

Direct Democracy, Voter Turnout and Representative Behavior

Essays in Political Economics

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Zusammenarbeit mit Koautoren und Vorveröffentlichungen

Die vier Kapitel dieser Dissertation basieren auf Arbeiten, die bereits an anderer Stelle – entweder als Working Paper oder in wissenschaftlichen Fachzeitschriften – publiziert wurden. Die folgende Aufzählung gibt Aufschluss über diese Vorveröffentlichungen. Ebenso wird aus ihr deutlich, welche (und wie viele) Ko-Autoren an welchem Projekt beteiligt waren.

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Chapter 0

General Introduction

0.1 Preliminary Remarks

Political economics (often also called public choice) studies the interplay of economics and politics. In the *New Palgrave Dictionary of Economics*, Gordon [Tullock](#) (2008) – one of the pioneers in the field – notes:

[...] Public choice uses economic tools to deal with the traditional problems of political science.

A similar approach is taken in this dissertation. Albeit being an economist by training, I have long been interested in classical political science questions. In my research, I try to employ economic methods to explain political phenomena.

The way of thinking varies between economists and political scientists. Therefore, both fields can profit from researchers that have overlapping interests: Economists are introduced to new topics. For them, it is interesting to see that their methods and assumptions can be transferred to a completely different field. Political scientists, on the other hand, can benefit from new answers to longstanding questions. My goal is to contribute to a fruitful collaboration of both fields. Although the focus of my work is largely empirical, theory always works as a guide in the background.¹

The political phenomena I seek to explain are as diverse as the use of citizen initiatives (Chapter 1), approval for direct democracy (Chapter 2), whether people

¹For example, Chapter 3 is, broadly speaking, an empirical test of the theory of the calculus of voting ([Downs, 1957](#); [Riker and Ordeshook, 1968](#)).

go to vote or not (Chapter 3) or the behavior of members of the German parliament (Chapter 4). The first two chapters put their focus on the functioning of direct democracy, while the last two chapters deal with questions related to representative democracy. In this way, this thesis is split in two parts.

In what follows, I shall give a short introduction to the topics covered in each chapter of this dissertation, thereby highlighting the key results and outlining the specific contribution to the political economics literature.

0.2 A Guide through the Chapters

Chapter 1: Signature Requirements and Citizen Initiatives

Chapter 1 looks at regulations governing direct democratic participation at the municipal level in Germany. It is entitled *Signature Requirements and Citizen Initiatives: Quasi-Experimental Evidence from Germany*. Since 1995, Bavaria allows its citizens to propose initiatives and thereby to influence the political agenda by themselves. However, the lawmakers were concerned that misuse of direct democratic instruments could lead to gridlock and capture of the initiative process by minorities. Therefore, a signature requirement was introduced: To bring an initiative to a vote, citizens have to prove that a sufficiently large fraction of the electorate supports their cause, which is demonstrated by the collection of a minimum required number of signatures.

In this chapter, we ask whether the signature requirement has "bite": Do lower signature requirements boost the number of observed citizen initiatives? Put differently, how many initiatives are being prevented due to a higher signature requirement? Throughout the chapter, we are guided by economic intuition and reasoning: The signature requirement should be a part of the cost function of the proposer. If this cost increases (more signatures need to be collected), we would expect that people are more likely to refrain from launching an initiative. If this is the case, the situation is comparable to a market where quantities are traded at certain prices. If the price goes up, people buy less of the product. The "price" of an initiative proposal is nothing else than the signature requirement. If this requirement gets tighter, we would expect that people react to this signal and this is indeed what we find.

To identify the effect of the signature requirement on the number of citizen ini-

tiatives, we rely on a regression discontinuity design (RDD)². The use of this methodology is possible due to special institutional provisions in the Bavarian municipal code. The lawmakers made the signature requirement depend on population size in a discontinuous way. At various population thresholds, signature requirements are reduced stepwise. This allows us to compare municipalities just above the cutoff (with the lower signature requirement) with municipalities just below the cutoff (with the higher signature requirement). Under the assumption that all unobserved characteristics develop smoothly with the population variable, municipalities just below the cutoff serve as a valid counterfactual for municipalities just above. The jump in the probability of observing an initiative at the threshold consequently measures a local average treatment effect.

The results suggest that signature requirements matter: Reducing the signature requirement by 1 percentage point increases the probability of observing an initiative petition in a given city-year by 8-10 percentage points. This finding holds for different window sizes around the cutoff and several polynomial specifications of the running variable. Furthermore, a nonparametric regression approach with varying kernels and different bandwidths yields very similar results. Other outcomes like initiative elections and initiatives leading to a decision are affected as well. This leads us to conclude that the signature requirement is important not only at the stage of initiative proposal, but also in determining whether a proposal leads to a political decision.

This chapter contributes to the literature in two ways. First, it uses a novel research design. Past studies trying to estimate the effect of signature requirements have usually relied on cross-sectional variation in a regression control framework. This identifies correlations at best. We offer the first quasi-experimental evidence on the question and are therefore quite confident that our estimates can be interpreted as causal. Second, with our focus on Germany, we show that the mechanisms governing direct democracy that have been investigated in the literature also work in a country that has much less experience with direct democratic decision making. In this way, we provide additional external validity to existing studies on the use of direct democracy (Eder et al., 2009; Barankay et al., 2003; Matsusaka and McCarty, 2001). The bulk of these studies has focused on the US or on Switzerland, the two countries with the longest history of direct citizen participation.

²This methodology has first been proposed by [Thistlethwaite and Campbell \(1960\)](#).

Chapter 2: Voting for Direct Democratic Participation

Chapter 2 is also concerned with direct democracy, but takes one step back: Instead of looking at the use of citizen initiatives, it focuses on the introduction of direct democracy. Entitled *Voting for Direct Democratic Participation: Evidence from an Initiative Election*, it uses a unique institutional setting in the German state of Bavaria. This state had provisions for direct citizen involvement on the state level since the end of World War II. However, state politicians were reluctant to introduce the possibility of direct democracy also at the local (municipal) level. In 1995, citizens took matters into their own hands: Via a statewide referendum, they forced a vote on the introduction of direct democracy at the local level. Put differently, Bavarian citizens used state level provisions for direct democracy to introduce similar provisions at the local level.

To the best of my knowledge, this is the only case worldwide where direct democracy was introduced by a direct democratic procedure itself. Because we know approval rates for the initiative in all 2,056 Bavarian municipalities, we observe – for the first time – revealed preferences for direct democracy. This is a major contribution to the literature on preferences for direct democracy. Usually, people state in surveys that they deem direct citizen involvement desirable (Bowler and Donovan, 1994). However, these survey answers entail no real consequences and it has been shown that respondents often give biased answers to comply with perceived social norms (Kahneman and Knetsch, 1992; Diamond and Hausman, 1994; Funk, 2012). The statewide initiative on the introduction of direct democracy is a real decision with real policy consequences. By taking a closer look at it, we can hence learn something about the differences between stated and revealed preferences in the realm of direct democracy.

The main goal of the empirical exercise in this chapter is to explain approval for direct democracy. Given that we observe differing approval rates across municipalities, it seems natural to ask what explains these differences. Are these standard demographic factors? Or political constellations? Interestingly, the governing party at the state level (which is also historically the strongest party in Bavaria) – the CSU – campaigned against the initiative. They promoted their own counterproposal that included a weakened version of citizen participation with high hurdles and prohibitive requirements. We observe in the data that in municipalities where the CSU is strong, approval for the initiative is lower. The point estimate suggests that for each percentage point increase in the CSU vote

share (measured at the last state election), approval for the initiative goes down by 0.61 percentage points. Thus, our results provide tentative support for the view that dissatisfaction with politics is not linked to a specific elected governing party. Rather it seems to be the case that dissatisfaction with representative democracy in general is a driving factor for voting for direct democracy.

The vote share of the governing party is the best predictor of approval for direct democracy. Included as the only regressor, it is able to explain almost 40 percent of the variation in approval rates. In addition, we go into great detail to demonstrate the robustness of this finding. For example, we show by tests in the spirit of [Oster \(2014\)](#) that selection on unobservables must be incredibly high to reverse our findings. Furthermore, Bayesian Model Averaging exercises support the conclusion that the vote share of the governing party should be included in each possible model as a regressor. To address potential endogeneity concerns, we also conduct an IV approach where we instrument the governing party vote with a measure for agricultural soil surface in the municipality. Finally, we use sample splits to rule out potential ecological fallacy arguments.

Chapters [1](#) and [2](#) are interconnected. While Chapter [1](#) tries to explain the use of direct democratic instruments, Chapter [2](#) explains the adoption and thereby measures revealed preferences for direct democracy. Of course, one can also go one step further and model the use of direct democracy as a function of the revealed preference measured in the statewide initiative, thereby establishing a direct link between the two chapters. This project is currently work in progress and not part of this dissertation. However, I will describe the project shortly in the General Conclusion (Section [5.4](#)) when I talk about future research.

Chapter [3](#): Turnout and Closeness

Chapter [3](#) is not embedded in the field of direct democracy like the previous two chapters. Named *Turnout and Closeness: Evidence from 60 Years of Bavarian Mayoral Elections*, it focuses on a crucial aspect of representative democracy, namely elections. The starting point of this study is the famous calculus of voting developed by [Downs \(1957\)](#) and [Riker and Ordeshook \(1968\)](#). These authors try to explain why people go to vote. In their model, individuals weigh the benefits against the costs of voting and only turn out if the first is greater than the latter. If elections are sufficiently close, potential benefits of voting are much higher because the probability of influencing the outcome with the own vote is larger.

As a consequence, close elections should have higher turnout rates. In this paper, I provide a thorough empirical test of this prediction using a rich dataset spanning more than 60 years of mayoral elections.

The obvious challenge, however, is how to measure electoral closeness. Given that I employ data on small scale mayoral elections, there are no opinion polls available prior to election day that would tell the voters something about the level of competitiveness. An improved measurement of electoral closeness is probably the main contribution to the literature of this study. Thanks to special institutional provisions in the German state of Bavaria, mayoral elections can have two rounds: If there is no candidate who obtains more than 50 percent of the vote in the first round, then a runoff election is held two weeks later with the two leading candidates from the first round. Now, closeness from the first round can be used as a signal for closeness of the second round. This measure will be in the information set of voters on election day. Previous studies using the *ex-post* realized closeness of the election had to make strong assumptions about rational expectations of voters, which are not necessary in my case.

The results suggest that closeness does indeed matter for electoral participation: A one standard deviation increase in closeness increases turnout by 1.68 percentage points, which corresponds to $\frac{1}{6}$ of a standard deviation in this variable. Importantly, I show that *Revealed Closeness*, which makes use of two-round elections, yields approximately the same results as the common *ex-post* measures employed in the literature so far. Due to the panel structure of the data, I can also evaluate a historical *ex-ante* measure of closeness. I contribute to the literature by offering a first comparison of three differently calculated indicators (*ex-ante*, *ex-post*, and two-round) for electoral closeness. Importantly, this comparison is conducted within one institutional framework.

As an additional heterogeneity check, I merge the voting data with data on election day precipitation by the *German Weather Service* (DWD). Rain on election day is a factor that – in the framework of the calculus of voting – increases the costs of voting. I am able to show that rain on election day reduces turnout *on average*, but not if the election is close. These findings suggest that electoral closeness works as an important mediator. In the General Conclusion (Section 5.2), I discuss the policy implications of this result. There, I also present another paper that is currently work in progress which uses rain on election day as an instrumental variable to gauge the partisan effects of voter turnout (Section 5.4).

The chapter fits well into the field of political economics, as the classic rational choice paradigm from economics is applied to the domain of voting, an area that used to attract primarily political scientists. Seen this way, it takes a similar approach as the widely known studies by [Becker \(1978\)](#), who developed an economic approach to explain various aspects of human behavior.

Chapter 4: Outside Earnings, Absence and Activity

Chapter 4 features the last paper of this dissertation and is entitled *Outside Earnings, Absence and Activity: Evidence from German Parliamentarians*. Here, we investigate the behavior of politicians after they have been elected. Members of parliament (MPs) are relatively free in interpreting their mandate. There is no specific job description. Therefore, it is at their personal discretion how much they work and how they allocate their time to competing activities. More specifically, politicians are allowed to follow sideline jobs in addition to their parliamentary work. This could, for example, be speeches at corporate events, positions in supervisory boards or a continuation of self-employment (e.g. in the case of lawyers).

We ask the question whether politicians with sideline jobs shirk in parliament. Put differently, are work opportunities outside and inside parliament substitutes? In economic terms, we estimate the labor supply of politicians. Analog to the question of a labor economist who asks by how much working hours decrease after becoming a parent, we ask by how much politicians reduce their parliamentary work when outside earnings increase.

This study became possible thanks to new disclosure requirements after an adjudication from the German Supreme Court in 2007. The judges forced German MPs to publish the details of their outside earnings. The relevance of the topic is still high. In the 2013 federal election campaign, many commentators agreed that the candidate of the social democrats (Peer Steinbrück) lost to conservative incumbent Angela Merkel because of his strikingly high outside earnings. If voters punish outside activities, politicians face a tradeoff between earning additional money on the one hand and reducing their chance of re-election (and thereby foregoing the benefits associated with holding office) on the other hand.

The contribution of Chapter 4 to the literature is twofold. First, we measure parliamentary activity in an improved way. Most existing studies only use attendance rates as a measure for parliamentary effort. We also have information on other

activities like oral contributions, interpellations, speeches and group initiatives. Admittedly, each of these variables has its shortcomings, but taken together they can provide a relatively complete picture of individual specific effort. Second, we make use of the panel structure of the data. As we observe German MPs' outside earnings and parliamentary activity in each year of the 2009-2013 legislative period, we are able to run fixed effect regressions. Hence, we can identify the effect of outside earnings on activity using only the within variation in the data. This implies that we can control for individual-specific unobserved characteristics (like ability), provided that these characteristics stay constant over time.

The results do not indicate that outside earnings are correlated with absence rates and speeches; but they do suggest that outside earnings are somewhat negatively correlated with oral contributions, interpellations, and group initiatives. We propose that the results for Germany do not corroborate evidence on other countries such as Italy (see [Gagliarducci et al. \(2010\)](#)) because party discipline, institutions, and political cultures differ across countries.

0.3 Brief Overview

To wrap up, [Table 1](#) summarizes the content of this dissertation in a concise way. Apart from providing title, research question and main findings for each chapter, it also informs the reader about the data used and the applied methodology. Information on co-authors is included as well. The last two rows indicate where the content of the four chapters in this dissertation has been published before, be it as a working paper or a journal article.

Table 1: Overview

	Chapter 1	Chapter 2	Chapter 3	Chapter 4
Title	Signature Requirements and Citizen Initiatives: Quasi-Experimental Evidence from Germany	Voting for Direct Democratic Participation: Evidence from an Initiative Election	Turnout and Closeness: Evidence from 60 Years of Bavarian Mayoral Elections	Outside Earnings, Absence and Activity: Evidence from German Parliamentarians
Co-Author(s)	Ronny Freier	Ronny Freier, Magdalena Pallauf, David Stadelmann		Björn Kauder, Niklas Potrafke
Research Question	Are signature requirements an effective tool to steer direct democratic participation?	What explains revealed preferences for direct democracy?	Is turnout higher in close elections?	Do moonlighting politicians shirk in parliament?
Data Source	<i>Mehr Demokratie</i> database on citizen initiatives; Bavarian State statistical office for population data	<i>Volksbegehren</i> data and municipal characteristics from the Bavarian State statistical office	Mayoral election data from the Bavarian State statistical office; Weather data from DWD	Outside Earnings from individual websites of German MPs; Activity in parliament from <i>Die Zeit</i>
Time Period	1995 - 2008	1995	1946 - 2009	2009 - 2013
Data Structure	Panel	Cross Section	Panel	Panel
Identification Method	Regression Discontinuity Design (RDD)	Instrumental Variables Method (IV)	Fixed Effects Regression	Fixed Effects Regression
Main Results	Reducing the signature requirement by 1 percentage point increases the probability of observing an initiative petition in a given city-year by 8-10 percentage points.	Support for direct democracy is related to dissatisfaction with representative democracy in general rather than with an elected governing party.	A one standard deviation increase in electoral closeness increases turnout by 1.68 percentage points, which corresponds to $\frac{1}{6}$ of a standard deviation in this variable.	Outside earnings are not correlated with absence rates and speeches, they are however negatively correlated with oral contributions, interpellations and group initiatives.
Discussion Paper(s)	Arnold and Freier (2013)	Arnold et al. (2015) ; Arnold et al. (2014a)	Arnold (2015)	Arnold (2013) ; Arnold et al. (2014b)
Journal Article	Arnold and Freier (2015b)	Submitted to a journal	Revise and Resubmit at the <i>Scandinavian Journal of Economics</i>	Arnold et al. (2014c)

Chapter 1

Signature Requirements and Citizen Initiatives: Quasi – Experimental Evidence from Germany*

1.1 Introduction

Direct democratic participation by citizens is often considered a useful complement to purely representative decision making.³ While a number of countries including the United States and Switzerland have used direct democratic instruments for a long time, countries such as Germany and transnational bodies such as the European Union only recently adopted constitutional provisions for citizen initiatives. Generally, one can observe a trend toward more citizen involvement.⁴ It is therefore critical to understand the institutional rules that govern the process of citizen participation.

The power of citizen initiatives to influence politicians' behavior depends crucially on the specific provisions of the legislation defining how initiatives are proposed.

*This chapter is based on joint work with Ronny Freier. A similar version has been published in *Public Choice*, see [Arnold and Freier \(2015b\)](#).

³See [Lupia and Matsusaka \(2004\)](#) and [Matsusaka \(2005\)](#) for general reviews of the functioning of direct democracy.

⁴For example, the German Pirate Party uses an online voting tool called “Liquid Democracy” to arrive at all party positions. Also, major infrastructural projects in Germany are often planned in cooperation with citizen associations to avoid public protest at a later stage.

In theory, a citizen who would like to propose an initiative will compare the benefits and costs of doing so. While it is hard to quantify the potential benefits (which depend on the probability of success), it is relatively straightforward to say something about the costs. By law, the proposer has to collect a certain number of signatures from fellow citizens who support her cause. Once this requirement is met, the initiative can be put to a popular vote. Since collecting signatures is costly, we would expect that laws stipulating a higher signature requirement make it less likely that the initiative will be proposed. The idea of the signature requirement is to impose some costs on the proposer to prevent the initiative process from being captured by minorities and to avoid overuse. However, a signature requirement that is too high hinders citizen involvement and may lead to frustration with the political process. The optimal choice of a signature requirement regime is therefore an important one for policymakers.

This paper aims to identify one part of the proposer's cost function. We estimate the causal effect of the number of required signatures on the probability that an initiative will be proposed. In other words, we ask how many initiatives are prevented from being proposed owing to a higher signature threshold, all else equal. Our identification strategy is based on discontinuous changes in the number of signatures required around several population thresholds specified in the constitution.

Our paper relates to three different strands of literature. First, and most importantly, there exists a small literature dealing with the impact of signature requirements on the use of initiatives. A seminal paper in this area is [Matsusaka and McCarty \(2001\)](#). Looking at a cross-section of US states, the authors report that higher signature requirements reduce the number of citizen-proposed initiatives. Similar evidence for the German Länder is provided by [Eder et al. \(2009\)](#), while [Barankay et al. \(2003\)](#) find no significant relationship between institutional openness and use of initiatives in Swiss cantons.⁵

A second strand of literature addresses the policy outcomes of direct democracy. Generally, it has been argued that initiatives – by shifting the agenda-setting power to the citizens – help to bring policies closer to the median voter's ideal point. This theory was first proposed by [Romer and Rosenthal \(1978\)](#) and was

⁵Recently, [Boehmke et al. \(2013\)](#) have shown that in US states, the length of the gridlock interval (the area in the policy space where no reforms are possible) is a better (positive) predictor of initiative use than signature requirements, which turn out to be insignificant.

further refined by [Gerber \(1999\)](#). Empirical validation of the theory has been provided by [Matsusaka \(2010\)](#), who finds that congruence between the median voter's preferred policy and the policy actually implemented is higher in states that allow for citizen initiatives. A large branch of this literature deals specifically with the fiscal effects of direct democracy ([Matsusaka, 1995](#); [Feld and Matsusaka, 2003](#); [Funk and Gathmann, 2013, 2011](#); [Blume et al., 2009](#); [Matsusaka, 2004](#)). For Germany, [Asatryan et al. \(2013\)](#) report that popular initiatives increase local government expenditures.

Third, on a methodological level, our paper fits into the empirical political economics literature that uses regression discontinuity designs based on constitutionally prescribed population thresholds. [Pettersson-Lidbom \(2001, 2012\)](#) was the first to use this particular design in a study of Sweden and Finland in which the number of council members in a municipality is constitutionally defined by various thresholds for the size (number of inhabitants) of the municipality.⁶

Our contribution to the literature is twofold: First, our data and research design allow for a first causal estimate of the effect of the signature requirement on initiative use. By adopting a regression discontinuity design, we improve on the existing literature by offering a novel identification strategy. Second, with our focus on Germany, we show that the mechanisms of direct democracy documented in the literature also work in a country that has much less experience with direct citizen participation. Our study hence validates and generalizes existing studies, most of which employ US or Swiss data.

For our empirical investigation, we use data on initiatives at the municipal level in Bavaria from 1995 through 2008. We combine these data with standard demographic variables for the municipalities, including population size in the respective years. Regarding the methodology, we use a regression discontinuity design. By comparing only municipalities close to the thresholds where the signature require-

⁶Population thresholds have also been used to identify the effects of wage changes for politicians ([Ferraz and Finan, 2009](#); [Gagliarducci and Nannicini, 2009](#)), the allocation of transfers ([Litschig and Morrison, 2010](#); [Brollo et al., 2009](#)), representative versus direct democracy ([Hinerich and Pettersson-Lidbom, 2010](#)), fiscal rules and policy ([Grembi et al., 2012](#)), female participation in politics ([Campa, 2012](#)), and voter turnout ([Eggers, 2013](#); [Tukiainen and Lyytikäinen, 2013](#)).

ment changes, we control for unobserved characteristics.⁷ This empirical strategy is intended to provide for quasi-random allocation of units into treatment and control group.

Our results suggest that municipalities falling into the area where the signature requirement drops to the next lower category have a roughly 8–10 percentage point higher probability – depending on the specification we use – of experiencing at least one initiative in a given year compared to the municipalities that still have the higher signature requirement. This effect is robust for different window sizes around the cutoff for various polynomial specifications and also for several choices of bandwidth if we use a nonparametric regression approach. Furthermore, placebo tests at fake thresholds or at thresholds where other institutions change (but not the signature requirement) yield no significant estimates, reassuring us that it is indeed the effect of the signature requirement we are measuring.

The remainder of this paper is organized as follows: Section 1.2 introduces the institutional setting. Section 1.3 presents the data and our empirical strategy. Section 1.4 describes our main results along with several robustness checks, and Section 1.5 concludes the analysis.

1.2 Institutional Setting

The municipal level is the lowest tier of government in Germany.⁸ In Bavaria, there are 2,056 independent municipalities. Generally, the affairs of the municipalities are jointly managed by an elected mayor and an elected town council. The responsibilities of the municipal tier of government include administration, public order, infrastructure, cultural institutions, and public transport.⁹

In the 1990s, most of the German states amended their municipal codes to allow for direct citizen participation in the policy-making process. We focus on Bavaria,

⁷One unobserved factor could be the degree of urbanization, for example. If urban milieus (i.e. larger cities) have a higher preference for direct democratic participation, lower signature requirements could be spuriously correlated with initiative activity. We therefore keep population size constant by looking at small windows around the cutoff where the signature requirement changes.

⁸Below the federal level there are 16 states, followed by about 12,500 municipalities which are organized into about 450 counties.

⁹Note that German municipalities are often also in charge of administering funds allocated to them by higher tiers of government, e.g., for public schools or social services. In these areas, municipalities may have only limited discretion.

the state that has seen roughly 40% of all initiative activity in Germany. The Bavarian legislation on citizen initiatives was introduced on November 1, 1995.¹⁰ It is set out in Article 18a of the Bavarian municipal code.¹¹

For an initiative to succeed, citizens have to overcome two hurdles: In the first step, citizens propose a petition for an initiative (the so-called *Bürgerbegehren*). The proposer of such an initiative then has to collect a minimum number of signatures from people who support her cause. Only if this condition is met and if the subject of the initiative is applicable¹² for a direct vote, the second step of the procedure is initiated: An initiative election, or *Bürgerentscheid*, is held. If a majority of citizens vote in favor of the proposal and this majority represents a sufficiently large fraction of the electorate,¹³ the proposal becomes law and has the same standing as a decision taken by the municipal council.

In our analysis, we make use of the fact that the constitutionally prescribed signature requirement for a successful *Bürgerbegehren* varies by municipality size. Policy makers were concerned that it may be more difficult to collect a certain percentage of signatures in a large, socially heterogeneous city than in a small village. The state authority, thus, made the signature requirement a function of population size. This function is not smooth. It displays discrete jumps at several population thresholds. In Bavaria, the signature requirement varies between 10% and 3%. It goes down in 1%-steps at several population cutoffs.¹⁴ The exact key is displayed in Panel 1 of Table 1.3 in the appendix.

At first sight, a 1% jump in the signature requirement may seem too small to have

¹⁰The introduction of direct legislation in German states is often accompanied by other institutional changes, as noted by Blume et al. (2011). In this regard, Bavaria is an ideal setting for evaluating the functioning of direct democracy. Bavaria offers a setting in which other features like direct election of mayors, vote aggregation and splitting or the treatment of small parties (explicit electoral hurdles) have been stable for many years before the introduction of the tool of citizen initiatives. In contrast, other states often changed those features and those reforms sometimes coincide with the direct democracy reforms. The introduction of the initiative is thus “unique” in Bavaria compared to the other German states, which is why we refrain from pooling states and concentrate on Bavaria.

¹¹See www.gesetze-bayern.de for more information.

¹²Each state has a list of topics that are eligible for initiatives. Note that one reason that we observe a relatively large number of initiatives in Bavaria is that the state law allows for many different aspects of local government policy to be decided by citizen initiatives.

¹³This so-called “quorum” was introduced on April 1, 1999 and hence 3.5 years after it became possible to propose initiatives. Twenty percent (15%) of the electorate has to vote “yes” in municipalities below 50,000 (100,000) inhabitants for the proposal to become law. Above 100,000 inhabitants, 10% is sufficient.

¹⁴Around 500,000 inhabitants, there is a 2% jump, but this affects only the cities of Munich and Nuremberg.

a significant effect on the number of proposed initiatives. We argue, however, that these jumps may indeed constitute important shifts. Consider the threshold of 50,000 inhabitants, at which the signature requirement falls from 7% to 6%. In absolute numbers, this implies that the proposer of an initiative has to collect 3,000 instead of 3,500 signatures from a population of 50,000. This corresponds to a marked decrease of $\frac{3,500-3,000}{3,500} = 14.29\%$ in the number of required signatures. Furthermore, there is good reason to assume that the variable costs of collecting signatures are convex. Passionate supporters of an idea will sign the petition without hesitation in the beginning; however, as the number of signatures collected increases, it is increasingly difficult to find new voters who are willing to sign the petition. Put differently, the cost of collecting the first 500 signatures is lower than the cost of collecting the marginal 500 signatures imposed by the higher signature requirement. Therefore, we expect the changes in this requirement to have non-trivial effects.¹⁵

Importantly for our study, the municipal code in Bavaria includes a number of institutional rules or constitutional features that depend on population thresholds (Ade and Freier, 2011). Overall, 15 different aspects of municipal governance are determined at certain thresholds, which in part overlap with the thresholds used in this analysis.¹⁶ We summarize the institutional setting in Table 1.3 in the appendix. Among other rules, population thresholds are used to define the number of council members, the wages and type of positions of politicians, as well as the set of tasks and fiscal transfers for which a municipality is responsible. This particular setting constitutes an important challenge for the identification of the causal effects in this analysis. In the robustness section, we will highlight a number of tests to illustrate that the results reported in this paper are indeed driven by the signature requirement rules and not by other discontinuous institutional changes at the same thresholds.

¹⁵An anonymous referee correctly pointed out that fixed costs in setting up the signature collection campaign might play a crucial role and lead to economies of scale. Our argument applies only to the variable costs of collecting signatures. On the margin, proposers in municipalities above and below the cutoff incur the same fixed costs.

¹⁶These thresholds have been determined by lawmakers in the ministries and are written down in the municipal code. Importantly, most of the thresholds existed long before the introduction of direct democracy in Bavaria. They were thus determined in a time when population numbers of municipalities were quite different, making it unlikely that they are endogenous from today's perspective.

1.3 Data and Empirical Strategy

1.3.1 Data and Descriptive Statistics

To answer our research question, we combine data from two different sources. First, we use a dataset on direct democratic activity in Germany that was developed jointly by the Universities of Wuppertal and Marburg. The nonprofit association *Mehr Demokratie e.V.* makes these data publicly available on its website.¹⁷

In the data, we observe all citizen initiatives in Bavaria from 1995 through 2008. Each initiative is coded as one observation, giving us roughly 2,000 data points to start with. Figure 1.1 in the appendix shows the yearly number of initiatives proposed since 1995. It illustrates that direct democratic activity saw an initial spike just after introduction of the law and then quickly leveled off to an average of around 100 initiatives per year.

For each proposed initiative in the dataset, we observe a number of further variables. Apart from the year of the proposal, we also know the subject of the initiative and the outcome of the process, i.e., whether an election was held and if so, whether it was successful or not. Also, having a unique identifier for each municipality in every year, we can match the data with our second dataset: the yearly official statistics collected by the Federal Statistical Office. From those data, we can deduce the population size of that municipality at that time.¹⁸

In Bavaria, there are 2,056 municipalities (including 25 county-free cities), which we observe over a period of 14 years (from 1995 to 2008). This gives us 28,784 city-year observations. Since we can only use citizen-proposed initiatives and not council-initiated referendums (for which the signature requirement is not applicable) in our analysis, we are left with about 1,800 direct democratic procedures that we match to the respective city-years. Hence, the unconditional probability of observing an initiative in a given municipality in a given year is about 6.25%.

For our analysis, the identification in the regression discontinuity design will come from observations that lie close to the respective population thresholds where the signature requirement changes. Since it is not ex ante clear what “close”

¹⁷See <http://www.mehr-demokratie.de/bb-datenbank.html>.

¹⁸We use the official population figure for the current year as the crucial measure. On the basis of this information we then determine what kind of signature requirement was in place and whether a municipality belongs to the treatment or control group.

means, we will use different samples ranging from +/- 5% to +/- 25% from the thresholds.¹⁹ In the 5% sample, we are left with 704 observations, while 2,477 (4,316) observations remain in the 15% (25%) samples.²⁰ The treatment and control groups turn out to be of similar size. Note that sample size is substantially reduced under the regression discontinuity approach, which is due primarily to the rather small average size of Bavarian municipalities (the median city has 2,815 inhabitants) and the signature requirement starts to change only at 10,000 inhabitants.

Descriptive statistics (mean, standard deviation, minimum and maximum) of all our outcome and explanatory variables can be found in Table 1.4 in the appendix.

1.3.2 Empirical Strategy

To estimate a causal effect of having a lower signature requirement on the number of proposed initiatives, we use a regression discontinuity framework. Following [Pettersson-Lidbom \(2012\)](#), we use population thresholds at which the signature requirements change discontinuously to identify our treatment effect.

Relying on observations close to the respective population thresholds, we estimate separate regression functions for the treatment and control groups and interpret the jump at the cutoff where treatment changes from 0 to 1 as the causal effect of the treatment. From a computational perspective, it is convenient to subtract the population threshold, c , from the actual population figure, i.e., to work with $X - c$ rather than with X .

We estimate the following basic model:

$$Y_{it} = \alpha + \tau D_{it} + f(X_{it} - c_{it}) + Z_{it}\beta + \epsilon_{it}, \quad (1.1)$$

where Y_{it} is an outcome variable related to initiative activity in municipality i in year t , X_{it} is population size and c_{it} is the respective (nearest) threshold. $f(\cdot)$ is some (parametric) polynomial function of $(X_{it} - c_{it})$, which varies on both sides of the threshold. Several functional forms will be used to estimate the treatment effect robustly. Control variables are summarized in Z_{it} . Finally, D_{it} is a dummy

¹⁹Distance to the threshold is measured in percentage points. For the threshold of 10,000 inhabitants, the +/- 5% sample will include all municipalities that have a population between 9,500 and 10,500 inhabitants.

²⁰We also run conventional OLS regressions with all observations in Section 1.4.3 as a robustness check.

indicating treatment (being above the threshold). If our theoretical reasoning is correct and lower signature requirements make initiatives cheaper to propose, we would expect the coefficient of interest τ to be positive and significantly different from zero.

One possible disadvantage of estimating the treatment effect parametrically is that observations further away from the cutoff also determine the value of the regression function at the cutoff. This happens because a *global* criterion – the sum of squared residuals – is minimized. Of course, this problem can be alleviated by including higher-order polynomials in the regression equation, allowing for a more flexible fit. Nevertheless, it is quite common to account for the *local* nature of the regression discontinuity estimator by estimating the model non-parametrically using kernel techniques. We use local linear regressions with a triangular or rectangular kernel, as is standard in the literature. We use a data-driven cross validation procedure to find the optimal bandwidth that minimizes the mean squared error of the regression.²¹

1.4 Results

This section is divided into three parts. First, we discuss our main results. Second, we examine the validity and robustness of the RDD estimates presented. Here, we present standard RDD tests as well as a detailed analysis that focuses on ruling out other potential confounders. Third, we discuss a conventional OLS approach as an alternative to the RDD results.

1.4.1 Main Results

Panel 1 of Table 1.1 shows our main estimates of the treatment effect τ (see Equation 1.1). The dependent variable is a dummy Y_{it} that takes the value of 1 if municipality i experienced at least one initiative²² in year t . The treatment effect can thus be interpreted as the change in the probability of observing at least one initiative in a given city-year. We highlight four RDD specifications. Column 1 shows the results in the narrow sample of +/- 5% using only a linear polynomial. Columns 2 and 3 extend the sample to +/- 15% and +/- 25%, respec-

²¹Here, we follow Imbens and Kalyanaraman (2012).

²²When we write initiative in this section, we always mean initiative petition – i.e., the first stage of the process (see Section 1.2).

tively, using quadratic and cubic control functions. In column 4, we implement the nonparametric specification with an optimal bandwidth as described above.

Table 1.1: Estimates of the Treatment Effect

	(1) RDD 5%	(2) RDD 15%	(3) RDD 25%	(4) RDD 25%
Panel 1: Dep. Variable - Prob(At Least One Initiative)				
Treatment	0.102** (0.047)	0.081** (0.040)	0.102** (0.042)	0.083** (0.035)
Panel 2: Dep. Variable - Number of Initiatives				
Treatment	0.162** (0.068)	0.118** (0.058)	0.159*** (0.060)	0.122*** (0.046)
Panel 3: Dep. Variable - Number of Initiative Elections				
Treatment	0.072* (0.038)	0.051 (0.034)	0.070** (0.035)	0.054** (0.025)
Panel 4: Dep. Variable - Number of Initiatives Leading to a Decision				
Treatment	0.096** (0.048)	0.076* (0.041)	0.100** (0.042)	0.075** (0.031)
Control function Specification	Linear Parametric	Quadratic Parametric	Cubic Parametric	Optimal Bandwidth Nonparametric
N	704	2477	4316	4316

Notes: Heteroscedasticity-robust standard errors in parentheses. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The treatment variable is defined as having a lower signature requirement.

The effect of treatment is positive (as expected) and significant throughout. Depending on window size and control function, the estimates range between 0.08 and 0.10. Having a lower signature requirement thus increases the probability of observing at least one initiative in a given city-year by 8-10 percentage points. Note that this measures an average effect, since we pooled the data for all thresholds.²³

The point estimates presented in Table 1.1 are strikingly large. Given that the

²³We also estimated a model in which we give individual dummies to each of the five population thresholds and estimate separate effects (results not reported). Owing to the smaller sample size at the individual thresholds, the results are mostly insignificant. However, the point estimates are very similar to our main effects and range from 6.6 percentage points at the 10,000 to 10.0 percentage points at the 30,000 thresholds. The point estimates seem to increase slightly with the size of the population, but they are statistically not significantly different from one another.

unconditional probability of observing an initiative is 15 percent in the 5% discontinuity sample, an 8 percentage point increase in probability roughly corresponds to a 50% premium on the unconditional probability of observing an initiative in the treatment group.

To complement the above analysis, we also estimate the effect of a lower signature requirement on several other outcome variables related to initiative activity. Mainly, we expect the signature requirement to affect whether a proposal for an initiative is made. However, we also ask whether the rule for signatures changes the overall number of initiatives proposed, the number of initiative elections that are ultimately held, as well as the number of initiatives that will lead to a decision.²⁴

We present estimates for these alternative outcome variables in Panels 2-4 of Table 1.1. Panel 2 displays estimates for the number of initiatives as outcome. As the dependent variable now varies between 0 and 3 (and not between 0 and 1), the coefficient of the treatment effect is larger in size and still significant. In Panel 3 we show the results for the number of actual initiative elections. Not all initiatives will lead to an election because the proposed issue may not be eligible for a direct vote, the signature requirement is not met, or other factors intervene in the process. However, also for this outcome we observe positive and significant effects of the lower signature requirement on the order of 5–7 percentage points. Finally, Panel 4 takes the number of initiatives leading to a decision as our outcome measure. Some initiatives are successful although never put to a vote. This happens when the municipal council accepts the initiative proposal and adopts a new policy in the spirit of the initiative. The council decision then renders a direct vote on the topic superfluous. The outcome variable in Panel 4 captures – in addition to normal initiative elections – the initiatives that were translated into new policies by the city council without being put to a popular vote. Our estimates here are again as large as 10 base points and significant throughout. We conclude that the signature requirement is important not only at the stage of initiative proposal, but also in determining whether a proposal leads to a political decision.

Figure 1.2 in the appendix visualizes the jump in the four different outcome variables and is the analog to Table 1.1. In the graph, we plot the deviation in log

²⁴Note that we implement the RDD using OLS for all outcomes. Alternatively, we also tested logit and ordered logit models when appropriate (results not reported). The results came out to be very similar, which is why we chose to keep the model parsimonious. Furthermore, none of the observations had predictions falling outside the 0 – 1 range in the OLS approach.

population from the threshold against the outcomes in bins of 1%. The graph highlights that the outcome differences are large for observations just above and below the threshold before they quickly return to a lower level.

1.4.2 Validity and Robustness of the RDD Results

In this subsection, we evaluate the validity and robustness of our results. Due to space constraints, we summarize our results here briefly without providing tables.²⁵

The regression discontinuity approach outlined above hinges on the assumption that municipalities have imprecise control over the assignment variable. We conduct two standard tests to evaluate the validity of this assumption. First, we plot histograms of the assignment variables and find no evidence of sorting in the population density around the thresholds.²⁶ Second, we confirm that important predetermined variables capturing political participation in the municipalities are balanced above and below the thresholds.²⁷

In a next step, we conduct a variety of robustness checks to demonstrate the stability of our results. First, we run multiple tests at placebo thresholds (-10% to +10% deviation from the true threshold, in steps of 1 percentage point), and find no effects there (as anticipated). Second, we experiment with different window sizes, different polynomials of the control function and different bandwidths (in the non-parametric model) to test the robustness of the model specifications. The treatment effect is always significantly positive and of comparable size. Third, we also drop individual thresholds one at a time to guarantee that results are not

²⁵The interested reader can find all tables and figures in the working paper version of this article ([Arnold and Freier, 2013](#)). Also, all results are available upon request.

²⁶Also, based on a formal test ([McCrary, 2008](#)), we cannot reject the null hypothesis of no sorting. Note that our results are markedly different from the evidence on sorting in German municipalities presented by [Ade and Freier \(2011\)](#), who use data that include earlier periods. In fact, the sorting in [Ade and Freier \(2011\)](#) is observed mostly in the 1980s (which is before our data period). We can think of three explanations for the non-existence of sorting in our (later) data. First, the large influx of migrants from former East Germany after 1990 made it harder to exert control over population statistics ([Freier et al., 2013](#)). Also, because of that influx, municipalities that had negative population growth before 1990 (and hence strong incentives to manipulate the population data) are now showing solidly positive growth. Second, the introduction of electronic administration with automatic voter registration and de-registration makes it more difficult to cheat. Third, any existing sorting incentive is likely to be most prevalent just before elections. Initiatives are also proposed, however, at non-election times when there is no benefit in sorting.

²⁷In particular, we test the number of valid votes, voter turnout, as well as council seats held by the Christian Social Union (CSU) and the Social Democratic Party (SPD), all in the 1990 elections.

driven by one threshold alone. Fourth, we include a time trend (also interacted with the treatment) in our model to adjust the model for the fact that the early periods saw an initial spike in the use of citizen initiatives. All robustness checks lead us to conclude that the effect of the signature requirement on the number of citizen initiatives is estimated robustly.

To conclude our robustness tests, we go into great detail to rule out other potential confounders. As highlighted in Table 1.3, the signature requirement rule is not the only treatment that changes at the population thresholds. To exclude the possibility that alternative mechanisms are driving our effects, we go through a number of tests.

First, we estimate our model at the thresholds of 1,000, 2,000, 3,000, and 5,000 inhabitants, where (among others) council size and wages of mayors change discontinuously but signature requirements remain the same. As expected,²⁸ the effects are close to zero and largely insignificant.²⁹ Second, we include council size, municipal spending,³⁰ as well as the participation quorum³¹ in the model to make sure that our treatment effect survives the inclusion of these additional controls. Finally, we exploit a unique opportunity to use a placebo outcome as a falsification test. Here, we use data on council-initiated referendums as an outcome variable. Importantly, these referendums do not have to fulfill a signature

²⁸As pointed out by an anonymous referee, it is not a priori clear that we should *expect* a zero effect in this placebo test. Other policy changes, e.g., council size or wage of the mayor, could very well exert an effect by themselves. However, the fact that we do not see such effects makes us confident that the other policy changes are indeed not troublesome for our identification strategy.

²⁹The idea of this test is the following. Council size changes both at the thresholds where the signature requirement changes and at the other thresholds of 1,000, 2,000, 3,000, and 5,000. If council size would indeed drive our effects, we would expect to observe this influence at the lower thresholds as well, where the signature requirement does not change.

³⁰Egger and Koethenburger (2010) and Ade and Freier (2011) are concerned with the effect of council size on public spending in the same Bavarian data – although in earlier years. The former paper argues that large spending effects occur as a result of council size changes at population thresholds. To ensure that our results on the probability of observing an initiative are not driven by council size changes or resulting changes in spending, we test whether directly controlling for these variables makes a difference in our analysis. Note that directly controlling for spending may be problematic in our setting. Asatryan et al. (2013) estimate a RDD using spending as an outcome variable, showing that initiative use positively impacts municipal expenditures. Thus, spending may be considered endogenous. For this reason, we include spending only as a robustness check.

³¹We control for the quorum in two different approaches: First, we extend our model to include indicator variables for higher/lower quorums and before / after introduction of the quorum. Second, we estimate our model including the quorum as a continuous variable. In both cases, the treatment effect stays similar in size and retains its significance.

requirement. Thus, we should not see significant differences in this outcome variable at the thresholds that change the signature requirement. Indeed, we find insignificant point estimates that are close to zero.³²

1.4.3 OLS Approach

The drawback of the regression discontinuity design is that sample size is reduced considerably. By focusing only on municipalities close to the cutoff, valuable information in the data is not exploited. However, one could also argue that conditional on population, the signature requirement is not correlated with the error term any more. Then it would be more efficient to estimate conventional OLS regressions using the whole sample, with the signature requirement as the main explanatory variable and a flexible polynomial function of population size as control variable.

Table 1.2 shows the results of this exercise. The expectation of the dependent variable is the probability of observing at least one initiative in a given city-year. The main explanatory variable is the signature requirement – measured in percentage points – which varies between 3 (largest municipalities) and 10 (smallest municipalities) depending on population size. Column (1) features a simple bivariate model, column (2) includes population size as a control variable and column (3) adds a more flexible cubic population polynomial. Fixed effects for the seven administrative regions and 96 counties in Bavaria are included in columns (4) and (5), respectively.³³

The results are consistent with the RDD results and go in the expected direction: A higher signature requirement leads to a lower probability of experiencing an initiative. The point estimates suggest that a one percentage point increase in the signature requirement leads to a decrease in the probability of observing an initiative of between two and six percentage points. While the size of the coefficient changes somewhat across specifications, sign and significance always point

³²To the extent that other confounding discontinuous rules at the population thresholds affect processes of direct democratic decision making, they should also have influenced the outcome of council-initiated referendums. The fact that this is not the case supports our analysis.

³³Note that it does not make much sense to include municipal fixed effects either in the RDD or in the OLS approach. With municipal fixed effects, the coefficient of the signature requirement is identified only by municipalities that cross the threshold from one side to the other during the sample period. There are very few observations for which this holds. Furthermore, under the RDD assumptions local randomization close to the cutoff holds independent of eventual fixed effects or additional control variables.

in the same direction: The signature requirement is an important limiting factor in the use of direct democratic instruments.

Note that the coefficients from the OLS regressions using the whole sample are somewhat smaller than the local average treatment effects that the RDD identifies using only a small window size around the threshold. Still, the treatment effect in column (3) of the main table and the most comparable coefficient from the OLS regression (cubic population polynomial and no fixed effects) are statistically not distinguishable from one another as their confidence bands overlap.

Table 1.2: OLS Results

	Dependent Variable: Prob(At least one initiative)				
	(1)	(2)	(3)	(4)	(5)
Signature Requirement	-0.021*** (0.002)	-0.014*** (0.003)	-0.066*** (0.008)	-0.066*** (0.008)	-0.023*** (0.009)
Population Polynomial	No	Linear	Cubic	Cubic	Cubic
Fixed Effects	No	No	No	Admin. Distr.	County
N	29778	29778	29778	29778	29778
R ²	0.01	0.01	0.02	0.02	0.06

Notes: Heteroscedasticity-robust standard errors in parentheses. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Population is divided by 100.000 to ease readability. The variable *Signature Requirement* varies between 3 and 10, depending on population size of the respective municipality in question. There are seven administrative districts and 96 counties in Bavaria.

1.5 Conclusion

In this paper, we study the causal effect of reducing the signature requirement on the probability of observing a citizen initiative. Based on data for Bavaria, we apply a regression discontinuity design using population thresholds at which the signature requirements are reduced stepwise.

We find that, directly at the thresholds, a drop in the number of signatures required causes the probability of an initiative to increase sizably and significantly. While the baseline probability of observing an initiative in a municipality is about 15% in a given year, the lower signature requirement increases this probability by 8-10 percentage points. Hence, the effects appear to be substantial.

Interestingly, the main effect seems to be confined to observations that are relatively close to the threshold. While this is of no concern for the internal validity of

our estimates, it raises questions about the external relevance. Because the effects quickly return to lower levels just past the thresholds, that part of the effect must indeed be attributed to a local effect in which the threshold itself constitutes a specific treatment. If being close to the threshold (or just above) makes citizens particularly aware that initiatives are relatively cheap to propose, it may be in fact the additional awareness rather than the lower costs that account for part of the effect.

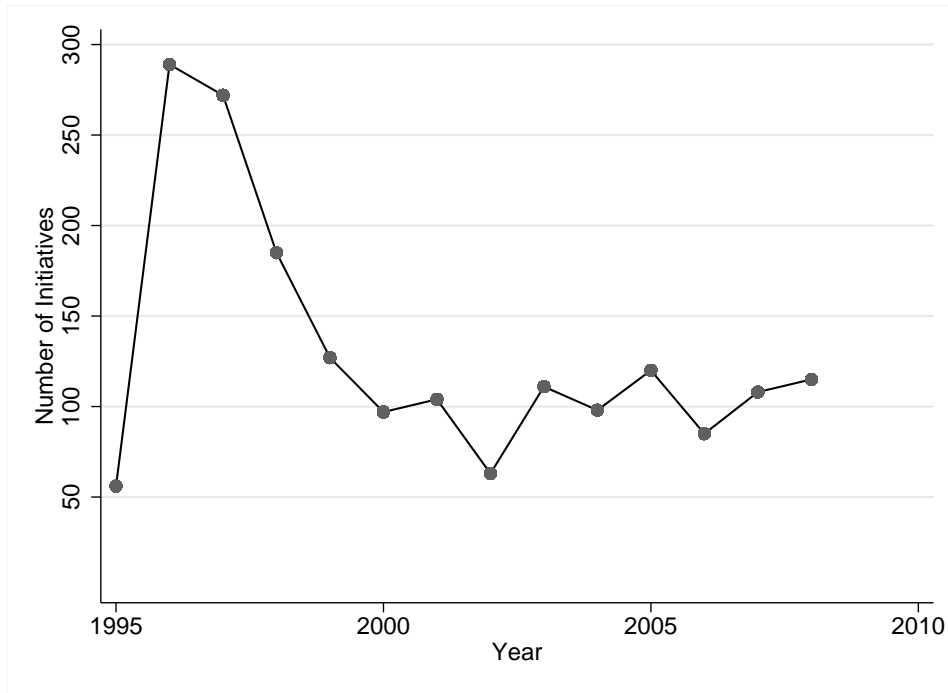
Our results are relevant to policymakers who aim to design the rules and institutions that regulate features of direct democracy. Generally, we consider direct democratic decision making desirable because it provides an inherent link between the rules under which democratic processes operate and the policies that are implemented. However, policy makers have long recognized that hurdles such as signature requirements are also needed to avoid the overuse of such direct democratic institutions. Our paper highlights that signature requirements are indeed an effective way of influencing direct democratic activity. Policymakers must carefully evaluate the effects of those signature requirements with the objective of regulating the amount of local popular initiatives to a level that is neither too high nor too low.

Appendix – Additional Tables and Figures

Table 1.3: Population Thresholds in Bavaria

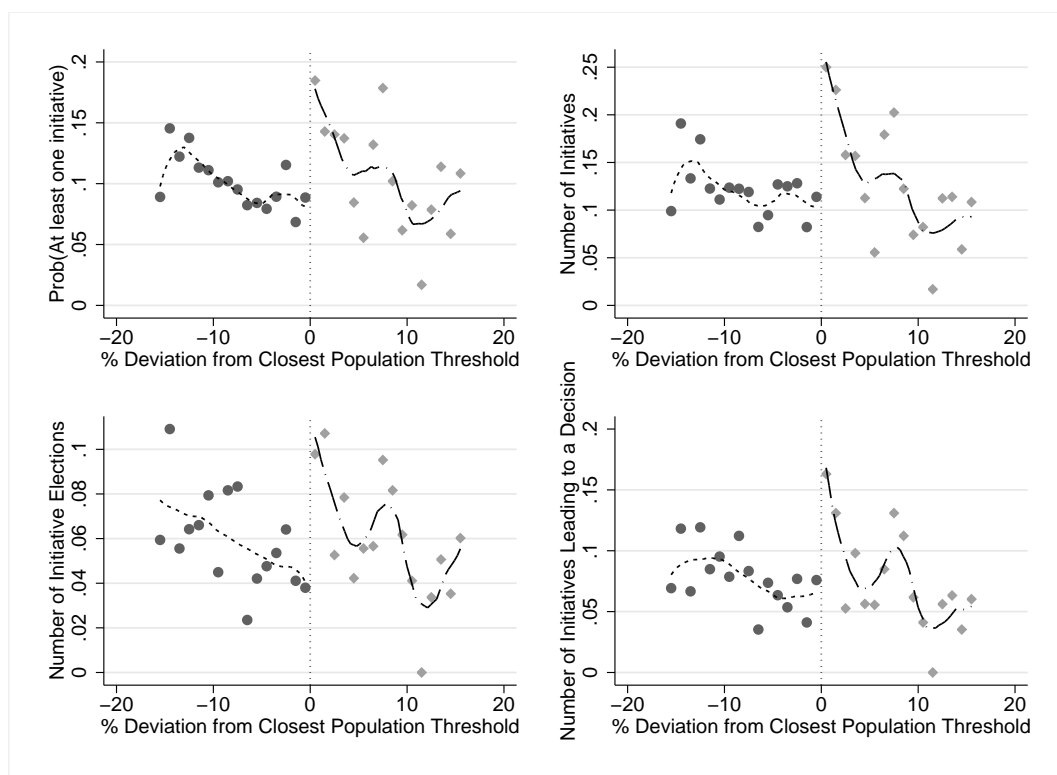
	Population thresholds at # of inhabitants (in thousands)										
	1	2	3	5	10	20	30	50	100	200	500
<i>Panel 1: Thresholds relevant for citizen initiatives</i>											
Signature requirement up to threshold (in percent)					10	9	8	7	6		5
Initiative election quorum up to threshold (in percent)								20	15		
<i>Panel 2: Thresholds defining local institutions</i>											
Council size	<i>x</i>	<i>x</i>	<i>x</i>	<i>x</i>	<i>x</i>	<i>x</i>	<i>x</i>	<i>x</i>	<i>x</i>	<i>x</i>	<i>x</i>
Full-time council members					<i>x</i>						
Wage of elected civil servants					<i>x</i>		<i>x</i>	<i>x</i>	<i>x</i>		
Wage of full-time mayors		<i>x</i>	<i>x</i>	<i>x</i>	<i>x</i>		<i>x</i>	<i>x</i>	<i>x</i>		
Wage of part-time mayors	<i>x</i>		<i>x</i>	<i>x</i>							
Referendum quota								<i>x</i>		<i>x</i>	
City districts										<i>x</i>	
Open council					<i>x</i>					<i>x</i>	
Accounting committee				<i>x</i>							
Mayor status				<i>x</i>	<i>x</i>						
<i>Panel 3: Thresholds defining budgeting rules</i>											
County free city								<i>x</i>			
Status of larger city							<i>x</i>				
Vehicle Tax				<i>x</i>							
Fiscal equalization				<i>x</i>	<i>x</i>			<i>x</i>	<i>x</i>		

Notes: Source: Table as presented in Ade and Freier (2011, p. 8, Table 1). For detailed descriptions of the specific rules that apply at those thresholds, consult Ade and Freier (2011, pp. 24-25, Tables 6 and 7).

Figure 1.1: Initiatives in Bavaria Over Time**Table 1.4:** Descriptive Statistics for Main Variables of Interest

Variable	Mean	Standard Deviation	Min	Max
Panel 1: Outcome Variables				
Prob(At Least One Initiative)	.1068119	.3089097	0	1
Number of Initiatives	.1283596	.403027	0	3
Number of Initiative Elections	.0644115	.2757455	0	3
Number of Initiatives Leading to a Decision	.0836423	.3130255	0	3
Panel 2: Explanatory Variables				
Population	29606.58	33761.22	7790	128380
Council Size	28.44522	9.783652	20	50
Total Expenditures (in 1000 Euros)	49874.93	60490.96	5257.373	526911.4

Notes: The table shows descriptive statistics for all variables of interest in the 25% discontinuity sample. Council-initiated referendums have been dropped from the sample since the signature requirement is not applicable for them.

Figure 1.2: Graphical Representation of Main Results

Notes: The unit of observation is a city-year. The points represent local averages of the outcome variable in bins of 1%. The lines are obtained using kernel-weighted local polynomial regressions.

Chapter 2

Voting for Direct Democratic Participation: Evidence from an Initiative Election[†]

2.1 Introduction

Look at opinion surveys by leading pollsters and you will find that contemporary citizens are relatively dissatisfied with the political system and interested in more direct democratic participation, possibly even much more direct participation. The interpretation, evaluation and discussion of such survey results on representative versus direct democracy has been largely an elite affair and political consequences are rarely drawn. This is comprehensible because stated support for direct democracy in opinion surveys need not reflect actual preferences for direct democratic participation. Hypothetical situations and erroneous self-reflections combined with moral satisfaction (Who will state that she is against democratic participation?) may introduce bias and make it difficult to draw concrete policy conclusions.

Ironically, yet coherent with the prevailing view of representative democracy, citizens have seldom been allowed to directly decide themselves on extensions of direct democratic participation rights. While political scientists and political economists have explored many facets of direct democracy, no empirical investigation so far

[†]This chapter is based on joint work with Ronny Freier, Magdalena Pallauf and David Stadelmann. A similar version has been published as a CREMA Working Paper, see [Arnold et al. \(2015\)](#). The paper has also been submitted to a journal.

exploits revealed preferences for direct democracy by looking at a direct democratic decision to implement more extensive direct democratic rights. Obviously, analyzing such a process of extending direct democracy would be revealing and our contribution attempts to fill this literature gap.

We analyze a constitutional change in the German State of Bavaria in 1995 where citizens, not politicians, granted themselves more say in politics at the *local level* through a constitutional initiative election at the *state level*. The constitutional change was accepted by a majority of Bavarian citizens and introduced important direct democratic instruments for municipalities, including municipal initiative petitions (*Bürgerbegehren*) and municipal initiative elections (*Bürgerentscheid*) which did *not* exist prior to 1995. The governing party in the state legislature campaigned against the systematic extension of direct participation rights.³⁴ This unique institutional setting allows us to focus on revealed preferences for direct democracy instead of analyzing opinion surveys, i.e. we focus on actual behavior of citizens in a real political decision with real consequences regarding the extension of direct democracy.³⁵

Dissatisfaction with representative democracy is regularly stated as a reason for the support of more direct democracy. The institutional setting allows us to explore factors which drive revealed preferences for more direct democratic participation at the local level. Importantly, we can distinguish between dissatisfaction with representative democracy in general, and distrust in the governing political party, in particular. Empirical results for over 2,000 Bavarian municipalities show that support for direct democracy is *not* positively associated with suspicion and a lack of support of the elected governing party as common perception might suggest. Rather, stronger electoral support for the governing party at the state level is related to relatively lower levels of revealed preferences for direct democracy. We employ an instrumental variable approach to account for endogeneity³⁶ and the

³⁴Public pressure on this matter forced the governing party to give up their general position against citizen participation and they subsequently campaigned for their own proposition of a weakened version of citizen participation.

³⁵Note that there is no direct economic relevance of this topic. However, we aim to make a contribution to the political science literature. Nevertheless, we think that also economists can learn something from our analysis, as uncovering differences between stated and revealed preferences can help researchers from various fields that employ survey data in their empirical applications.

³⁶Unobserved factors that influence both the preference for direct democracy and the strength of the governing party in a municipality could lead to spurious correlation of the variables in question.

results indicate that the causal relationship most likely runs from stronger electoral support to voting against direct democracy. Over and above the IV strategy, we also test whether potentially unobserved variables affect the robustness of our empirical results. Furthermore, a Bayesian approach highlights the importance of electoral support as a main variable of interest to explain preferences for direct democracy. The overall evidence is suggestive for the view that dissatisfaction with politics is not linked to a specific elected governing party but rather to representative democracy in general which complements the existing literature on interpretations for support of direct democracy.

This paper is structured as follows: Section 2.2 presents theoretical considerations and discusses the related literature. Section 2.3 presents the institutional setting and the initiative at the state level for the extension of direct democracy. We discuss the data and the empirical strategy in Section 2.4 and present results in Section 2.5. Section 2.6 offers concluding remarks.

2.2 Theoretical Considerations and Literature

In many countries, regions and local jurisdictions citizens rely increasingly on initiatives, referendums and other forms to direct democracy to take political matters into their own hands.³⁷ Numerous studies analyzed the effects that direct democratic participation and decisions have on policy outcomes (see, among others, Noam 1980; Frey 1994; Feld and Matsusaka 2003; or recently Matsusaka 2005, 2008, 2010 for overviews). However, empirical evidence is comparatively scarce on the support for the introduction of direct democratic instruments. In particular, revealed preferences for direct democratic participation rights are usually unobserved. Historically, the extension of direct democratic participation rights seems to be associated with discontent with politicians in general (see Piott 2003), who then grant more participation rights in an effort to appease voters. We contribute to the literature on the demand for direct democracy by analyzing a decision of voters (instead of politicians) to grant themselves more direct democratic participation rights.

Opinion polls in numerous countries show strong, almost overwhelming, demand of citizens for more direct influence on policy decisions (Bowler and Donovan 1994). According to surveys of the International Social Survey Program in 2004

³⁷For the chances and challenges of direct democracy, see Kirchgässner (2015).

over 60 percent of survey respondents in the United States, Canada, Great Britain, and Spain agree or strongly agree that "referendums are a good way to decide important political questions." In Austria, Germany and Switzerland support is over 80 percent. Other national and international pollsters offer similar numbers (Leininger 2015). Due to potential bias of surveys, the literature on the public approval of direct democracy is inconclusive (see, e.g. Dalton et al. 2001; Donovan and Karp 2006; Bowler et al. 2007). Craig et al. (2001) show that survey answers for the extension of direct democracy differ markedly by the way the question is asked: If citizens make a distinction between the political elite and themselves, i.e. "Us versus Them", large majorities agree with direct democracy while support is markedly weaker when asked about normal people's capacity of participating. According to Dyck and Baldassare (2009) support for direct democracy varies on whether the survey questions focus on the abstract institution rather than concrete details and Collingwood (2012) shows that support is lower when respondents are initially asked questions on ballot propositions. Thus, it is, unfortunately, not clear that opinion polls for more direct democracy actually correspond to citizen preferences for it and whether they over- or understate actual preferences. Instead of focusing on hypothetical support for direct democracy in opinion polls, our contribution analyses an actual ballot proposition for more direct democracy that lead to an important real constitutional change.

Extensive literature highlights the discrepancy between outcomes of opinion surveys and true preferences for politically important topics. The absence of reflected attitudes on certain issues can lead to improvised answers in surveys and opinions may change in relatively short periods of time (Zaller and Feldman 1992; Diamond and Hausman 1994). Preceding questions, the interview manner, the context, among others, all influence the answers which may lead to different survey outcomes or seeming preference indication of survey respondents (Diamond et al. 1993; Hanemann 1994; List 2002). Proposed policies in surveys tend to be considered hypothetical and real costs and policy consequences are only partly taken into account or even unknown. Such hypothetical bias combined with moral satisfaction can cause the survey results on politically relevant topics to be widely inaccurate (Kahneman and Knetsch 1992; Diamond and Hausman 1994; Neill et al. 1994; Cummings et al. 1997; Murphy et al. 2005; Funk 2012). Regarding prevailing norms, this may be particularly true for surveys on direct democracy.

Focusing on actual initiative elections provides a way to elicit revealed voter preferences directly. Initiative elections permit citizens to judge legislative proposals,

rank them against the status quo, and they entail real policy outcomes (e.g. [Schneider et al. 1981](#); [Hersch and McDougall 1988](#); [Frey 1994](#); [Garrett 1999](#); [Portmann et al. 2012](#); [Brunner et al. 2013](#); [Carey and Hix 2013](#)). Voters in initiative elections put more time into thinking about a ballot decision and the incentive to state true preferences is higher as their decisions entail real consequences. [Schlaepfer et al. \(2004\)](#) and [Schlaepfer and Hanley \(2006\)](#) compared stated preferences in professional surveys which were conducted before the awareness of an upcoming referendum with the decisions in the actual referendum and find that preferences indicated in surveys are largely incompatible with referendum decisions. By analyzing an initiative election on the introduction of direct democracy, we avoid challenges of surveys and obtain a direct measure for revealed preferences of voters regarding the support for direct democracy.

Higher educated people are more aware of ballot propositions and have a stronger opinion on them ([Bowler and Donovan 1994](#)). Uninformed voters tend to abstain from voting in initiative elections. Thereby, initiative elections oversample the informed population ([Osborne and Turner 2010](#); [Stadelmann and Torgler 2013](#)). However, informed answers are also overrepresented in surveys as ill-informed survey respondents often do not answer questions such that both, initiative elections and surveys, bias the outcome towards the opinion of informed voters ([Althaus 1996](#)). As open public debates precede an actual decision in initiative elections, information uncertainty is generally lower than for survey respondents ([Frey 1994](#); [Lupia 1994](#)) and abstaining from a ballot is more closely associated with true indifference. Moreover, information is more easily accessible ahead of initiative elections than for surveys and voters generally tend to be better informed when they can participate more directly in political decisions ([Feld and Kirchgaessner 2000](#); [Benz and Stutzer 2004](#)).

As in any political process, particular groups may try to influence referendum decisions through campaigning ([Lupia 1994](#)). [Bohnet and Frey \(1994\)](#) and [Frey \(1994\)](#) argue that referendums fulfill individual preferences and are able to break the cartel of politicians directed against voters. Nevertheless, governments and politicians may influence how citizens vote in referendums. [Bowler and Donovan \(1994\)](#) suggest that endorsements by political parties and politicians serve as a channel of information and that they have an influence at the ballot (see also [Lupia 1994](#); [Shachar and Nalebuff 1999](#); [Stadelmann and Torgler 2013](#)). Moreover, partisanship increases the probability of forming opinions by statements of the political elite ([Eichenberger and Serna 1996](#)). Results by [Treichsel and Sciarini](#)

(1998) suggest an impact of political elites on voting outcomes in referendums and [Smith and Tolbert \(2001\)](#) argue that political party affiliation is the most important influence on voting decisions. For initiative elections, parties tend to become involved when the issue affects the party's ideology and parties attract voters by taking a side to withdraw voter support from another party ([Smith and Tolbert 2001](#)). [Jenssen and Listhaug \(2001\)](#) note that voters in initiative elections may take positions on an issue based on party cues. Our setting allows us to contribute to this literature by taking account of party positions with respect to an extension of direct democracy and we can analyze the influence of parties on revealed preferences for direct democracy. Thereby, we extend the literature on the reasons for support of direct democracy by investigating whether suspicion of elected governing party explains actual support for direct democracy (see, e.g., [Gerber 1999](#) or [Dalton et al. 2001](#)) or rather dissatisfaction with representative democracy in general.

2.3 Institutional Setting

2.3.1 Direct Democracy at the State Level and Political Parties

Germany implemented a party-centered representative democracy at the national level after 1945. Direct democratic participation rights such as initiative petitions or initiative elections have been included in some State (*Länder*) constitutions. In particular, the State of Bavaria grants comparatively extended direct democratic participation rights to its citizens. In 1946, a two-stage legislation with initiative petitions and elections had been implemented for decisions at the state level. An initiative petition (*Volksbegehren*) constitutes an attempt of citizens to change or adapt a law or a constitutional amendment. It is addressed to the State Parliament (*Landtag*). Before the initiative petition gets submitted to voters for a decision in an initiative election (*Volksentscheid*), two signature requirements need to be fulfilled: In a first step, 25,000 signatures of eligible voters are required to have the legal admissibility of the initiative formally tested. In a second step, 10 percent of the electorate have to sign the initiative petition up to 14 days before it is submitted to the State Parliament. The State Parliament has the right to formulate a counterproposal to the initiative. If the State Parliament rejects the initiative or submits a counterproposal, a popular vote in an initiative election is

necessary. The proposal with the relative majority is accepted and becomes law.³⁸ Constitutional amendments need to fulfill an approval quorum of 25 percent of eligible voters. Until 2014, 19 referendums were held in Bavaria at the state level.

The political party landscape in Bavaria is influenced by the conservative Christian Social Union (CSU) which held an absolute majority in the State Parliament and Government from 1962 until 2008 and regained an absolute majority of seats in 2013. The party only competes in the State of Bavaria. Its spectrum of supporters is broad and ranges through all strata of society (Pappi 2011). The Social Democrats (SPD) represent the party for the working class at the federal level in Germany, however, the working class votes in the same proportions for the SPD and the CSU in Bavaria. The Greens and the Liberals (FDP) enjoy electoral support at the state and municipal level in Bavaria. In state level elections their vote shares lie between approximately 2 to 8 percent.³⁹ Since 1998 the Free Voters (Freie Wähler), a conservative party next to the CSU gained between 4 and 10 percent in state elections.

2.3.2 Introducing Direct Democracy at the Local Level

While the Bavarian constitution grants extended direct democratic participation rights at the state level, the situation at the local level has been entirely different until 1995. In 1951, the incorporation of local level initiative petitions (*Bürgerbegehren*) and local level initiative elections (*Bürgerentscheid*) was not ratified by the state legislature (see Bierl 1995 and Bayerischer Landtag 1995c, p. 308 for details). In the following 40 years, multiple motions to implement direct democracy at the local level by smaller parties were all inhibited by the governing party, i.e. the CSU.⁴⁰ At the time, the CSU advanced numerous arguments against direct democratic participation at the local level against the Greens and other supporters of direct democracy, e.g. that a minority might overrule a majority and that municipalities would lose their ability to govern. CSU officials expressed concern that direct democracy at the local level would not match with the system of

³⁸In 2000, the Bavarian state election law (*Landeswahlgesetz*) was changed due to a decision of the Bavarian Constitutional Court. If a referendum and a counterproposal are submitted, voters have now more than one vote and in case more than one option wins a majority of the votes, a tie-break vote is necessary (Bayerisches Gesetz- und Verordnungsblatt Nr. 15/2000, p. 365).

³⁹After a term in the State government the Liberals did not manage to win any seats in the 2013 election.

⁴⁰Famous but failed attempts to expand direct democracy include proposals in 1981, 1985, 1987, and 1991 (Bayerischer Landtag 1991 pp. 400)

representative democracy (CSU Parteitag 1982) and that local direct democratic decisions would be "hijacked by demagogues and pied pipers" (Bayerischer Landtag 1995b p.893).⁴¹ The CSU won an outright majority in both state elections of 1994 and 1998, with 52.8 percent and 52.9 percent, respectively. Between the two state elections, in 1995, direct democracy at the local level was introduced through an initiative election at the state level.

In 1995, a year after the election, the citizen's association for "More Democracy" (*Mehr Demokratie in Bayern e.V.*) formulated an initiative to introduce direct democracy at the local level through a constitutional amendment at the state level, i.e. the state constitution needed to be changed to allow direct democracy at the local level. At the state level such an initiative was possible according to the existing state constitution as outlined above. The initiative petition passed the signature requirement with 13.7 percent of the electorate and was submitted to the State Parliament. The proposition was considered citizen-friendly with extensive participatory and decisive rights as well as low hurdles (see Bayerischer Landtag 1994a; Bayerischer Landtag 1994b). Parliamentary criticism towards the initiative was intense and it was particularly targeted at missing quora of approval, the non-exclusion of certain policy areas from an initiative election, and certain politicians argued that "all this would serve special interests" (Bayerischer Landtag 1995c, p. 311).

As the initiative petition passed the state signature requirement, the governing party CSU formulated a counterproposal (see Bayerischer Landtag 1995a). This alternative proposal would have introduced important steps against the extension of direct democratic rights and instruments at the local level. In particular, it included a quorum requirement and subject exclusions within a legislative instead of a constitutional framework (Bayerischer Landtag 1995b, 889-895) which would have made it prone to arbitrary changes ex-post. A rigorous debate in parliament followed and the counterproposal was accused as a "bluff package" (Bayerischer Landtag 1995d p. 893). With the majority CSU voting for its own measure while the opposition voted for the initiative in the State Parliament, the initiative was not directly ratified (64 votes for the initiative, 89 against it) and the counterproposal as well as the initiative had to be put to the polls. In the

⁴¹The protocol of the plenary session in 1995 states: "Der Bürgerentscheid ist eine Spielwiese für Volksverführer und Demagogen. Die vom Volk gewählten Vertreter in den kommunalen Parlamenten sollen entmachtet und die Mehrheiten von aktionistischen Minderheiten terrorisiert werden."

intense public debate ahead of the initiative election, opposition parties, in particular the Greens, supported the initiative of *More Democracy* as did some other 50 small associations.⁴² However, CSU dominated large municipal head organizations “*Landkreistag*”, “*Städtetag*”, and “*Gemeindetag*” for counties, cities and municipalities all rejected the initiative in favor of the counterproposal. The referendum took place on October 1, 1995. The CSU counterproposal was rejected by the voters with only 38.7 percent supporting it. A majority of 57.8 percent of voters supported the original initiative. 3.4 percent rejected both amendments. The new constitutional amendment was implemented directly afterwards on November 1, giving Bavarian municipalities the most extensive direct participation rights in Germany.⁴³ In 2010, 15 years after the implementation, Bavaria counted over 1700 local initiative petitions and 900 local initiative elections, making it the German State with the most vivid direct democratic activity at the local level.⁴⁴ Anecdotal evidence also suggests that other German states modeled extensions of direct democratic participation rights from Bavaria.

This unique institutional setting permits us to analyze the extension of direct democracy through a direct democratic process. Citizens themselves initiated a constitutional reform through an initiative petition at the state level to extend direct democratic participation rights at the local level. Past attempts to extend direct democracy through representative democracy all failed as the governing party blocked them. We can, thus, analyze how support for the governing party and other factors drive citizens to vote for more or less direct democratic participation, i.e. instead of relying on opinion polls to measure potential support of direct democracy, we analyze revealed preferences for a real constitutional proposal.

⁴²The Social Democrats officially supported the initiative. Informal interviews and personal conversations with former social democratic mayors, however, make us believe that this position was rather taken in opposition to the governing CSU than out of support for more local direct democracy.

⁴³In 1999, the Bavarian Constitutional Court decided upon the introduction of a quorum. Since then, local initiative elections are only successful when they achieve a quorum of approval which depends on the population size of the municipality.

⁴⁴Today, at the national level CSU politicians often argue for more direct democratic participation.

2.4 Data and Empirical Strategy

2.4.1 Descriptive Statistics

The state of Bavaria consists of 2056 municipalities, including 25 county-free cities. We obtained official results of the *More Democracy* initiative election (*Volkentscheid*) from the Bavarian State Office for Statistics (*Bayerisches Landesamt für Statistik und Datenverarbeitung*). On the level of the individual municipality, we observe revealed preferences for direct democracy by approval rates of the initiative election as well as approval rates for the counterproposal advanced by the State Parliament.

As our main variable of interest, we analyze the governing party vote shares (CSU) in the prior state election of 1994, $VS^{CSUState}$. The temporal closeness of the 1994 state election and the 1995 state-wide initiative election contributes to ruling out changes in party loyalty over time. Moreover, the CSU obtained virtually the same state-wide support in 1998 as in 1994 and the support levels in municipalities are highly correlated for the two elections. We also gathered CSU vote shares in the last municipal elections in 1990, $VS^{CSULocal}$. Since the CSU does not run in all municipal elections, we code a dummy indicating whether the CSU appeared on the ballot. Furthermore, we collected information on voter turnout in the state election, the preceding municipal election and the initiative election which allows us to measure general interest in politics, political culture, and citizen engagement. Turnout for the state level initiative election measures the effect of voter mobilization on revealed preferences for direct democracy at the local level. The effect of a municipality's support for the opposition party The Greens – which was the main political advocate for more direct citizen engagement – is captured by the Green vote share in the state election. Moreover, we have information on the number of parties running in the municipal election and whether the directly elected mayor is a member of the CSU or not.

We capture demographic, socio-economic and other differences among municipalities by the following variables: $\text{Log}(\text{Population})$ size distinguishes between urban municipalities and rural ones. The age distribution is captured by the variables *Share young* (under 18 years) and *Share elderly* (over 65 years). The strength of the economy is proxied by the rate of *Employment* and the municipal financial situation is reflected by the level of *Per Capita Debt*. We include the share of *Catholics* which stems from the 1987 census to measure conservatism and control

whether a municipality is a *University Town*. Interest heterogeneity within the municipality is accounted for by the amount of *In-migration* from East Germany and neighboring states between 1987 and 1995. With the *Share of Agricultural Soil Surface* we have a variable to proxy preferences for conservative parties.

Table 2.1 shows descriptive statistics for all variables used in the analysis. Average approval for the initiative by More Democracy is greater than 50 percent, reflecting the fact that the initiative was successful.⁴⁵ The CSU is the dominant party on the state level, garnering on average almost 60 percent of the vote in the 1994 election. At the municipal level, the CSU faces stronger competition by local parties and conservative citizen groups⁴⁶ and it is less successful with an average vote share of 22.4 percent. As the party stands for election in just 56 percent of the municipalities, this implies an average vote share of about 40 percent conditional on running. Osborne and Turner (2010) suggest that in common value environments referendums lead to higher welfare than a social planner because indifferent voters do not participate in the ballot. Turnout for the initiative by *More Democracy* was at 37.9 percent on average. This corresponds to other referendums in Bavaria⁴⁷ and similar participation rates are observed in countries with extensive direct participation rights such as Switzerland (Stadelmann et al. 2013). Turnout is highest in local elections (roughly 80 percent) and about 10 percentage points lower in state elections.

2.4.2 Empirical Strategy and Expected Effects

To estimate ceteris-paribus effects of CSU strength and other factors on the revealed preference for direct democracy in the initiative election, we employ a regression control framework. Hence, we estimate the following model:

$$PreferencesDD_i = \alpha + \beta_1 \cdot VS_i^{CSUState} + \beta_2 \cdot VS_i^{CSULocal} + X_i' \gamma + \theta_k + \epsilon_i \quad (2.1)$$

⁴⁵Note that the average weighs all municipalities equally, i.e. independent of population size. We therefore observe a small difference between average approval in our sample (54.2 percent) and the official result of the initiative (57.8 percent).

⁴⁶Some local groups affiliate with the state CSU such that there is only no registered list bearing the term “CSU” in local elections

⁴⁷Total turnout was 43.8 percent for a change in the waste disposal law in 1991 and 39.9 percent for a constitutional reform and abolishment of the Senate in 1998. Recent turnout for referendums in 2013 was higher because state elections were held the same day.

Table 2.1: Summary Statistics

Variable	Mean	Std. Dev.	Min.	Max.	N
Share Yes for Direct Democracy	0.541	0.089	0.239	0.764	2031
Governing Party Vote Share (State Election)	0.585	0.090	0.255	0.832	2031
Governing Party Vote Share (Municipal Election)	0.222	0.214	0	0.806	2031
CSU Runs	0.556	0.497	0	1	2031
Green Party Vote Share (State Election)	0.052	0.023	0.008	0.211	2031
CSU Mayor	0.471	0.499	0	1	2031
Number of Parties	3.569	1.554	1	13	2031
Turnout Initiative	0.380	0.068	0.150	0.798	2031
Turnout (State Election)	0.690	0.055	0.426	0.907	2031
Turnout (Municipal Election)	0.812	0.057	0.600	0.995	2026
Log(Population in 1000)	1.052	0.828	-1.565	3.933	2031
Per Capita Debt	581.7	466.0	0	8889	2031
Share Employed	0.200	0.147	0.008	1.383	2028
Share Young	0.222	0.025	0.118	0.294	2031
Share Elderly	0.142	0.028	0.057	0.385	2031
In-Migration 1987-1995	0.123	0.082	-0.206	0.729	2031
Share Catholic	0.746	0.264	0.035	0.988	2031
University	0.003	0.059	0	1	2031
Share Of Agricultural Soil Surface	0.565	0.161	0.025	0.921	2031

Notes: Descriptive statistics for all 2031 Bavarian municipalities. The 25 county-free cities are excluded from the sample. The share of employed people can be larger than one due to commuting workers from neighboring municipalities. Source: Own calculations.

where $PreferencesDD_i$ is the approval rate for the initiative which reflects revealed preferences for direct democracy, α is a constant. β_1 is the central coefficient of interest capturing the effect of $VS_i^{CSUState}$, i.e. the vote share of the governing CSU in the state election. β_2 captures the influence of the CSU strength in a municipality. X_i' is a vector of control variables, θ_k is a fixed effect for administrative region k and ϵ_i is an error term. Administrative region fixed effects capture economic, demographic, social and cultural differences between the seven regions (e.g. Upper Franconia vs. Swabia). The unit of observation is the individual municipality, indexed by i ($i = 1, 2, \dots, 2031$). We always estimate robust standard errors.

The governing CSU campaigned for its own counterproposal and it was the only party campaigning against the initiative. Support for direct democracy depends on trust in politicians. If citizens generally trust the governing party's performance and its cues, and if dissatisfaction with government is low, we expect $\beta_1 < 0$ (and also $\beta_2 < 0$), i.e. CSU strength should then have, ceteris paribus, a negative influence on approval for the initiative. If, on the other hand, the electorate is dissatisfied and suspicious of the governing party (Bowler et al. 2007), they may demand more direct control, i.e. the influence of the CSU vote share would then

be positive.⁴⁸ Traditionally, elections at the municipal level are less influenced by trust to a specific party compared to state elections where parties matter relatively more than individual politicians. Thus, we expect the (absolute) municipal CSU vote share to have a smaller influence on the initiative election for direct democracy than the (absolute) state CSU vote share, i.e. $|\beta_1| > |\beta_2|$.

Turnout at the state level initiative measures the effect of mobilization within municipalities for direct democracy. The effect of overall mobilization on support for direct democracy is theoretically ambiguous as it is unclear whether supporters or opponents of direct democracy are easier to mobilize with campaigning, such that support for direct democracy and turnout for the initiative are jointly determined. However, low turnout in the previous state election can be interpreted as sign of dissatisfaction with politics in general, such that we expect a negative relationship between turnout in the state election and support for direct democracy. The same expectation holds for turnout in the preceding municipal election, though, the absolute effect should be lower as individual politicians matter more than parties at the local level.

When analyzing approval for direct democracy and interpreting the effects of party strength, we need to control for a number of other variables which may influence preferences for direct democracy and support for the governing party at the same time. The strength of the opposition Green which supported the initiative should have a positive influence on voting for the initiative of *More Democracy*. The “New Politics” theory (Inglehart 1999, 1990) suggests that support for direct democracy should be more common among the younger, more urban, better educated and less conservative population. Therefore, we expect *Share young*, *Log Population*, *University* to have a positive influence on the dependent variable and *Share elderly* and *Share Catholic* to have a negative effect on revealed preferences for direct democracy. Economic factors such as debt levels and employment opportunities as well as in-migration may affect support for the governing party and for direct democracy but the sign of their effect is theoretically ambiguous.

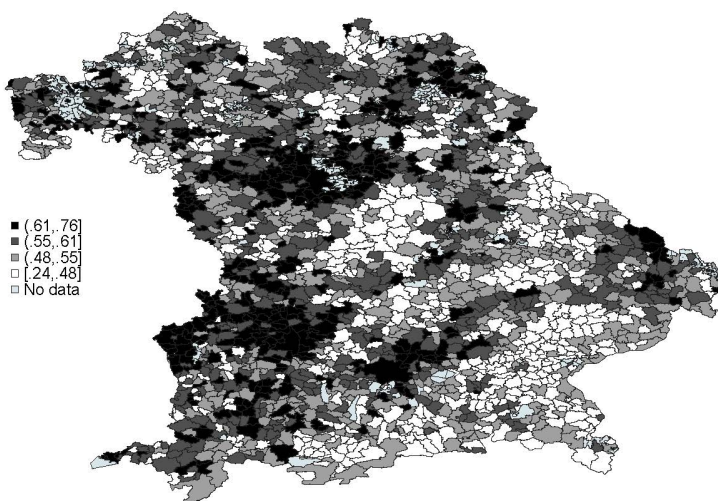
⁴⁸It could be argued that the electorate may vote for the initiative because voters might have an interest in setting the political agenda. However, this argument leaves open the question why a higher strength of the CSU (or other parties) should be associated with more demand for direct democracy if not because of discontent with the governing party. Importantly, discontent with the governing party does not need to result in lower support for it in elections as electoral support depends on the alternatives offered by other parties.

2.5 Results

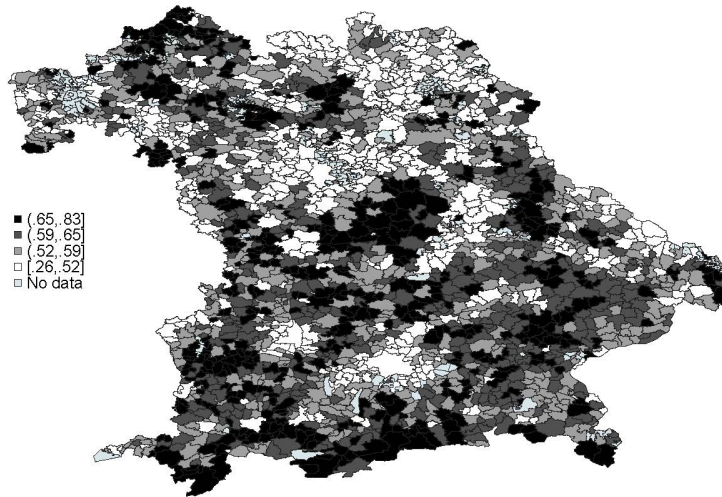
2.5.1 Baseline Results

Figures 2.1, 2.2 and 2.3 highlight the motivation and a central result of our contribution. Figure 2.1 shows the revealed preferences for more direct democracy at the local level and Figure 2.2 contrasts it with the CSU strength across the municipalities in Bavaria. A negative relationship between CSU strength and approval for direct democracy is directly discernible. Municipalities with higher support for the CSU typically had substantially lower approval rates for direct democracy. As the CSU was and still is the governing party and campaigned actively against the extension of direct democracy, we interpret this as first evidence against the dissatisfaction hypothesis regarding governing parties, i.e. municipalities with higher support for the governing CSU trusted their representatives and voted relatively more against the extension of direct democracy. The relationship becomes more apparent when looking at the scatterplot in Figure 2.3 which visualizes the negative correlation between the CSU vote share and approval for direct democracy in the referendum. Although the overall level of support for direct democracy is high throughout, we detect a strong negative correlation ($\rho = -0.6208$ and $p - Value = 0.000$) between the strength of the governing party and the revealed preference for direct democracy.

Figure 2.1: Initiative Approval: Graphical Evidence



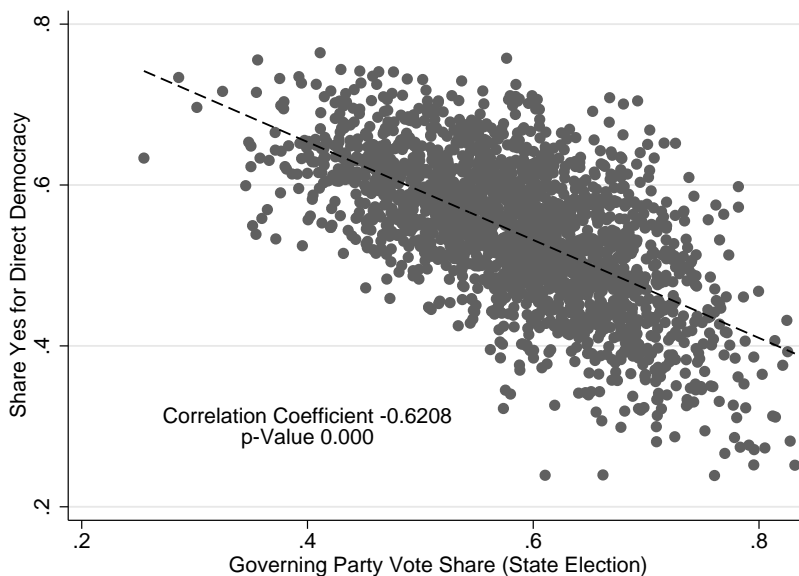
Notes: This graph depicts approval for the initiative. Each shade of grey depicts a different quartile of the vote distribution.

Figure 2.2: Governing Party Strength: Graphical Evidence

Notes: This graph depicts the strength of the CSU in the 1994 state election. Each shade of grey depicts a different quartile of the vote distribution.

Table 2.2 presents econometric evidence. Specification (1) gives the baseline model with the yes share for the *Direct Democracy* initiative, $PreferencesDD_i$ as dependent variable and the governing party vote share in the preceding state level election, $V_i^{CSUState}$, as the single explanatory variable. CSU strength in a municipality has a negative effect on the approval for direct democracy in the referendum. The point estimate suggests that for each percentage point increase in the CSU vote share in the state election, approval for the initiative by *More Democracy* goes down by 0.61 percentage points. This is a substantial association when taken at face value: Although the initiative was with 57.8 percent clearly accepted, a difference in only approximately 1.4 standard deviations in the CSU vote strength would have sufficed to prevent the extension of direct democratic participation in Bavarian municipalities. We will show that the sign and the magnitude of the estimated relationship is highly robust, it stems from a change in the CSU strength, it can most likely be interpreted causally and unobservables are unlikely to explain the effect.

To ensure that these results are not driven by demographic or socio-economic differences between municipalities which affect CSU strength and preferences for direct democracy at the same time, we add control variables in specification (2). We observe that the literature's expectation regarding the control variables broadly tend to hold: a higher share of elderly and Catholics is negatively associated with approval of more direct democracy at the local level. More urban municipalities

Figure 2.3: Revealed Preferences for Direct Democracy and Governing Party Strength

Notes: The figure shows a scatterplot of CSU strength against the approval rate in the state initiative election. Each dot represents a municipality. The dashed line is a linear fit. Source: Own calculation.

show higher levels of support for direct democracy. A municipality's financial situation, having a university and the share of employed are not significantly related to support levels. Importantly, qualitative and quantitative results for the CSU strength at the state election a year prior to the initiative remain virtually identical when compared to specification (1).

Specification (3) adds the CSU vote share at the previous municipal election, $VS_i^{CSU Local}$ and whether it runs as a party while specification (4) controls for political variables related to parties and support for the initiative. CSU strength at both levels (state and local) has a negative influence on support for direct democracy. A ceteris paribus increase in the CSU vote share in the state election of one percentage point is associated with a 0.594 percentage points lower support for direct democracy and for each percentage point increase in the CSU vote share in the municipal election, approval declines by 0.082 percentage points. As expected, $|\beta_1| > |\beta_2|$, i.e. the influence of the CSU strength at the state level is larger than at the municipal level. The null hypothesis of equality of the two coefficients is rejected with a p-value of 0.000.

In specification (4) the vote share of the Green Party is a positive predictor of voting for direct democracy. The Greens were the strongest advocates among state parties of more direct citizen participation at the local level in Bavaria. As

mayors are directly elected in Bavaria, it could be expected that they do not have any significant influence on support for direct democracy which is fully consistent with our findings. Mayors are already under direct control independent of their parties.⁴⁹ However, the number of competing parties in a municipal election proxies dissatisfaction with the political system in general and is, consequently, positively related to support for direct democracy. Turnout for the initiative has a negative impact on approval rates for the initiative election.⁵⁰ As expected, turnout in the state election and in the municipal election are both negatively correlated with approval for direct democracy but only turnout at the state level has a statistically significant effect. Again, the inclusion of these political control variables does not affect the statistical significance of our main variable of interest, the magnitude of the effect remains unchanged and CSU strength in state election has an absolutely higher effect than CSU strength in municipal election, i.e. $|\beta_1| > |\beta_2|$ (p-value = 0.000). Dissatisfaction rather seems to be related to the political system in general than with the elected governing party.

Not only the magnitude of the CSU vote share is important but also its explanatory power. This can best be seen by comparing the R^2 of the regressions in columns (1)-(4). CSU strength at the state level generates an R^2 of already 39 percent in column (1). More importantly, while the R^2 rises to 0.58 when including further a full set of control variables (i.e. the regional fixed effects), the strength of the governing party is still the single best predictor of voting in the initiative for an extension of direct democracy. This result suggests that party cues are an important predictor even in a direct democratic decision.

State and municipal elections took place before the vote on the initiative. To address potential endogeneity concerns, we implement an instrumental variable approach in specifications (5) and (6) of Table 2.2. We instrument the CSU vote share in the state election with the share of agricultural soil surface in the municipality. Historically, the CSU has been a party strongly present in rural areas. Thus, the identification idea relies on the assumption that the conservative CSU was traditionally strong in areas with more agriculture. This is confirmed by

⁴⁹An alternative explanation is that the effect of the CSU mayor is already captured by her/his party.

⁵⁰We are aware of the endogeneity of this variable. Still, we think that reporting this interesting correlation is of value to the reader. Reassuringly, results remain entirely stable when dropping turnout for the initiative from the model.

Table 2.2: Explaining the Initiative Outcome

Specification	(1)	(2)	(3)	(4)	(5)	(6)
	Share Yes	Share Yes	Share Yes	Share Yes	Share Yes	Share Yes
	OLS	OLS	OLS	OLS	IV	IV
Governing Party Vote Share (State Election)	-0.609*** (0.018)	-0.607*** (0.021)	-0.594*** (0.021)	-0.570*** (0.022)	-0.791*** (0.159)	-0.809*** (0.167)
Governing Party Vote Share (Municipal Election)			-0.082*** (0.017)	-0.057*** (0.020)		-0.025 (0.031)
CSU Runs			0.031*** (0.008)	0.023*** (0.009)		0.007 (0.015)
Green Party Vote Share (State Election)				0.203*** (0.078)	0.001 (0.169)	-0.024 (0.175)
CSU Mayor				0.002 (0.003)	0.001 (0.003)	0.002 (0.003)
Number of Parties				0.003** (0.001)	0.005*** (0.001)	0.004*** (0.002)
Turnout Initiative				-0.104*** (0.029)	-0.090*** (0.032)	-0.086*** (0.032)
Turnout (State Election)				-0.169*** (0.039)	-0.163*** (0.041)	-0.160*** (0.041)
Turnout (Municipal Election)				-0.035 (0.033)	-0.050 (0.036)	-0.053 (0.036)
Log(Population)		0.011*** (0.002)	0.012*** (0.002)	0.005* (0.003)	-0.001 (0.005)	-0.000 (0.005)
Per Capita Debt		-0.000 (0.000)	-0.000 (0.000)	-0.000 (0.000)	-0.000 (0.000)	-0.000 (0.000)
Share Employed		-0.023** (0.010)	-0.021** (0.011)	-0.021* (0.011)	-0.017 (0.011)	-0.017 (0.012)
Share Young		0.045 (0.090)	0.037 (0.090)	0.035 (0.090)	0.277 (0.201)	0.294 (0.202)
Share Elderly		-0.375*** (0.067)	-0.376*** (0.067)	-0.409*** (0.064)	-0.276** (0.118)	-0.264** (0.119)
In-Migration 1987-1995		0.009 (0.021)	0.008 (0.021)	-0.008 (0.021)	-0.022 (0.025)	-0.025 (0.024)
Share Catholic		-0.022** (0.009)	-0.018** (0.009)	-0.018** (0.009)	0.013 (0.026)	0.018 (0.026)
University		-0.002 (0.019)	-0.000 (0.017)	-0.002 (0.016)	-0.001 (0.018)	-0.002 (0.017)
Constant	0.897*** (0.010)	0.925*** (0.024)	0.916*** (0.024)	1.079*** (0.036)	1.136*** (0.051)	1.140*** (0.053)
Administrative Region Fixed Effects	no	yes	yes	yes	yes	yes
First Stage F-Test (p-Value)	-	-	-	-	30.60	29.45
N	2031	2028	2028	2023	2023	2023
R ²	0.39	0.55	0.55	0.58		

Notes: Heteroscedasticity-robust standard errors in parentheses. Significance Levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

the first stage regressions with an F-Test value of more than 30. The identifying assumption for the second stage to work is that the share of agricultural soil surface influences approval for direct democracy through no other channel than CSU strength. If one accepts this assumption⁵¹, then instrumental variable estimates are consistent and have a causal interpretation, i.e. we can exploit the exogenous variation in CSU strength due to differences in agricultural soil use to explain preferences for direct democracy. We always include socio-economic and

⁵¹We are aware of the potential shortcomings of our instrumental variable, which is why we refrain from calling the IV regressions our main specification. Rather, column (4) of Table 2.2, the OLS regression with the full set of covariates, is our focal point.

demographic control variables to control for the potentially omitted channels of influence. The results are reassuring: Coefficients of the CSU vote share are quantitatively in the same ballpark but slightly larger (in absolute value) than in the OLS approach. With the IV specification, it is estimated that a one percentage point increase in CSU strength leads to an approval rate of the initiative election that is approximately 0.80 percentage points lower.

2.5.2 Robustness Checks

The baseline results are robust to a variety of alternative specifications as shown in Tables 2.3 to 2.6. Robustness tests include the full set of control variables as well as administrative region fixed effects.

Weighting and Analyses of Subsamples

As municipalities have different population sizes, regressions where each observation has the same impact on the estimated coefficient can be misleading. We therefore replicate our main results from columns (4) and (6) of Table 2.2 using population weights. This implies that a municipality with 1000 inhabitants will have twice the effect on the coefficient of interest than a municipality with only 500 inhabitants. The results can be found in columns (1) and (2) of Table 2.3. It becomes evident that population weighting does not affect our quantitative or qualitative results. If anything, the OLS and IV coefficient are now closer together.

Specification (3) looks at the subsample of 775 municipalities with fewer than 2000 inhabitants to ensure that results are not driven by small municipalities. Specification (4) includes county-free cities into the sample.⁵² In both specifications the influence of CSU strength at the state and the municipal level remains statistically significant, the absolute magnitude remains comparable and the influence of strength at the state level is higher ($|\beta_1| > |\beta_2|$).⁵³

Analyzing municipal samples with differential strength of the CSU at the state level does not affect the statistical significance nor the magnitude of the CSU

⁵²For these 25 cities, we are lacking mayoral election results as well as the number of parties running for the municipal council, which is why we excluded them in the baseline specification. Here, we include them to show that they do not bias the general results.

⁵³As we use only one cross-section, we do not have a natural level to cluster the observations. Given that the municipalities are organized in counties, we tried to cluster the standard error by county (71 clusters) to allow for cross-dependencies on this level (results not reported). The results remain entirely robust.

strength on support for direct democracy (specifications (5) and (6)). In particular, for municipalities where the CSU was close to the 50 percent benchmark (column 6), its effect remains quantitatively similar to other specifications which lends support to the hypothesis that dissatisfaction with the political system is not directly related to the governing party.

It is interesting to note that when only estimating the influence of the CSU strength at the local level (specification (7)), i.e. without including the CSU vote share in the state election, the coefficient β_2 remains negative and statistically significant, it increases slightly in absolute size but remains comparable to specifications where $VS_i^{CSUState}$ is also included.

Selection on Unobservables and Bayesian Model Averaging

To further demonstrate the robustness of our results to potential endogeneity issues due to omitted variables, we conduct robustness tests in the spirit of [Oster \(2014\)](#) in Table 2.4. The basic idea of this test is that we can learn something about potential bias from unobservables by looking at coefficient movements after including observed control variables into the regression. Intuitively the logic behind the approach is the following: If the inclusion of *observed* covariates increases the explanatory power of the model substantially, i.e. the R^2 goes up, but leaves the coefficient of interest almost unaltered, i.e. β_1 remains stable, then one may assume that potential *unobserved* variables would not affect the coefficient either, since the included controls are already the ones the researcher deemed most important in terms of reducing endogeneity issues. We apply this approach and calculate the so-called *identified set* under a proportional selection assumption and a given maximum potential value of R^2 . If this set excludes zero, the coefficient estimates reach a level of robustness “in the range of what would be seen if the treatment was randomized” ([Oster 2014](#)), i.e. the results are likely not to suffer from endogeneity due to unobservables. As required by the literature, we compare movements in the coefficient of $VS^{CSUState}$ and the R^2 from the most parsimonious specification (without controls) to the full model (with all observed covariates) and then calculate the bias adjusted coefficient $\beta_1^{*'} = \tilde{\beta}_1 - \tilde{\delta} \frac{(\hat{\beta}_1 - \tilde{\beta}_1)(R_{max} - \tilde{R})}{(\tilde{R} - \tilde{R})}$ that constitutes the bound of the identified set. The results are very reassuring: We find that the identified set always excludes zero, independent of the degree of selection on unobservables (the choice of $\tilde{\delta} = \{0.5, 0.75, 1\}$). Hence, even under the most con-

Table 2.3: Robustness: Initiative Outcome

Sample / Specification	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Share Yes	Share Yes	Share Yes	Share Yes	Share Yes	Share Yes	Share Yes
	OLS PW	IV PW	Pop < 2000	County Free	$V_{SCSU} > Mean$	$0.45 < V_{SCSU} < 0.55$	CSU Local
Governing Party Vote Share (State Election)	-0.539*** (0.025)	-0.691*** (0.248)	-0.594*** (0.035)	-0.565*** (0.021)	-0.609*** (0.046)	-0.553*** (0.081)	
Governing Party Vote Share (Municipal Election)	-0.071*** (0.019)	-0.044 (0.050)	-0.049 (0.040)	-0.072*** (0.017)	-0.074** (0.029)	-0.091** (0.039)	-0.132*** (0.023)
CSU Runs	0.027*** (0.008)	0.015 (0.023)	0.018 (0.018)	0.031*** (0.007)	0.032** (0.013)	0.037*** (0.017)	0.063*** (0.010)
Green Vote Share (State Election)	0.124 (0.076)	0.012 (0.188)	0.228 (0.150)	0.220*** (0.077)	0.324*** (0.124)	0.077 (0.134)	0.745*** (0.094)
CSU Mayor	0.004 (0.003)	0.004 (0.003)	0.001 (0.005)		0.001 (0.004)	0.005 (0.005)	0.002 (0.003)
Number of Parties	0.001 (0.001)	0.002 (0.003)	0.005* (0.003)		0.003* (0.002)	0.002 (0.002)	0.000 (0.001)
Turnout Initiative	-0.036 (0.032)	-0.032 (0.034)	-0.184*** (0.050)		-0.198*** (0.048)	-0.033 (0.048)	-0.147*** (0.036)
Turnout (State Election)	-0.207*** (0.045)	-0.201*** (0.048)	-0.102* (0.061)	-0.177*** (0.038)	-0.243*** (0.059)	-0.043 (0.059)	-0.192*** (0.045)
Turnout (Municipal Election)	-0.022 (0.037)	-0.035 (0.044)	-0.107** (0.054)	-0.019 (0.032)	0.010 (0.052)	-0.139** (0.055)	0.006 (0.038)
Administrative Region Fixed Effects	yes	yes	yes	yes	yes	yes	yes
Baseline Controls	yes	yes	yes	yes	yes	yes	yes
N	2023	2023	775	2048	1045	554	2023
R ²	0.64	0.63	0.55	0.58	0.47	0.39	0.41

Notes: Baseline controls include all other variables employed in Table 2.2 and an intercept. Heteroscedasticity-robust standard errors in parentheses. Significance levels: * p < 0.10, ** p < 0.05, *** p < 0.01.

servative assumptions ($\tilde{\delta} = 1$) and $R_{max} = 1$, our results are robust to potential omitted variable bias.⁵⁴

Table 2.4: Oster (2014) Tests: Potential Bias from Unobservables

Proportional Selection Assumption	$\tilde{\delta} = 0.5$	$\tilde{\delta} = 0.75$	$\tilde{\delta} = 1$
Uncontrolled $\hat{\beta}_1$	-0.61	-0.61	-0.61
Controlled $\tilde{\beta}_1$	-0.57	-0.57	-0.57
Uncontrolled \hat{R}^2	0.39	0.39	0.39
Controlled \tilde{R}^2	0.58	0.58	0.58
Identified set $[\tilde{\beta}_1, \beta_1^{*'}]$	[-0.57, -0.53]	[-0.57, -0.50]	[-0.57, -0.48]
Zero excluded from identified set?	yes	yes	yes

Notes: This procedure of assessing potential bias from unobserved variables by looking at movements in coefficients and R^2 when including observed covariates has been developed by Oster (2014). It is based on previous work by Altonji et al. (2008, 2005). The uncontrolled $\hat{\beta}_1$ comes from column (1) in Table 2.2, while the controlled $\tilde{\beta}_1$ can be found in column (4). $\beta_1^{*'}$ is calculated with an assumed value of $R^{max} = 1$.

Employing Bayesian Model Averaging (BMA, see Raftery et al. 1997; Raftery 1995; Hoeting et al. 1999) we show in Table 2.5 that the governing party vote share, $V^{CSUState}$, is highly relevant for explaining preferences for direct democracy when evaluated over different combinations of independent variables. The central idea behind Bayesian Model Averaging is to estimate the distribution of an unknown parameter of interest across a large number of different models (model space). This allows us to calculate conditional means and standard deviations which can be interpreted similarly to common regression coefficients and standard errors. More importantly, we can calculate a posterior inclusion probability for every variable which can be interpreted as the probability that a specific variable is included in a model. Thereby, it serves as an indicator for the importance of the specific variables over the whole model space for a prior inclusion probability of 1/2. The estimated posterior probability of inclusion also reflects how often the respective variables turn out to be of explanatory importance when testing all models in the model space. Empirical results show that the CSU vote share has the maximal posterior inclusion probability (99.9 percent). The posterior conditional mean over all estimates performed in BMA suggests that the mass of coefficients for the $V^{CSUState}$ is concentrated around -0.549 which closely corresponds to our previous results. The CSU vote share at the previous municipal election has also a

⁵⁴Reasoning the other way around, one can also ask which value of δ would be needed to make the identified set include zero. We find that this maximum possible value of δ is 6.49, implying that selection on unobservables would need to be more than six times as large as selection on observables to bias our coefficient in a way that the sign would change.

high posterior inclusion probability and its posterior conditional mean is negative and statistically significant. As before, the (absolute) municipal CSU vote share has a smaller influence on preferences for direct democracy than the (absolute) state CSU vote share, i.e. $|\beta_1| > |\beta_2|$. Thus, the Bayesian Model Averaging approach suggests that our results are robust when evaluated over a large model space of different combinations of independent variables.

Table 2.5: Bayesian Model Averaging

	Inclusion probability	Conditional coefficient	Conditional SE
Governing Party Vote Share (State Election)	99.9	-0.549	0.021
Governing Party Vote Share (Municipal Election)	77.4	-0.057	0.033
CSU Runs	49.4	0.027	0.011
Green Party Vote Share (State Election)	99.9	0.448	0.073
CSU Mayor	1.3	-0.001	0.003
Number of Parties	74.6	0.004	0.001
Turnout Initiative	2.1	0.024	0.029
Turnout State Election	99.9	-0.303	0.033
Turnout Municipal Election	83.9	-0.104	0.031
Log(Population)	13.5	0.006	0.003
Per Capita Debt	1.4	1.59E-06	3.11E-06
Share Employed	1.2	-0.004	0.011
Share Young	99.9	0.355	0.083
Share Elderly	99.9	-0.303	0.073
In-Migration 1987 - 1995	28.6	-0.051	0.021
Share Catholic	99.9	-0.041	0.006
University	1.4	-0.012	0.024

Notes: The posterior inclusion probability represents the likelihood that a variable is included in the model. The conditional coefficient and the conditional SE are conditional on inclusion of the variable in the model and represent the posterior mean of coefficients and the posterior standard deviations. BMA results were obtained using the software of the R Project for Statistical Computing with the BMA package.

Exploiting Turnout and Vote Shares

Our results suggest a negative causal effect of the governing party vote share on support for direct democracy. To ensure that the effect stems from higher individual levels of support instead of different compositions of the electorate (ecological fallacy), we control for opposition parties and turnout in the initiative election. Table 2.6 provides additional support for our interpretation: We estimate the relationship for four subsamples of high/low CSU vote shares and high/low turnout in municipalities. Changes in the $VS^{CSUState}$ are likely to stem from higher individual levels of support in municipalities where the CSU vote share is high and where turnout in the initiative election is high (first quadrant), as the voting population in these municipalities is composed of individual CSU supporters. We observe that the coefficient of the variable $VS^{CSUState}$ for this subsample is -0.66 which closely resembles previous estimates. If individual voters from the opposition would drive

our result or low turnout rates would drive our effect, we should expect a high absolute coefficient for municipalities with low CSU vote shares and low turnout in the initiative. This is not the case as shown in the fourth quadrant: Increasing $V S^{CSU State}$ decreases the support for direct democracy in this subsample too, but the effect is with 0.46 percentage points slightly lower than in the first quadrant. This suggests that individual levels of support for the governing party matter. The second and the third quadrant provide results for municipalities where the CSU vote share is high but turnout is low and where the CSU vote share is low but turnout is high, respectively. All effects are qualitatively and quantitatively similar to earlier estimates, confirming once more the robustness of our baseline results.

Table 2.6: Ecological Fallacy: Governing Party Vote Share Coefficients

	<i>High Initiative Turnout</i>	<i>Low Initiative Turnout</i>
<i>CSU Vote Share High</i>	-0.656*** (0.059)	-0.555*** (0.078)
<i>CSU Vote Share Low</i>	-0.619*** (0.070)	-0.463*** (0.053)

Notes: Heteroscedasticity-robust standard errors in parentheses. Significance Levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Each cell reports the coefficient of the governing party vote share on voting for direct democracy from a separate regression. We estimate the same models as in Table 2.2, column (4). The columns divide the sample into municipalities below and above the median turnout at the initiative. The rows divide the sample into municipalities where the governing party is stronger or weaker than the median municipality in terms of CSU vote share.

2.5.3 Refinements

Empirical evidence so far suggests that political dissatisfaction is rather related to the political system than to the governing elected party. In Table 2.7 we investigate further differential hypotheses to support this interpretation.

Mobilization for and approval of the initiative for an extension of direct democracy are jointly determined. If the governing party was able to cast doubt on the benefits of direct democracy and if it mobilized people against the initiative, we would expect that in municipalities with a relatively high turnout, CSU strength should play a relatively higher role compared to municipalities with a relatively lower turnout for the initiative. Specifications (1) and (2) provide support for this view and specification (3) estimates an interaction effect between turnout and ruling party strength. Lower levels of turnout moderate the effect that CSU

strength has on revealed preferences for direct democracy. At average turnout levels an increase of 1 percentage point in the governing party vote share reduces support for direct democracy by $(0.235 + 0.874 \cdot 0.690 =)$ 0.838 percentage points in the approval rate. At a reduced turnout rate of 0.580 (two standard deviations reductions) the effect is reduced to 0.742 percentage points.

Table 2.7: Refinements: Initiative Outcome

Sample / Specification	(1)	(2)	(3)	(4)	(5)
	Share Yes	Share Yes	Share Yes	Share Yes	Share Yes
	Turnout > Mean	Turnout ≤ Mean	Interaction	Interaction	Interaction
Governing Party Vote Share (State Election)	-0.612*** (0.029)	-0.545*** (0.032)	-0.235** (0.100)	-0.558*** (0.025)	-0.663*** (0.041)
Governing Party Vote Share (Municipal Election)	-0.047* (0.026)	-0.086*** (0.032)	-0.054*** (0.020)	-0.057*** (0.020)	-0.060*** (0.020)
CSU Runs	0.014 (0.011)	0.037*** (0.014)	0.021** (0.009)	0.023*** (0.009)	0.024*** (0.009)
Green Vote Share (State Election)	0.110 (0.096)	0.220* (0.125)	0.204*** (0.078)	0.203*** (0.078)	0.203*** (0.078)
CSU Mayor	0.002 (0.004)	0.003 (0.004)	0.002 (0.003)	0.019 (0.018)	0.002 (0.003)
Number of Parties	0.006*** (0.002)	0.001 (0.002)	0.003** (0.001)	0.003** (0.001)	-0.014** (0.006)
Turnout (State Election)	-0.260*** (0.056)	-0.215*** (0.045)	-0.158*** (0.039)	-0.167*** (0.039)	-0.164*** (0.039)
Turnout (Municipal Election)	-0.036 (0.051)	-0.022 (0.044)	-0.028 (0.033)	-0.035 (0.033)	-0.042 (0.033)
Turnout Initiative			0.394*** (0.147)	-0.105*** (0.029)	-0.104*** (0.029)
Interaction Turnout * CSU Vote Share			-0.874*** (0.252)		
Interaction CSU Mayor * CSU Vote Share				-0.029 (0.031)	
Interaction Number of Parties * CSU Vote Share					0.028*** (0.010)
Constant	1.159*** (0.053)	1.008*** (0.054)	0.867*** (0.070)	1.071*** (0.037)	1.141*** (0.041)
Administrative Region Fixed Effects	yes	yes	yes	yes	yes
Baseline Controls	yes	yes	yes	yes	yes
N	1023	1000	2023	2023	2023
R ²	0.63	0.53	0.58	0.58	0.58

Notes: Heteroscedasticity-robust standard errors in parentheses. Significance Levels: * p < 0.10, ** p < 0.05, *** p < 0.01.

Having a mayor of the ruling CSU party in a municipality has, *ceteris paribus*, no effect on support for direct democracy as mayors are already directly elected. Consequently, we would also expect that CSU mayors should not moderate the influence of CSU strength in the state level on revealed preferences for direct democracy. Specification (4) shows that this is the case. The interaction effect of the CSU Mayor and the vote share is insignificant and negligible regarding its magnitude while the baseline effect of the vote share in the state election remains statistically significant and similar in size compared to earlier specifications.

Political competition at the local level expressed by the number of parties should, however, have the moderating effect (specification (5)). *Ceteris paribus*, more

parties at the municipal level are a sign for general political discontent and are associated with support for more direct democracy. More local political competition in interaction with support for the CSU at the state level has a positive influence on voting for the initiative. This finding is consistent with the control function of direct democracy at the local level: While people may trust the ruling party at the state level, relatively more distrust of politics in general is associated with more demand for direct democratic participation as strength of the governing party increases (positive interaction term with negative baseline effect of governing party vote share).

2.6 Conclusion

Direct democratic participation is a relevant policy issue in many countries around the world and citizens seem to be interested in more direct democracy when looking at opinion surveys on the subject. Nowadays, almost no politician argues openly against more citizen participation prior to elections but promises for direct democracy are seldom fulfilled after elections.

Voters in Bavaria decided in a constitutional initiative election at the state level in 1995 to grant themselves more direct democratic participation rights at the local level. This unique setting allows us to directly analyze revealed preferences for direct democracy instead of relying on opinion surveys. Employing revealed preferences complements other analyses which employ opinion surveys and questions regarding the extension of direct democracy. It allows new and different insights because the decision we analyze is binding and has been implemented after the successful initiative election. In particular, the institutional setting allows us to explore factors which drive revealed preferences for more direct democratic participation at the local level and we can explore whether dissatisfaction with representative democracy or rather dissatisfaction with the governing party at the state level (and often at the local level too) is related to actual voter support for direct democracy.

Empirical results show that support for direct democracy at the local level is negatively associated with support for the elected governing party. In fact, electoral support for the governing party at the local level in the previous state election is the strongest single predictor for the actual extension of direct democracy. Instrumental variable results point to a causal interpretation of this finding. Numerous

robustness analyses and testing differential hypotheses lead to the same conclusion. Alternatively to the IV, we also account for potential endogeneity issues and find that the effect of the strength of the governing party is most likely not affected by unobservables. Thus, our results provide tentative support to the view that dissatisfaction with politics is not linked to a specific elected governing party. Rather it seems to be the case that dissatisfaction with representative democracy in general is a driving factor for voting for direct democracy.

Chapter 3

Turnout and Closeness: Evidence from 60 Years of Bavarian Mayoral Elections[‡]

3.1 Introduction

The well-known calculus of voting ([Downs, 1957](#); [Riker and Ordeshook, 1968](#)) postulates that the probability of influencing the electoral outcome is an important determinant in the turnout decision. It predicts that electoral participation should increase in the closeness of the race. Put differently, when "more is at stake," people have an extra incentive to go to the polls.

However, it is not straightforward to measure electoral closeness in empirical applications. How do people know whether the election is competitive or not? Several approaches are used in the literature. Most existing studies rely on *ex-post* realized closeness and assume rational expectations of the voters. Apart from making a strong assumption, additional endogeneity concerns arise. If vote shares appear on the right hand side and turnout on the left hand side of the empirical model, the total number of votes cast appears on both sides of the equation. Turnout and *ex-post* closeness may, therefore, be spuriously correlated ([Cox, 1988](#)). As a consequence, other researchers employ *ex-ante* historic closeness measures from previous elections to avoid these issues. However, elections are not necessarily

[‡]A similar version of this chapter has been published as a DIW Discussion Paper, see [Arnold \(2015\)](#). I have been invited to Revise & Resubmit the paper for publication in the *Scandinavian Journal of Economics*.

comparable across time, especially if the set of candidates changes. The longer the electoral period, the less useful is the information from a previous election. A third approach therefore relies on pre-election opinion polls to gauge voters' perception of electoral closeness. However, these data are typically only available for large-scale federal elections. It is unclear whether electoral closeness exists in this context at all, because even a razor-thin victory in a large electorate may translate into vote margins of tens of thousands of votes between winner and runner up.

In this paper, a different approach is used. I employ two-round elections to identify closeness effects. Mayoral elections in the German state of Bavaria provide an ideal institutional setting: According to the local constitution in Bavaria, if no mayoral candidate is able to get an absolute majority of the vote in the first round, a second (runoff) election is held two weeks later with the two leading candidates from the first round. Now, closeness of the first round can be taken as a proxy for closeness of the second round. This measure, which I call *Revealed Closeness*, avoids the drawbacks of other commonly used measures in the literature. *Revealed Closeness* is predetermined and recent at the same time, whereas other *ex-ante* (*ex-post*) measures only fulfill the first (second) criterion. Another advantage of Bavarian mayoral elections lies in the small size of the electorate. Municipalities in Bavaria can have as few as 17 eligible voters. The largest city in the data still has fewer than 40,000 eligible voters. Thus, I can evaluate closeness effects in a setting where the probability of being the decisive voter is non-negligible, which is rarely the case in large-scale federal elections.

Using data on all mayoral elections in 2031 Bavarian municipalities between 1946 and 2009, I find that closeness does indeed matter for electoral participation: A one standard deviation increase in closeness increases turnout by 1.68 percentage points, which corresponds to $\frac{1}{6}$ of a standard deviation in this variable. Importantly, I show that *Revealed Closeness*, which makes use of two-round elections, yields approximately the same results as the common *ex-post* measures employed in the literature so far. Due to the panel structure of the data, I can also evaluate a historical *ex-ante* measure of closeness. I contribute to the literature by offering a first comparison of three differently calculated indicators (*ex-ante*, *ex-post*, two-round) for electoral closeness. Importantly, this comparison is conducted within one institutional framework.

Heterogeneous effects of other electoral stimuli, depending on closeness, are also identified. For example, rain on election day reduces turnout *on average*, but

not if the election is close. Furthermore, the negative effect of constituency size on turnout is also mitigated by closeness. These findings suggest that electoral closeness works as an important mediator. Other determinants of turnout can thus be thought of as second rank – they matter only if the race is not close. Implications for election campaigns are in contrast to conventional wisdom: Usually, candidates running in several constituencies concentrate their mobilization efforts on close races. My findings suggest, however, that electoral stimuli work best in uncompetitive races and question whether they have an effect in close races at all.

The remainder of this paper is organized as follows: Section 3.2 shortly presents the theoretical background. Section 3.3 reviews the empirical literature on turnout and electoral closeness. The institutional setting of Bavarian mayoral elections and the data are described in Section 3.4. Section 3.5 holds descriptive statistics and introduces the empirical strategy. Results are presented in Section 3.6. Finally, Section 3.7 concludes.

3.2 Theoretical Considerations

Why should electoral closeness incite people to turn out to vote? The calculus of voting developed by Downs (1957) and Riker and Ordeshook (1968) contends that individuals compare the benefits and costs of voting in a rational choice manner.⁵⁵ Let the net benefit of voting be described by the following expression:

$$Y = \pi B + D - K \tag{3.1}$$

Three factors determine Y , the net benefit of voting: πB is the expected individual benefit of voting which depends on the outcome of the election. It is comprised of π , the probability that a single vote will be decisive, and B , the specific benefits that materialize if the preferred candidate wins and takes office. D is a payoff that realizes independent of the electoral outcome. It is often called the "civic duty" component of the calculus of voting. K are the (opportunity) costs of going to the polls.

An individual decides to cast a ballot if the benefits outweigh the costs of voting, i.e. if $Y > 0$. Where does electoral closeness affect the turnout decision? The answer is that individuals will have a different perception of the parameter π in close races. If one's preferred candidate wins (or loses) for sure, π is 0, and a single

⁵⁵For a summary of economic theories on voter turnout, see Dhillon and Peralta (2002).

vote does not change that. As a consequence, πB reduces to zero and does not influence the turnout decision any more. However, if the race is close, one vote can be decisive and $\pi \in (0, 1]$. Vice versa, abstaining could lead to a marginal loss of one's preferred candidate.⁵⁶ Therefore, close races offer the possibility of discontinuously getting the additional benefit B , which motivates people to turn out to vote. Put differently, the potential pivotalness of a single vote increases in the closeness of the race. Let closeness be denoted by c . Then we have

$$\frac{\partial \pi}{\partial c} > 0. \quad (3.2)$$

As closeness positively impacts π , and π itself has a positive effect on the turnout decision, closeness and turnout are positively related. Thus,

$$Y = \pi(c)B + D - K \quad (3.3)$$

and

$$\frac{\partial Y}{\partial c} = \frac{\partial Y}{\partial \pi} \frac{\partial \pi}{\partial c} = B \frac{\partial \pi}{\partial c} > 0. \quad (3.4)$$

Ceteris paribus, electoral closeness increases turnout by affecting the probability of a single vote being pivotal.

3.3 Empirical Literature

The interplay of closeness and turnout is a longstanding subject of debate in economics and political science.⁵⁷ The empirical focus is mostly on higher level

⁵⁶Some argue that even in close elections, the probability of being pivotal is essentially zero. As [Schwartz \(1987\)](#) put it, "saying that closeness increases the probability of being pivotal [...] is like saying that tall men are more likely than short men to bump their heads on the moon." I provide two answers to this objection. First, constituency size can be *extremely* small in Bavarian mayoral elections. Some municipalities have no more than 20 eligible voters. While closeness may play no role in state-wide ballots, it can make a difference in small Bavarian municipalities. Second, there is ample evidence that individuals have difficulties in handling small probabilities and hence overestimate the likelihood of their vote being decisive ([Kahneman and Tversky, 1979](#); [Fehr-Duda and Epper, 2012](#)). Closeness thus increases turnout depending on the *perceived* probability of being pivotal, not the *actual* probability of being pivotal.

⁵⁷A specific branch of the literature deals with the partisan effects of voter turnout. [Hansford and Gomez \(2010\)](#) use election day rain as an instrument for turnout and find that higher turnout helps the Democrats in US presidential elections. In a similar fashion, [Gomez et al. \(2007\)](#) estimate that one inch of rain reduces turnout in a county by roughly 0.8 percent. Related, one inch above normal rain gives the Republican candidate an extra 2.5 percent of the vote. For Germany, [Arnold and Freier \(2015a\)](#) find that social democrats profit from higher turnout levels. Conservatives, on the contrary, seem to suffer under higher electoral participation.

elections. [Endersby et al. \(2002\)](#) find that closeness positively impacts turnout in the 1993 / 1997 Canadian federal elections, controlling for campaign expenditures. The same effect is established for the 1982 US House election by [Cox and Munger \(1989\)](#). [Shachar and Nalebuff \(1999\)](#) develop a structural model to evaluate feedback effects between closeness, turnout and mobilization efforts of political leaders. They estimate that a one percent increase in electoral closeness increases turnout by 0.34 percent. In Germany, a small positive closeness effect is found for the federal elections from 1983 to 1994 ([Kirchgässner and Zu Himmern, 1997](#)). However, the robustness of the effect seems to be confined to West Germany. [Grofman et al. \(1998\)](#) argue that in US Senate and House elections turnout is not maximized at maximal closeness (i.e. the 50-50 split of the vote), but at a Republican share greater than 50 percent. According to the authors, this happens because Republican partisans are generally more likely to vote. For Swiss referendums between 1981 and 1999, mobilization effort is a better predictor of turnout than expected closeness ([Kirchgässner and Schulz, 2005](#)). All these studies use an *ex-post* measure of electoral closeness. For a review and meta-analysis of all empirical studies explaining voter turnout, see [Geys \(2006\)](#).

A more direct test of the people's reaction to the parameter π in the calculus of voting is provided by [Tukiainen and Lyytikäinen \(2013\)](#). Using exogenous variation in pivotal probabilities that occur at population thresholds where council size changes discontinuously, the authors show that turnout is higher just above the thresholds where the probability of being pivotal increases and conclude that "voters are rational." [Andersen et al. \(2014\)](#) find that in Norway, people are more likely to vote when more is at stake in the election. Due to topography-determined exogenous hydropower income of some municipalities, they can evaluate how people respond to changes in the size of the parameter B in the calculus of voting. According to their estimates, a change in hydropower income from minimum to maximum increases turnout by six percentage points. [Fraga and Hersh \(2010\)](#) provide a first estimate of the interaction between closeness and inclement weather on election day using county data for US Presidential elections. In the calculus of voting, rain works as an exogenously imposed cost on the act of voting, that is, it is equivalent to an increase in the parameter K . They find that rain negatively impacts turnout on average, but this effect is mitigated in close elections. Closeness is calculated both with an *ex-ante* as well as an *ex-post* measure.

There is a small and recent literature that also uses two-round elections to identify closeness effects ([Fauvelle-Aymar and François, 2006](#); [Simonovits, 2012](#); [Garmann,](#)

2014; Indridason, 2008; De Paola and Scoppa, 2014).⁵⁸ I contribute to this literature and extend it by offering two innovations: First, the long panel data allow to credibly evaluate measures of lagged closeness and provide sufficient variation to include electorate fixed effects into the model. Second, the use of precipitation data is still quite rare in turnout applications. I offer a first estimate of the interaction effect between electoral closeness and inclement weather on election day for an election outside the United States.⁵⁹

3.4 Institutional Setting and Data

Bavaria is Germany's largest state by area and has about 12.6 million inhabitants (as of June 2014). The governing structure is organized into several administrative layers: In total, there are 2056 municipalities (including 25 county-free cities), 96 counties, and 7 administrative regions. Hence, municipalities constitute the smallest organizational unit of government. They are governed by a mayor (executive branch) and a town council (legislative branch). The mayor is also a full member of the town council and serves as its president. Once elected, the mayor is the highest representative of the municipality and executes decisions taken by the council. Larger municipalities have full-time mayors that are employed as public servants. Smaller municipalities (with less than 5000 inhabitants) often have honorary mayors.⁶⁰

Since the end of World War II, each Bavarian municipality directly elects its mayor. Each citizen above the age of 18 is allowed to vote. Running as a candidate is possible from age 21 onwards. To win the election in the first round, an absolute majority of votes is necessary. If no candidate is able to gather more than 50

⁵⁸Two-round elections are also used to measure so-called bandwagon effects (Kiss and Simonovits, 2014; Ade and Freier, 2013) or to study the formation of electoral pacts (Blais and Indridason, 2007).

⁵⁹The data on Bavarian mayoral elections employed in this paper have been used beforehand. Freier (2011) estimates party incumbency effects in the order of 38-40 percentage points in the probability of winning the next mayoral election. In a related paper, Freier and Thomasius (2012) find that more qualified candidates receive higher vote shares. Furthermore, social-democratic mayors seem to reduce local taxes, while conservative mayors increase taxes (Freier and Odendahl, 2012). Schild (2013) asks whether female mayors allocate municipal budgets to different categories than their male counterparts and finds no significant differences. All these studies rely on Regression Discontinuity Designs (RDD) to estimate causal effects. On the validity of RDD in electoral contexts, see Eggers et al. (2015), who also use the data on Bavarian mayoral elections.

⁶⁰More detailed information on tasks and status of the mayor can be found in the Bavarian municipal code (*Gemeindeordnung, GO*), articles 34-39, accessible via www.gesetze-bayern.de.

percent of the votes, then a runoff election is held two weeks after the first election. The two leading candidates from the first round advance to the second round. Whoever gathers a majority of votes in the runoff election is the newly elected mayor. A mayoral term lasts six years.⁶¹ Around 85 percent of the elections are held in March or the beginning of April. The remaining 15 percent are distributed relatively evenly over the year.⁶²

I have data on all Bavarian mayoral elections between 1946 and 2009. With 27015 observations in total and 2031 municipalities,⁶³ this makes about 13 elections per municipality on average. The data are from the state statistical office and include a variety of information. For example, I know all candidates' names, gender, profession and party affiliation. Furthermore, information on the exact election day, the number of eligible voters, the number of voters who turned out and individual candidate vote shares are available. I also know the status of the mayor (full-time vs. honorary) and whether a runoff election was held or not.

I combine these mayoral election data with precipitation data from 559 weather stations in Bavaria. These daily time series data are recorded and published by the German Weather Service (*Deutscher Wetterdienst, DWD*). As I know geographic locations of municipalities and weather stations as well as the exact election dates, I can tell exactly how much it rained on election day in a given municipality (which is assigned to the nearest weather station). Figure 3.1 in the appendix shows a map of Bavaria with all weather stations marked by blue dots.

3.5 Descriptives and Empirical Strategy

Before looking at the data, one note of caution is in order. To this day, the literature has not yet agreed on one commonly used measure of electoral closeness.⁶⁴

⁶¹The detailed electoral rules are laid down in the Bavarian electoral law for municipalities and counties (*Gemeinde- und Landkreiswahlgesetz, GLKrWG*), articles 39-52, accessible via www.gesetze-bayern.de.

⁶²The "official" election date is in March. However, if an incumbent resigns or retires before the end of the electoral period, early elections can be called.

⁶³Mayoral election results for the 25 county-free cities are missing.

⁶⁴See [Endersby et al. \(2002\)](#) for development of a competitiveness index and a comparison of different closeness indicators in Canadian federal elections. See [Kayser and Lindstaedt \(2015\)](#) for a cross-nationally applicable measure of electoral competitiveness.

Most authors use variations of vote margins between winner and runner-up.⁶⁵ The disadvantage of this measure is its interpretability. When vote margins are introduced as closeness proxy in a regression framework, signs are reversed. Higher closeness is equivalent to *smaller* vote margins and vice versa. To ease readability, I use a variation of the vote margin as an indicator for closeness:

$$Closeness = 1 - (vs_1 - vs_2) \quad (3.5)$$

where vs_i is the vote share of the candidate finishing i^{th} place. With this definition, the closeness variable is bounded between 0 and 1 and has a positive sign. A value of 1 indicates maximum closeness: There is a tie for first and second place. A value of 0, on the contrary, signals that the election was not close at all. Either the first place candidate was able to garner all the votes, or there was simply no contender.

Descriptive statistics of all variables used in the analysis can be found in Tables 3.2, 3.3 and 3.4 in the appendix. I distinguish between three different types of elections: Table 3.2 contains information on "uncontested elections," i.e. elections with low competition where one candidate is able to gather more than 50 percent of the vote in the first round. Most elections are uncontested. Table 3.3 holds the same statistics for "contested elections." These are first round elections where no single candidate is able to receive an absolute majority of votes. Finally, Table 3.4 shows descriptive statistics for runoff elections. These elections automatically follow contested elections and only the two leading candidates from the first round are allowed to run in the second round.

Several differences emerge between the different samples. Turnout is lower in runoff elections, indicating voter fatigue and dissatisfaction when one's preferred candidate does not reach the second round. Nevertheless, the overall turnout levels – at about 80 percent – are quite high compared to state or federal elections. Closeness is highest in contested elections and lowest in uncontested elections, as one would expect. The distributions of turnout and closeness in the three different samples are visualized in Figure 3.2 in the appendix. Elections are more likely to be contested when electorate size is larger and when more candidates

⁶⁵In a methodological note, Cox (1988) warns that vote margins – when introduced as explanatory variables in a regression framework – are spuriously correlated with the dependent variable turnout. He suggests to use raw vote margins instead. Note that his criticism does not apply to my approach, because I use vote margins from the first round and turnout from the second round. My closeness measure is thus predetermined and not endogenous to turnout.

decide to run. Surprisingly, elections for honorary mayor positions are contested more often than the ones for full-time mayor positions. The maximum number of eligible voters is 38461 and hence quite small. This reflects the fact that mayoral elections in the 25 county-free cities, which constitute the largest cities, are not recorded in the dataset. Females are more likely to run in contested elections, although their overall share of the candidate pool is never larger than 15 percent. The well-known lack of women in politics (Lawless and Pearson, 2008; Ferreira and Gyourko, 2014) is strikingly visible in Bavarian mayoral elections.⁶⁶

Given that weather is essentially random, it is not surprising that the rain variables do not show substantial differences in the various subsamples. In Table 3.2, rain on election day amounts to 1.9 millimeters on average. However, this includes all election days with no rain at all, which constitute almost 60 percent of the sample. Conditional on some rain at all, average rainfall on election day rises to $\frac{1.868}{0.585} = 3.193$ millimeters.⁶⁷ The quality and accuracy of the weather data are captured by the distance to the nearest weather station. As the grid of weather stations is quite dense in Bavaria (see Figure 3.1), there is no municipality located further away than 14 kilometers from the next station. Average distance is below 5 kilometers.⁶⁸

Due to the panel structure of the data and the special institutional setting where some (but not all) elections have a second (runoff) round, I can evaluate three different measures of closeness and compare them:

Lagged Closeness takes closeness from the preceding regular election as the crucial measure. As this is lagged by six years or one electoral period, it is known to the voters on election day (it is *ex-ante*). However, it contains information on an election with possibly completely different circumstances / political constellations than the current election.

Actual Closeness takes on the value of the *ex-post* realized closeness of the current

⁶⁶Related, Freier and Thomasius (2012) find that female candidates in Bavaria suffer an electoral disadvantage of roughly five percent in vote share.

⁶⁷As most elections take place in (relatively) rainy March, the variation in rain across time and space is quite high.

⁶⁸Note that the number of observations is somewhat smaller for the rain variables. This is due to the fact that some weather stations were inaugurated only later in the sample period. In principle, I could match uncovered early elections to weather stations further away and retain all observations. However, this comes at the cost of bringing more noise into the weather variables. Given that precipitation (and especially rain showers) can be quite local, I decided against the matching over longer distances.

election. As this is generally unknown on election day, one has to assume rational expectations of the voters if this measure is supposed to influence their decision to turn out or not.

Revealed Closeness makes use of two-round elections. Closeness of the first round is taken as a signal for competitiveness of the second round two weeks later. This measure is superior to the two previous ones: It is recent (contrary to *Lagged Closeness*) and predetermined (contrary to *Actual Closeness*) at the same time.

The basic empirical model looks as follows:

$$T_{it} = \alpha + \beta C_{it} + X'_{it}\omega + \theta_i + \epsilon_{it}. \quad (3.6)$$

The dependent variable T_{it} is turnout in municipality i at date t . I define turnout as the number of voters that go to the polls divided by the number of eligible voters. C_{it} is the closeness of the election, according to the three different concepts introduced above and calculated as in Equation 3.5. For actual and lagged closeness, I use the sample of uncontested elections. To evaluate the effect of revealed closeness on turnout, I use the sample of runoff elections and add closeness from contested elections two weeks prior. The vector of control variables X includes a third order time trend, a third order polynomial in the number of eligible voters, a dummy for concurrent elections, dummies for female candidates and honorary mayor positions as well as the lagged dependent variable (to account for "usual" levels of electoral participation). I also add dummies for the number of candidates. Fixed effects for each municipality i are included in θ_i , where $i = 1, 2, \dots, 2031$. Standard errors are robust to heteroscedasticity.

3.6 Results

3.6.1 Main Results

Table 3.1 shows regressions of turnout on the three different measures of electoral closeness.

Column (1) holds the results for *Lagged Closeness* using the sample of uncontested elections. One can see that lagged closeness exerts a small *negative* impact on turnout, which is unexpected and stands in contrast to the theoretical considerations introduced above. I argue, however, that this measure is confounded

Table 3.1: Main Results

Sample	(1)	(2)	(3)
	Turnout	Turnout	Turnout
	<i>Uncontested Elections</i>		<i>Runoff Elections</i>
Lagged Closeness	-0.012*** (0.001)		
Actual Closeness		0.053*** (0.002)	
Revealed Closeness			0.234*** (0.020)
Turnout in Preceding Election	0.213*** (0.011)	0.188*** (0.010)	0.865*** (0.042)
Concurrent Election	0.067*** (0.006)	0.066*** (0.007)	0.071*** (0.024)
Female Candidate(s)	-0.000 (0.002)	-0.000 (0.002)	0.001 (0.005)
Honorary Mayor	0.002 (0.002)	0.003 (0.002)	0.004 (0.005)
Two Candidates	0.057*** (0.001)	0.025*** (0.002)	
Three Candidates	0.067*** (0.002)	0.036*** (0.002)	0.004 (0.009)
Four Candidates	0.071*** (0.004)	0.040*** (0.004)	-0.001 (0.010)
Five Candidates	0.065*** (0.008)	0.034*** (0.008)	-0.014 (0.010)
Six Candidates	0.083*** (0.011)	0.052*** (0.010)	0.001 (0.012)
Seven Candidates	0.103*** (0.011)	0.073*** (0.008)	0.007 (0.012)
Constant	0.690*** (0.011)	0.701*** (0.010)	-0.147*** (0.045)
Municipal Fixed Effects	yes	yes	yes
Third Order Electorate Size Polynomial	yes	yes	yes
Third Order Time Trend	yes	yes	yes
N	20582	20582	1859
R ²	0.64	0.65	0.83

Notes: Standard errors robust to heteroscedasticity. Significance Levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The data cover all mayoral elections in 2031 Bavarian municipalities from 1946-2009.

with incumbency effects that materialize over time. Assume that the last election was very close (i.e. a high value of *Lagged Closeness*). It is quite likely that this last election was a hard-fought race for an open seat. Then one of the candidates

won and took office. In the current election, he is the incumbent and profits from substantial incumbency advantages, as shown in Freier (2011). Therefore, the race is not close and people abstain from voting. Thus, a high value of *Lagged Closeness* is associated with low levels of turnout today due to incumbency effects that materialize between the two elections. In a setting like Bavaria, where incumbency effects are important in magnitude,⁶⁹ measures like *Lagged Closeness* are of limited value.

Column (2) presents results for *Actual Closeness*. Here, the coefficient is in line with theoretical expectations. If closeness switches from 0 to 1 (i.e. from minimum to maximum), turnout increases by 5.3 percentage points. Another way to look at the coefficient is the following: A one standard deviation increase in *Actual Closeness* (0.325) leads to a turnout rate that is $0.053 \cdot 0.325 \approx 1.72$ percentage points higher, which roughly corresponds to a $\frac{1}{5}$ standard deviation increase in this variable.

Finally, column (3) shows results for the measure of *Revealed Closeness*, which makes use of two-round elections. Also here, the coefficient of interest is positive and significant, as anticipated. It is also four to five times as large as in column (2). However, one has to keep in mind that *Revealed Closeness* only varies between 0.5 and 1, because it is calculated from the contested election two weeks prior where no single candidate was able to garner more than 50 percent of the vote. Putting the size of the coefficient into perspective, a one standard deviation increase in *Revealed Closeness* (0.072) leads to a turnout rate that is $0.234 \cdot 0.072 \approx 1.68$ percentage points higher. This corresponds to roughly $\frac{1}{6}$ of a standard deviation in this variable.

Figure 3.3 in the appendix shows the slope coefficients of lagged, actual and revealed closeness in the turnout equation and thereby visualizes the findings from Table 3.1.

The coefficients of the control variables also merit a short discussion. Turnout in the preceding election has a large and significant impact on current turnout in all three specifications. Given that the preceding election happened six years ago in columns (1) and (2), it is not surprising that the coefficient is much larger in column (3), where the preceding election was much more recent, namely two weeks

⁶⁹Freier (2011) estimates that incumbency status increases the probability of winning the next election by 38-40 percentage points.

ago. If another election (Federal, State or European level) is held on the same day as the mayoral election, turnout is approximately seven percentage points higher, as captured by the dummy variable *Concurrent Election*. Statistically, it does not seem to make a difference for electoral participation whether female candidates run for office or not. The same holds true for the status of the mayor: Races for honorary and full-time positions show turnout rates that are not statistically different from each other.⁷⁰

The dummies for the number of candidates show the expected signs. More candidates lead to higher turnout rates, *ceteris paribus*. As discussed in the theory section, this effect can be due to higher aggregate mobilization efforts or to a higher likelihood of finding a candidate on the ballot sheet that is close to one's own political views. In runoff elections (column (3)), only two candidates remain. The number of candidates in the first round does not seem to have an impact on turnout levels in the second round.

All models include fixed effects for each municipality, a third order polynomial to control for electorate size and a third order time trend to capture structural developments in turnout rates over time. The coefficients are not reported in detail, but some remarks are in order.

Turnout and electorate size are negatively related.⁷¹ In line with theory, turnout is lower in larger municipalities where the probability of being pivotal is close to zero. Figure 3.4 in the appendix graphically shows the negative slope. Furthermore, turnout rates are trending downwards over time.⁷² This trend – visualized in Figure 3.5 in the appendix – is consistent with evidence from other developed democracies like the US (see for example [Gentzkow \(2006\)](#)). Interestingly, electoral closeness seems to have increased over time, at least in uncontested elections. This finding is lacking a theoretical explanation so far. The R^2 of the regressions is quite high and lies between 0.64-0.83, highlighting the large explanatory power of the right hand side variables.⁷³

⁷⁰As mayor status depends on municipal size and I already control for the number of eligible voters, this finding is not too surprising.

⁷¹To be more specific, the coefficient of the linear term of *Voters* is negative, the square is positive and the cube is again negative.

⁷²Also here, the linear term of the *Year* variable is negative, the square is positive, and the cube is again negative.

⁷³Dropping the lagged dependent variable decreases the R^2 by 10-15 percent, depending on the specification.

3.6.2 Heterogeneous Effects

It is worthwhile to take a closer look at other variables that affect the costs and benefits of voting. I have already shown that electorate size negatively affects turnout by reducing the probability of one vote being decisive, i.e. by reducing π in the calculus of voting. Rain on election day is a factor that increases the costs of voting ($K \uparrow$) and should therefore negatively influence turnout. In what follows, I test if voters react differently to larger electorate sizes or rain on election day when the race is closer.

I therefore estimate the following interaction specification

$$T_{it} = \alpha + \beta C_{it} + \gamma V_{it} + \delta R_{it} + \rho C_{it} V_{it} + \psi C_{it} R_{it} + X'_{it} \omega + \theta_i + \epsilon_{it} \quad (3.7)$$

where V_{it} is the number of eligible voters, R_{it} is rain in millimeters and ρ and ψ are coefficients of interaction effects of these two variables with the closeness variable. All other variables and parameters are described in Equation 3.6 and all controls from the baseline regression are included.

Table 3.5 in the appendix holds the results. In columns (1) - (3), I interact the three closeness measures with the number of eligible voters in the municipality. Columns (4) - (6) show analogous results when closeness is interacted with the amount of rain on election day (measured in millimeters). The interacted variables are always included linearly, too.

As evident from columns (2) and (3) in Table 3.5, constituency size (as measured by the variable *Voters*) has a negative impact on turnout, but this effect is mitigated in close elections, as shown by the positive sign of the interaction term. This holds for the measures of actual and revealed closeness, whereas the coefficient of *Lagged Closeness* again has the opposite sign. The regressions with the rain variables in columns (4) - (6) also reveal some interesting insights. First, rain on election day reduces turnout, albeit at a small scale.⁷⁴ The coefficient in column (5) implies that 10 millimeters of rain reduce turnout by one percentage

⁷⁴Election days with no rain at all also have lower turnout levels, as can be seen by the negative coefficient of the *No Rain* dummy. This puzzling result could be due to an omitted variable: If days with no rain at all are especially sunny and warm, opportunity costs of voting might again go up because people want to make use of the good weather. Although data on temperatures and hours of sunshine are available, the grid of weather stations recording these data is much looser than the grid recording the precipitation data. In order to prevent the inclusion of noisy data, I focus on the rain variables.

point.⁷⁵⁷⁶ However, given the maximum amount of precipitation recorded on election day (84.3 millimeters), it becomes clear that weather can be a non-negligible factor. Importantly, I control for location-specific general precipitation patterns by including the average amount of rain that usually falls on election day in a municipality into the model. Rain levels above (or below) that level cannot be anticipated by the voters and hence act as a random cost (benefit) on the act of voting. Again, the negative effect of rain is mitigated when closeness is higher, as indicated by the positive signs of the interaction terms in columns (5) and (6).

These results suggest that the competitive context of an election plays an important role. On rainy days, people vote less *on average*, but not if the race is close. Larger electorates have lower turnout rates in uncompetitive environments, but the number of voters does not matter as much when some candidates have equal chances of winning. My findings show that electoral closeness is an important mediator when it comes to the functioning of electoral stimuli. Figure 3.6 in the appendix shows the different slope coefficients of electorate size and rain in uncompetitive vs. competitive environments and thereby visualizes the findings from Table 3.5.

3.6.3 Robustness

Until now, all models have been estimated with municipal-level fixed effects. Hence, the closeness effects were largely identified from the within variation in the data. The results remain entirely robust without the municipal fixed effects. Including county or administrative region fixed effects does not change the qualitative or quantitative results either.⁷⁷ Furthermore, I clustered the standard errors at these higher levels. The statistical significance of the findings was never affected (all results available upon request).

3.6.4 Attenuation Bias and IV Strategy

Using *Revealed Closeness* addresses the endogeneity issues that would otherwise arise when relying on *Actual Closeness* only. Garmann (2014), however, argues

⁷⁵This effect is of similar magnitude as the effects reported by Arnold and Freier (2015a) for municipal and state elections in the German state of North-Rhine Westphalia.

⁷⁶I also experimented with a quadratic specification of the rain variable, but the square term never reached conventional levels of statistical significance.

⁷⁷Bavaria has seven administrative regions and 96 counties.

that also first-round competition measures voters' expectation of electoral closeness only with an error, thereby creating an errors-in-variables problem. To get rid of potential remaining attenuation bias, he suggests to use an IV strategy where closeness of the second round is instrumented with closeness from the first round. As a further robustness check, I provide such estimates in Table 3.6 in the appendix. *Revealed Closeness* is a strong predictor of *Actual Closeness*. An F-Statistic of more than 100 indicates that the instrument is highly relevant. The effect of instrumented closeness on turnout is consistent with the main findings in Table 3.1, albeit being somewhat larger.

3.7 Discussion

In this paper, I examine whether turnout is higher in close elections. Using an *ex-ante*, an *ex-post* and a two-round measure of electoral closeness, I find that closeness does indeed matter: If the election is very competitive, people go to the polls at a higher rate. This is a surprising result, given the *actual* probability of influencing the outcome with one's own vote: In all 27015 Bavarian mayoral elections between 1946 and 2009, there are only 33 cases where one vote would have changed the outcome. This corresponds to a probability of being pivotal in the order of $\frac{33}{27015} = 0.0012 \approx 0.12$ percent. A tentative explanation for the positive closeness effect is that voters' turnout decision is based on the *perceived* probability of being pivotal, not the *actual* probability. Likewise, one could interpret voting in relatively close elections as a type of insurance against the worst case: Even though the probability of being pivotal is close to zero, voters want to avoid the trouble of not having voted and realizing that their vote would have changed the outcome after all.

Furthermore, I provide evidence that other electoral stimuli, like electorate size and rain showers on election day, affect turnout differently depending on the competitive context of the election. In this sense, closeness works as a mediator, making other costs of voting less important in the turnout decision. Implications for campaigners and candidates are straightforward: If rain impacts turnout only conditional on (non-)closeness, it is quite likely that other classic mobilization tactics like get-out-to-vote campaigns or advertisements work better or worse depending on the closeness of the race. If the competitive context is "first order," election campaigns should take this into account.

My results also have implications for the literature on electoral closeness. First, I show that the two measures *ex-post Actual Closeness* and *ex-ante Revealed Closeness* display comparable effects on turnout. Therefore, the many studies relying only on *ex-post Actual Closeness* – due to data limitations and lack of two-round elections – do not make a mistake. However, the results also show that historical measures of closeness dating back to a previous election (*Lagged Closeness*) are potentially confounded with incumbency effects and therefore biased. Of course, this interpretation of the findings is based on the assumption that *Revealed Closeness* identifies the true effect of electoral closeness on turnout.

Appendix

Table 3.2: Summary Statistics: Uncontested Elections

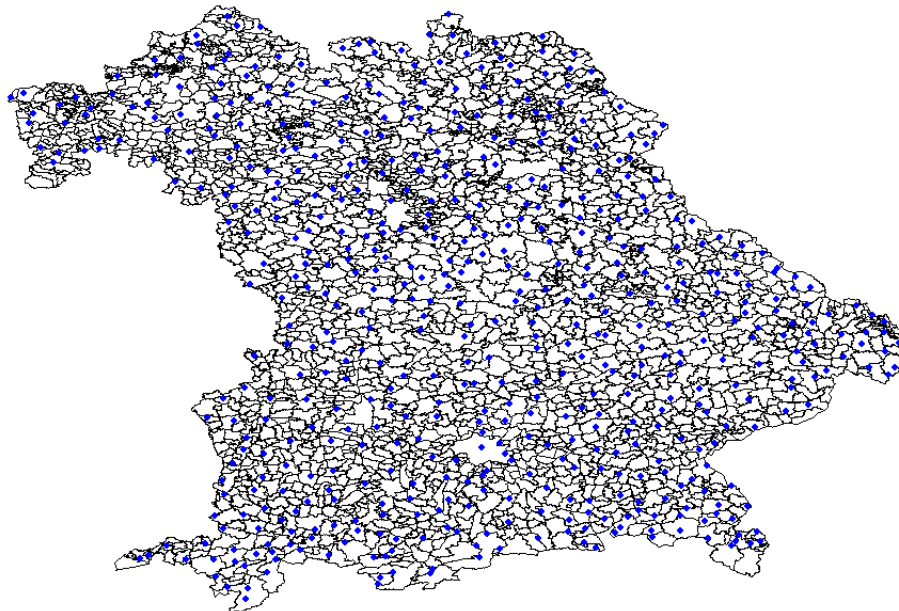
Variable	Mean	Std. Dev.	Min.	Max.	N
Turnout	0.813	0.091	0.222	1	22813
Closeness	0.394	0.325	0	1	22813
Voters	2086.371	2639.56	17	38461	22813
Number of Candidates	1.632	0.756	1	7	22813
Concurrent Election	0.009	0.095	0	1	22813
Female Candidate(s)	0.037	0.189	0	1	22813
Honorary Mayor	0.262	0.44	0	1	21079
No Rain	0.585	0.493	0	1	18791
Rain in mm	1.868	4.128	0	84.3	18791
Day Station Average Rain in mm	2.254	1.021	0.1	11.743	18791
Distance to Nearest Weather Station in km	4.835	2.324	0.025	13.899	22813
Altitude in m	464.891	170.589	107	1832	18791

Table 3.3: Summary Statistics: Contested Elections

Variable	Mean	Std. Dev.	Min.	Max.	N
Turnout	0.808	0.089	0.423	1	2284
Closeness	0.904	0.072	0.515	1	2284
Voters	3518.558	4023.755	20	34694	2284
Number of Candidates	3.105	0.858	1	7	2284
Concurrent Election	0.014	0.118	0	1	2284
Female Candidate(s)	0.145	0.353	0	1	2284
Honorary Mayor	0.483	0.5	0	1	2284
No Rain	0.636	0.481	0	1	1870
Rain in mm	1.585	3.778	0	32.8	1870
Day Station Average Rain in mm	2.171	0.895	0	8.18	1870
Distance to Nearest Weather Station in km	4.761	2.432	0.025	13.899	2284
Altitude in m	455.498	161.768	110	1832	1870

Table 3.4: Summary Statistics: Runoff Elections

Variable	Mean	Std. Dev.	Min.	Max.	N
Turnout	0.758	0.106	0.351	0.974	1892
Closeness	0.867	0.106	0.198	1	1892
Voters	3988.797	4228.991	144	34658	1892
Number of Candidates	1.999	0.023	1	2	1892
Concurrent Election	0.005	0.069	0	1	1892
Female Candidate(s)	0.078	0.268	0	1	1892
Honorary Mayor	0.483	0.5	0	1	1873
No Rain	0.588	0.492	0	1	1657
Rain in mm	2.386	4.716	0	39.9	1657
Day Station Average Rain in mm	2.239	0.943	0.05	8.898	1657
Distance to Nearest Weather Station in km	4.768	2.469	0.025	13.899	1892
Altitude in m	455.879	163.552	110	1832	1657

Figure 3.1: Grid of Weather Stations in Bavaria

Notes: This map presents the spatial distribution of all 559 weather stations in Bavaria. The black lines depict the borders of all 2056 municipalities. Each blue dot stands for a weather station.

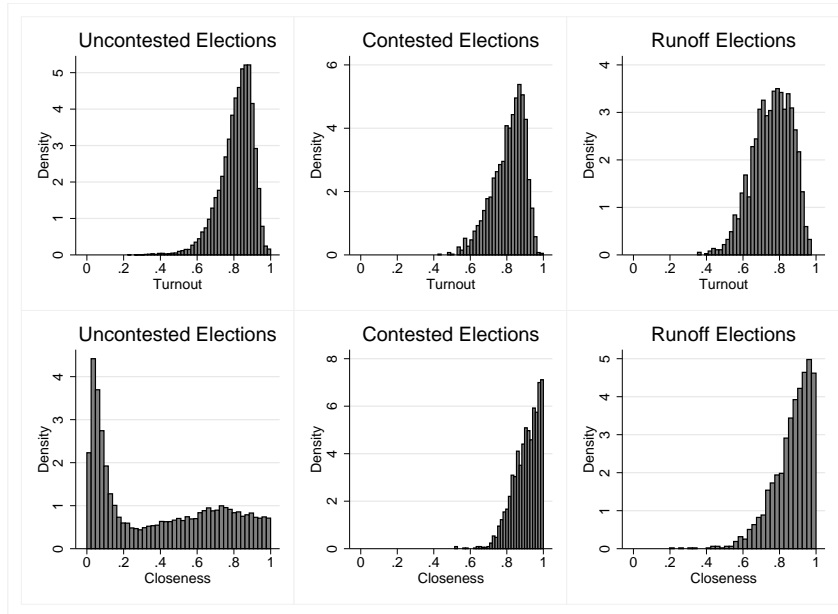
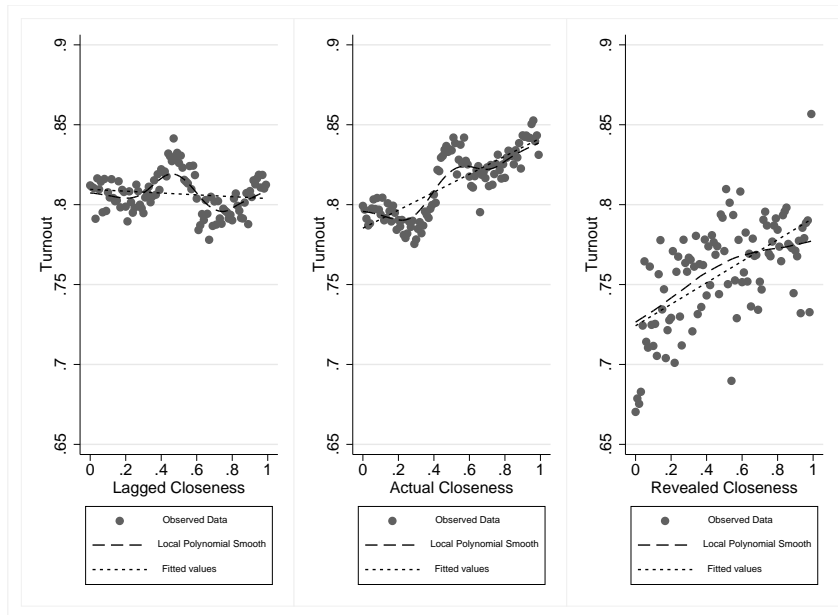
Figure 3.2: Distributions of Turnout and Closeness**Figure 3.3:** Turnout and Closeness

Figure 3.4: Turnout and Electorate Size

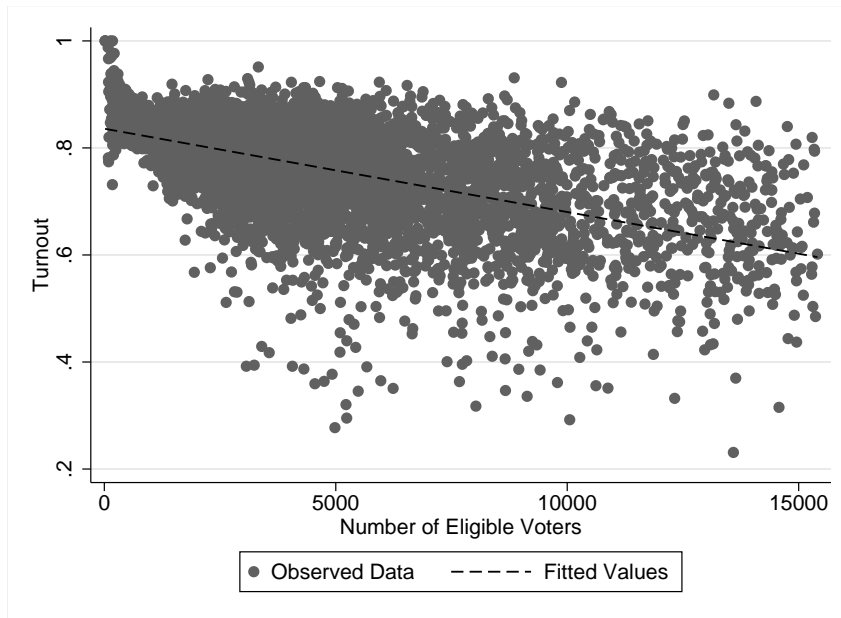
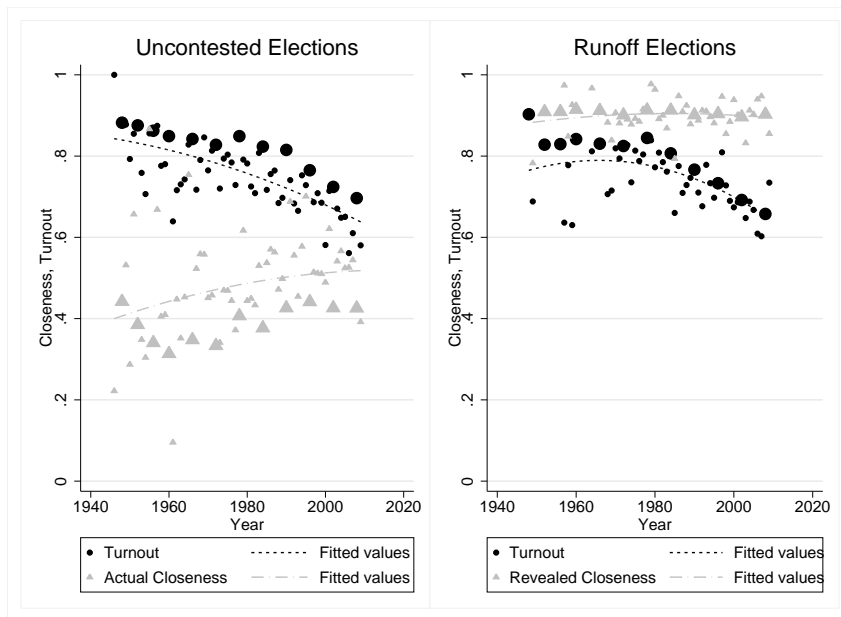


Figure 3.5: Turnout and Closeness Over Time



Notes: Bold circles and triangles represent years in which a major election took place (roughly 2000 municipalities), while the smaller circles and triangles represent years which do not belong to the usual election cycles (generally less than 100 observations). The fit lines are generated using equal weighting of all years.

Table 3.5: Heterogeneous Effects: Electorate Size and Weather

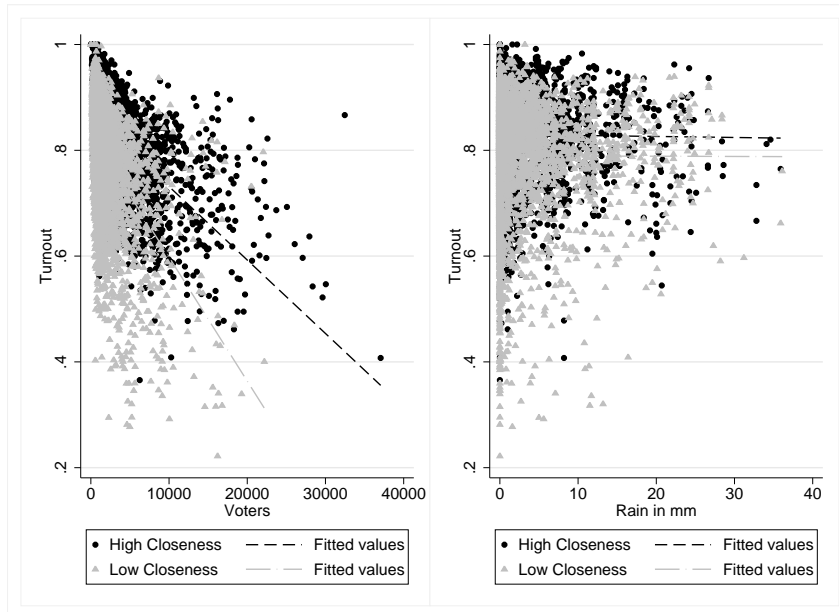
	(1)	(2)	(3)	(4)	(5)	(6)
	Turnout	Turnout	Turnout	Turnout	Turnout	Turnout
Sample	<i>Uncontested Elections</i>	<i>Runoff Elections</i>	<i>Runoff Elections</i>	<i>Uncontested Elections</i>	<i>Runoff Elections</i>	<i>Runoff Elections</i>
Lagged Closeness	-0.007*** (0.002)			-0.011*** (0.002)		
Actual Closeness		0.037*** (0.003)			0.051*** (0.003)	
Revealed Closeness			0.214*** (0.020)			0.222*** (0.021)
Turnout in Preceding Election	0.217*** (0.011)	0.190*** (0.010)	0.856*** (0.041)	0.195*** (0.013)	0.175*** (0.011)	0.842*** (0.045)
Voters	-0.157*** (0.016)	-0.199*** (0.016)	-0.111** (0.045)	-0.172*** (0.017)	-0.175*** (0.017)	-0.022 (0.047)
No Rain				-0.004*** (0.001)	-0.003*** (0.001)	0.002 (0.004)
Rain in mm				-0.000* (0.000)	-0.001*** (0.000)	-0.011*** (0.004)
Day Station Average Rain in mm				-0.003*** (0.000)	-0.003*** (0.001)	0.003 (0.001)
Constant	0.456*** (0.010)	0.500*** (0.010)	-0.197*** (0.027)	0.485*** (0.012)	0.519*** (0.011)	-0.204*** (0.029)
<i>Interaction Effects</i>						
Lagged Closeness * Voters	-0.026*** (0.009)					
Actual Closeness * Voters		0.068*** (0.010)				
Revealed Closeness * Voters			0.073*** (0.017)			
Lagged Closeness * Rain				0.000 (0.000)		
Actual Closeness * Rain					0.001*** (0.000)	
Revealed Closeness * Rain						0.013*** (0.004)
All Controls from Baseline Regression	yes	yes	yes	yes	yes	yes
N	20582	20582	1859	17386	17386	1632
R ²	0.64	0.65	0.83	0.64	0.65	0.83

Notes: Standard errors robust to heteroscedasticity. Significance Levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The data cover all mayoral elections in 2031 Bavarian municipalities from 1946-2009. The coefficient of *Voters* has been multiplied by 10^4 to ease readability.

Table 3.6: IV Estimation

Dependent Variable	(1) (First Stage) Actual Closeness	(2) (Second Stage) Turnout
Revealed Closeness	0.622*** (0.059)	
Actual Closeness		0.375*** (0.039)
Constant	0.231*** (0.079)	
All Controls from Baseline Regression	yes	yes
First Stage F-Statistic (p-Value)	109.72 0.00	- -
N	1244	1244
R ²	0.19	0.72

Notes: Standard errors robust to heteroscedasticity. Significance Levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure 3.6: Heterogeneous Effects: Electorate Size and Weather

Notes: This graph visualizes the interaction between electorate size and turnout (left panel) as well as rain and turnout (right panel) depending on closeness for uncontested elections. Municipalities belong to the high / low closeness group if they rank above / below the 75 / 25 percent quantile of the closeness variable.

Chapter 4

Outside Earnings, Absence and Activity: Evidence from German Parliamentarians[§]

4.1 Introduction

Some politicians acquire experience in private business before, during, and after being active in politics. Members of parliament (MPs) in Germany, for instance, are allowed to continue with the business activities that they were engaged in before entering politics. It is also conceivable that politicians use their prominence and networks to earn money by, for example, giving speeches. Politicians have to split their efforts between their obligations as an MP and their outside activities, generating a time trade-off. Earning outside income may thus influence how MPs fulfill their parliamentary responsibilities (which include, for example, attending plenary sessions in parliament, committee meetings, giving speeches in parliament, and offering consultation sessions for citizens).⁷⁸

Outside earnings and parliamentary activity were a hot issue during the German election campaign of 2013. The Social Democrat Peer Steinbrück accumulated

[§]This chapter is based on joint work with Björn Kauder and Niklas Potrafke. A similar version has been published in the *European Journal of Political Economy*, see [Arnold et al. \(2014c\)](#).

⁷⁸[Soule and Clarke \(1970\)](#) and [Hitlin and Jackson \(1977\)](#) examine amateurs and professionals in the 1968 Democratic national convention and the 1974 Democratic mid-term conference. [Jones et al. \(2002\)](#) describe how electoral rules in Argentina influence political careers. [Mattozzi and Merlo \(2008\)](#) investigate why careerpoliticians and political careers exist.

substantial outside earnings by giving speeches at corporate events, and commentators conjectured that such outside activities would negatively affect parliamentary effort. Steinbrück ran as a candidate for chancellor in the 2013 federal election against the conservative incumbent Angela Merkel. Commentators agreed that Steinbrück's outside earnings were one of the reasons why the Social Democrats lost the election.

We investigate how outside earnings of German federal parliament (*Bundestag*) members were associated with absence and parliamentary activity over the 17th legislative period, 2009-2013. We focus on the 17th legislative period because the German Supreme Court adjudicated in 2007 that MPs have to publish their outside activities. There is no study that examines the nexus between outside earnings and parliamentary activity in Germany. In contrast to studies for other countries (e.g. [Gagliarducci et al. \(2010\)](#) for Italy), we also use measures of parliamentary activity that go beyond the absence rate, namely speeches, oral contributions, interpellations, and group initiatives. We use a panel data set to control for MPs' unobserved ability. The results do not indicate that outside earnings are correlated with absence rates and speeches; but they do suggest that outside earnings are somewhat negatively correlated with oral contributions, interpellations, and group initiatives. We propose that the results for Germany do not corroborate evidence on other countries such as Italy because party discipline, institutions, and political cultures differ across countries.

4.2 Related Studies

Moonlighting is expected to influence politicians' activities in parliament: when MPs pursue outside activities, they have less time to spend on pure parliamentary activities.⁷⁹ In Italy, outside earnings have been shown to increase absence from parliament using the pre-parliament incomes of freshmen politicians as an instrumental variable for outside earnings. High-ability citizens are, however, more likely to run for office when they are allowed to keep their outside earnings ([Gagliarducci et al., 2010](#)).⁸⁰ Allowing moonlighting may hence influence the selection of (high-ability) candidates into politics.

⁷⁹For a survey on moonlighting by politicians, see [Geys and Mause \(2013\)](#). [Bender and Lott Jr \(1996\)](#) review the literature on legislator voting and shirking.

⁸⁰[Merlo et al. \(2009\)](#) also use the pre-parliament incomes of politicians as an instrumental variable for outside earnings. See [Nannicini et al. \(2013\)](#) on how Italian voters punish absence.

Two issues are likely to influence the quality of politicians: differences in opportunity costs induce low-quality candidates to run with higher probability than high-quality candidates (selection effect), and higher salaries for politicians improve politicians' performance due to an incentive effect (Besley, 2004; Caselli and Morelli, 2004; Messner and Polborn, 2004).⁸¹ In Italy, higher wages have attracted more educated candidates (Gagliarducci and Nannicini, 2013). In Finland, higher salaries for politicians have increased the quality of female candidates, but not the quality of male candidates (Kotakorpi and Poutvaara, 2011).

Experts examine which determinants influence MPs' attendance and activities in parliament. In Italy, for example, MPs who have no political experience prior to entering parliament have been shown to attend votes less often when outside opportunities increase. MPs who do have political experience prior to entering parliament have not, by contrast, been found to attend votes less often when outside opportunities increase (Fedele and Naticchioni, 2015). MPs from governing parties were absent less often in parliament compared to MPs from opposition parties (Gagliarducci et al., 2010; Galasso and Nannicini, 2011). MPs elected into parliament via the majoritarian tier were absent less often than MPs elected into parliament via the proportional tier (Gagliarducci et al., 2011). MPs who won the mandate in contestable districts were absent from parliament less often because parties allocated their best politicians to contestable districts (selection effect, see Galasso and Nannicini (2011)). German MPs who won the mandate in the 2009 election in contestable districts were also absent from parliament less often (Bernecker, 2014).⁸² In the European parliament, increasing salaries have been shown to increase absence rates and to decrease the number of questions (Mocan and Altindag, 2013). Fisman et al. (2012), by contrast, do not find that salaries influenced attendance in the European parliament; legislative output, however, increased when MPs attended parliamentary meetings more frequently.⁸³

⁸¹Peichl et al. (2013) find that German MPs earn 35-65% more than private sector executives, but not more than top-level private sector executives. See Kotakorpi et al. (2014) on the returns of political office in Finland. See also Bordignon et al. (2013) on the self-selection of politicians and Evrenk et al. (2013) on the quality of politicians in party-primaries.

⁸²German MPs who won the mandate in the 2005 election in contestable districts had lower outside earnings (Becker et al., 2009). Female MPs had fewer additional jobs, particularly in the private sector (Geys and Mause, 2012a). Physically attractive female MPs had, however, more additional jobs (Geys, 2013b). MPs from rightwing parties had more outside activities than MPs from left-wing parties (Mause, 2009). Geys and Mause (2012b) portray the nexus between outside earnings and electoral control. Geys (2013a) discusses how electoral cycles influence outside activities of MPs in the United Kingdom.

⁸³In the United States' congress, MPs were found to exhibit lower voting participation in their last term (Lott, 1990). In the United Kingdom, politicians' expense claims and parliamentary

How outside earnings influence absence from and activity in parliament remains as an empirical question.

4.3 Institutional Background

4.3.1 Absence from Recorded Votes and Activity in Parliament

There are various voting procedures in the German Bundestag. We rely on the only voting procedure that reveals the voting behavior of each MP, namely recorded votes. 218 recorded votes took place between the beginning of the legislative period in 2009 and the end of the legislative period in 2013. We thus use absence rates in these recorded votes for each MP in each year of the legislative period.⁸⁴

We use four measures of parliamentary activity. Firstly, we use speeches, including both speeches actually given and speeches placed on record. We acknowledge that MPs determine the number of speeches jointly with their factions. We assume, however, that the willingness to give a speech is strongly correlated with the actual number of speeches given. MPs may well place speeches on record, which are included in our measurement of the number of speeches.⁸⁵

Secondly, we use oral contributions, including for example interposed questions, replies, declarations, and heckling. Oral contributions measure all kinds of oral activities during plenary sessions excluding pre-formulated speeches. We expect oral contributions to measure individual-specific effort. In contrast to speeches, oral contributions occur spontaneously during debates and are thus difficult to prepare by staffers.

activity were positively correlated, indicating that politicians are intrinsically motivated ([Besley and Larcinese, 2011](#)).

⁸⁴The federal parliament also publishes information on MPs' excused absences. MPs are, however, allowed to excuse themselves for being absent without any reason. We therefore cannot distinguish between excuses with and without good reason and do not distinguish between excused and unexcused absences in the empirical analysis. When MPs are absent, they have to incur wage cuts of up to 100 euros per day.

⁸⁵In the course of the Euro crisis some MPs gave speeches in parliament even though their view on the Euro crisis, such as bailout packages, was in contrast to the faction leaders' views. Frank Schäffler from the reigning FDP, for example, gave speeches in parliament opposing the faction leaders' and the government's view on bailout packages.

Thirdly, we use the number of small and large interpellations. MPs submit interpellations in a written form. Interpellations are intended to retrieve information from the government. Fourthly, we use the number of group initiatives, including bills, different types of applications, and reporting. To be sure, it remains questionable how much effort individual MPs put into interpellations and group initiatives because MPs often jointly pose interpellations and, by definition, jointly prepare group initiatives.⁸⁶

We acknowledge the shortcomings of the individual variables measuring absence or activity of MPs. Using all four of these variables jointly may, however, to some extent offset these shortcomings and thus allows deriving conclusions from how outside earnings are correlated with MPs' effort.

4.3.2 Outside Earnings of MPs

The German federal parliament decided in August 2005 that MPs must publish their outside activities and outside earnings.⁸⁷ The law now requires MPs to publish their sources and levels of outside earnings. Table 4.1 shows the eight categories into which MPs have to classify their outside activities. We only consider categories two to five because these are the only categories that involve a trade-off between outside earnings and attendance/activity in parliament. These categories contain income from employment and self-employment (such as income from speeches), income from positions in corporations (such as positions on supervisory boards), income from positions in public entities (such as county councils or churches), and income from positions in societies and foundations. We do not include income from holding a position in the government or a party when calculating outside earnings as income from holding a position in the government or a party does not describe "outside" earnings. MPs have to publish outside earnings from each activity in a coded way as a monthly or yearly income. Level 1 includes outside earnings of between 1,000 and 3,500 Euros, Level 2 includes outside earnings of between 3,500 and 7,000 Euros, and Level 3 includes outside earnings of above 7,000 Euros. Disclosure requirements also include unpaid activities and activities remunerated at less than 1,000 Euros. In the empirical analysis, we use

⁸⁶For the German states, Braendle and Stutzer (2013) show that the number of interpellations increases with the share of public servants in parliament.

⁸⁷Nine MPs called the Supreme Court to adjudicate on the legitimacy of the law. The president of the federal parliament decided to wait for the Supreme Court verdict before publishing MPs' outside earnings. In July 2007 the German Supreme Court adjudicated that MPs have to publish their outside activities and outside earnings.

the lower bounds of these income categories: Level 1 describes outside earnings of 1,000 Euros, Level 2 of 3,500 Euros, and Level 3 of 7,000 Euros.

Table 4.1: Reporting of Outside Earnings

#	Description	Example
1	Professional career before membership in parliament	College Professor
2	Employment and self-employment	Speech
3	Positions in corporations	Supervisory Board
4	Positions in public entities	Church
5	Positions in societies and foundations	Development aid agency
6	Agreements on future activities and pecuniary advantages	Absorption of tuition fees
7	Participations in corporations	Law firm
8	Donations	–

Notes: Outside earnings from employment and self-employment are officially named outside earnings "beside the mandate".

We acknowledge that using outside earnings does not include how much time an MP invested in earning outside income, and that outside earnings thus serve only as a proxy for the time invested.

4.3.3 The German Political Party Landscape and Federal Elections

Two major political parties characterize the political spectrum in Germany: the leftist SPD and the conservative CDU. In Bavaria, Germany's largest federal state by area, the conservatives are not represented by the CDU, but by their sister party, the CSU. No party competition emerges between the CDU and the CSU, and they form one faction in the federal parliament. We therefore refer to both parties collectively as the CDU in the empirical analysis. All of Germany's federal chancellors have been members of one of these two major blocs: the SPD and the CDU.

The much smaller FDP and the Greens (Bündnis 90/Die Grünen) have played an important role as coalition partners. Although the SPD has formed coalitions with all of the other three parties, the CDU has never formed a coalition with the Greens at the federal level. The CDU has formed coalitions with the SPD and the FDP.

In federal elections, voters cast two votes in a personalized proportional representation system. The first vote determines which candidate is to obtain the direct

mandate in one of the 299 electoral districts with a simple majority. The second vote determines how many seats the individual parties receive in parliament. Each party that received at least 5% of the second votes obtains a number of the 598 seats in the parliament that corresponds to the party's second vote share.⁸⁸ Candidates voted into the parliament with the first vote (direct mandate) obtain their seats first. Candidates from party lists obtain the remaining seats. When the number of direct mandates exceeds the party's vote share, the party obtains excess mandates. Because the other parties did not obtain equalizing mandates in the elections before 2013, excess mandates made it possible for an individual party to receive a larger number of seats as compared to the number of seats this party would have received based on the second vote result.

4.4 Empirical Analysis

4.4.1 Descriptive Statistics

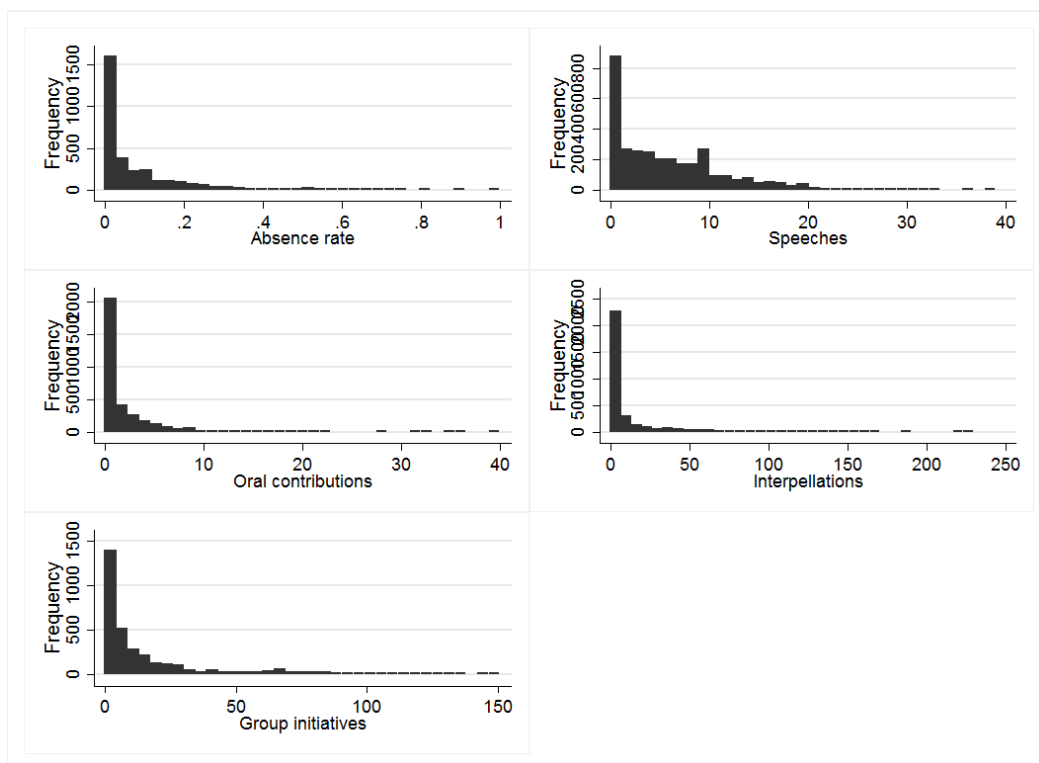
We use data from the website of the German federal parliament, from MPs' personal websites, and from the German newspaper "*Die Zeit*" for the period 2009-2013 (17th legislative period).⁸⁹

Figure 4.1 shows histograms of the absence rate and parliamentary activities for the individual MPs. The histograms portray skewed distributions of the absence rate, speeches, oral contributions, interpellations, and group initiatives. These distributions are skewed to the right, meaning that most MPs display low or average levels of activity, while there are some MPs that tend to be very active in parliament.

The upper left panel of Figure 4.2 shows that left-wing politicians were more frequently absent in the 17th legislative period of the federal parliament than their colleagues from other parties. MPs from the Left Party did not attend 15.3% of the recorded votes on average, MPs from the SPD did not attend 10.6% of the recorded votes, and MPs from the Greens did not attend 7.8% of the recorded votes per year. Right-wing politicians, in contrast, had lower absence rates. MPs from the FDP did not attend 6.5% of the recorded votes on average and MPs from

⁸⁸Candidates obtain a direct mandate even if their party fails to reach the 5% clause. If a party obtains less than 5% of the second votes, but at least three direct mandates, the party obtains a number of seats in the parliament according to the party's second vote share.

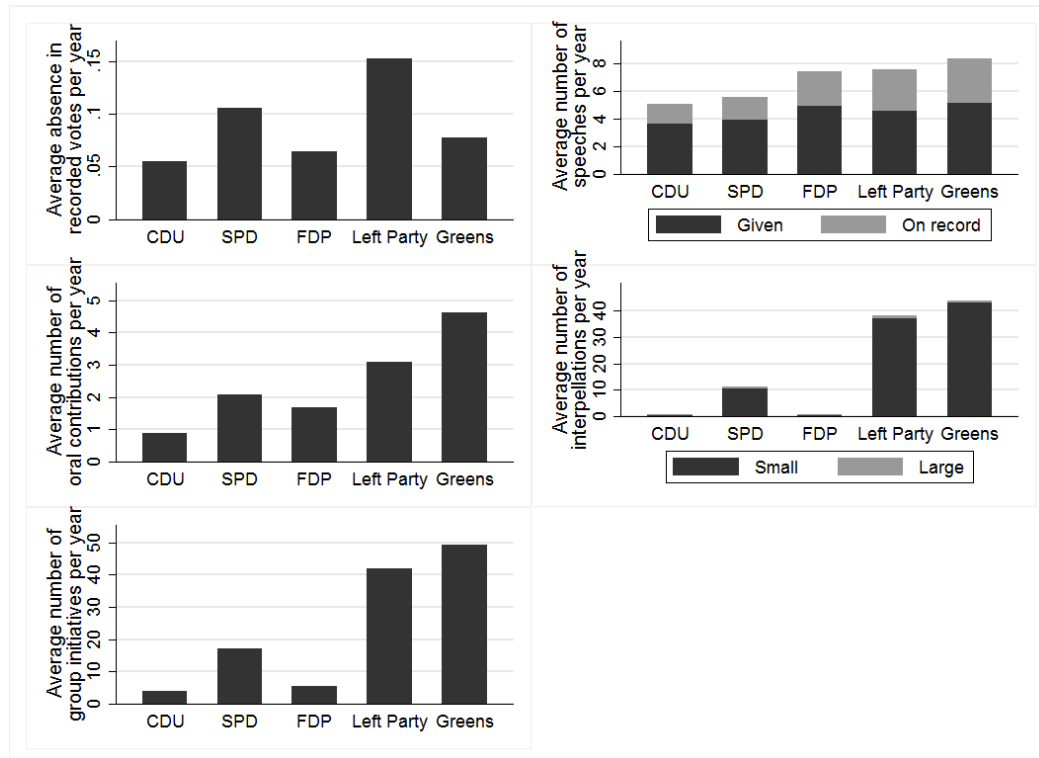
⁸⁹We compiled information on outside earnings in August 2013.

Figure 4.1: Distributions of Absence and Activities

the CDU did not attend 5.5% of the recorded votes per year. It is conceivable that right-wing politicians were absent less often than left-wing politicians because the CDU and FDP were the governing parties and therefore they had to ensure their majority in parliament (see also [Gagliarducci et al. \(2010\)](#) and [Galasso and Nannicini \(2011\)](#)).⁹⁰ MPs from opposition parties have, by contrast, lower incentives to attend votes because the opposition parties fail to form a majority in parliament in any event.

The upper right panel of Figure 4.2 shows that MPs from small parties were more active in giving speeches. MPs from the Greens gave 8.4 speeches on average, Left Party MPs gave 7.6 speeches, and FDP MPs gave 7.4 speeches per year. MPs from large parties, by contrast, gave fewer speeches. SPD MPs gave 5.6 speeches on average and CDU MPs gave 5.1 speeches per year. MPs from small parties may well give more speeches to attract attention. The number of minutes for speeches allocated to the factions is, however, proportional to the factions' size. MPs from small parties may thus have given shorter speeches.

⁹⁰In Italy, left-wing politicians have shown lower absence rates, while belonging to the government coalition is controlled for ([Gagliarducci et al., 2010](#)).

Figure 4.2: Party Affiliation and Activity

The center left panel of Figure 4.2 shows that left-wing politicians were more active in giving oral contributions. MPs from the Greens contributed on average 4.6 times, Left Party MPs contributed 3.1 times, and SPD MPs contributed 2.1 times per year. Right-wing politicians, in contrast, made oral contributions less frequently. FDP MPs contributed on average 1.7 times and CDU MPs contributed 0.9 times per year. It is conceivable that leftwing politicians contributed more often because the SPD, Left Party, and Greens were the opposition parties.

The center right panel of Figure 4.2 shows that politicians from opposition parties were more active in interpellating. MPs from the Greens interpellated on average 43.9 times, Left Party MPs interpellated 38.1 times, and SPD MPs interpellated 11.4 times per year. Politicians from governing parties, by contrast, interpellated less often. CDU MPs interpellated on average 0.6 times and FDP MPs also interpellated 0.6 times per year.

The lower left panel of Figure 4.2 shows that politicians from opposition parties were more active in preparing group initiatives. MPs from the Greens prepared on average 49.3, Left Party MPs prepared 42.0, and SPD MPs prepared 17.1 group initiatives per year. Politicians from governing parties, by contrast, prepared fewer

group initiatives. FDP MPs prepared on average 5.4 and CDU MPs prepared 4.0 group initiatives per year.

Figure 4.3 shows that outside earnings were substantially higher among right-wing politicians. CDU MPs earned on average 7,900 Euros in outside income, while FDP MPs earned 4,400 Euros in outside income per year. Left-wing politicians, by contrast, had lower outside earnings. SPD MPs earned 3,000 Euros in outside income on average, Left Party MPs earned 1,400 Euros, and MPs from the Greens earned 300 Euros in outside income per year. For MPs from all parties, outside earnings from employment and self-employment were the most important source of outside earnings.⁹¹

Figure 4.3: Outside Earnings by Party

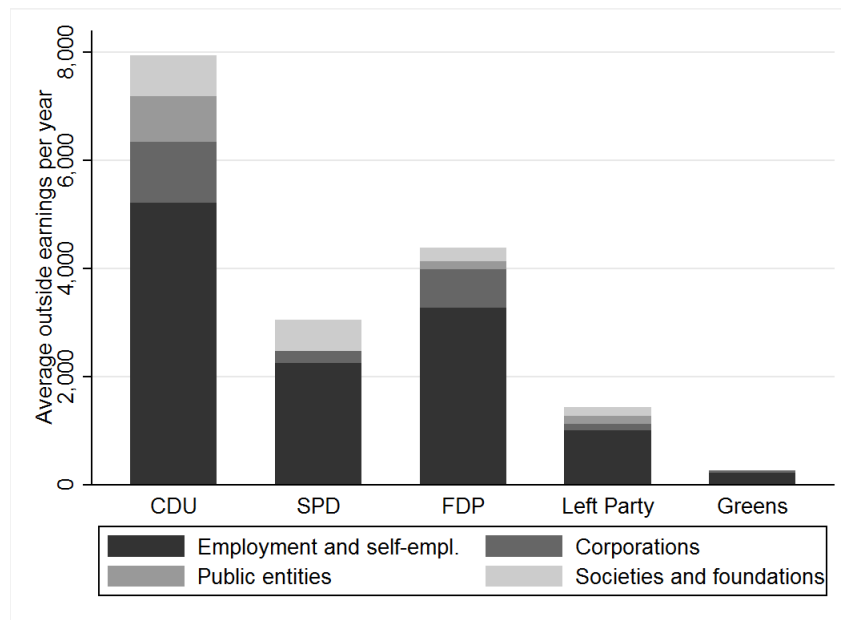
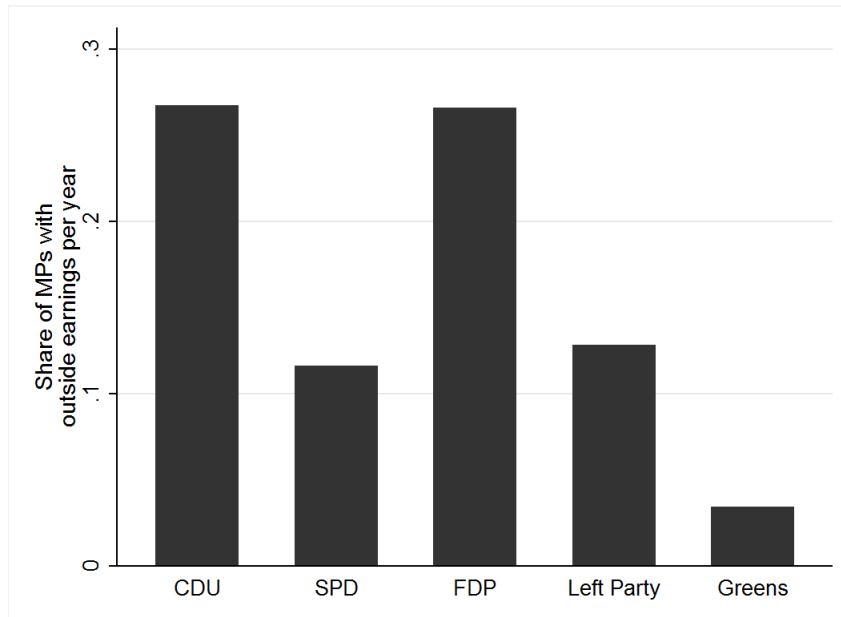


Figure 4.4 shows that the share of MPs with outside earnings was substantially higher among right-wing parties. The CDU had 26.7% of MPs with outside earnings and the FDP had 26.6% of MPs with outside earnings per year. Left-wing parties, on the other hand, had lower shares of MPs with outside earnings. The Left Party had 12.9% of MPs with outside earnings, the SPD had 11.7% of MPs, and the Greens had 3.5% of MPs with outside earnings per year.

⁹¹Considering only MPs with positive outside earnings, CDU MPs earned on average 29,700 Euros in outside income and FDP MPs earned 16,500 Euros in outside income per year. SPD MPs earned on average 26,100 Euros in outside income, Left Party MPs earned 11,100 Euros, while MPs from the Greens earned 7,600 Euros.

Figure 4.4: Share of MPs with Outside Earnings

The descriptive statistics in Table 4.2 show that absence rates varied between 0 and 100%, the number of speeches varied between 0 and 39, the number of oral contributions varied between 0 and 40, the number of interpellations varied between 0 and 230, and the number of group initiatives varied between 0 and 151 per year among MPs.⁹² Outside earnings varied between 0 and 296,000 Euros per year among MPs.

Table 4.3 shows the correlation coefficients between absence, speeches, oral contributions, interpellations, group initiatives, and outside earnings. As expected, outside earnings are positively correlated with absence rates and negatively correlated with speeches, oral contributions, interpellations, and group initiatives. The correlation coefficients are small and range between 0.06 and 0.12 in absolute values. The pairwise correlations between outside earnings and parliamentary effort are statistically significant at the 1% level. The correlation coefficients are largest between speeches, oral contributions, interpellations, and group initiatives (0.24 to 0.80).

⁹²Oskar Lafontaine from the Left Party did not attend 10 out of 10 votes in 2009 because he was ill.

Table 4.2: Descriptive Statistics

Variable	Obs.	Mean	Std. Dev.	Min	Max
Absence	3131	0.08	0.14	0	1
Speeches	3131	6.21	5.78	0	39
Oral contributions	3131	1.97	3.18	0	40
Interpellations	3131	12.50	26.25	0	230
Group initiatives	3131	16.90	25.11	0	151
Outside earnings (total)	3131	0.05	0.17	0	2.96
Outside earnings (employment and self-employm.)	3131	0.03	0.15	0	2.96
Outside earnings (corporations)	3131	0.01	0.04	0	1.05
Outside earnings (public entities)	3131	0.00	0.04	0	0.84
Outside earnings (societies and foundations)	3131	0.00	0.04	0	0.84
CDU	3131	0.38	0.49	0	1
SPD	3131	0.24	0.42	0	1
FDP	3131	0.15	0.36	0	1
Left Party	3131	0.12	0.33	0	1
Greens	3131	0.11	0.31	0	1

Notes: Outside earnings measured in 100,000 Euros.

Table 4.3: Correlation Coefficients

Variable	Absence	Speeches	Oral Contributions	Interpellations	Group Initiatives	Outside Earnings
Absence	1					
Speeches	-0.08***	1				
Oral contributions	-0.03	0.37***	1			
Interpellations	0.06***	0.24***	0.39***	1		
Group initiatives	0.07***	0.36***	0.46***	0.80***	1	
Outside earnings	0.07***	-0.06***	-0.08***	-0.10***	-0.12***	1

Notes: Significance Levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

4.4.2 Empirical Strategy

The baseline panel data model has the following form:

$$\text{MP's Effort}_{ijt} = \beta_j \text{Outside Earnings}_{it} + \eta_i + \theta_t + u_{it} \quad (4.1)$$

with $i = 1, \dots, 652$; $j = 1, \dots, 5$; $t = 1, \dots, 5$.

MP's Effort $_{ijt}$ describes the share of recorded votes not attended (absence; $j = 1$), the number of speeches given in parliament ($j = 2$), the number of oral contributions ($j = 3$), the number of interpellations ($j = 4$), or the number of group initiatives ($j = 5$) by MP i in year t (t runs from 1 to 5 because of the five calendar years from 2009 to 2013). Outside Earnings $_{it}$ describes outside earnings in 100,000 Euros. Due to the fact that data on outside earnings are only published in categories, we use the lower bounds of these categories when calculating outside earnings (see Section 4.3.2). η_i is an MP fixed effect, θ_t is a time fixed effect, and u_{it} describes an error term.⁹³

We estimate fixed effects models with standard errors robust to heteroskedasticity (Huber-White sandwich standard errors – see Huber (1967) and White (1980)).⁹⁴ The panel includes the five calendar years from 2009 (September; beginning of the legislative period) to 2013 (September; end of the legislative period). We include all MPs in this 17th legislative period of the German federal parliament. We thus also include MPs who left the parliament within the legislative period and their successors. The sample includes 652 MPs and 3,131 observations.

4.4.3 Regression Results

Table 4.4 shows the results. Using the absence rate as the dependent variable, the coefficient of outside earnings in column (1) is positive, but does not turn out to be statistically significant. Using speeches as the dependent variable, the coefficient of outside earnings in column (2) is negative, but does also not turn out to be statistically significant. Column (3) shows a negative correlation between outside earnings and the number of oral contributions. The coefficient of outside

⁹³Note that potential simultaneity bias could be a problem in this application, even if we condition on MP specific fixed effects. Therefore, we interpret our results as correlations rather than causal effects. In Section 5.3, I briefly talk about the shortcomings of this study and how they could be addressed with better data.

⁹⁴We are aware of the fact that linear models are not perfectly suited for censored dependent variables.

earnings is statistically significant at the 10% level. The numerical meaning of the coefficient is that the number of oral contributions decreases by 0.08 when outside earnings increase by one standard deviation (17,140 Euros). Column (4) shows a negative correlation between outside earnings and the number of interpellations. The coefficient of outside earnings is statistically significant at the 5% level. The number of interpellations decreases by 0.68 when outside earnings increase by one standard deviation. Column (5) shows a negative correlation between outside earnings and the number of group initiatives. The coefficient of outside earnings is statistically significant at the 1% level. The number of group initiatives decreases by 1.06 when outside earnings increase by one standard deviation.

Table 4.4: Main Results

	(1)	(2)	(3)	(4)	(5)
	Absence	Speeches	Oral Contributions	Interpellations	Group Initiatives
Outside earnings (total)	0.009 (0.059)	-0.537 (1.125)	-0.467* (0.264)	-3.944** (1.564)	-6.177*** (2.115)
2010	0.006 (0.007)	6.253*** (0.202)	2.348*** (0.143)	16.378*** (1.116)	22.255*** (1.247)
2011	0.013* (0.008)	7.345*** (0.221)	2.226*** (0.133)	14.643*** (1.082)	21.559*** (1.039)
2012	0.016** (0.008)	7.532*** (0.236)	2.179*** (0.112)	14.422*** (1.063)	20.885*** (0.970)
2013	0.023*** (0.008)	5.448*** (0.207)	1.280*** (0.081)	6.123*** (0.506)	11.198*** (0.548)
MP Fixed Effects	yes	yes	yes	yes	yes
Observations	3,131	3,131	3,131	3,131	3,131
Number of n	652	652	652	652	652
R-squared within	0.00624	0.454	0.192	0.204	0.315
R-squared between	0.00479	0.00400	0.00751	0.0143	0.0253
R-squared overall	0.00331	0.225	0.0802	0.0611	0.125

Notes: 2010 to 2013 describe time fixed effects (reference category: 2009). Robust standard errors in parentheses. Significance Levels: * p < 0.10, ** p < 0.05, *** p < 0.01.

The year dummies show that the absence rate was higher in 2011, 2012 and 2013 compared to 2009 (reference category). For speeches, oral contributions, interpellations, and group initiatives, the year dummies show that activities were higher in the 2010-2012 period compared to 2009 and 2013, because the year dummies for 2009 and 2013 do not cover full years at the beginning and the end of the legislative period.

Why is it that outside earnings are (negatively) correlated with the number of oral contributions, interpellations, and group initiatives, but not significantly related with the absence rate and the number of speeches? MPs with outside earnings

may well reduce their time spent on parliamentary work. It is conceivable that MPs with outside earnings reduce less important and less visible activities, such as oral contributions, interpellations, and group initiatives. MPs, by contrast, avoid reducing more visible activities, such as attendance in parliament and the number of speeches.

Table 4.5 shows the results for MPs from individual parties. For the CDU, FDP, and the Greens, the results do not show that outside earnings are correlated with the absence rate or activity in parliament. For the SPD, columns (3) and (4) show a negative correlation between outside earnings and the number of oral contributions, and between outside earnings and the number of interpellations. The coefficients of outside earnings are statistically significant at the 5% and 1% level. The numerical meaning of the coefficients is that the number of oral contributions decreases by 0.11 and the number of interpellations decreases by 0.61 when outside earnings increase by one standard deviation (18,315 Euros). For the Left Party, columns (1) and (2) show a positive correlation between outside earnings and the absence rate, and between outside earnings and the number of speeches. The coefficients of outside earnings are statistically significant at the 1% level. The absence rate increases by 0.03 and the number of speeches increases by 0.71 when outside earnings increase by one standard deviation (4,284 Euros). Outside earnings do not turn out to be statistically significant when we use group initiatives of MPs from individual parties as dependent variable.

Table 4.5: Results for Individual Parties

	(1)	(2)	(3)	(4)	(5)
	Absence	Speeches	Oral Contributions	Interpellations	Group Initiatives
Outside earnings (total): CDU (Obs.: 1,197)	0.077 (0.048)	-0.440 (1.170)	0.065 (0.269)	1.432 (1.225)	-0.963 (0.814)
Outside earnings (total): SPD (Obs.: 738)	-0.047 (0.051)	-0.585 (1.661)	-0.602** (0.304)	-3.312*** (1.155)	-1.930 (1.293)
Outside earnings (total): FDP (Obs.: 470)	-0.096 (0.106)	2.379 (7.720)	0.104 (4.101)	-0.070 (1.089)	4.500 (5.133)
Outside earnings (total): Left Party (Obs.: 381)	0.641*** (0.138)	16.668*** (3.689)	-2.400 (1.671)	-16.421 (25.068)	-26.192 (29.923)
Outside earnings (total): Greens (Obs.: 345)	-0.019 (0.120)	-4.924 (10.860)	-0.906 (3.830)	33.988 (27.686)	21.229 (13.787)

Notes: Each row describes a party-specific estimation with (non-reported) individual and time fixed effects. Robust standard errors in parentheses. Significance Levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

4.4.4 Robustness

Estimating a fixed-effects model does not allow for including time-invariant variables, such as gender and party affiliation. To exploit the variation in MPs' time-invariant characteristics, one thus may estimate a random-effects model. Estimating a random-effects model, however, gives rise to a potential bias from omitted (time-invariant) variables, such as unobserved heterogeneity in MPs' abilities. We thus do not rely on results from a random-effects model.

The results of the fixed-effects model may suffer from reverse causality bias, because MPs decide simultaneously on outside activities and on attendance or activity in parliament. In an instrumental variable approach, one may well use potential market incomes of MPs as an instrumental variable for outside earnings (see [Gagliarducci et al. \(2010\)](#) and [Merlo et al. \(2009\)](#)). There are no data available on the incomes earned by individual German MPs before they entered parliament. Matching data on the politicians' occupations before becoming an MP or the politicians' occupations learned with data on average incomes of occupations helps to describe potential market incomes. We do, however, not rely on results from a 2SLS model, because potential market incomes do not account for the different abilities of MPs, which is likely to give rise to different potential incomes (unobserved heterogeneity). There is another concern about the 2SLS approach. We cannot rule out that potential market incomes influence the absence rate or the activity through a channel other than outside earnings. We thus cannot ensure that the exclusion restriction is fulfilled.

We tested whether the results change when outside earnings are measured in logs.⁹⁵ Replicating Table 4.4 renders the coefficient of outside earnings to be positive and statistically significant when we use the absence rate as the dependent variable and the coefficients of outside earnings to lack statistical significance when we use the number of oral contributions and the number of interpellations as dependent variables.

We also tested whether the results change when we measure outside earnings, absence rates, the number of speeches, the number of oral contributions, the number of interpellations, and the number of group initiatives in logs. Replicating Table

⁹⁵In taking the natural logarithm of outside earnings (in Euros), absence rates (in percentage points), speeches, oral contributions, interpellations, and group initiatives (numbers), we use $\log(0) = 0$.

4 renders the coefficient of outside earnings to be positive and statistically significant when we use the absence rate as the dependent variable and the coefficient of outside earnings to lack statistical significance when we use the number of interpellations as the dependent variable.

We tested whether the results change when we consider absence rates measured in days with recorded votes, rather than as measured by the number of recorded votes. Measuring absence rates in days may capture outside opportunities more precisely because the number of recorded votes per day varies between 1 and 20: not attending one day with 20 recorded votes gives rise to less outside opportunities compared to not attending 20 days with one recorded vote each.⁹⁶ Replicating Table 4.4 does not change the inferences.

We tested whether including/excluding MPs without outside earnings in the whole legislative period changes the inferences. Replicating Table 4.4, excluding MPs who did not have outside earnings in the whole legislative period renders the coefficients of outside earnings to lack statistical significance when we use the number of oral contributions, the number of interpellations, and the number of group initiatives as dependent variables.⁹⁷

We tested whether including/excluding MPs with low/high outside earnings changes the inferences. The results show that excluding MPs with low outside earnings does not change the inferences. Excluding MPs with high outside earnings renders the outside earnings variable to be statistically significant when we use the absence rate as dependent variable.

4.5 Conclusion

We examine how German MPs' outside earnings are correlated with their absence and parliamentary activity. We assess MPs' effort empirically by drawing on new data on German MPs: in July 2007, the German Supreme Court adjudicated that MPs must publish details of their outside earnings. Our dataset covers the 17th legislative period from 2009 to 2013. Using absences during recorded votes, and

⁹⁶We account for MPs who attended some, but not all recorded votes on a day: when an MP did not attend two out of four recorded votes, for example, we consider the MP as having not attended half the day.

⁹⁷MPs without outside earnings during the entire legislative period do not help identifying the nexus between outside earnings and parliamentary effort, but may influence the coefficients of the model via covariates that change over time.

the number of speeches, oral contributions, interpellations, and group initiatives as indicators for effort and running fixed-effect panel regressions, the results do not show that outside earnings are correlated with absence and speeches. Outside earnings are, however, somewhat negatively correlated with oral contributions, interpellations, and group initiatives.

Our results do not corroborate findings based on Italian data, where MPs' outside earnings were shown to significantly increase absence rates ([Gagliarducci et al., 2010](#)). Three explanations spring to mind why results based on German and Italian data differ.

Firstly, penalties for missing recorded votes differ in Germany and Italy. In Germany, moonlighting MPs face wage cuts of up to 100 Euro for each session of recorded votes they miss (see §14 *Abgeordnetengesetz*). In Italy, missing recorded votes does not decrease MPs' salaries. The baseline salary is also much higher in Italy ([Mause, 2014](#)). Consequently, even if there were penalties for missed recorded votes, MPs' salaries are relatively less dependent on attendance. It is thus conceivable that results differ across countries because MPs only react to the incentives they face.

Secondly, differences in party discipline may well influence MPs' behavior. While party discipline in Europe, and especially in Germany, is generally quite strong, Italy is an exception to this rule. For example, over the period 1996-2000, one fourth of all deputies in the Italian parliament switched parties at least once ([Heller and Mershon, 2008](#)). A lack of party identification may explain why we observe a negative relationship between outside earnings and attendance rates in Italy.

Thirdly, Germany and Italy have very different political cultures. A political culture may be self-reinforcing ([Beniers and Dur, 2007](#)). If attending recorded votes is regarded as an indispensable duty in Germany while being substitutable for any other activity in Italy, our results are plausible. Differences in political culture may be measured, for example, by social capital, trust or legal origin ([Boix and Posner, 1998](#); [Bjørnskov, 2010](#)).

Our findings, however, must be interpreted with caution. Since we run fixed-effects panel regressions, the effect of outside earnings on parliamentary activity is identified by MPs who earn differing amounts of outside income over the course of the years. About 75% of all MPs do not have outside earnings. Many of the

MPs that have outside earnings earn the same amount each year. MPs with zero or constant outside earnings do not contribute to identify the effect. If we run party-specific regressions, even more cases are left out and identification is based on a handful of MPs. The variation in the data we can use is thus small.

Ideally, we would like to extend our sample to the legislative period 2013-2017. Thanks to new and stricter disclosure rules, variation in reported outside earnings in the legislative period 2013-2017 is probably much higher. However, these data will only be available as of 2017 onwards.

Disclaimer

In 2013 we started to write two individual papers on the issue investigated in this paper. Felix Arnold published his paper as DIW Discussion Paper "German MPs' Outside Jobs and Their Repercussions on Parliamentary Effort" (see [Arnold \(2013\)](#)) and presented his paper at the Economic Policy Seminar of the FU Berlin. Björn Kauder and Niklas Potrafke presented their paper "Outside earnings, attendance, and activity: Do German MPs meet their obligations?" at the Public Economics Workshop 2013 in Munich, the Annual Congress of the International Institute of Public Finance 2013, an internal conference at the Ifo Institute, and at the Australasian Public Choice Conference 2013. In the beginning of 2014, we merged the two papers.

Chapter 5

General Conclusion

5.1 Concluding Remarks

I want to congratulate the reader who kept reading until this point. This effort – which I highly appreciate – shall not go unrewarded. The General Conclusion wraps up the dissertation by discussing policy implications of the different chapters in Section 5.2. The limitations and weaknesses of my work (from a subjective point of view) are mentioned in Section 5.3. Finally, Section 5.4 gives an outlook on upcoming research projects that have been motivated by this dissertation.

5.2 Policy Implications

Chapter 1 evaluated the effect of signature requirements on the occurrence of citizen initiatives. We found that an increase in the signature requirement leads to substantial reductions in the number of observed citizen initiatives. This is a stunning result when put into context. Bavaria is said to be one of the most liberal states when it comes to the regulation of direct democracy. The hurdles that proposers have to overcome are – for the most part – lower than in other German states. Even where conditions are favorable, we show that the signature requirement has "bite". This implies that the hurdles imposed on proposers are usually binding.

A thought experiment also links this result to Chapter 2. As described there, the governing party (CSU) campaigned against the initiative for more direct democratic participation and launched a counterproposal that featured higher hurdles in terms of signature requirements, quora and regulations regarding the proposi-

tion process of initiatives. Today, Bavaria alone is the state that saw roughly 40 percent of all initiative activity in Germany. Most observers ascribe this to the liberal law. The results from Chapter 1 suggest that if the counterproposal had been adopted, Bavaria would probably not be leading the statistics with regards to initiative activity.

These observations together imply that politicians need to be careful when designing direct democratic institutions. Of course, there is a tradeoff: On the one hand, citizen participation is desirable as it ensures that the median voter can implement her preferred policies. On the other hand, marginal topics and minority interests should be prevented from being put to a vote, as overuse of citizen initiatives can also lead to voter fatigue. We show that signature requirements are an effective tool to steer direct democratic activity. The "optimal" choice of a signature requirement, however, depends on the politician's (or the voter's – see Chapter 2) preferences.

Chapter 3 provided evidence that turnout is higher in close elections. Furthermore, other factors like rain on election day or district size were negatively related to turnout. The main policy implications can be drawn from interaction effects of closeness with the two latter variables, however. The regression results suggest that closeness has a moderating effect: For example, rain on election day reduces turnout on average, but not if the race is close. Put differently, the competitive context of an election plays a major role for the functioning of other electoral stimuli.

This is valuable information for campaigners. They try to influence election outcomes to the favor of their candidate by means of advertisements, campaign rallies, get-out-to-vote leaflets and several other mobilization instruments. If closeness mitigates the effects of rain on election day and constituency size, it is well possible that the functioning of the electoral stimuli employed by campaigners is also affected in some way. Therefore, the competitive context of an election can be thought of as being "first order": If electoral stimuli work best in uncompetitive environments, it is questionable whether campaigners should concentrate their efforts on close races. A given amount of campaign spending might buy more votes in uncompetitive elections. However, marginal votes are more important in razor thin first-past-the-post contests and it might be worthwhile to spend more money on them. My results suggest that electoral closeness is a factor that is to be taken into account when planning election campaigns.

Chapter 4 investigates the interplay of outside earnings and parliamentary activity for German members of parliament (MPs). The results suggest that higher outside earnings are not negatively associated with visible activities like attendance during recorded votes or giving speeches. However, the data show a negative correlation between outside earnings and other activities that are not so well known by the public. These activities include oral contributions, interpellations and group initiatives. The fact that politicians allocate their time differently to visible and non-visible activities indicates a persisting lack of transparency. Therefore, developments like the project `abgeordnetenwatch.de` – a website dedicated to monitoring the behavior of German MPs – are generally useful.

When I started writing Chapter 4, MPs had to report their outside earnings in three broad categories. The exact amounts in Euros were thus unknown to the public and the researcher. I think it is a good development that stricter disclosure requirements⁹⁸ are in place now. For the public, this brings the advantage of increased accountability. For the researcher, measurement error in empirical investigations using outside earnings as explanatory variable is reduced.

5.3 Limitations

In this section, I want to briefly outline the limitations and weaknesses of the research projects that are part of this dissertation.

In Chapter 1, we found substantial effects of lower signature requirements on the probability of observing citizen initiatives. Indeed, these effects are strikingly large and seem to be confined to observations that are relatively close to the threshold where the signature requirement changes. They return to lower levels further away from the threshold. It is thus conceivable that part of the effect must be attributed to a local effect in which the threshold itself constitutes a specific treatment. If being close to the threshold (or just above) makes citizens particularly aware that initiatives are relatively cheap to propose, it may be in fact the additional awareness rather than the lower costs that account for part of the effect. As our analysis cannot tell these two mechanisms apart from each other, the results have to be interpreted with caution.

A common concern against the results in Chapter 2 is the so-called *ecological*

⁹⁸MPs have to report their outside earnings in 10 categories now. The parliament could not agree on publication of the exact figures, however.

inference fallacy argument. We want to explain individual preferences for direct democracy. However, we only observe voting data aggregated to the municipal level. Taking information on a whole population and deducing inferences for single individuals within this population can be problematic. Although we do our best to rule out such arguments (and show some evidence for the validity of our interpretation in Table 2.6), we can never ultimately be sure.

In Chapter 3, I argue that electoral closeness positively affects the probability of being the pivotal voter. This is certainly true. However, the relationship between closeness and pivotalness is far from linear. The probability of being pivotal should increase exponentially with closeness. The regression approach I employ only accounts for linear effects of closeness on turnout. As this was also a concern of one anonymous referee, I plan to show the link between closeness and pivotalness graphically in a revised version of the paper.

The main weakness of the empirical analysis in Chapter 4 is probably a simultaneity problem: Parliamentary activity and outside earnings are jointly determined. The results might thus suffer from reverse causality bias. We are aware of this shortcoming and interpret our results as correlations only. A possible solution would be to find an instrumental variable that provides exogenous variation in outside earnings. There exists a study for Italy (Gagliarducci et al., 2010) that uses pre-parliament income from private sector occupations as an IV for outside earnings. However, such data are not available in our case, as German MPs need not disclose how much they earned before entering parliament.

A further limitation of our study is that the fixed effects estimation makes it impossible to estimate coefficients of variables that do not change over time. Nevertheless, it would be interesting to know how gender or party affiliation interact with outside earnings and parliamentary activity.

5.4 Further Research

The work on this thesis has inspired some new research projects that I briefly want to mention here.

Chapters 1 and 2 deal with direct democracy in the German state of Bavaria. I am currently working on two other projects that are directly related to these chapters.

The first project establishes an immediate link between preferences for citizen involvement and the use of direct democracy. It is joint work with David Stadelmann and Ronny Freier. Here, we try to explain the use of citizen initiatives in Bavaria between 1995 and 2011 as a function of the approval rate in the referendum on the introduction of direct democracy in 1995. We therefore combine the data used in Chapters 1 and 2 of this thesis. Preliminary results suggest that the association is positive, as anticipated. Hence, municipalities with a larger preference for direct democracy also use the instruments more frequently once they are available. This further strengthens the conjecture from Chapter 2 that referendum results are a valid measure for revealed preferences for direct democracy.

Another project that is currently work in progress focuses on the effect of citizen initiatives on housing supply. It is joint work with Thorsten Martin and Ronny Freier. Many people claim that citizen initiatives are often used to prevent infrastructure projects from happening. We investigate whether initiatives have a negative effect on the approved residential area in Bavarian municipalities. To account for the endogeneity of initiative occurrence, we rely on spillover effects from neighboring municipalities in an instrumental variable setting. The preliminary results suggest that initiatives indeed reduce the approved residential area.

Chapter 3 has also inspired a new research project. While the main focus in the chapter was on electoral closeness, I have also shown that rain exerts a negative impact on turnout. This fact is used in a new project which is joint work with Ronny Freier. Here, we use rain on election day as exogenous variation in turnout to identify the partisan effects of electoral participation. Which party (if any) profits from higher voter turnout? Results from the IV estimation show that in municipal and state elections in the German state of North-Rhine Westphalia, it is primarily the social democrats who profit from higher voter turnout. This comes at the expense of the conservatives, whose vote share declines. The paper is currently *Revise & Resubmit* at *Electoral Studies*. A DIW Discussion Paper is also available ([Arnold and Freier, 2015a](#)).

Another project I am currently working on uses vote shares as dependent variables as well. In joint work with Alexandra Avdeenko, we are interested in electoral success of the Left Party in German Federal Elections. Interestingly, we find that this party consistently underperforms in areas close to the old inner-German border. Our conjecture is that voters punish repression they experienced in the period of German Separation. As the Left Party is the successor of the *Ancien*

Regime in the GDR, people ascribe their repression experiences to this party. To underpin this mechanism with data, we show some evidence that (i) repression was particularly high in areas close to the inner-German border, (ii) people associate the Left Party with the old ruling party (SED) from the GDR and (iii) the old ruling party is largely seen as responsible for repression and crimes committed by the state security (STASI) in the GDR.

Finally, one could easily imagine an update of Chapter 4 after the 18th legislative term in 2017. The problem with the current analysis is that party specific effects cannot be disentangled from opposition vs. government specific effects. Having data on two legislative periods in a row would make this possible, as there are some parties in government in the 18th legislative term that used to be in opposition the term before. However, the data for this analysis will only become fully available in the end of 2017.

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Summary

Chapter 1

Signature requirements are often used as hurdles to prevent overuse of direct democratic instruments such as citizen initiatives. We evaluate the causal effect of lowering signature requirements on the number of observed citizen initiative petitions. Based on municipal-level data for Germany, we make use of changes in signature requirements that occur at specific population thresholds to build an identification strategy using a regression discontinuity design. We find that reducing the signature requirement by 1 percentage point increases the probability of observing an initiative petition in a given city-year by 8–10 percentage points.

Chapter 2

We study a constitutional change in the German State of Bavaria where citizens, not politicians, granted themselves more say in politics at the local level through a state initiative election. This institutional setting allows us to observe revealed preferences for direct democracy and to identify factors which explain these preferences. Empirical evidence suggests that support for direct democracy is related to dissatisfaction with representative democracy in general rather than with an elected governing party.

Chapter 3

One prediction of the calculus of voting is that electoral closeness positively affects turnout via a higher probability of one vote being decisive. I test this theory with data on all mayoral elections in the German state of Bavaria between 1946 and 2009. Importantly, I use constitutionally prescribed two-round elections to measure electoral closeness and thereby improve on existing work that mostly uses *ex-post* measures that are prone to endogeneity. The results suggest that electoral

closeness matters: A one standard deviation increase in closeness increases turnout by 1.68 percentage points, which corresponds to $\frac{1}{6}$ of a standard deviation in this variable. I also evaluate how other factors like electorate size or rain on election day affect turnout differentially depending on the closeness of the race.

Chapter 4

We examine moonlighting by politicians in Germany. In July 2007, the German Supreme Court adjudicated that members of parliament (MPs) have to publish details of their outside earnings. Using panel data models, we investigate how outside earnings are correlated with absence and parliamentary activity. The results do not indicate that outside earnings are correlated with absence rates and speeches; but they do suggest that outside earnings are somewhat negatively correlated with oral contributions, interpellations, and group initiatives. We propose that the results for Germany do not corroborate evidence on other countries such as Italy because party discipline, institutions, and political cultures differ across countries.

German Summary

Kapitel 1

Um dem übermäßigen Gebrauch von Instrumenten wie Bürgerbegehren vorzubeugen, werden oftmals sogenannte Unterschriftenhürden vorgeschrieben. Wir evaluieren den kausalen Effekt einer Senkung der Unterschriftenhürde auf die beobachtete Anzahl an Bürgerinitiativen. Basierend auf deutschen Gemeindedaten nutzen wir diskrete Sprünge in der Unterschriftenhürde an bestimmten Bevölkerungsgrenzen, um den Effekt mittels eines Regression Discontinuity Designs (RDD) zu identifizieren. Die Ergebnisse legen nahe, dass eine Senkung der Unterschriftenhürde um einen Prozentpunkt die Wahrscheinlichkeit, eine Initiative in einem gegebenen Stadt-Jahr zu beobachten, um acht bis zehn Prozentpunkte erhöht.

Kapitel 2

Wir untersuchen die Einführung direkter Demokratie in Bayern. Dort haben die Bürger selbst – nicht Politiker – über die Ausweitung direkt demokratischer Partizipation auf Kommunalebene mithilfe eines landesweiten Referendums entschieden. Diese institutionelle Besonderheit erlaubt es uns, offenbarte Präferenzen für direkte Demokratie zu messen und anschließend zu untersuchen, welche Faktoren diese Präferenzen erklären. Die empirischen Befunde legen nahe, dass die Befürwortung direkter Demokratie mit einer generellen Unzufriedenheit mit repräsentativer Demokratie einhergeht und nicht mit Ablehnung der regierenden Partei.

Kapitel 3

Eine Vorhersage der Theorie des rationalen Wählers ist, dass die Knappheit der Wahl einen positiven Effekt auf die Wahlbeteiligung ausübt, da die Wahrscheinlichkeit, mit der eigenen Stimme die Wahl zu beeinflussen, in diesem Fall höher

ist. Ich teste diese Theorie mit Daten zu allen Bürgermeisterwahlen in Bayern zwischen 1946 und 2009. Um ein Maß für Knappheit der Wahl zu erlangen, nutze ich aufeinander folgende Erstrundenwahlen und Stichwahlen. Dies stellt eine Verbesserung gegenüber bisherigen Studien dar, die vor allem auf potenziell endogene *ex-post* Maße vertrauen. Die Resultate zeigen, dass die Knappheit der Wahl eine wichtige Rolle spielt: Steigt die Knappheit um eine Standardabweichung, so erhöht sich die Wahlbeteiligung um 1.68 Prozentpunkte, was einem Sechstel einer Standardabweichung in dieser Variable entspricht. In einer Heterogenitätsanalyse zeige ich außerdem, wie andere Faktoren wie die Wahlbezirksgröße oder Regen am Wahltag die Wahlbeteiligung abhängig von der Knappheit unterschiedlich beeinflussen.

Kapitel 4

Wir untersuchen die Nebentätigkeiten von deutschen Politikern. Im Juli 2007 hat das Bundesverfassungsgericht verfügt, dass alle Bundestagsabgeordneten ihre Nebeneinkünfte offenlegen müssen. Mit Paneldatenmodellen zeigen wir, wie Nebeneinkünfte mit Abwesenheit und parlamentarischer Aktivität korrelieren. Die Resultate zeigen nicht, dass Nebeneinkünfte mit Abwesenheitsquoten und der Anzahl an Reden korreliert sind; sie zeigen aber, dass Nebeneinkünfte negativ mit Wortbeiträgen, Anfragen und Gruppeninitiativen korrelieren. Wir vermuten, dass die Resultate aus Deutschland nicht die Evidenz aus anderen Ländern wie zum Beispiel Italien bestätigen, weil Parteidisziplin, Institutionen und politische Kulturen zwischen den Ländern unterschiedlich sind.

Declaration

Erklärung gem. §4 Abs. 2 der Promotionsordnung

Hiermit erkläre ich, dass ich mich noch keinem Promotionsverfahren unterzogen oder um Zulassung zu einem solchen beworben habe, und die Dissertation in der gleichen oder einer anderen Fassung bzw. Überarbeitung einer anderen Fakultät, einem Prüfungsausschuss oder einem Fachvertreter an einer anderen Hochschule nicht bereits zur Überprüfung vorgelegen hat.

Ich erkläre außerdem, dass ich meine Dissertation selbstständig verfasst habe.

Berlin

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