical election cycle lasts only five years. The identifying variation for the coefficient four years before an election therefore only comes from municipalities in the state of Bavaria as well as from one extended electoral cycle in the states of North Rhine-Westphalia and Brandenburg, respectively. Reduced sample: restricted to the period 2006–2019 and municipalities with at least five thousand inhabitants due to data availability issues for municipal core budgets. Baseline investment of SOEs (core budgets) is EUR 150.26 (EUR 314.59) per capita.

Figure 3.7 shows the results for both the core budget analysis discussed above and the adjusted SOE analysis at the municipal level: The pre-election year effect for SOEs is EUR 5.11 (or 3.4% in relative terms), similar to the effect estimated at the firm level and for the larger sample. All in all, simple back-of-the-envelope calculations then suggest that we significantly underestimate the *total* PBC in public investment when only considering core budgets: The *total* effect for the pre-election year is on average more than a third (39.6%) higher when SOEs are also included in addition to the core budgets. Using the per capita investment figures for the *full* SOE sample as a baseline, this figure rises to 94.8% (i.e., the effect of the election cycle in euro values is almost as large for SOEs as for the core budgets). The assessment, that we underestimate the total PBC in municipal investment by about one third if we focus only on core budgets can therefore be taken as a lower bound estimate. These conclusions apply to the sample of municipalities that are the main owners of at least one SOE. In sum, our results on public investment through SOEs and core budgets suggest that both are attractive instruments for politicians in the sense that both are used strategically in the context of municipal elections in order to secure the positions of politicians' and possibly also SOE managers.

As noted above, in order to assess the relevance of SOEs for the *overall* PBC in public investment at the municipal level, regardless of whether a municipality owns an SOE or not, one would also have to examine the PBC in municipalities without an SOE. This goes beyond the possibility of this version of the paper. Yet, from a theoretical point of view, core budgets without an SOE might exhibit a stronger PBC in investment than their SOE-owning counterparts as they cannot finance parts of these activities in SOEs. This would decrease the average relative importance of SOEs for the *overall* PBC compared to the estimates presented in this section. Further research is needed to test this hypothesis.

## 3.6 Discussion and Conclusion

We examine the investment behavior of the public sector at the municipal level in Germany when two instruments are available: SOEs and the core budget of governments as the municipal owners of SOEs. While SOEs are influenced by public decision makers in their agenda setting and are in principle part of the public sector, their role and activities are much less frequently discussed in public and academic debates or in official statistics, which typically concentrate on governments' core budgets (see also Asatryan et al., 2022). We argue that the relatively hidden financing activities of SOEs, among other things, make them particularly attractive as an instrument for electioneering and investigate whether SOEs' investment dynamics follow the electoral cycle of their owners, with higher investment in the run-up to an election in order to increase the re-election chances of incumbent politicians.

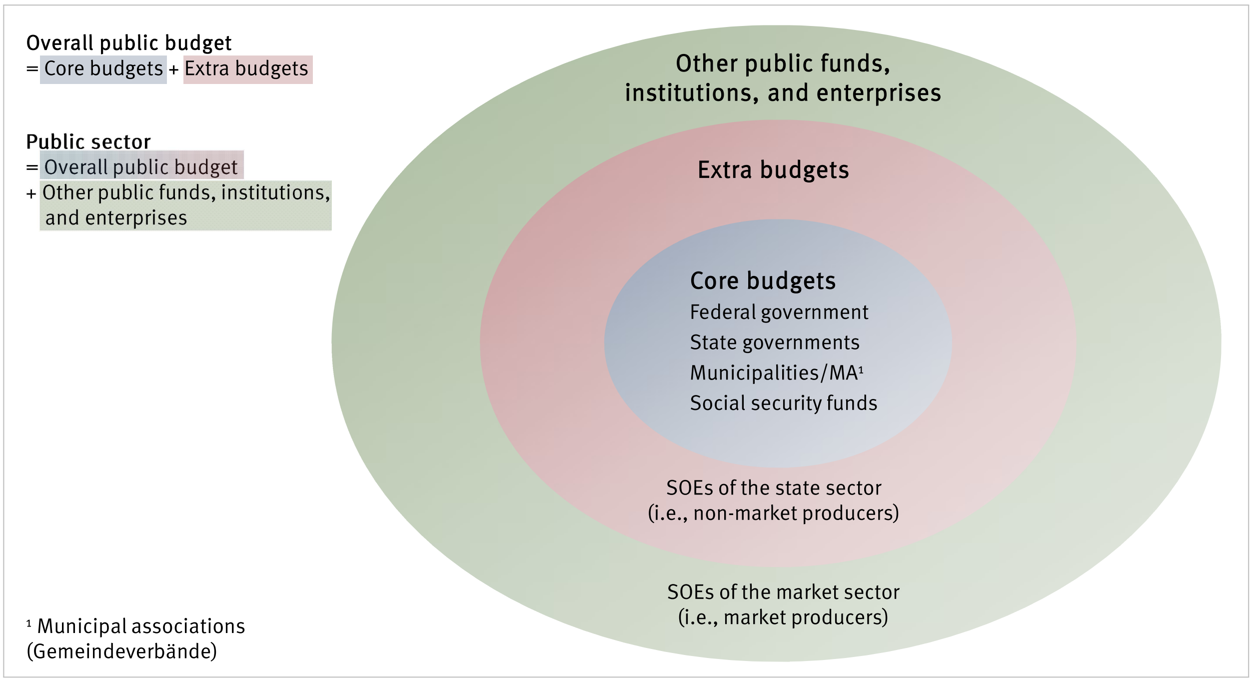
Our empirical strategy exploits the fact that council election years differ across German states, allowing us to disentangle election-induced investment increases from general trends over time. Using administrative microdata on both SOEs and core budgets over a maximum period of 18 years (2002–2019), we identify robust PBCs in both instruments that are of similar magnitude in relative terms: Per capita investment by SOEs (core budgets) is 3.4% to 4.1% (4.1%) higher in the year preceding an election, relative to the election year that serves as our baseline in an event study modeling the entire electoral cycle of local governments. Various robustness tests and heterogeneity analyses support the interpretation that public investment – through both SOEs and core budgets – is strategically used to improve the re-election prospects of incumbent counselors.

Our results speak in favor of the hypothesis that municipal council elections are the driving force behind the identified dynamics in public investment. This leads us to conclude that political influence, not just market forces, has a significant impact on SOE investment. Previous research has shown that this is likely to lead to inefficiencies (e.g., Alok and Ayyagari, 2020). Following Asatryan et al. (2022), we also argue that heavy use, combined with the politicization of SOE activity, leads to significant intransparency with respect to public sector finances. From a policy perspective, this calls for careful attention to the institutional details that regulate the outsourcing behavior of core budgets in order to avoid inefficiencies and deficiencies in the effective governance of outsourced units.

While our analysis provides a comprehensive overview of PBCs in municipal-level investment in Germany, we identify four relevant and directly related extensions that may be the focus of future versions of the paper. First, returning to our theoretical discussion in the introduction, we can say little about the relative importance of *internal* (SOE managers) versus *external* (political owners) motives for our results. One way forward is to follow Li et al. (2020) and use Bureau van Dijk's Amadeus database, which also contains information on board members of a subsample of SOEs, to link board turnover to electioneering and election outcomes. Second, a natural extension of the data is to include all German municipalities, regardless of whether they own an SOE or not (and regardless of population size). This facilitates a discussion of the more general and overall relevance of SOEs for the PBC in public investment. Third, a perennial challenge in the PBC literature concerns the financing perspective. In essence, the question is whether a PBC leads to a longer-term increase in debt or whether a PBC is "financially neutral" in the sense that investment is simply strategically shifted across the electoral cycle (intertemporal shifting) or across expenditure categories (e.g., Drazen and Eslava, 2010), which may nevertheless give rise to inefficiencies. The identification challenge is that the counterfactual for investment and other financial variables in the absence of electoral incentives is unobservable. The focus on SOEs, with richer data on the financial situation of the enterprises relative to core budgets, may provide an opportunity for new insights on the financing part, if a suitable control group can be identified. This also relates to the fourth and final aspect: Recent studies have shown that public investment can crowd-in or crowd-out private investment, depending on the institutional setting or the type of investment (e.g., Bircan and Saka, 2021; Morais et al., 2021; Datta, 2023). In light of this recent evidence, we intend to examine the effect of the PBC in municipal public investment on private investment at the regional level to learn about the broader economic impact of PBCs.

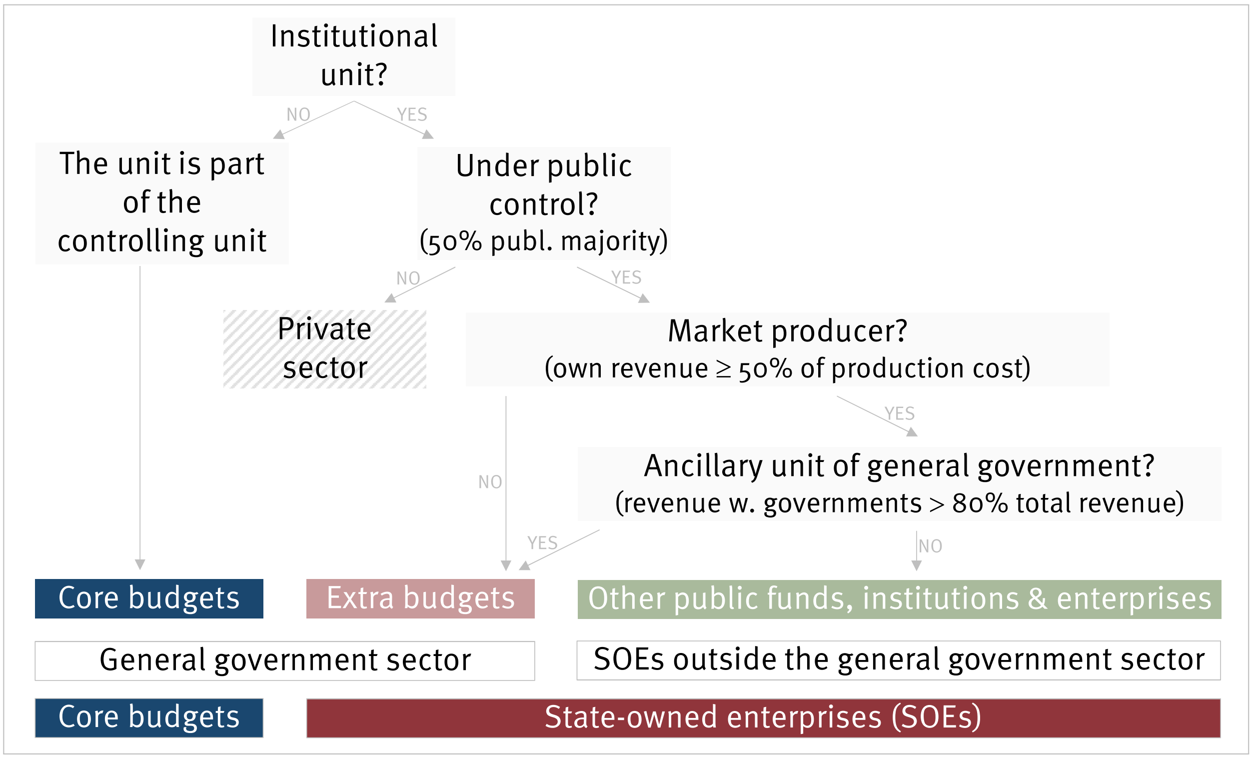
# Appendix

**Figure A3.1:** The public sector in Germany – sector accounts




Notes: Own figure based on Federal Statistical Office (2019). The term "SOE" describes and comprises all units from the two outer layers (i.e., market- and non-market producers).

**Figure A3.2:** Categorization scheme for government sector accounts




Notes: Own figure based on Schmidt et al. (2017). The figure summarizes the criteria for the classification of public institutional units (any institution or enterprise subject to some form of government involvement) into core budgets, non-market-producer SOEs, and market-producer SOEs. Ancillary units of general government are companies that generate more than 80% of their revenue by making business with government units.

**Table A3.1:** Election cycle in SOE investment – individual states excluded

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) | (13) | 14) |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Election year $-4$ | 0.013 | 0.013 | -0.002 | 0.009 | 0.017 | 0.008 | 0.017 | 0.016 | 0.014 | 0.019 | 0.015 | 0.014 | 0.016 | 0.014 |
| | (0.031) | (0.032) | (0.033) | (0.037) | (0.034) | (0.032) | (0.032) | (0.043) | (0.032) | (0.033) | (0.032) | (0.032) | (0.031) | (0.032) |
| Election year $-3$ | 0.008 | 0.008 | -0.001 | 0.004 | 0.014 | 0.004 | 0.013 | 0.010 | 0.008 | 0.017 | 0.006 | 0.009 | 0.006 | 0.006 |
| | (0.017) | (0.018) | (0.019) | (0.018) | (0.021) | (0.018) | (0.018) | (0.018) | (0.017) | (0.018) | (0.017) | (0.017) | (0.017) | (0.017) |
| Election year $-2$ | 0.030* | 0.025 | 0.026 | 0.037** | 0.027 | 0.029* | 0.031* | 0.033* | 0.031* | 0.041** | 0.028* | 0.029* | 0.031* | 0.030* |
| | (0.016) | (0.017) | (0.018) | (0.017) | (0.019) | (0.016) | (0.017) | (0.017) | (0.016) | (0.016) | (0.016) | (0.016) | (0.016) | (0.016) |
| Election year $-1$ | 0.041** | 0.040** | 0.038** | 0.048*** | 0.043** | 0.039** | 0.037** | 0.038** | 0.042** | 0.048*** | 0.038** | 0.038** | 0.041** | 0.039** |
| | (0.016) | (0.017) | (0.018) | (0.017) | (0.018) | (0.017) | (0.018) | (0.017) | (0.016) | (0.017) | (0.016) | (0.016) | (0.016) | (0.016) |
| Election year $+1$ | -0.003 | -0.005 | -0.027 | -0.001 | 0.010 | -0.007 | -0.005 | 0.007 | -0.005 | 0.005 | -0.005 | -0.000 | -0.001 | -0.003 |
| | (0.016) | (0.018) | (0.017) | (0.016) | (0.018) | (0.016) | (0.017) | (0.018) | (0.016) | (0.017) | (0.016) | (0.016) | (0.016) | (0.016) |
| SOE fixed effects | x | x | x | x | x | x | x | x | x | x | x | x | x | x |
| Year fixed effects | x | x | x | x | x | x | x | x | x | x | x | x | x | x |
| SOE controls | x | x | x | x | x | x | x | x | x | x | x | x | x | x |
| Municipality controls | x | x | x | x | x | x | x | x | x | x | x | x | x | x |
| Number of observations | 103,412 | 99,869 | 96,257 | 86,334 | 95,735 | 93,949 | 80,098 | 93,092 | 100,912 | 98,482 | 100,111 | 96,589 | 99,740 | 99,776 |
| R-squared | 0.766 | 0.765 | 0.766 | 0.750 | 0.765 | 0.756 | 0.788 | 0.767 | 0.766 | 0.765 | 0.769 | 0.768 | 0.768 | 0.767 |
| Excluded state | – | *Schleswig-Holstein* | *Lower Saxony* | *North Rhine-Westphalia* | *Hesse* | *Rhineland-Palatinate* | *Baden-Württemberg* | *Bavaria* | *Saarland* | *Brandenburg* | *Mecklenburg-Vorpommern* | *Saxony* | *Saxony-Anhalt* | *Thuringia* |

Notes: *** p<0.01, ** p<0.05, * p<0.1. Results correspond to Equation 3.1. Standard errors clustered by municipality. Column (1) represents the main specification (see also model (3) in Table 3.2). Columns (2) to (14) exclude all observations that belong to the respective state one by one.

**Table A3.2:** Election cycle in SOE investment – heterogeneity by economic sectors (selection)

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|---|---|---|---|---|---|---|---|
| NACE sector | D | E | H | L | N | P | R |
| Election year −3 | 0.064 | -0.008 | -0.121 | -0.015 | 0.032 | 0.024 | -0.089 |
| | (0.041) | (0.022) | (0.149) | (0.061) | (0.118) | (0.123) | (0.079) |
| Election year −2 | 0.029 | 0.045** | -0.004 | -0.026 | -0.011 | 0.032 | 0.026 |
| | (0.038) | (0.021) | (0.143) | (0.060) | (0.135) | (0.114) | (0.071) |
| Election year −1 | 0.086** | 0.053** | 0.161 | -0.036 | 0.057 | 0.094 | 0.028 |
| | (0.037) | (0.022) | (0.144) | (0.058) | (0.117) | (0.112) | (0.068) |
| Election year +1 | 0.018 | -0.005 | -0.135 | -0.037 | -0.082 | 0.016 | -0.089 |
| | (0.036) | (0.022) | (0.143) | (0.058) | (0.080) | (0.123) | (0.072) |
| Baseline investment in EUR per capita | 102.17 | 101.61 | 44.94 | 63.66 | 47.75 | 6.10 | 32.64 |
| SOE fixed effects | x | x | x | x | x | x | x |
| Year fixed effects | x | x | x | x | x | x | x |
| SOE controls | x | x | x | x | x | x | x |
| Municipality controls | x | x | x | x | x | x | x |
| Number of observations | 10,633 | 42,187 | 1,906 | 14,785 | 2,559 | 1,186 | 8,116 |
| R-squared | 0.703 | 0.650 | 0.715 | 0.647 | 0.803 | 0.723 | 0.650 |
| Description of NACE sector | Energy supply, (electricity, gas) | Water supply, sewerage, waste managem. | Transportation and storage | Real estate activities | Administrative and support service activities | Education | Arts, entertainment, recreation |

Notes: *** p<0.01, ** p<0.05, * p<0.1. Results correspond to Equation 3.1. Separate regressions for each economic sector (measured by the first digit of the NACE classification system). Standard errors clustered by municipality.

**Table A3.3:** Summary statistics – core budgets

| Variable | N | Mean | Std. dev. | 1st perc. | Median | 99th perc. |
|---|---|---|---|---|---|---|
| Population | 24,735 | 29,097 | 67,249 | 4,902 | 14,088 | 294,627 |
| Share population >65 years | 24,735 | 0.212 | 0.032 | 0.149 | 0.209 | 0.309 |
| Share working age population | 24,735 | 0.617 | 0.022 | 0.554 | 0.618 | 0.674 |
| Population density | 24,735 | 485.3 | 476.8 | 45.2 | 314.7 | 2,271.1 |
| Turnout (in previous election) | 24,735 | 0.531 | 0.073 | 0.369 | 0.529 | 0.710 |
| *Share of seats in local council by political party* | | | | | | |
| CDU/CSU | 24,735 | 0.357 | 0.131 | 0.000 | 0.360 | 0.667 |
| SPD | 24,735 | 0.241 | 0.122 | 0.000 | 0.236 | 0.545 |
| FDP | 24,735 | 0.038 | 0.048 | 0.000 | 0.025 | 0.196 |
| B90/GRÜNE | 24,735 | 0.066 | 0.067 | 0.000 | 0.062 | 0.244 |
| PDS/DIE LINKE | 24,735 | 0.027 | 0.064 | 0.000 | 0.000 | 0.306 |
| AfD | 24,735 | 0.004 | 0.020 | 0.000 | 0.000 | 0.108 |
| Others/indep. counselors | 24,735 | 0.267 | 0.213 | 0.000 | 0.222 | 1.000 |
| Accrual accounting system | 24,735 | 0.493 | 0.500 | 0 | 0 | 1 |

Notes: Confidentiality policies of the German statistical offices do not permit the reporting of minima and maxima. Instead, we report the 1st and 99th percentiles. Reduced sample: restricted to the period 2006–2019 and municipalities with at least five thousand inhabitants due to data availability issues for municipal core budgets.

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Hiermit erkläre ich, dass ich die eingereichte Dissertation mit dem Titel "Essays on Empirical Public Economics" selbständig angefertigt und die benutzten Hilfsmittel vollständig und deutlich angegeben habe.

(Justus Nover)

## Curriculum Vitae

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