Technische Universität Dortmund

Dissertation

The Role of Political Institutions and Representation in Economic Policy - An Empirical Analysis

vorgelegt von

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Inauguraldissertation
zur Erlangung des akademischen Grades

Doctor rerum politicarum

der Technische Universität Dortmund,
Wirtschafts- und Sozialwissenschaftliche Fakultät

September 2014

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Institution:
Technische Universität Dortmund
Wirtschafts- und Sozialwissenschaftliche Fakultät
Lehrstuhl Öffentliche Finanzen

Diplom-Kaufmann, Diplom-Volkswirt Sebastian Garmann: *The Role of Political Institutions and Representation in Economic Policy - An Empirical Analysis*

Acknowledgments

This dissertation was written while I was a doctoral student at the Ruhr Graduate School in Economics and at the Chair of Public Economics at the University of Dortmund under the supervision of Prof. Dr. Wolfram F. Richter. I am very thankful to him for his advice, constant support, and effort. Whenever the need arose, he took the time to discuss any difficulties. Furthermore, I want to thank Tobias Seidel and Kornelius Kraft for not hesitating to become a part of my thesis committee. Financial support from the Ruhr Graduate School in Economics is gratefully acknowledged.

This thesis has strongly benefitted from comments of editors and anonymous reviewers of different academic journals. I am grateful for their invaluable effort which has helped to improve this thesis. I further want to thank the whole Chair of Public Economics at the University of Dortmund for the collaboration during the last three years.

Most of all, I want to thank Julia and my family for the encouraging support during the last years.

Publication Details

- Chapter 1 is forthcoming in Applied Economics.
- Chapter 3 has been published in Ecological Economics (Ecological Economics Vol. 105, 1-10, 2014).
- Chapter 4 is forthcoming in Food Policy.

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Part I

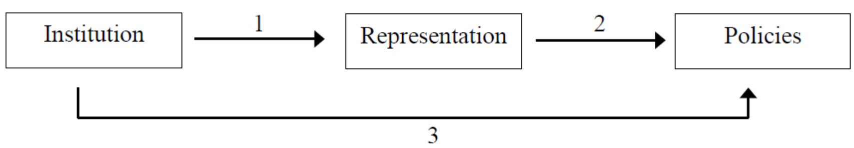
Introduction

Introduction

In recent years, there has been a growing interest in studying whether and how institutions and representation can influence political decision-making. The study of different political institutions can be classified within the broader field of "New Institutional Economics" that has emerged since the middle of the 20^{th} century (see Voigt, 2011). The interest in the effects of representation on policies partly originated from the perception that the simple Downsian median voter approach (see Downs, 1957), which is often used in theoretical models to describe outcomes of political processes, in many cases does not provide a convincing approximation to real-world decision-making. This approach circumvents the formulation of a detailed political model by simply assuming that the median voter decides over policy, i.e., political representation, for example through political ideology or the form of government, does not matter. There is growing empirical evidence that this assumption is inappropriate. Most prominently, Lee et al. (2004) have found that voters elect rather than affect policies in elections for the US house of Representatives.

This thesis is composed of four self-contained chapters that make independent contributions to the question of how different forms of institutions and representation can affect policy. Figure 1, inspired by Besley and Case (2003), illustrates the three strands of the empirical literature dealing with institutions, representation, and policy choices. Arrow 1 stands for (empirical) studies that examine the link between institutions and representation. Examples from the

FIGURE 1: Institutions, representation, and policy



existing literature include studies related to the effects of electoral rules on minority representation (see Trebbi et al., 2008) and on the validity of Duverger's law (see Fujiwara, 2011). However, this link is not covered in this thesis.

Arrow 2 stands for studies that examine the effect of political representation on policy. For example, existing studies have investigated the impact of women's leadership on the provision of local public goods (see Chattopadhyay and Duflo, 2004) and the effect of partisan representation in the US Congress on the distribution of federal funds (see Albouy, 2013). Arrow 2 will be covered in chapters 1 and 3 of this thesis. Specifically, I investigate whether government ideology and the form of government influence policy.

Arrow 3 represents the reduced-form effect of institutions on policy, which can be direct or mediated through the effect of political representation. Examples in the empirical literature representing arrow 3 include Persson and Tabellini (2004), who investigate the effect of constitutional rules on the size of government, and Funk and Gathmann (2011), who study the effect of direct democratic institutions on public spending. In this thesis, arrow 3 is represented by chapters 2 and 4.[1]

In chapter 1 of this thesis, I investigate whether and how coalition governments causally influence fiscal policies using data from German municipalities. There is a sizeable literature theorizing that the form of government has an influence on the chosen policies (see, e.g., Roubini and Sachs, 1989; Perotti and Kontopoulos, 2002; Schaltegger and Feld, 2009), and in turn testing whether this theory has any empirical content. A preeminent problem in this literature is that the form of government is typically not randomly assigned to political units. It is plausible that electoral choices of voters are to a significant extent driven by voter preferences regarding fiscal policy, and that this introduces endogeneity concerns. Moreover, it is likely that there are many other unobservable factors correlated with both fiscal policy and electoral outcomes. This could lead to an

[1]It might be debatable whether the number of legislators as considered in chapter 2 represents an institution or should be perceived as representation. In some election systems, the number of legislators is itself affected by the electoral choices of the voters and might thus be perceived as representation. This is, for example, the case when excess seats can occur. In chapter 2 of this thesis, the number of legislators, however, is determined prior to the election. Therefore, it should be considered as an institution. In line with this argument, Pettersson-Lidbom (2012) also considers the number of legislators as an institution.

omitted variable bias. A prime example of such an omitted variable is political culture.

The main contribution of chapter 1 is to use a quasi-experiment that introduces exogenous variation into the probability of being governed by a coalition government. This can be exploited in a Regression Kink Design. Applying the Regression Kink Design to a panel data set of German local governments obtained from the state of North-Rhine Westphalia, it is assumed that local governments close to the kink point (the 50% vote share of the strongest party) are equal in all observable and unobservable covariates except for the treatment probability. My results show that this empirical strategy is an important innovation, as quasi-experimental estimates differ to a large extent from simple OLS estimates: While OLS estimates suggest that coalition governments increase expenditures and taxes, the reverse is true when endogeneity issues are addressed.

Chapter 2 is concerned with the interactions of institutions. It is natural to believe that not only do different institutions exert independent influences on policy, but that the effectiveness of a particular institution is conditioned by the existence of other institutions. Understanding the interactions of institutions is thus an important endeavor (see Acemoglu, 2005). However, estimating the *causal* effect of a combination of institutions is complicated by the need to find quasi-experiments in (at least) two dimensions.

Chapter 2 exploits such a rare opportunity. Specifically, I investigate how the nomination scheme of the city manager, the head of the public administration in municipalities of the German state Hesse, influences the effects of the municipal council's fragmentation on public spending and taxation. The nomination scheme could be an important determinant of the strength of fragmentation effects for at least two reasons: First, the existing literature on government fragmentation is suggestive (but not conclusive) of the fact that the effects of government fragmentation on spending could be to a large extent dependent on the underlying political and administrative system. Baqir (2002) provides arguably the most encompassing analysis in this literature; however, he employs rather questionable instrumental variables to tackle potential endogeneity issues. Second, Pettersson-Lidbom (2012) theoretically argues that fragmentation can play a role in local government systems, because a budget-maximizing public administration can be better monitored with more councilors. My empirical analysis tests this argument of an agency-problem. Obviously, more councilors are only necessary to monitor the public administration if the latter is independent from the council such that it can diverge from the spending preferences of the council. My findings suggest that a reform of the nomination scheme implies an increase in the independence of the public administration, and therefore, one should observe (more) significant effects of the number of councilors after the reform.

Moreover, an important innovation of chapter 2 is that it empirically focuses on the interplay between the local council as the local legislature and the public administration. Local governments in developed countries typically consist of these two separate institutions. Thus, it is important to understand the interplay between these bodies of local governments. However, such kind of empirical research is non-existent so far. Theoretical studies on this topic include those of Rauch (1995) and Vlaicu and Whalley (2013). They also describe the relationship between the public administration and the local council as a principal-agent relationship, and my empirical results are in line with this interpretation.

It should be mentioned that many existing studies in political economy use national-level data compared to local-level data as employed in chapters 1 and 2 of this thesis. Local government data have the advantage that these originate from comparable political units that are observed under the same institutional framework. Thus, estimates are less likely to suffer from unobserved heterogeneity. This is a large advantage compared to national-level data, especially if claims for causality should be made. However, local-level data have the obvious disadvantage that they cannot be used invariably, for example, in cases where either the outcome or the explanatory variable of interest only varies at a higher level of government, as is the case in chapters 3 and 4 of this thesis.

In chapter 3, I study the effects of representation on environmental outcomes. Specifically, using a cross-country data set and static as well as dynamic panel data methods, I investigate to what extent political ideology and government fragmentation are important determinants of CO2 emission reduction processes. Besides the natural relevance of this topic in an era of climate change, it is important to study partisan effects in environmental policy because it is unclear that the latter can be placed on the typical left-right scale of the political spectrum. Left-wing as well as right-wing parties have both incentives and disincentives to promote environmental protection. For example, left-wing parties might be incentivized to protect the environment because their typical clientele is more often affected by the negative consequences of pollution. On the other hand, these parties might shy away from environmental protection because it might threaten employment opportunities in heavy-polluting sectors. By contrast, right-wing parties might not be incentivized to promote environmental protection because this might lead to adjustment costs for entrepreneurs. However, these parties are often Christian parties (at least in the sample of Western democracies that I study), and are therefore concerned with the integrity of the divine creation. This could strengthen their interest in environmental protection (see Potrafke, 2011).[2]

In line with these ideas, I find that the (perhaps most pragmatic) center-governments reduce emissions to the largest extent. This analysis once again indicates that the Downsian median voter theorem does not always hold. Studying partisan effects in environmental policy is also helpful because this policy field is arguably less subject to reverse causality concerns than others (e.g., social policy): environmental policy is not a major motivator in most elections. Moreover, voters are likely to be poorly informed about who is responsible for the outcomes of environmental policy. Thus, it is not likely that governments will be turned out of office because of the environmental policies they pursue. My exercise in chapter 3 therefore provides a strong test of the existence of partisan effects.

My results in chapter 3 also show that the organization of government represented by the number of parties can have important (but to some extent less significant) policy consequences: Emissions are reduced to a lesser extent if the number of parties that constitute the government is high. This shows that the degree of government fragmentation plays a crucial role in adjustment processes as hypothesized by Alesina and Drazen (1991).

Chapter 4 is concerned with a classical example of special-interest policy. It is well known that - at least in democracies - there is a systematic bias in

[2]Interestingly, there exist both right-wing as well as left-wing Green parties. For example, in Germany, the most popular Green party (*Bündnis 90/Die Grünen*) can be categorized as a left-wing party, while the ÖDP (*ökologisch-demokratische Partei*) can be classified as a center-right Green party. However, in contrast to the former, the latter has not been very successful in elections.

trade policies: Trade policies tend to generously support farmers, for example, through price distortions (see Persson and Tabellini, 2002). In developing countries, this bias in favor of the agricultural sector has historically been far less apparent. However, in the last two decades, the developing countries have modified trade policies in favor of agriculture (see Anderson et al., 2013). Chapter 4 analyzes how a change in a bundle of institutions - represented by the globalization process - affects this kind of special-interest policy. Precisely, I investigate whether the globalization process has affected agricultural support through price distortions or direct payments made to domestic farmers.

Globalization does not just affect one specific institution, but several others. For example, globalization might affect the existence of trade barriers, shape political institutions (for example, might cause a growing importance of supra-national institutions), or might be responsible for a change in economic or social culture.[3] Globalization could affect not only farmers but also food consumers in various negative ways, thus creating a rationale for politicians to increase agricultural support in the globalization process. However, there are also theoretical arguments for why agricultural support should be lowered in the course of globalization, thus making the effects of globalization on agricultural support ultimately an empirical question.

An econometric challenge in chapter 4 is that globalization is not likely to be exogenous to agricultural support or trade policies in general. Both reverse causality and omitted variables could plausibly bias simple OLS estimates. Moreover, measurement error could be a problem as it is questionable whether the employed KOF indices are perfect proxies for globalization. Both issues have received little attention in the existing globalization literature. I deal with these issues by employing system and difference GMM estimators that are able to generate internal instruments for globalization. My results suggest that globalization significantly increases agricultural support.

To summarize, this thesis empirically shows that both institutions and representation can have important effects on policy. A careful empirical examination of these effects is necessary to resolve the theoretical ambiguities. From the methodological point of view, the thesis shows that causality is a major issue in the political economy literature. Thus, each chapter of this thesis takes causality

[3]This corresponds to a broad interpretation of institutions as suggested by North (1990). He defines institutions as any "humanly devised constraints that structure human interaction."

issues seriously and develops empirical strategies that address endogeneity to the largest possible extent.

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Part II

Chapters

Chapter 1

The causal effect of coalition governments on fiscal policies: evidence from a regression kink design

1.1 Introduction

Coalition governments seem to be the rule rather than the exception in democratic countries. In fact, 24 of the 28 countries in the European Union recently have more than one party in their national government. Contrary to conventional wisdom, coalition governments occur not only in proportional election systems, but in majoritarian systems as well. A prominent example of such a coalition is that which was established in the United Kingdom by the conservatives and liberals in 2010. Despite the importance of coalition governments in the political process, no clear consensus has yet emerged in the literature regarding the causal effects that they might have on fiscal policy.

The lack of consensus on the subject is unsurprising, considering the inherent difficulty in measuring such effects empirically. First, the form of government is not randomly assigned to political units (such as local governments as in the present case study): This creates problems of reverse causality and omitted variable bias. Second, in proportional election systems individual parties are rarely close to holding the absolute majority of neither the vote share nor the seat share. Third, it is difficult to utilize simple Regression Discontinuity methods to establish causality in the present case: In proportional election systems, as in my case study, there is no discontinuity at the 50% vote share of the strongest party (i.e. the party with the most votes in the municipal elections) as, by contrast, would be the case in a two-party system.

In this study, I develop an empirical strategy to estimate the causal effect of coalition governments on fiscal policies. I overcome the problems stated above by using a Regression Kink Design (RKD) and municipal data from the state of North Rhine–Westphalia (NRW), Germany. The RKD makes use of the fact that, because of the D'Hondt seat allocation method employed in the municipal election system, the probability of holding the absolute majority of the seats exhibits a kink at the 50% threshold of the vote share of the strongest party[1], which can be exploited in an instrumental variables (IV) strategy. Moreover, in my panel data set there are almost as many single-party governments as there are coalition governments such that problems associated with the existence of a proportional election system can be overcome.

Theoretically, the model put forth by Weingast et al. (1981) predicts that government spending should be higher under coalition governments than under single-party governments as the number of decision-makers in a government is positively associated with the amount of spending. The underlying argument is framed as a political common pool problem: Their model assumes that the budget is a common pool for all political actors. These political actors value the benefits of projects for their own interest groups more than the costs associated with them, leading to an overutilization of the public budget and consequently a bias toward overspending.[2] Empirically, results are less clear-cut: Existing empirical studies (Roubini and Sachs, 1989; Edin and Ohlsson, 1991; De Haan and Sturm, 1994; Perotti and Kontopoulos, 2002; Ashworth and Heyndels, 2005; Bawn and Rosenbluth, 2006; Persson et al., 2007; Schaltegger and Feld, 2009) come to conflicting conclusions. Most importantly, though, it is unclear to what extent the potential problems of omitted variable bias and reverse causality

[1] I use an absolute seat majority of the strongest party in the local council as a proxy for single-party governments as the form of government is not observable in the data set. If the strongest party had no seat majority, the municipality was considered to be governed by a coalition government. More details on this procedure are given in section 1.2.2.

[2] In a recent contribution, Primo and Snyder (2008) question the findings of Weingast et al. (1981) and show that under alternative assumptions, even a reverse Law of $1/n$ can hold, i.e. there exists a negative relationship between government fragmentation and spending.

have been resolved in these studies.[3]

More generally, the present study is related to a recent literature studying the aforementioned common pool problem in political economy (Tyrefors Hinnerich, 2009; Schaltegger and Feld, 2009; Egger and Köthenbürger, 2010; Pettersson-Lidbom, 2012). Moreover, the study fits in with recent studies in political economy that use methods from the program evaluation literature to measure the effects of political institutions on economic outcomes (Pettersson-Lidbom, 2008; Gagliarducci and Nannicini, 2013; Tyrefors Hinnerich and Pettersson-Lidbom, 2014). Furthermore, this study is related to a new body of literature that utilizes the RKD to generate credibly causal estimates (Dahlberg et al., 2008; Simonsen et al., 2010; Nielsen et al., 2010; Card et al., 2012; Lundqvist et al., 2014). This study is one of the very first to implement such a design. Moreover, to the best of my knowledge, this is the first study to use a RKD in the case of a binary (as opposed to a continuous) treatment.

The main conclusion of this study is that coalition governments do not spend more, but rather significantly less, than single-party governments. Looking at different sub-categories of the budget, I find that the lower spending in the case of coalition governments is mainly driven by a decrease in spending on public administration. Moreover, I find that coalition governments generate lower tax revenues.

The remainder of the chapter is structured as follows: Section 1.2 consists of a description of the institutional framework in NRW and the data used for this study. Section 1.3 presents the empirical strategy. In section 1.4, I present the results and check their robustness. Finally, section 1.5 concludes.

1.2 Institutional framework and data

1.2.1 Institutional framework

In Germany, municipalities are the lowest administrative unit of government but are, however, of considerable economic importance: They have the right to self-government and are responsible for roughly one-third of total German

[3]In a paper that was written concurrently to this one, Freier and Odendahl (2012) estimate the coalition effect using computer simulated counterfactuals. They also find some evidence that coalition governments spend less. However, their results seem to be quite dependent on the specification used.

government spending. Furthermore, they are free to set three different tax rates: a tax on business profits (*Gewerbesteuer*), on agricultural land (Property Tax A), and on business and private land (Property Tax B). Aside from levying taxes and receiving grants from higher tiers of government, municipalities finance themselves through debt, fees, and financial contributions. German municipalities are responsible for culture, elementary schools, economic promotion, and the maintenance of municipal roads, among other things.

NRW, the state upon which this study focuses, is the most populous German state with approximately 17.5 million inhabitants spread across 396 municipalities. Of these, 23 municipalities double as counties (*kreisfreie Städte*). Because these municipalities have greater autonomy and different spending responsibilities than cities that belong to a county, they are not comparable to the other municipalities, and therefore, they have been excluded from the study. This leaves 373 municipalities that I observe over the 1985–1999 period. This period consists of three legislative terms, each lasting five years. The elections for these legislative terms were in the years 1984, 1989, and 1994. Because elections were held toward the end of the year[4], it is reasonable to assume that each newly elected government could influence only the variables in the year following the election for the first time, i.e. the relevant years for the legislative term 1984–1989 are 1985, 1986, 1987, 1988, and 1989.

During the investigation period, NRW had a closed-list, mixed-member proportional election system. The number of seats in a municipal council was determined by population size and ranged from 21 to 69 seats. Seats were allocated using the D'Hondt method and only parties with a minimum vote share of 5% were considered in the seat allocation process. In 1999, a large reform of the municipal election system took place in NRW, which changed the political system in several ways: Among other things, there was a switch from a council-manager to a mayor-council system, the minimum vote share was abolished, and the seat allocation method was changed. Because of the dramatic differences in institutional settings that this reform created, only data until 1999 was included.

Two parties mainly characterize the political spectrum of NRW: in almost all municipal elections from 1985 to 1999, either the center-right Christian Democratic Union (CDU) or the center-left Social Democratic Party (SPD)

[4]The concrete dates were September 30, 1984; October 1, 1989; and October 16, 1994.

was the strongest party. Only in the case of nine elections did a third party achieve the majority of votes. These third parties were always voter associations. Because these nine elections seem to be highly non-partisan, and because the political system in these municipalities might be non-comparable to other municipalities, these nine cases have been dropped from the analysis.[5] The results, however, remain virtually unchanged if they are included. Two smaller parties are of importance in local politics in NRW: The liberal Free Democratic Party (FDP) and the Greens. These parties are legislatively represented in many municipal councils. If a coalition has to be built, the Greens are traditionally close to the SPD, while the FDP is close to the CDU. There are also municipality specific voter associations and other parties in NRW, but, compared to other German states, they are relatively rare and seldom represented in councils.

1.2.2 Data

For this study, I use yearly electoral, financial, and population data for NRW from the Statistical Office of NRW. The following eight outcome variables are used:

- Total expenditures
- Spending on personnel
- Spending on public administration
- Material spending
- Tax revenues
- Multiplier Property Tax A
- Multiplier Property Tax B
- Multiplier Business Tax

I express all expenditure variables and tax revenues in per capita terms in constant 2005 prices (EUR). The multipliers for the three local tax rates, which the municipalities are free to set, are denoted in percentage points. Because the histograms typically show a right-skewed distribution of the variables (as can be seen in figure 1.1), a logarithmic transformation is applied to all of the outcome variables for the estimation. As a result, the influence that the type of government has on these variables will have a percentage interpretation.

[5]Since each election results in 5 years of data, 45 observations have therefore been dropped.

FIGURE 1.1: Distribution of outcome variables

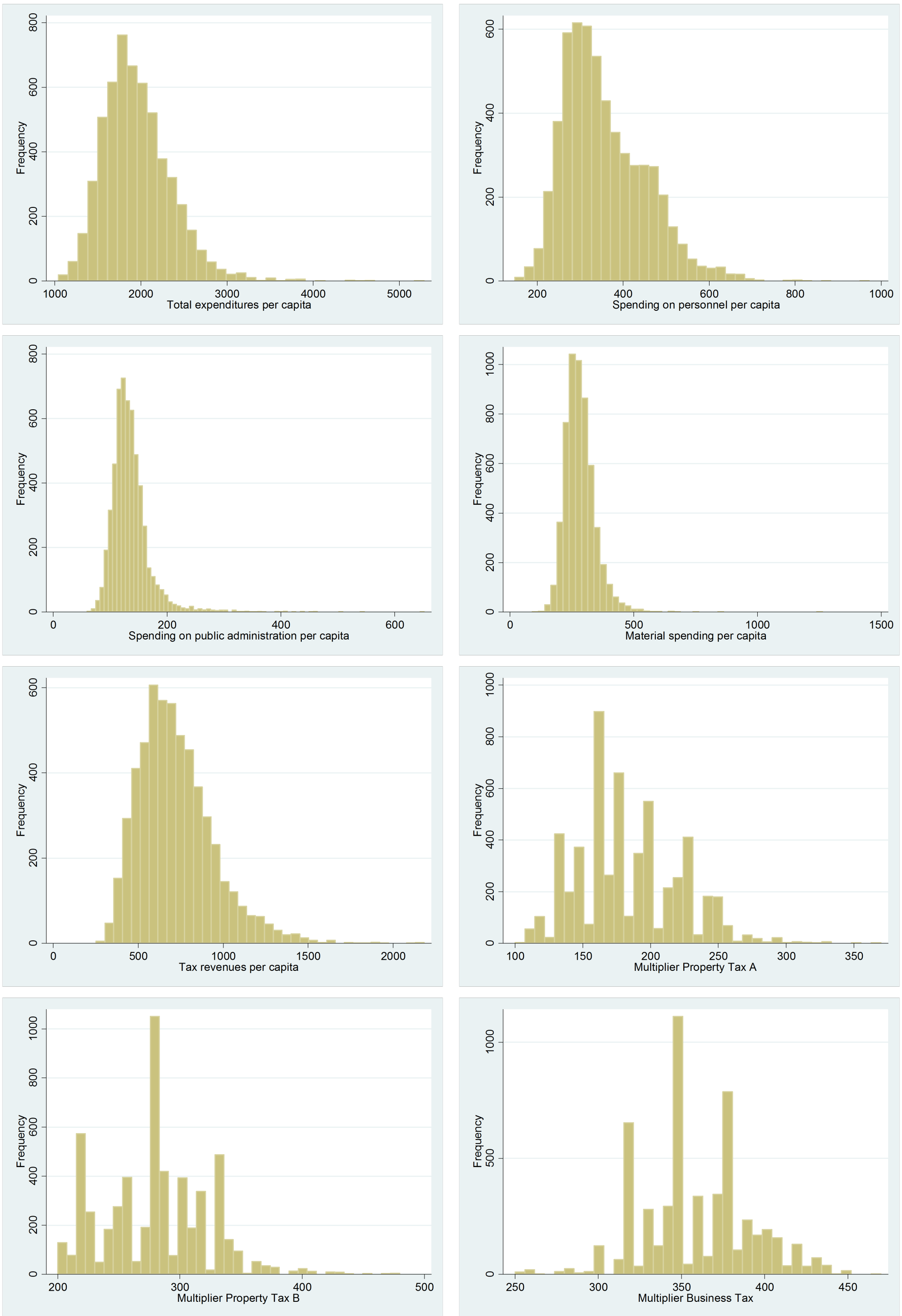


Table 1.1: Summary statistics

	Mean	Std. Deviation	Min	Max
Outcome variables				
Total Expenditures	1958.99	401.80	1038.63	5289.8
Spending on Personnel	357.32	96.17	147.95	972.88
Spending on public administration	135.96	36.15	59.50	646.77
Material Spending	283.49	61.56	89.58	1244.37
Tax revenue	723.91	223.53	251.54	2182.45
Multiplier Property Tax A	183.96	38.00	100	370
Multiplier Property Tax B	280.60	43.21	200	480
Multiplier Business Tax	359.54	32.50	250	470
Assignment variable				
Vote share of the strongest party	0.4917	0.0719	0.3256	0.8114
Covariates				
Population size	26295.89	22425.53	3752	148870
Proportion of people below 15	0.1741	0.0204	0.1211	0.2610
Proportion of people above 65	0.1377	0.0229	0.0787	0.2187
Council size	37.92	7.92	21	69
Population density	3.97	3.83	0.39	22.27
Share of foreigners	0.0613	0.0316	0.0068	0.2192

Notes: Expenditure variables are expressed in constant 2005 prices in per capita terms. The multipliers of the tax rates are expressed in percentage points. The table only contains municipalities with a relevant population size less than 25000. There are 6308 observations that fulfil this requirement.

Summary statistics for these eight variables and the main explanatory variables of this chapter can be found in table 1.1.

The data set offers several advantages: First, in contrast to political units in cross-country studies, local governments in NRW are very homogenous and operate under a unified framework. Second, it is a very large panel data set. The estimates are based on more than 5500 observations, and there are enough data points in each interval near the 50% threshold to reliably estimate the propensities of coalition governments. See table 1.2 for an overview. Third, the number of cases in which the strongest party received an absolute majority

TABLE 1.2: Propensity of Coalition Governments

Interval of the vote share of the strongest party	Number of elections	Propensity of Coalition Governments
$(0.51; 0.52]$	56	0
$(0.50; 0.51]$	60	0
$(0.49; 0.50]$	80	0.0625
$(0.48; 0.49]$	77	0.3636
$(0.47; 0.48]$	61	0.5902
$(0.46; 0.47]$	60	0.9333
$(0.45; 0.46]$	61	0.9836
$(0.44; 0.45]$	63	0.9841
$(0.43; 0.44]$	56	1
$(0.42; 0.43]$	45	1

Notes: The table shows the number of elections that resulted in a given interval of the vote share of the strongest party plus the propensity that the strongest party did not receive the absolute majority of the seats for each interval of the vote share.

of seats is almost equal to the number of cases in which no party received an absolute majority, which is rather untypical for proportional election systems.

One disadvantage of the data set is that the election data contains no information about the coalitions that have been built in a specific municipality (or whether a coalition was built at all). I would like to point out that almost all related studies using local government data face the same problem.[6] Therefore, I proxy for coalition governments with a variable indicating whether the strongest party in the local council does not hold an absolute majority of the seats, i.e. whether the seat share of the strongest party is not larger than 50%.

A problem with this proxy could be that with a seat share below 50%, parties might prefer minority governments to coalitions. Still, in minority governments, as well as in coalition governments, it is necessary to bargain between (at least) two parties to decide on policy. It is, therefore, reasonable to believe that in minority governments, the same bargaining inefficiencies exist as in "typical" coalition governments. Consequently, I define a coalition government as any kind of government in which such a bargaining over policy between different

[6]The only exception I am aware of is Ashworth and Heyndels (2005).

parties must take place.[7]

Another potential problem is that parties with an absolute majority could decide to build a coalition. However, for the following three reasons, this problem may be regarded as a non-issue: First, theoretical research (Baron and Diermeier, 2001) finds that, in the absence of a crisis, only minimum winning coalitions (those in which every party in government is needed to secure an absolute majority) should form, which makes coalitions unlikely if one party has an absolute majority. Second, in contrast to the local governments in most other German states, a competitive democracy exists in NRW as opposed to a concordance democracy (Holtkamp, 2008). It is well known that a competitive democracy is associated with a strong party system and minimum winning coalitions as opposed to broad-based, consensus orientated coalitions. Third, for German *state* governments, one can observe which coalitions have been built, and in the last 40 years, there has never been a case in which a party with an absolute majority decided to form a coalition government. Thus, the problem stated above does not seem to be very significant in the present political context, such that I argue that a seat share of over 50% is a reasonable approximation of a single-party government.

Most importantly, even if there were outliers which did not comply with the rule used to proxy for coalition governments (i.e., even if some parties with an absolute majority built coalition governments or parties without an absolute majority built single-party governments), my approach would still identify an intention-to-treat (ITT) effect, i.e. the causal effect of the offer of treatment on the outcome variables. Moreover, the ITT effect is a lower bound of the treatment-on-the-treated (TOT) effect when one accounts for an innocuous assumption. The TOT effect can be calculated as (Angrist and Pischke, 2008)

$$TOT = \frac{ITT}{\text{Compliance rate treatment group} - \text{Compliance rate control group}} \tag{1}$$

Under the assumption that a larger percentage of municipalities in the treatment group (i. e., those municipalities in which the strongest party has a seat share of no more than 50%) are treated (i.e., are governed by a coalition government) than municipalities in the control group (i.e., those municipalities in which

[7] The classification of minority governments as coalition governments is not unusual in the literature and has, for example, been used by Solé-Ollé and Sorribas-Navarro (2008).

the strongest party has a seat share larger than 50%), the TOT effect obviously must be larger (in absolute terms) than the ITT effect derived in this study. Moreover, if not many political units deviate from the rule specified above (i.e., if the percentage treated in the treatment group is close to 1 and the percentage treated is close to 0 in the control group), the ITT effect is very close to the TOT effect.

1.3 Identification strategy

1.3.1 Regression Kink Design

The main interest of this study is the causal effect of the type of government on different fiscal policies, and therefore, the relationship I am interested in is given by

$$y_{it} = \alpha + \beta C_{it} + \epsilon_{it} \tag{2}$$

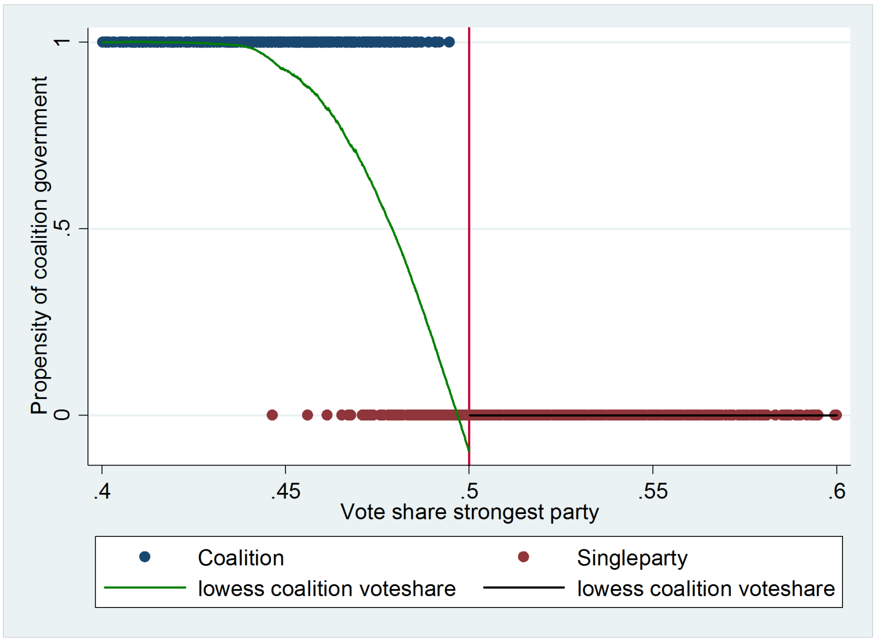
where y_{it} is a fiscal outcome in municipality i in year t and C_{it} is a dummy equal to one if the seat share of the strongest party is smaller than 0.5 and zero otherwise. α is a constant, and ϵ is an error term with the usual properties. However, if I were to estimate this equation by OLS, the estimated parameters would likely be biased due to potential problems of omitted variable bias and reverse causality.

To generate exogenous variation in C_{it}, I use that the treatment probability is a kinked function of the vote share of the strongest party. Figure 1.2 nicely illustrates the empirical strategy. Figure 1.2 shows the propensity of a coalition government as a function of the vote share of the strongest party.[8] The slope change of the function at the 50% vote share threshold of the strongest party is caused by a feature of the election system of NRW: The D'Hondt seat allocation method that was in place during the time period of interest fulfills the absolute majority condition in the case of an odd council size.[9] The absolute majority condition states that a party that gets more than 50% of the votes necessarily

[8]Figure 1.2 also shows that there is no discontinuity in the probability of a coalition government at the threshold.

[9]Note that an odd council size is prescribed by law in NRW.

FIGURE 1.2: Propensity of coalition governments



Notes: Lines in green and in black are Lowess curves over the observations with vote share of the strongest party below and above 0.5, respectively.

receives more than 50% of the seats. This means that it is not possible to get more than 50% of the votes and receive less than 50% seats. On the other hand, it is possible to get less than 50% of the votes but more than 50% of the seats. The latter can happen because parties with less than 5% of the votes are not considered in the seat allocation process or when votes between other parties are split to the best advantage of the strongest party. Thus, the probability of a coalition government is zero when the vote share of the strongest party is above 50%, while the probability of a coalition government is a positive, but a decreasing function of the vote share when the vote share of the strongest party is below 50%.

The effects of the absolute majority condition can also be seen directly by investigating the results from the elections in 1984, 1989, and 1994 (see table 1.3): There were many more cases in which the strongest party held more than 50% of the seats than cases in which the strongest party got more than 50% of the votes. Additionally, table 1.4 shows the election results organized by the frequency with which each of the two largest parties had been the strongest party in the three municipal elections.

TABLE 1.3: Election periods and election results

Election Period	Seat share strongest party > 0.5	Seat share strongest party ≤ 0.5	Vote share strongest party > 0.5	Vote share strongest party ≤ 0.5
1985-1989	240	133	203	170
1990-1994	187	186	136	237
1995-1999	175	198	108	265
Sum	602	517	447	672

Notes: The table shows how often municipal elections resulted in seat and vote shares above or below 0.5. Overall, there have been 1119 elections in the investigation period.

TABLE 1.4: Election results split up by identity of strongest party

	Seat share strongest party > 0.5	Seat share strongest party ≤ 0.5	Vote share strongest party > 0.5	Vote share strongest party ≤ 0.5
Strongest Party: CDU	422	309	320	411
Strongest Party: SPD	177	202	124	53

Notes: The table shows how often municipal elections resulted in seat and vote shares above or below 0.5 for the two different strongest parties. I dropped those 9 observations where a third party was the strongest party. This leaves 1110 election results.

The absolute majority condition thus induces a slope change in the treatment probability when the strongest party achieves the 50% threshold of the vote share. The RKD that I use is based on the claim that political units slightly above the kink point (50% vote share of the strongest party) are similar in all respects to political units slightly below the kink point except for the treatment probability. Suggestive evidence for this claim can be found by inspecting whether the baseline covariates of political units slightly above the kink point do not differ significantly from that of those slightly below it. Here, it is important to use only those covariates that are themselves unaffected by the treatment. I consider the following covariates to be valid: population size, population density, the share of foreigners, the proportion of people aged below 15, the proportion of people aged above 65, and the size of the municipal council.[10] All these baseline covariates are measured before treatment takes place (i.e., at the beginning of the years 1984, 1989 and 1994).

Tables 1.5 and 1.6 illustrate the investigation as to whether the baseline covari-

[10]The size of the municipal council is determined by the relevant population size before the election. It is therefore, by design, a pre-determined covariate.

Table 1.5: Means of baseline covariates for coalition and single-party governments

	Coalition governments Means	Single-party government Means	Difference in means
Population size	28201.21	24670.49	3530.72***
Proportion of people below 15	0.1704	0.1773	-0.0068***
Proportion of people above 65	0.1417	0.1344	0.0074***
Council size	38.79	37.18	1.61***
Population Density	4.23	3.74	0.49***
Share of foreigners	0.0651	0.0581	0.0069***

Notes: *Significant at the 10 percent level, **Significant at the 5 percent level, ***Significant at the 1 percent level.

Table 1.6: Means for baseline covariates when the vote share of the strongest party is within 2 percentage points from the 50%-threshold

	$0.48 < v_{it} \leq 0.5$ Means	$0.5 < v_{it} \leq 0.52$ Means	Difference in means
Population size	29041.29	28358.09	683.68
Proportion of people below 15	0.1712	0.1711	0.0001
Proportion of people above 65	0.1383	0.1398	-0.0015
Council size	39.05	38.66	0.39
Population Density	4.38	4.43	-0.05
Share of foreigners	0.0624	0.0614	0.0010

Notes: *Significant at the 10 percent level, **Significant at the 5 percent level, ***Significant at the 1 percent level.

ates are indeed balanced *close to the kink point*: In table 1.5, I provide the means for the baseline covariates for coalition governments (Column 1), single-party governments (Column 2), and the difference between these means (Column 3). As expected, for all of the variables a *t*-test rejects the null hypothesis of equal means. However, when the range of observations is restricted to a vote share within 2 percentage points of the 50% threshold (as is shown in table 1.6), the *t*-test can no longer reject the null hypothesis of equal means for the pre-determined variables. This indicates that the municipalities whose strongest party held a vote share slightly above or below the threshold are indeed similar except for the treatment probability.

1.3.2 Implementation

In this section, I will formalize my RKD approach. In the empirical strategy, I make the identifying assumption that the direct effect of the vote share of the strongest party on the outcome variables differs from the kinked relationship between treatment probability and vote share described above, i.e. I assume that there is no slope change in the direct effect of the vote share on the outcomes variables at the 50% threshold. Under this condition, I can use the functional form between treatment probability and vote share as an instrument for the dummy variable C_{it}. The first and second stage in the two-stage least squares estimation procedure are given by the following two equations:

$$\begin{aligned} C_{it} &= \alpha_0 + \sum_{p=1}^{\overline{p}} \alpha_p (v_{it} - 0.5)^p + \sum_{p=1}^{\overline{p}} \beta_p (v_{it} - 0.5)^p D_{it} + \lambda_t + \epsilon_{it} \\ y_{it} &= \gamma_0 + \gamma_1 \hat{C}_{it} + \sum_{p=1}^{\overline{p}} \delta_p (v_{it} - 0.5)^p + \lambda_t + u_{it} \end{aligned} \tag{3}$$

where the vote share of the strongest party, v_{it}, is the forcing variable in the RKD, λ_t is a year-fixed effect, and D_{it} is a dummy variable that takes the value of 1 if $v_{it} \leq 0.5$ and 0 otherwise.[11]

This global polynomial approach is my preferred specification as it uses all available data points and is therefore most efficient under the assumption that the control function is not misspecified. To rule out the possibility that

[11]Note that centering the assignment variable v_{it} on the threshold 0.5 is standard in the literature (Dahlberg et al., 2008) and ensures that the effect is measured at the threshold. Further, note that I need not include municipality-fixed effects for two reasons: First, the within-variation of C_{it} is not very large. Second, in RKDs, as well as in Regression Discontinuity Designs, the source of identification is a comparison between those units slightly above and those slightly below the threshold (Lee and Lemieux, 2011). Adding municipality-fixed effects would introduce more restrictions than necessary without any gain in identification (Tyrefors Hinnerich and Pettersson-Lidbom, 2014).

the results are a product of a misspecification of the control function, I use polynomials ranging from the third to the fifth order as a way to determine whether the estimated coalition effect is sensitive to the choice of the control function. In addition, I also present the results from local linear regressions in which the sample is restricted to observations close to the threshold. The corresponding estimating equations read

$$\begin{aligned} C_{it} &= \alpha_0 + \alpha_1(v_{it} - 0.5) + \beta_1(v_{it} - 0.5)D_{it} + \lambda_t + \epsilon_{it},\ -h < v_{it} - 0.5 < h \\ y_{it} &= \gamma_0 + \gamma_1 \hat{C}_{it} + \delta_1(v_{it} - 0.5) + \lambda_t + u_{it},\ -h < v_{it} - 0.5 < h \end{aligned} \tag{4}$$

I choose varying bandwidths $h \in (0.3, 0.4, 0.5)$ of the vote share of the strongest party.[12] In all specifications, I include year-fixed effects and cluster the standard errors at the municipality level to allow for arbitrary serial correlation of the standard errors (Bertrand et al., 2004).

In the context of the IV estimation approach, I can formulate the conditions that need to be fulfilled to estimate the causal effect of coalition governments on fiscal policy: First, there needs to be a kink in the treatment probability, i.e. the instruments $\sum_{p=1}^{\bar{p}} \beta_p(v_{it} - 0.5)^p D_{it}$ need to be relevant, which can be evaluated by the Kleibergen–Paap F-statistic. Second, the exclusion restriction needs to hold, i.e. once I control for the direct effect of the vote share on the outcome variables, I can leave out the term $\sum_{p=1}^{\bar{p}} \beta_p(v_{it} - 0.5)^p D_{it}$ in the second stage (i.e. there is no kink in the direct relationship between the vote share and the outcome variables). With one instrument, as in the local linear regression approach, this assumption is untestable. However, because I use higher-order polynomials in the global polynomial approach, I can test the exclusion restriction with the Hansen–Sargan overidentification test, i.e. the J-Test (under the assumption that one instrument is valid).[13]

Card et al. (2012) derive necessary and sufficient conditions for the RKD to

[12]While, for example, Imbens and Kalyanaraman (2012) develop data-driven approaches for the selection of the optimal bandwidth in the Regression Discontinuity Design, for the RKD such approaches do not yet exist. Note, also, that it does not make sense to use a bandwidth smaller than 3% of the vote share, because with smaller bandwidths the number of coalition governments in the sample becomes too small, as can be inferred from table 1.2.

[13]The results from the J-Test, however, have the disadvantage that they are not based on multiple instruments coming from different dynamics. It is not clear to what extent this affects the results of the J-Test. Thus, one should interpret them with caution.

identify a treatment-on-the-treated parameter, i.e. to guarantee that the exclusion restriction holds. First, it is necessary that there is no *precise* manipulative sorting at the 50% threshold: It should not be possible for political units to sort themselves *precisely* over or under the threshold. In other words, there should be some random component in the vote share of the strongest party. Second, although the vote share of the strongest party may have a direct effect on the outcome variables (which is very likely to be the case), there should be no kink in this relationship at the threshold. This implies that there should be no kink at the threshold for any pre-determined covariates. This can be tested by checking whether the estimates from specification (3) are sensitive to the inclusion of the baseline covariates. Another way to check for the presence of a kink in the baseline covariates is to use each of the covariates as dependent variables in RKD specifications across a range of bandwidths and polynomial orders in order to see if coalition governments *systematically* affect the baseline covariates. I present these validity tests in section 1.4.2.

The derivations of Card et al. (2012) are, however, only suited to cases that feature a continuous treatment variable. In the present case, however, C_{it} is a binary treatment variable. Fortunately, Dong (2012) has shown that the RKD can even be applied with a binary treatment indicator where the identification comes from a kink in the treatment probability.[14]

1.4 Results

1.4.1 Results from the regression kink design

In this section, I present empirical evidence on the effect of coalition governments on fiscal policy. Table 1.7 shows the results of the empirical model (3). Each cell in table 1.7 represents one estimate/standard error pair plus the p-value of the J-Test (where available). Column 1 of table 1.7 presents the results of the OLS estimation procedure. The OLS results either show no effect or confirm the theoretical hypothesis that spending is significantly greater under

[14] In other words, the RKD estimand, as detailed by Card et al. (2012), depends on the derivative of the continuous treatment variable, while the estimand I use depends on the derivative of the expected value of the binary treatment variable, the treatment probability (Dong, 2012).

TABLE 1.7: Results from the parametric estimation approach

	OLS	RKD	RKD	RKD
Log (Total spending)	-0.005 (0.013) -	-0.053 (0.026)** *0.407*	-0.032 (0.031) *0.582*	-0.034 (0.030) *0.514*
Log (Spending on Personnel)	0.046 (0.022)** -	-0.075 (0.040)* *0.563*	-0.042 (0.047) *0.876*	-0.05 (0.047) *0.862*
Log (Spending on Public Administration)	0.020 (0.015) -	-0.067 (0.029)** *0.517*	-0.073 (0.036)** *0.484*	-0.082 (0.035)** *0.482*
Log (Material spending)	-0.001 (0.015) -	-0.023 (0.028) *0.773*	-0.031 (0.033) *0.146*	-0.034 (0.033) *0.293*
Log (Tax revenues)	0.092 (0.022)*** -	-0.064 (0.042) *0.304*	-0.094 (0.050)* *0.285*	-0.093 (0.050)* *0.170*
Log (Multiplier Property Tax A)	-0.003 (0.004) -	-0.035 (0.030) *0.183*	-0.024 (0.039) *0.185*	-0.019 (0.039) *0.729*
Log (Multiplier Property Tax B)	0.021 (0.009)** -	-0.015 (0.018) *0.589*	-0.005 (0.022) *0.579*	-0.004 (0.021) *0.679*
Log (Multiplier Business Tax)	0.011 (0.006)* -	-0.017 (0.011) *0.712*	-0.012 (0.013) *0.680*	-0.011 (0.013) *0.768*
Sample	Full	Full	Full	Full
Degree of polynomial	None	Third	Fourth	Fifth
Kleibergen-Paap F statistic	-	1240.29	745.51	603.19
Observations	5550	5550	5550	5550
Number of clusters	372	372	372	372

Notes: Each cell presents an estimate/standard error-pair from a separate regression plus the p-value of the J-Test (in italics). All regressions include year-fixed effects. Standard errors clustered at the municipality-level are reported in parentheses. *Significant at the 10 percent level, **Significant at the 5 percent level, ***Significant at the 1 percent level coalition governments. The most obvious result is a large and highly significant effect on tax revenues: The OLS estimates suggest that tax revenues are 9.2% higher under coalition governments. I also find significantly positive effects for spending on municipal personnel as well as for the multipliers of two of the municipal tax rates.

Columns 2–4 show the two-stage least squares results. Moreover, these columns report the first stage Kleibergen–Paap F-statistic for the excluded instruments. The instruments are highly relevant in all cases such that problems of weak instruments should not be a concern. The J-Test is never significant at the 10% level or higher which indicates that the instruments are valid.

Looking at the RKD results suggests that the simple OLS estimates are likely to be extremely biased. For each outcome variable that I use, I find that coalition governments only have negative effects. The significance of these effects depends on the outcome variable. For both total and personnel expenditures, I find significant effects for one specification, suggesting that there may be some significant negative relationship between these variables and the presence of a coalition government.

My results (including those from the local linear regressions) strongly suggest that coalition governments mainly affect two outcome variables: First, expenditures on the public administration significantly decrease no matter which polynomial order I use. The estimated effects are economically relevant: Even the most conservative estimate suggests that a coalition government decreases spending on public administration by about 6%. The robustness of the estimates for different control functions suggests that the control function is not misspecified. General conclusions do not change no matter which polynomial order I use. Second, tax revenues are significantly lower under coalition governments than under single-party governments. Only one coefficient barely fails to be significant at the 10% level, but all other polynomial orders show a significantly negative effect. Again, the magnitude of these estimates points to economically relevant effects. For all other outcome variables, I find negative effects that are not significant in most specifications. The estimated effects are, in general, very stable across the different polynomial orders.

The results of the local linear regressions (presented in table 1.8) are mostly in line with the results of the global polynomial approach. First, except for

TABLE 1.8: Results from the local linear regressions

	RKD	RKD	RKD
Log (Total spending)	-0.053 (0.067)	-0.102 (0.045)**	-0.123 (0.042)***
Log (Spending on Personnel)	-0.074 (0.095)	-0.136 (0.064)***	-0.155 (0.060)***
Log (Spending on Public Administration)	-0.102 (0.078)	-0.108 (0.050)**	-0.122 (0.048)***
Log (Material spending)	0.027 (0.067)	-0.064 (0.042)	-0.105 (0.039)***
Log (Tax revenues)	-0.074 (0.105)	-0.124 (0.069)*	-0.131 (0.062)**
Log (Multiplier Property Tax A)	0.067 (0.087)	-0.060 (0.053)	-0.100 (0.046)**
Log (Multiplier Property Tax B)	-0.027 (0.042)	-0.034 (0.027)	-0.034 (0.026)
Log (Multiplier Business Tax)	-0.018 (0.026)	-0.026 (0.016)	-0.025 (0.016)
Bandwidth	±0.03	±0.04	±0.05
Kleibergen-Paap F statistic	85.6	376.64	615.5
Observations	1950	2490	2970
Number of clusters	230	265	287

Notes: Each cell presents an estimate/standard error-pair from a separate local linear regression. All regressions include year-fixed effects. Standard errors clustered at the municipality-level are reported in parentheses. *Significant at the 10 percent level, **Significant at the 5 percent level, ***Significant at the 1 percent level.

two cases, all the estimated coalition effects are negative. Second, coalition governments significantly decrease tax revenues and spending on public administration. These effects are highly significant except in the case where the bandwidth was ±0.03. This is because the standard errors strongly increase as the number of observations decreases, which happens when the bandwidth shrinks from ±0.04 to ±0.03. The point estimates, however, are still economically relevant and even larger (in absolute terms) than in the global polynomial approach. As a final point, the estimates of the other expenditure categories are higher (in absolute terms) in the local linear regressions than they are in the global polynomial approach and more often significant as well.

How can one explain the causal negative effect that coalitions have on tax revenues and public administration expenditures? Most of the theoretical literature, as well as most existing empirical studies, find that the degree of government fragmentation is *positively* associated with government spending. The first empirical study, however, to find negative effects on spending caused by fragmentation was that of Pettersson-Lidbom (2012). He suggests that the negative effects of fragmentation, measured in this case by council size, are due to agency problems between the public administration and the municipal council. Because more legislators are better able to monitor the public administration, excessive spending driven by a budget-maximizing bureaucracy decreases in the presence of a larger council. In the second chapter of this thesis, I also provide evidence that there exists an agency problem between the public administration and the municipal council. I find that fragmentation negatively affects spending when the public administration is relatively independent from the municipal council, while there is no significant relationship when the council is responsible for the administration's term length in office. It could be the case that the same underlying mechanism also holds in the case of coalition governments: When there are two or more parties in government, bureaucrats should be easier to monitor, which would lead to a decrease in expenditures. This mechanism might also hold for tax revenues: Although bureaucrats are not able to set these tax rates themselves, it could be possible for them to influence politicians with their recommendations. Bureaucrats might recommend higher tax rates because a larger public budget is associated with greater prestige for the bureaucracy (Niskanen, 1971). With more parties in government, however, it is possible that these recommendations would face greater opposition from politicians.

However, there are, of course, other explanations one can think of for the coalitions' negative impact on spending. For example, coalition governments change the way politicians are accountable to voters. With a single party in government, policy outcomes can quite accurately be attributed to that party. With more than one party in government, voters do not exactly know whom to hold accountable for the realized policies: This might result in less voter-friendly spending. As a second potential mechanism, coalition governments might find it more difficult to agree in controversial policy areas, suggesting that coalitions would tend not to approve spending projects in cases where there was not a perfect agreement. As a final example, coalitions might exhibit a different political culture than single-party governments. Decisions might be based on the expertise of more politicians, which might increase the efficiency of governance. Moreover, politicians in different parties tend to monitor each other more closely than politicians belonging to the same party, which might decrease political rents. This idea is in line with the literature that argues that political competition tends to reduce political rents (Svaleryd and Vlachos, 2009; Solé-Ollé and Villadecans-Marsal, 2012). More theoretical work is clearly necessary to explain the negative effects of coalition governments on public spending and to discriminate between possible mechanisms.

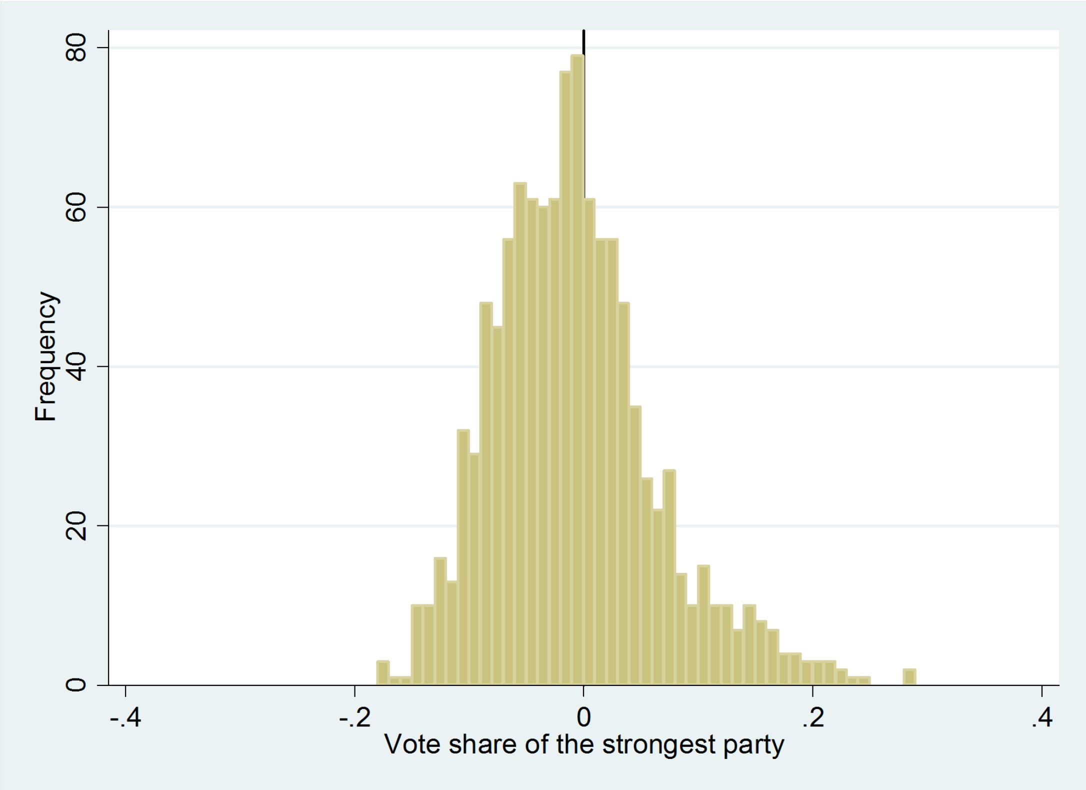
1.4.2 Robustness checks

I have checked the robustness of my results by testing the identifying assumptions of the RKD. As discussed in section 3, the RKD is not valid if political units are able to precisely manipulate the assignment variable in order to sort themselves above or below the 50% threshold. The assumption of absence of precise sorting can be evaluated by inspecting the density of the assignment variable. In the absence of precise sorting, the derivative of the density of the assignment variable should be smooth around the threshold. In figure 1.3, I provide histograms of the assignment variable with three different bandwidths. In none of these histograms, a "hole" in the distribution of the assignment variable is apparent. Moreover, it is reassuring to see that the number of political units slightly below the 50% threshold of the vote share of the strongest party is almost the same as the number slightly above the threshold if ones uses very small bandwidths of the vote share around the threshold. This suggests that political units did not strategically self-select above or below the threshold.

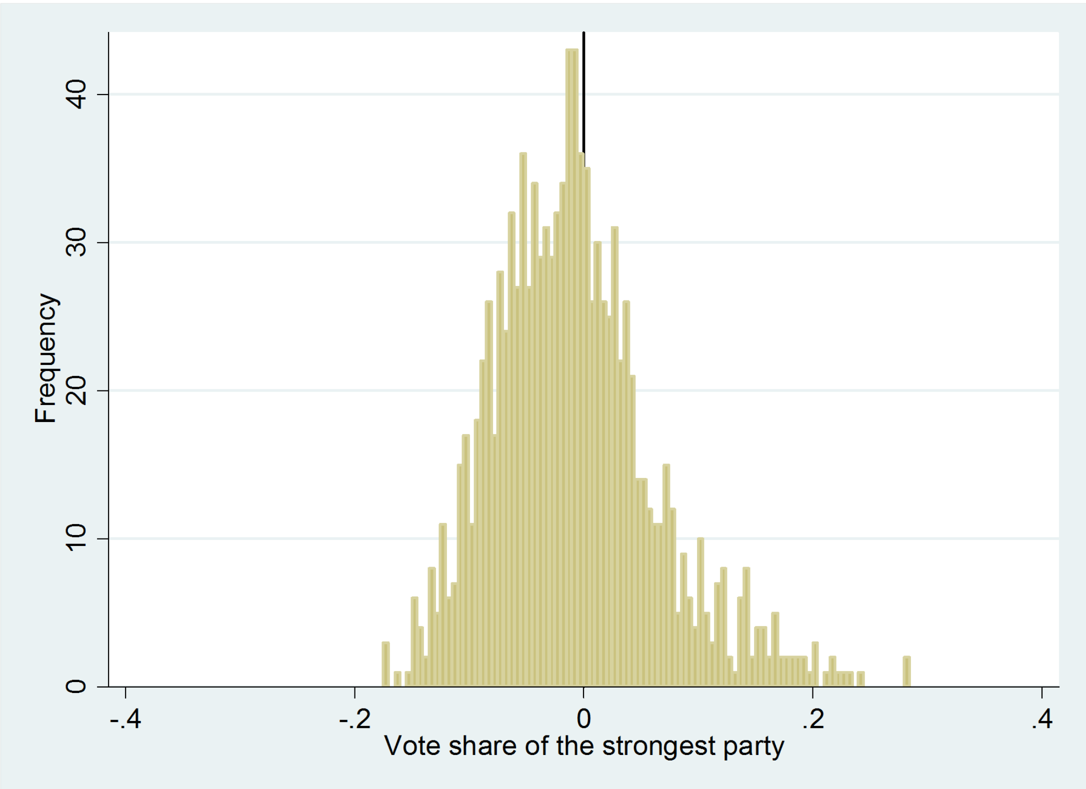
More formally, one can test the null hypothesis that there is no discontinuity at the threshold with a McCrary test (see McCrary, 2008). Figure 1.4 reports the results of the McCrary test. The null hypothesis that there is no discontinuity at the threshold cannot be rejected. Overall, there is strong evidence against manipulative sorting. Intuitively, this could have been expected because election manipulation should not be a concern in Germany. Moreover, in a recent contribution, Eggers et al. (2014) study more than 40000 closely contested elections from different countries (including Germany) and do not find any evidence of imbalances around the threshold.

The second identifying assumption of the RKD is that there is no kink in the

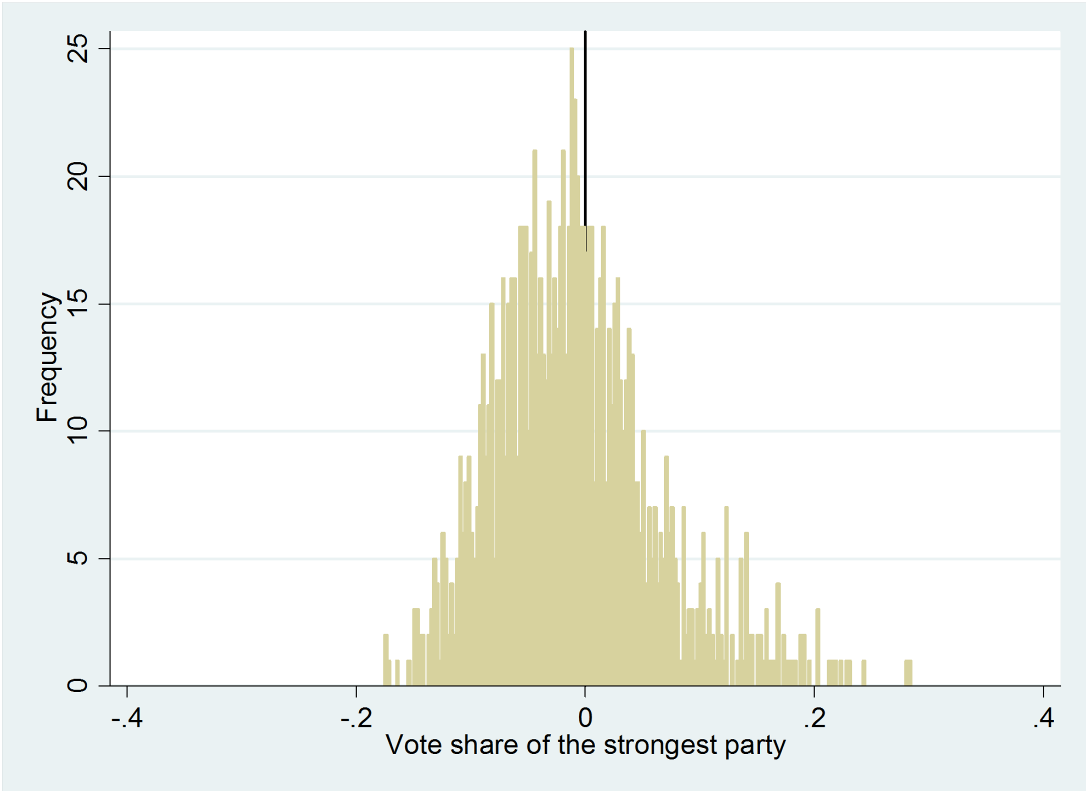
FIGURE 1.3: Histogram of the assignment variable (centered at 0)



(A) Bandwidth: 0.01



(B) Bandwidth: 0.005



(c) Bandwidth: 0.0025

direct relationship between the outcome variables and the vote share at the threshold, i.e. the marginal effect of the vote share of the strongest party on the outcomes is smooth. As explained in section 1.3, this implies that there should be no kink at the threshold for any pre-determined covariates. The fulfillment of this identifying assumption can therefore be tested by the inclusion of pre-determined covariates into the specification (3). If the baseline estimates are not sensitive to the inclusion of these covariates, then this should be strong evidence for the exclusion restriction to hold. Table 1.9 presents the results after inclusion of the proposed covariates: population size, population density, the share of foreigners, the proportion of people aged below 15 and above 65 and the logarithm of the size of the municipal council. The results show that almost all estimates are strikingly similar to the ones before the inclusion of the covariates. Also, none of the conclusions concerning statistical significance changes by including these covariates.

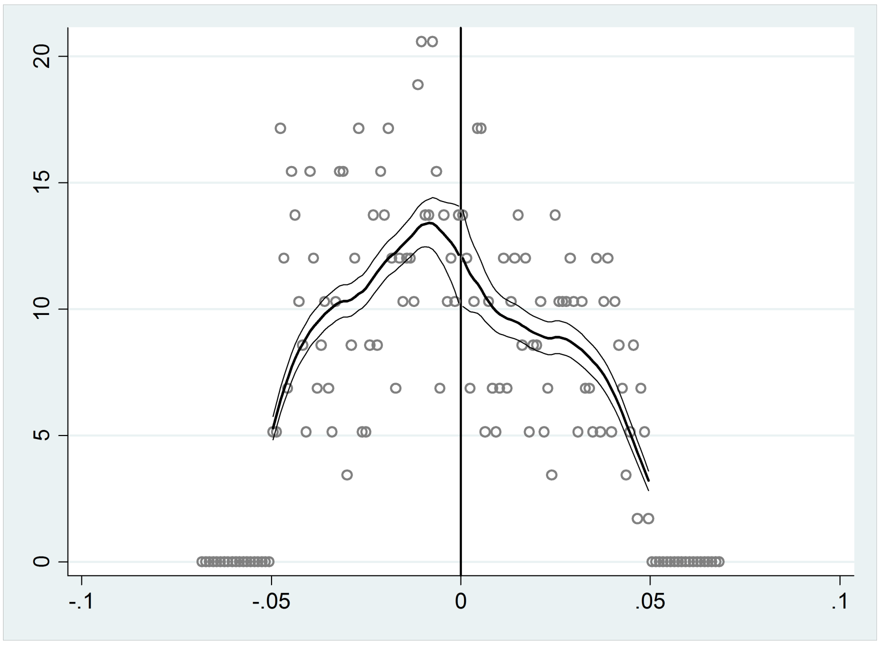
As explained above, another strategy to test whether there is a kink in the baseline covariates is to employ these covariates as dependent variables in specifications across a range of bandwidths and polynomial orders. Table 1.10 presents the corresponding results for the global polynomial approach

Table 1.9: Results from the parametric estimation approach when controlling for pre-determined covariates

	OLS	RKD	RKD	RKD
Log (Total spending)	-0.026 (0.011)** -	-0.052 (0.022)** *0.325*	-0.033 (0.026) *0.345*	-0.035 (0.025) *0.472*
Log (Spending on Personnel)	-0.003 (0.014) -	-0.064 (0.028)** *0.621*	-0.039 (0.034) *0.333*	-0.046 (0.033) *0.262*
Log (Spending on Public Administration)	0.006 (0.014) -	-0.051 (0.027)* *0.296*	-0.074 (0.034)** *0.425*	-0.082 (0.034)** *0.729*
Log (Material spending)	-0.016 (0.014) -	-0.026 (0.026) *0.784*	-0.033 (0.032) *0.068**	-0.036 (0.031) *0.151*
Log (Tax revenues)	0.047 (0.018)*** -	-0.051 (0.038) *0.525*	-0.072 (0.042)* *0.249*	-0.072 (0.043)* *0.277*
Log (Multiplier Property Tax A)	-0.015 (0.015) -	-0.029 (0.030) *0.224*	-0.020 (0.038) *0.173*	-0.016 (0.038) *0.626*
Log (Multiplier Property Tax B)	0.005 (0.007) -	-0.011 (0.014) *0.568*	-0.004 (0.017) *0.638*	-0.002 (0.017) *0.972*
Log (Multiplier Business Tax)	0.000 (0.005) -	-0.012 (0.009) *698*	-0.010 (0.011) *0.692*	-0.009 (0.011) *0.995*
Sample	Full	Full	Full	Full
Degree of polynomial	None	Third	Fourth	Fifth
Kleibergen-Paap F statistic	-	1166.39	695.21	571.20
Observations	5550	5550	5550	5550
Number of clusters	372	372	372	372

Notes: Each cell presents an estimate/standard error-pair from a separate regression plus the p-value of the J-Test (in italics). All regressions include year-fixed effects and population size, population density, the proportion of people aged below 15, the proportion of people aged above 65, the share of foreigners and the logarithm of the size of the municipal council as control variables. Standard errors clustered at the municipality-level are reported in parentheses. *Significant at the 10 percent level, **Significant at the 5 percent level, ***Significant at the 1 percent level.

FIGURE 1.4: McCrary-test of the vote share of the strongest party (centered at 0)



Notes: Weighted kernel estimation of the log density of the assignment variable, performed seperately on either side of the threshold (centered at 0). Point estimate of the discontinuity: -0.019, se: 0.121. Optimal bandwidth is calculated as in McCrary (2008).

and table 1.11 those for the local linear regressions. Only in the case of the proportion of people aged below 15, I find two significant treatment effects. One of these effects is marginally significant at the 10%-level, while the other one is significant at the 5%-level. However, for (at least) two reasons I do not believe that this invalidates my approach. First, the estimated effect is not systematic. The effect varies from being negative to being positive across different bandwidths in the local linear regression approach and is therefore far from being insensitive to the specifications used. Second, in total I estimate 36 coefficients of which one is significant at the 10%-level and one is significant at the 5%-level. These are even less significant coefficients than one would expect from a totally random distribution. Thus, I can conclude that there seems to be no clear evidence for a kink in the direct effect of the vote share on the outcome

TABLE 1.10: Balance test for baseline covariates: Global polynomial approach

	RKD	RKD	RKD
Population size	1965.74 (3602.76)	1679.9 (4400.34)	1482.29 (4343.11)
Population density	-0.485 (0.654)	-0.459 (0.796)	-0.548 (0.769)
Share of foreigners	-0.003 (0.005)	-0.002 (0.006)	-0.002 (0.005)
Log (Council size)	0.010 (0.033)	-0.023 (0.041)	-0.023 (0.040)
Proportion of young, 0-15	0.005 (0.003)*	0.003 (0.004)	0.003 (0.004)
Proportion of old, 65+	-0.002 (0.004)	-0.002 (0.004)	-0.002 (0.004)
Sample	Full	Full	Full
Degree of polynomial	Third	Fourth	Fifth
Kleibergen-Paap F statistic	1240.29	745.51	603.19
Observations	5550	5550	5550
Number of clusters	372	372	372

Notes: Each cell presents an estimate/standard error-pair from a separate regression. All regressions include year-fixed effects. Standard errors clustered at the municipality-level are reported in parentheses. *Significant at the 10 percent level, **Significant at the 5 percent level, ***Significant at the 1 percent level.

variables which suggests that the exclusion restriction is likely to be valid.[15]

1.4.3 Does the coalition effect depend on the ideology of the municipal government?

Another potential problem could be that the pure coalition effect that I am estimating might be confounded by a specific coalition effect. To be precise, a closer inspection of table 4 reveals that the center-right CDU is far more often capable of building a single-party government than the center-left SPD.

[15] Another robustness check would be to perform a falsification test where one estimates the coalition effect at "fake" kink points. However, as explained by Simonsen et al. (2010), this is not possible in the Regression Kink Design. The reason is that the functional form of the curve is unknown and potentially differs between real and "fake" kink points. Thus, one cannot use the functional form from the original kink point at any "fake" kink points.

TABLE 1.11: Balance test for baseline covariates: Local linear regressions

	RKD	RKD	RKD
Population size	-4064.98 (9324.01)	-4507.82 (5839.38)	-2532.33 (5527.75)
Population density	-0.706 (1.450)	-1.338 (1.051)	-0.431 (0.944)
Share of foreigners	-0.002 (0.013)	-0.006 (0.010)	-0.006 (0.008)
Log (Council size)	-0.103 (0.090)	-0.069 (0.054)	-0.030 (0.047)
Proportion of young, 0-15	-0.001 (0.006)	0.007 (0.005)	0.010 (0.004)**
Proportion of old, 65+	0.011 (0.010)	-0.002 (0.006)	-0.007 (0.005)
Bandwidth	±0.03	±0.04	±0.05
Kleibergen-Paap F statistic	85.60	376.64	615.50
Observations	1950	2490	2970
Number of clusters	230	265	287

Notes: Each cell presents an estimate/standard error-pair from a separate local linear regression. All regressions include year-fixed effects. Standard errors clustered at the municipality-level are reported in parentheses. *Significant at the 10 percent level, **Significant at the 5 percent level, ***Significant at the 1 percent level.

This could imply that the switch from a coalition to a single-party government is highly correlated with a rightward shift in the ideology of the municipal government.[16] Thus, the coalition dummy might express some effect that is caused by the ideology of the government rather than by the form of the government. Therefore, it makes sense to include the ideology of the municipal government into the regression in order to account for its influence on the results.

I follow Pettersson-Lidbom (2008) and assume a two-bloc system for the ideological orientation of the municipal governments: I assume a left-wing government to be in place if either the absolute majority of seats are held by the center-left

[16]The result that a coalition government generates significantly less tax revenues is evidence against the possibility of a shift in the ideology of the government to the right when there is a switch from a coalition to a single-party government. Intuitively, one would suppose that left-wing governments have causally higher revenues as has been found in Pettersson-Lidbom (2008).

TABLE 1.12: Classification of government identity

Number of elections resulting in			
Left-wing government	323	SPD SPD+Greens	177 146
Right-wing government	596	CDU CDU+FDP	422 174
Undefined majority	191		

Notes: The table shows how often the municipal governments in the period of investigation were classified to be left-wing or right-wing.

SPD or by a combination of the Greens and the SPD, while, conversely, I define a right-wing government as one in which the absolute majority of seats are held by the center-right CDU or by the CDU and the FDP together. In all other cases, I consider the ideology of the municipal government to be undefined. Table 1.12 gives further information on how often each kind of government occurred. Table 1.13 shows the results I obtain when I include a dummy variable for a left-wing government into the global polynomial regression and exclude all cases in which the ideology is undefined.[17]

The inclusion of the ideology of the municipal government only slightly changes the estimates. Most importantly, conclusions regarding the statistical significance of the effect of coalition governments on total expenditures and spending on public administration do not change. For tax revenues, coefficients decrease marginally in size such that the estimates barely fail to be significant. However, the estimates are still of an economically relevant magnitude. The small drop in size could be because now there are fewer observations as governments with undefined ideologies are excluded. Therefore, I conclude that the results are not driven by the ideology of the municipal governments, but are rather caused by their form, that is, whether they are a single-party government or a coalition.[18]

[17] Note that one, of course, cannot interpret the results of this robustness check as causal effects. The reason is that the ideology of the government is not a pre-determined variable.

[18] This does not, of course, mean that ideology does not play a role in government policies at the municipality level, but that the effect of the form of government on my outcome variables is not mediated by ideology.

Table 1.13: Results from the empirical model when controlling for government ideology

	OLS	RKD	RKD	RKD
Log (Total spending)	-0.035 (0.012)*** -	-0.058 (0.025)** *0.291*	-0.04 (0.030) *0.326*	-0.041 (0.029) *0.485*
Log (Spending on Personnel)	-0.017 (0.015) -	-0.051 (0.030)* *0.665*	-0.029 (0.038) *0.361*	-0.045 (0.037) *0.224*
Log (Spending on Public Administration)	-0.008 (0.014) -	-0.064 (0.031)** *0.808*	-0.078 (0.038)** *0.398*	-0.084 (0.038)** *0.612*
Log (Material spending)	-0.015 (0.015) -	-0.009 (0.028) *0.777*	-0.024 (0.034) *0.149*	-0.035 (0.033) *0.393*
Log (Tax revenues)	0.038 (0.020)* -	-0.059 (0.042) *0.469*	-0.068 (0.047) *0.181*	-0.066 (0.045) *0.235*
Log (Multiplier Property Tax A)	-0.025 (0.017) -	-0.014 (0.036) *0.181*	-0.017 (0.042) *0.240*	0.01 (0.044) *0.868*
Log (Multiplier Property Tax B)	-0.003 (0.008) -	-0.005 (0.017) *0.601*	0.000 (0.019) *0.631*	0.008 (0.021) *0.974*
Log (Multiplier Business Tax)	-0.007 (0.006) -	-0.010 (0.120) *0.380*	-0.008 (0.012) *0.560*	0.001 (0.013) *0.963*
Degree of polynomial	None	Third	Fourth	Fifth
Kleibergen-Paap F statistic	-	1080.43	479.30	226.75
Observations	4595	4595	4595	4595
Number of clusters	356	356	356	356

Notes: Each cell presents an estimate/standard error-pair from a separate regression plus the p-value of the J-Test (in italics). All regressions include year-fixed effects, population size, population density, the proportion of people aged below 15, the proportion of people aged above 65, the share of foreigners, the logarithm of the size of the municipal council and a dummy for left-wing governments as control variables and exclude all cases in which the government ideology is considered to be undefined. Standard errors clustered at the municipality-level are reported in parentheses. *Significant at the 10 percent level, **Significant at the 5 percent level, ***Significant at the 1 percent level.

1.5 Conclusion

In this study, I estimate the effect that coalition governments have on fiscal policies using municipal level data from NRW, Germany. The claim for causality can be made because the probability of a coalition government forming is a kinked function of the vote share of the strongest party. This allows me to estimate a RKD that assumes that the direct effect of the vote share on the outcomes differs from the effect of the vote share on the treatment probability. The RKD generates credible sources of exogenous variation under very reasonable and testable identifying assumptions, which allows me to mimic a randomized experiment quite closely. I find that once issues of reverse causality and omitted variable bias have been addressed with the RKD, coalition governments spend less than single-party governments, contrary to the theoretical hypothesis and the OLS estimates. The spending results are mainly driven by lower expenditures on public administration. Moreover, I find that coalition governments have lower tax revenues. These results are robust to typical specification tests in a RKD.

The negative effect of government fragmentation on fiscal policies is in line with recent studies that generate causal estimates of the influence of government fragmentation. The recent literature argues that the negative effects are due to agency problems between the public administration and the municipal council that could be better solved with more political decision-makers. The outcome variables that were mainly affected in this study are in line with this view. However, there is clearly a need for more theoretical work to explain these findings and to discriminate between alternative mechanisms that might eliminate or overshadow the common pool problem in the case where fragmentation is represented by the form of the government.

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Chapter 2

Elected or appointed? How the nomination scheme of the city manager influences the effects of government fragmentation

2.1 Introduction

Government fragmentation, i.e. the number of decision-makers in government such as the number of legislators, is often considered an important determinant of government size (see, e.g.,Weingast et al.,1981; Schaltegger and Feld, 2009). Despite massive attention in the empirical literature[1], however, no definitive conclusion has emerged regarding the causal effect of fragmentation on policy. Strikingly, Egger and Köthenbürger (2010) as well as Pettersson-Lidbom (2012) even apply the same identification strategy to different data sets and find completely different results. A main characteristic of the empirical literature is that it has measured the effect of fragmentation in completely different (local) political systems.

This chapter asks whether the underlying heterogeneity in local political systems can explain this diversity of the results. Specifically, using a large panel data

[1]This footnote gives a (non-complete) list of papers. In this literature, there exist different measures of government fragmentation. Besides the number of legislators as used in this chapter, the number of ministers and the number of parties in government are sometimes used (see, e.g., Roubini and Sachs, 1989;Perotti and Kontopoulos, 2002; Schaltegger and Feld, 2009). However, the number of legislators seems to be the most often used measure of government fragmentation (see, e.g., Gilligan and Matsusaka, 1995; Bradbury and Crain, 2001; Baqir, 2002; MacDonald, 2008; Egger and Köthenbürger, 2010; Pettersson-Lidbom, 2012).

set of 426 municipalities in the German state Hesse in the period 1985-2000, I compare the effect of the number of legislators on spending (i.e. the **council size effect**) in two distinct local political systems: In the first setting, the city manager - the head of the public administration in Hesse - is appointed by the municipal council. In the second setting, he is elected by the voters.[2] To establish causality, I combine a Regression Discontinuity Design (RDD) with a Difference-in-differences approach (DiD). The RDD uses that council size in Hesse is a discontinuous function of population size. The DiD approach exploits that the timing of the switch from appointment to election differed across municipalities due to a quasi-random phase-in period of the new system.

I indeed find that the effect of fragmentation differs in these two political systems: When the manager is appointed by the council, there is no relationship between the size of the municipal council and spending. When the manager is elected by the voters, there is a highly significant negative council size effect, i.e. the larger is the size of the council the smaller is the size of government.

The theoretical findings on the relationship between fragmentation and policy are as diverse as the existing empirical results. In a seminal contribution, Weingast et al. (1981) have argued that fragmentation leads to inefficiently high spending because each legislator tries to benefit his constituencies through pork-barrel spending but internalizes only a fraction of the associated costs. Primo and Snyder (2008), however, show that the model of Weingast et al. (1981) can even lead to inefficiently low spending. Recently, Pettersson-Lidbom (2012) has argued that agency problems between the council and a budget-maximizing public administration can be better solved with more legislators, thus creating a negative relationship between fragmentation and spending.

The specific mechanism behind the effects of fragmentation on policy is important to understand the welfare consequences of fragmentation. In pork-barrel type models such as Weingast et al. (1981) and Primo and Snyder (2008), both positive and negative effects of fragmentation lead to inefficiency. By contrast, if agency problems exist, more fragmentation can curb down inefficiently high

[2]An important predecessor of this chapter is Baqir (2002). He compares the effects of government fragmentation in mayor-council and council-manager systems in US cities and finds that mayor-council systems break the relationship between fragmentation and spending. However, in his paper political units might self-select into specific forms of local governments such that it is questionable whether the estimated effects can be given a causal interpretation.

spending.

I argue that my results point to the existence of agency problems between the public administration and the council. When the manager is appointed, he is completely accountable to the council. If the manager cares about staying in office, there will be, if any, a very restricted incentive to counteract the decisions of the council.[3] More legislators are therefore not necessary to monitor him. When the manager is elected by the voters, his tenure in office should be less dependent on the council and his political position should be stronger: He does not need a parliamentary majority behind him to stay in office. This should create (or further strengthen) agency problems between the municipal council and the manager compared to the case of appointment. Therefore, more council members are helpful to better monitor the public administration. This creates a negative relationship between the number of legislators and spending.[4]

To evaluate whether the different council size effects in these two political systems might really be caused by agency problems, I perform the analysis for different sub-categories of the budget as well as for local tax rates. Fittingly, the above conclusions only hold for expenditure categories over which the public administration has discretion. Since agency problems can only exist for those policy fields that the manager is able to influence, this is supportive of the proposed mechanism.

[3]This is similar in spirit to the initial intent of the inventor of the city manager plan in the United States as described by Stone et al. (1940): "By authorizing the council to hire and fire the city manager at its discretion, however, the city manager plan effectively gave the council control over administrative, as well as over legislative, policy."

[4]A question that arises when regarding the relationship between city manager and council as a principal-agent problem is why elections for the city manager should make a difference if voters might be able to discipline the city manager through elections for the city council: After all, the city manager is made more beholden after the introduction of elections to the same people who elect the council. A key assumption for elections to make a difference is the existence of incomplete information of the voters: If voters have perfect information, it should make no difference of whether the city manager is elected or appointed. If in this case the city council appoints an incompetent city manager, voters will simply punish the council in council elections. Thus, councils would not hire managers that voters would not elect. If voters, however, have incomplete information about the behavior of the local political actors, Vlaicu and Whalley (2013) show that it can make a difference of whether the manager is elected or appointed. A second explanation of why the nomination scheme of public officials matters is because separate elections for the council and the administrator might allow unbundling policy issues (see Besley and Coate, 2003).

This chapter's results have important policy implications: Knowledge about which political institutions can avoid inefficiencies through either pork-barrel spending or agency problems is helpful.[5] Moreover, mayor-council systems have been found to imply other council size effects than council-manager systems (Baqir, 2002). Is this caused by the nomination scheme of the public administration? The election of the head of the public administration is highly correlated with the existence of a mayor-council system[6], whereas in council-manager systems, it is more common that the city manager is appointed by the council. Therefore, disentangling the effect of the nomination scheme on the council size effect from the effect of the whole administrative system is in general difficult, but possible in the present setting as the reform only changed the former.

Additionally, the results have important implications for the validity of cross-country studies in the government fragmentation literature: A council size effect that differs depending on the political system illustrates the need to use data from a homogeneous institutional setting rather than conflating data from political units with different political systems as it is done particularly in cross-country studies.

More generally, this chapter contributes to the literature on fiscal policy determination in different forms of local governments (see, e.g, MacDonald, 2008; Coate and Knight, 2011; Vlaicu and Whalley, 2013). Up to now, *causal* empirical evidence on the interplay between the local executive (city manager or mayor) and the city council as the local legislature is scarce if not non-existent. This lack is unsurprising considering the econometric challenge to find (at least) two natural experiments. Moreover, the chapter is related to a growing literature investigating the role of the public administration in the decision-making process in governments (see e.g., Alesina and Tabellini, 2007; 2008; Ting, 2012). Third, the chapter is related to the literature in political economy that estimates

[5]This is particularly true if this relationship also holds for other measures of fragmentation in local governments. Consider for example the case of government fragmentation due to coalition governments. National policy-makers are not able to determine whether a municipality is governed by a single-party or a coalition government as this is the result of the municipal elections. But if there are negative effects from one of these forms of governments, national policy-makers might be able to mitigate these negative effects by simply changing the nomination scheme of the public administration.

[6]For example, Baqir (2002) reports that in his sample of US cities, 98% of the mayors in mayor-council systems are elected by the voters.

causal effects by making use of variants of the RDD (see, e.g., Fujiwara, 2011; Gagliarducci et al., 2011; Tyrefors Hinnerich and Pettersson-Lidbom, 2014). To the best of my knowledge, this paper is the first to use the RDD to measure interaction effects of separate institutions.

The rest of the chapter is structured as follows. In section 2, I describe the institutional framework in Hesse and the data set used. Section 3 presents the empirical strategy. In section 4, I first present evidence for the direct effect of the change in the nomination scheme on the outcome variables. Second, I present evidence that the change in the nomination scheme also has had effects via government fragmentation. Finally, section 5 concludes.

2.2 Institutional framework and data

2.2.1 Institutional framework

In Germany, municipalities are the lowest administrative unit of government, but are, however, of considerable economic importance: They are responsible for roughly one-third of the total German government spending and employ 40% of all state employees. Moreover, municipalities are free to set three different tax rates: a tax on business profits (*Gewerbesteuer*), a tax on agricultural land (Property Tax A) and a tax on land used by business and private households (Property Tax B). Municipalities are, for example, responsible for culture, elementary schools, economic promotion and maintenance of municipal roads.

In this chapter, I use a large panel data set covering the 426 municipalities of the state Hesse over the time period 1985-2000. Hesse is located in the center of Germany and has approximately 6 million inhabitants. Figure 2.1 shows a map of Hessian municipalities. The 1985-2000 period consists of four legislative terms, each lasting four years. Municipal council elections were in 1985, 1989, 1993 and 1997 each time at the beginning of the year.

FIGURE 2.1: Map of Hesse with municipal borders (in white)

Hesse has a closed-list[7], proportional election system. Legislators in the municipal council are elected at large, i.e. the whole municipality forms one electoral district. Importantly, due to the characteristics of the municipal election system

[7]A closed-list system (as compared to an open-list system) is given if voters can only vote on predecided party lists. Party lists determine which candidates get the seats that a party has been awarded in the city council. For example, if a party gets 5 seats in the municipal council, in a closed-list system the first 5 candidates on the party list will get these seats. In an open-list system, voters do not only vote for a party list, but also have some opportunity to influence the order of the candidates on the list (and can thus to a certain extent influence which specific candidate gets elected).

in Hesse, overhang seats are not possible.[8] After 2000, a large reform of the election system became law: For example, an open-list system was introduced and the barrier to entry[9] into the municipal council was abolished. Moreover, since 2001, municipalities can also, to a certain extent, influence the size of the municipal council. Because this reform created enormous differences in institutional settings, only data until 2000 are used.

The municipal constitution (*Gemeindeordnung*) in Hesse is the so-called magistrate constitution (Elsner, 1956; Schneider, 1981; Dreßler, 2010): This local constitution can be described as one with a clear dividing line between the decision and the implementation level in the political process. The municipal council has the ultimate decision power in all affairs that concern the municipality and is responsible for monitoring the public administration. The decisions of the council are implemented by a small magistrate (typically two members plus the manager) that leads the public administration.

The magistrate is presided by the manager and decides by majority. In case of a draw, the vote of the manager is pivotal. The manager has the right to allocate responsibilities to the other members of the magistrate. Thus, he can design the organization of the public administration in the way that is most compatible with his concepts. It is also important to note that the manager works full-time, while the other members of the magistrate typically work on an honorary basis. Thus, they are more time-constrained than the manager as they have to do their normal jobs besides working in the public administration. This is likely to create sizeable information advantages for the manager over the rest of the magistrate.

Importantly, the manager (as well as the other members of the magistrate) has no political function. He is an employee of the municipality and is not allowed

[8] Overhang seats can arise in mixed-member proportional election systems in which some members of the council are not elected by party lists but by (geographic) constituencies. Overhang seats are generated when a party gets less seats based on votes for the party list than it wins constituencies. In this case, the council may be larger than prescribed by law. Importantly, such a phenomenon cannot occur in Hesse because in contrast to many other German states, Hesse has no mixed-member proportional system. Otherwise, causal identification would be harder to achieve (the RDD would be fuzzy).

[9] A barrier to entry implies that only parties that have received more than a certain percentage of votes are considered in the seat allocation process. Thus, only those parties can receive seats in the municipal council. In Hesse, the barrier to entry was set at 5% of the votes.

to be member of the municipal council. In contrast to most other German states, the head of the public administration in Hesse has no voting rights in the municipal council and cannot veto against the decisions of the council. Accordingly, the decisions of the council also do not require approval by the manager or by the magistrate. Thus, the manager and the magistrate are clearly not part of the decision, but of the implementation process.

The position of the head of the public administration in Hesse is therefore equivalent to what the theoretical literature on local government systems (see Coate and Knight, 2011) defines as a city manager: As compared to a mayor in a mayor-council system, a city manager has no veto or voting rights and is not part of the municipal council. De facto, except for the existence of a small magistrate, the Hessian local government system is very close to the typical council-manager system in the United States.

This is unsurprising as local government systems in different German states were at least partly shaped by the identity of the allied power that had occupied that state after World War II. States that were occupied by the UK like for example North Rhine-Westphalia and Lower Saxony had (at least until the local government systems were reformed in the 1990s) systems that were close to the British analogue, while states like Hesse that were part of the US sector tended to adopt local government systems close to the US analogue. My results are therefore also informative for local governments in the US given the high similarity of the local government systems in both countries and because in council-manager systems in the US, it is also possible to have the city manager elected by the voters instead of appointed by the council.

However, while having no legislative power, for tasks that are necessary for the daily administration the magistrate has some discretion over expenditures: The magistrate can freely hire or dismiss municipal employees as long as this is in accordance with the employment plan of the council and does not stand in contrast to the council's general employment policy. This should give the magistrate some discretion over personnel expenditures. Moreover, the magistrate does not require approval of the council for daily administration tasks. These can be defined as tasks of small financial magnitude that are executed repeatedly. This should give the magistrate some discretion over material spending as this expenditure type consists of positions with these characteristics.

Before the reform, the members of the magistrate as well as the manager were appointed by the council with a 2/3 majority. After the reform, the manager was elected by the voters in a runoff election in which a candidate is elected if he receives more than 50% of the votes in the first round. If no candidate succeeds in the first round, only the top two candidates from the first round advance to the second round. The magistrate, however, was still appointed by the council. Thus, only the nomination scheme of the manager and neither the nomination scheme of the magistrate nor the responsibilities of any of the political actors changed after the reform. Moreover, also the manager's salary and pension entitlements (or those of any political actor) were not changed by the reform.

The council has several possibilities to enforce his decisions: Most importantly, the council is able to remove members of the magistrate from office. Before the reform, the council could also remove the manager from office. After the reform, only the voters can vote him out of office. Thus, the reform implied a strong decrease in the dependence of the manager on the council. This leads me to expect an agency problem after the reform that creates the necessity of many legislators monitoring the administration.

2.2.2 Data

For this chapter, I use electoral, financial, and population data from the Statistical Office of Hesse. The data is on a yearly basis. I express all expenditures variables in per capita terms in constant 2005 prices (EUR). Moreover, all expenditure variables are expressed in logarithms, because histograms typically show a right-skewed distribution of these variables (Figure 2.2). Therefore, the effect of the number of legislators on expenditures will have a percentage interpretation. I also use the multipliers of the three local tax rates as outcome variables, but do not transform them into logarithms. However, such a transformation does not affect the results. Summary statistics for the main variables used in this chapter can be found in table 2.1.

2.3 Identification strategy

I am interested in the interaction between the nomination scheme of the manager and the size of the municipal council and the influence of this interaction on

FIGURE 2.2: Distribution of outcome variables

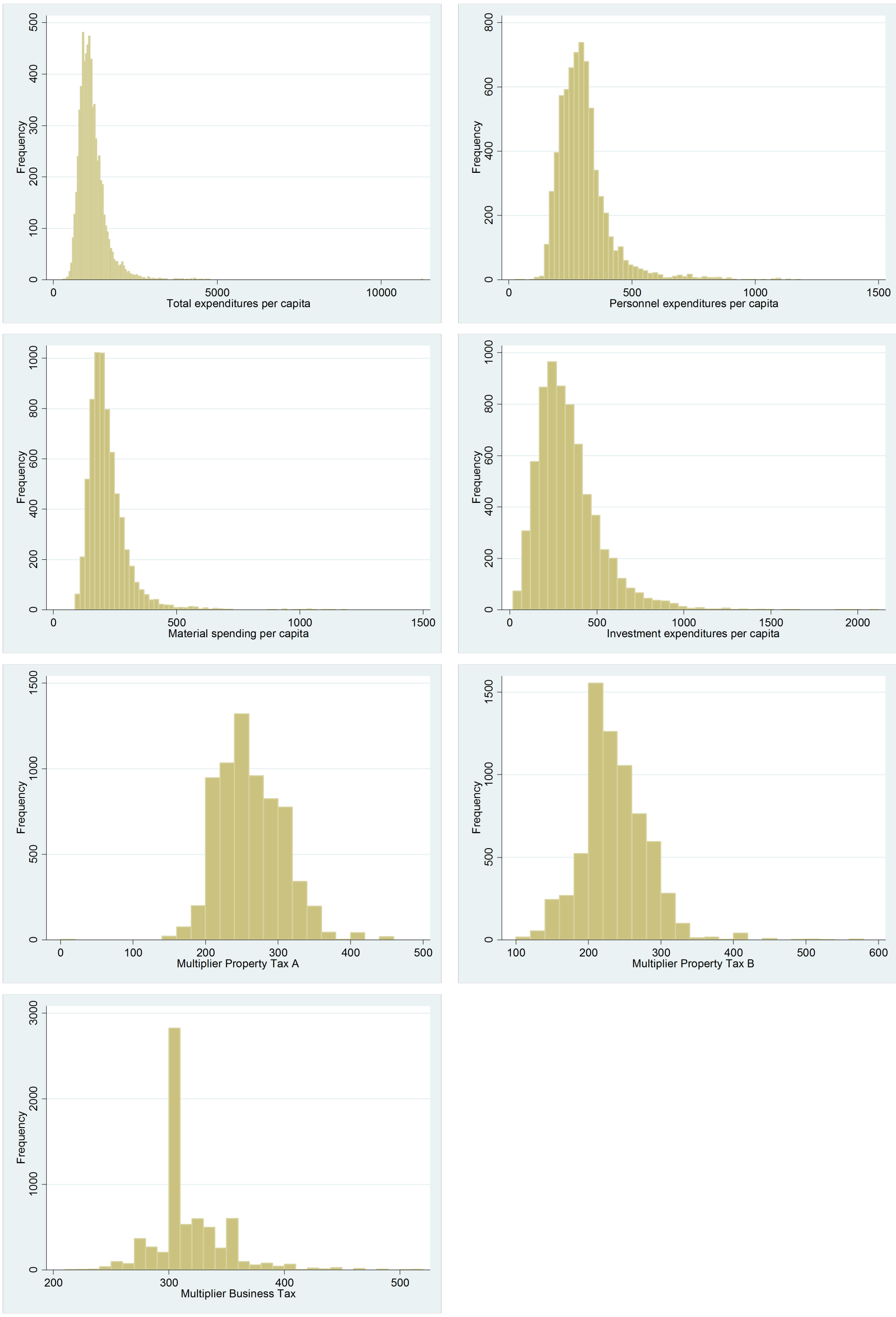


TABLE 2.1: Summary statistics

	Mean	Std. Deviation	Min	Max
Total spending	1131.6	371.1	279.1	11248
Personnel spending	285.4	74.6	24.8	833.1
Material spending	210.2	64.7	88.3	1023.2
Investment spending	341.3	184.3	18.4	2116.3
Property Tax A	257.7	43.1	0	450
Property Tax B	227.6	41	100	380
Business Tax	308.6	26	210	400
Council size	29.6	6.9	13	37
Proportion of old, 65+	0.154	0.024	0.082	0.272
Proportion of young, 0-15	0.163	0.016	0.106	0.227
Seat share CDU	0.32	0.124	0	0.71
Seat share SPD	0.444	0.135	0	0.867
Dummy: Absolute Majority	0.45	0.497	0	1

Notes: Expenditure variables are expressed in constant 2005 prices in per capita terms. The multipliers of the tax rates are expressed in percentage points. The table only contains municipalities with a relevant population size less than 25000. There are 6308 observations that fulfil this requirement.

fiscal policy. Formally, this relationship is given by

$$y_{it} = \alpha + \beta C_{it} + \gamma C_{it} D_{it} + \epsilon_{it} \tag{1}$$

where y_{it} is a fiscal outcome of a municipality i in year t, C_{it} is the size of the municipal council and D_{it} is a dummy variable that is equal to one if the municipality has an elected manager and zero otherwise. For a manager appointed by the council, the council size effect I am interested in is given by β. With an elected manager, the council size effect is given by $\beta + \gamma$.

In this chapter, I am interested in evaluating whether the conclusion that council size matters is a universal one. If council size was a significant determinant of fiscal policies under both political systems that I study, one would suspect both β and $\beta + \gamma$ to be significantly different from zero. Thus, I focus on the comparison of the marginal effects of the number of council members on fiscal policy under the two different political systems. In this way, I am able to observe whether the conclusions drawn in empirical studies can indeed be affected by the political system in place.

Table 2.2: The council size law

Relevant Population Size (pop)	Council size
pop $\leq$ 3000	15
3000 $<$ pop $\leq$ 5000	23
5000 $<$ pop $\leq$ 10000	31
10000 $<$ pop $\leq$ 25000	37
25000 $<$ pop $\leq$ 50000	45
50000 $<$ pop $\leq$ 100000	59
100000 $<$ pop $\leq$ 250000	71
250000 $<$ pop $\leq$ 500000	81
500000 $<$ pop $\leq$ 1000000	93
pop $>$ 1000000	105

However, if equation (1) is estimated directly, the estimated parameters will likely be biased. To solve potential endogeneity problems, one thus needs two natural experiments that introduce exogenous variation in the terms C_{it} and $C_{it}D_{it}$, respectively.

First, in Hessian municipalities, I can use that council size is a deterministic discontinuous function of population size. Table 2.2 shows the council size law according to paragraph 38(1) of the local constitution of Hesse. The idea of an RDD in this context is that municipalities with a population size slightly below a population threshold are similar in all respects to those with a population size slightly above a threshold. Any difference in outcomes between these municipalities can therefore be attributed to the council size.[10]

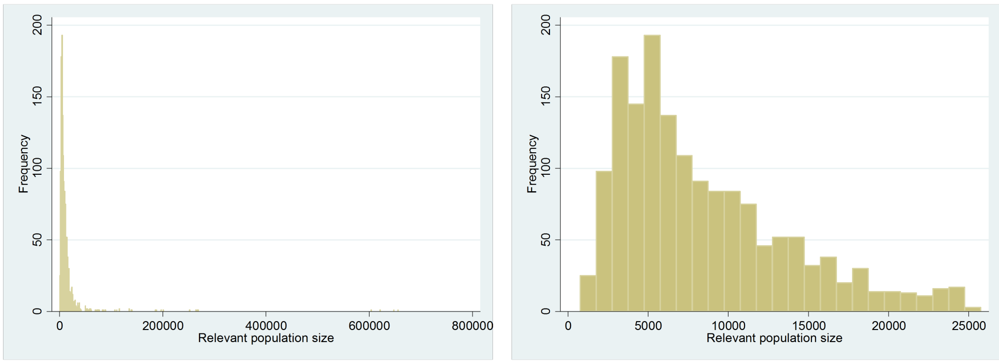
According to the local constitution of Hesse, the specific number of council members is determined by the **relevant population size** that the Statistical Office of Hesse had calculated and published at the last regular date before the exact date of the election was scheduled by the state government.[11]

Figure 2.3 shows the distribution of the relevant population size before elections. As can be seen, most municipalities have a population size below 25000. In

[10]An RDD using council size laws has recently been used by Egger and Köthenbürger (2010) and Pettersson-Lidbom (2012). For standard references on the RDD see Imbens and Lemieux (2008) and Lee and Lemieux (2011).

[11]The relevant population sizes for the four elections under investigation have been determined by the Statistical Office of Hesse on the following dates: June 30th, 1984; December 31st, 1987; March 31st, 1992; March 31st, 1996. Election dates have been: March 10th, 1985; March 12th, 1989; March 7th, 1993; March 2nd, 1997.

FIGURE 2.3: Histogram of relevant population size



(A) All Municipalities (B) Municipalities with Pop < 25000

fact, only 29 of the 426 municipalities never had a relevant population size below 25000 in the investigation period. Because the RDD requires many observations near the threshold, I drop those observations with population size over 25000 and focus on the three thresholds for municipalities with less than 25000 inhabitants: the 3000-, the 5000- and the 10000-threshold in the council size law.[12]

For the RDD to measure the causal effect of council size, it is necessary that there are no other changes at the 3000-, 5000- or 10000-threshold of the assignment variable (see Lee and Lemieux, 2011 and Ade and Freier, 2011 for a discussion on this topic). In Hesse there are changes in the population weight of the fiscal equalization law at the 5000- and 10000-population threshold and in the salary of the manager at the 10000-threshold. However, the assignment variables for the salary of the manager and for the weights in the fiscal equalization law differ from the assignment variable for council size: Council size is determined by the **relevant population size** that is measured at specific dates before the start of the legislative term. The weights in the fiscal equalization law and the salary of the manager are determined by the **current population size** and by the **current population size lagged one year**, respectively. Thus, at the three thresholds of the relevant population size, only council size changes which helps me to disentangle the council size effect from other confounding policy factors. Nonetheless, in the appendix, I also show that my results do not change

[12]After one election, a council in one municipality had 15 seats as prescribed by law but effectively only 13 council members simply because one party had not enough candidates to fill all the seats that it has received in the election. Importantly, my results are not sensitive to excluding these 4 observations.

when I directly control for the salary of the manager and the weights in the fiscal equalization law in the regressions.

Suppose that there were no change in the nomination scheme of the manager. Then, using the council size law, one could estimate the council size effect for the whole investigation period by

$$y_{it} = \alpha + \beta C_{it} + \delta' \mathbf{X}_{it} + f(Pop_{it}) + \epsilon_{it}, \quad (2)$$

where f is a polynomial function of the treatment-determining variable Pop, the **relevant population size** before an election, and $\mathbf{X}$ is a set of potential control variables.

To generate exogenous variation in $C_{it}D_{it}$, I use that the introduction of the election of the manager was implemented gradually over six years and that the timing of the first manager election for each municipality was arguably independent of observable and unobservable municipal characteristics.[13] More precisely, the decision of a change in the nomination scheme in Hesse evolved as follows: In 1991, the state government of Hesse launched a referendum to ask the citizens whether the manager in Hessian municipalities should be elected instead of appointed. The referendum resulted in a vote for the switch in the nomination scheme with a large majority. The reform should become law from 1993 on.

For my identification strategy, it is important that the date at which the term of the last appointed manager ended differed across municipalities and has not been under control of the state or local decision-makers in the year of the change in law, 1991. Because this date differed across municipalities, it was easier for the state government to implement a phase-in period of the new nomination scheme with a length of 6 years than to let all municipalities switch to the new system at a single date. Thus, municipalities switched to the new system when the term of the last appointed manager ended. Therefore, I can use those municipalities that had not been switched as counterfactual outcomes for those that already had an elected manager. In this way, I can disentangle general state-wide time effects from the introduction of the elected manager.

I code the variable D_{it} as follows: It is equal to one for the first time when

[13] For empirical studies also using a similar phase-in period of a switch in local government systems in a different contexts see Egger et al. (2011a, 2011b) and Ade (2014).

TABLE 2.3: Switch in the nomination scheme

Year	Number of municipalities with $D = 1$ for the first time
1993	45
1994	77
1995	85
1996	77
1997	63
1998	66
1999	13
Sum	426

the elected manager is likely able to effectively lead the public administration and from then on stays one until the year 2000. Manager elections take place throughout the year. If the first election of the manager is in the first half of the year, I assume that the elected manager is able to lead the public administration for the first time in the year of the election. If it is in the second half, I assume that the manager can effectively lead the administration from the next year on. For example, if the first manager election is in the first half of 1995, D_{it} is equal to one for the first time in 1995, while D_{it} is equal to one for the first time in 1996 if the first election is in the second half of 1995. Table 2.3 shows the distribution of the timing of the switch for the 426 municipalities.

I estimate a combination of a Regression Discontinuity Design with a Difference-in-Differences Approach:

$$y_{it} = \alpha + \beta C_{it} + \gamma C_{it} D_{it} + \delta' \mathbf{X}_{it} + \rho D_{it} + f(Pop_{it}) + f(Pop_{it}) D_{it} + \lambda_t + \epsilon_{it} \quad (3)$$

Thus, I control for direct effects of the nomination scheme on fiscal policies by including D_{it} directly into the regression and allow the polynomial function of the treatment-determining covariate to differ before and after the change in the nomination scheme.[14] I use polynomials ranging from the third to the fifth order to investigate whether the estimated council size effect is sensitive to the choice of the control function. In addition, I estimate the council size effect using only observations that are close to one of the population thresholds. I choose three different window sizes around the thresholds: ± 25, ± 20 and ± 10 percent, respectively. In all specifications, I include year-fixed effects λ_t and the proportion of people aged below 15 and above 65 in the specific municipality as control variables.[15] I cluster the standard errors at the municipality level to allow for arbitrary serial correlation within a municipality (see Bertrand et al., 2004).

Note that my approach differs from the typical RDD used in the literature where there is a single discontinuity. Here, (and in related papers, see Egger and Köthenbürger, 2010 and Pettersson-Lidbom, 2012) the data are pooled around several population thresholds because the number of political units close to a single threshold is typically too low to provide meaningful RDD estimates. By pooling the data, however, the model exploits a large variation in the assignment variable, i.e. population size, across jurisdictions which might affect the estimates. Nonetheless, in the robustness checks in section 2.4.3, I will show that the results are insensitive to this large variation in population size. It may be more appropriate to view the estimation approach without restriction in the variation of the assignment variable as a difference-in-differences approach where the first difference comes from the change in the nomination scheme and the second difference from the different number of

[14] The estimated model is similar to the Difference-in-Discontinuities Design estimated by Grembi et al. (2012) and Lalive et al. (2012). However, while their motivation is to disentangle one policy factor from other confounding policy factors, my aim is to estimate the combined effect of two features of the municipal system. Accordingly, they test for interactive effects between their policy factor of interest and other confounding policy factors as the presence of such interactions would invalidate their approach. By contrast, as I am interested in a combined effect of two policy factors, interactions between these policy factors do not invalidate my approach, but are at the core of the analysis.

[15] I also checked the robustness of the results by additionally including the seat shares of the two most important local parties, the center-right CDU and the center-left SPD as well as a dummy for absolute majorities (as a proxy for single-party governments) in the municipal council as control variables, although these are not pre-determined variables in a strict sense. The inclusion of these three additional control variables did not change the results and therefore I omit them to save space. The results are available from the author upon request.

legislators across municipalities.[16]

Before investigating the change in the council size effect, I measure the direct causal effect of the change in the nomination scheme on the outcome variables. If the switch in the nomination scheme of the manager results in higher spending for those expenditure categories over which the public administration has the most discretion, this will be evidence that the manager prefers different policies than the council and is indeed able to implement these policies after his independence from the council has increased. This is an important requirement for the existence of agency problems.

Besides that, the direct effect of a change in the nomination scheme is also interesting in its own right. There are many early empirical studies that have focused on local political institutions and their consequences for public policy, mainly investigating the reform movements in the United States in the early and mid-20$^{\text{th}}$ century (see, e.g., Booms, 1966; Morgan and Pelissero, 1980; Rauch, 1995).[17] In a recent contribution, Enikopolov (forthcoming) investigates whether elected or appointed public officials differ in the employment policies they pursue, but acknowledges that self-selection of local governments into different political systems cannot be ruled out in his setting.

To measure the causal effect of a change in the nomination scheme, I implement the following difference-in-differences estimation approach:

$$y_{it} = \mu_i + \rho D_{it} + \lambda_t + \delta' \mathbf{X}_{it} + \epsilon_{it} \tag{4}$$

where μ_i is a municipality-fixed effect. For the DiD approach, I use population size, council size[18], the proportion of people aged below 15 and above 65, the seat shares of the two major parties (the center-left SPD and the center-right

[16] I thank an anonymous reviewer for this comment. I have followed his suggestion and titled the estimates without restriction in the variation of the assignment variable in table 2.5 as DiD estimates.

[17] Rauch (1995) was also one of the first to explicitly describe the relationship between the city council and the public administration as a principal-agent problem in which incentives for the bureaucracy depend on the specific local government structure in place.

[18] Note that I allow the coefficient on the council size to differ before and after the reform as the analysis below clearly shows that this is appropriate. This way, I am able to measure the effect that is due to a change in the nomination scheme without confounding it with the effect that is due to a changing relationship between fragmentation and fiscal policies after the change in the nomination scheme.

CDU), and a dummy variable for absolute majorities in the municipal council as control variables.

For the difference-in-differences approach to be valid, it is required that those municipalities that have not switched to the new system yet can be regarded as valid counterfactuals for those municipalities that have already switched. This requirement implies that in absence of the treatment, the fiscal policies of the treated municipalities should have evolved on the same time path as of those municipalities that have not been treated. It is most likely to hold if the timing of the switch was indeed quasi-random such that municipalities did not self-select into the treatment or control group. Evidence for the validity of this assumption is presented in section 2.4.3.

2.4 Results

2.4.1 The direct effects of a change in the nomination scheme

Table 2.4 presents the results from the difference-in-differences approach (4). Total expenditures, personnel expenditures and material spending significantly increase when the manager is elected instead of appointed, while most other outcome variables are not affected. Only for the multiplier of the property tax B, I find a significantly negative effect. The effects for the outcome variables over which the public administration has the most discretion are highly significant and economically relevant. For example, material spending grows by more than 10% in response to the switch in the nomination scheme. These results provide evidence for the interpretation that the manager has different spending priorities than the municipal council and that he can use his increased independence from the council after the change in the nomination scheme to influence spending. The results thus point to agency problems between the public administration and the municipal council.

2.4.2 The change in the council size effect

In this section, I provide evidence on how the council size effect changes when there is a change in the nomination scheme by estimating the empirical model

TABLE 2.4: The direct effect of a change in the nomination scheme (Equation (4))

	ρ
Log (Total Expenditures)	0.0915 (0.0340)***
Log (Personnel exp.)	0.0657 (0.0297)**
Log (Material spending)	0.1031 (0.0385)***
Log (Capital expenditures)	-0.0007 (0.0975)
Multiplier Property Tax A	-4.5423 (3.8705)
Multiplier Property Tax B	-12.7461 (5.0712)**
Multiplier Business Tax	-4.2133 (4.0642)
Observations	6308

Notes: Expenditure variables are expressed in constant 2005 prices. The tax rate multipliers are expressed in percentage points. Standard errors are clustered at the municipality level. There are 397 clusters. The sample includes only municipalities with a relevant population size below 25000 in the time period 1985-2000. All regressions include year-fixed effects, municipality-fixed effects and the control variables mentioned in the text. *Significant at the 10 percent level, **Significant at the 5 percent level, ***Significant at the 1 percent level.

(3). Table 2.5 presents the results for the council size effect before (β) and after ($\beta + \gamma$) the change in the nomination scheme of the manager. Moreover, I report results on the coefficient γ. OLS results differ depending on the outcome variable and no clear pattern emerges neither regarding the significance nor the sign of the council size effect before and after the change in the nomination scheme. The comparison with the DiD results leads to the conclusion that the OLS estimates are severely biased.

In the following, I will consider the council size effect not only for total expenditures, but also for different types of expenditures for two reasons. First, the main pattern in the data might be more visible in specific types of expenditures than in aggregate expenditures because the latter consist to a significant extent of expenditure categories over which the city manager has no discretion.

Second, considering different types of expenditures allows shedding some light on the mechanism underlying the results. If agency problems between the public administration and the legislature are driving the results, then the negative effects after the reform should be existent in those expenditure categories over which the public administration has the most discretion. If

TABLE 2.5: Results from the empirical model (3)

		OLS	DiD	DiD	DiD
Log(Total Expenditures)	β	0.0067	-0.0053	-0.0061	-0.0061
		(0.0014)***	-0.0036	(0.0037)*	(0.0037)*
	γ	-0.0051	-0.0026	-0.0038	-0.0037
		(0.0013)***	(0.0047)	(0.0047)	(0.0047)
	$\beta+\gamma$	0.0016	-0.0080	-0.0099	-0.0098
		(0.0015)	(0.0042)*	(0.0043)**	(0.0043)**
Log(Personnel exp.)	β	0.0090	-0.0041	-0.0032	-0.0029
		(0.0015)***	(0.0036)	(0.0036)	(0.0035)
	γ	-0.0040	-0.0049	-0.0052	-0.0052
		(0.0012)***	(0.0050)	(0.0051)	(0.0051)
	$\beta+\gamma$	0.0050	-0.0090	-0.0084	-0.0081
		(0.0016)***	(0.0050)*	(0.0050)*	(0.0050)
Log(Material spending)	β	0.0040	-0.0045	-0.0057	-0.0055
		(0.0014)**	(0.0044)	(0.0044)	(0.0044)
	γ	-0.0057	-0.0090	-0.0084	-0.0082
		(0.0014)***	(0.0059)	(0.0061)	(0.0060)
	$\beta+\gamma$	-0.0018	-0.0135	-0.0141	-0.0137
		(0.0019)	(0.0056)**	(0.0059)**	(0.0057)**
Log(Capital expenditures)	β	-0.0061	-0.0015	-0.0025	-0.002
		(0.0024)**	(0.0061)	(0.0062)	(0.0063)
	γ	-0.0061	-0.0074	-0.0089	-0.0091
		(0.0032)*	(0.0107)	(0.0108)	(0.0109)
	$\beta+\gamma$	-0.0122	-0.0088	-0.0114	-0.0112
		(0.0033)***	(0.0104)	(0.0105)	(0.0106)
Multiplier Property Tax A	β	-1.4079	0.0672	0.2515	0.2656
		(0.2908)***	(0.7533)	(0.7675)	(0.7693)
	γ	0.2108	0.3388	0.3733	0.3733
		(0.1933)	(1.0268)	(1.0440)	(1.0488)
	$\beta+\gamma$	-1.1971	0.4060	0.6248	0.6389
		(0.2784)***	(0.8980)	(0.8929)	(0.8904)
Multiplier Property Tax B	β	-0.7698	1.0389	1.1750	1.1505
		(0.2627)***	(0.7340)	(0.7412)	(0.7421)
	γ	0.3904	-0.7867	-0.6932	-0.6882
		(0.1903)**	(0.8722)	(0.8911)	(0.8941)
	$\beta+\gamma$	-0.3793	0.2522	0.4818	0.4623
		(0.2540)	(0.7356)	(0.7402)	(0.7420)
Multiplier Business Tax	β	0.7209	-0.4320	-0.3664	-0.3849
		(0.1692)***	(0.3899)	(0.3959)	(0.3980)
	γ	0.0906	0.1342	0.2174	0.2021
		(0.1487)	(0.5896)	(0.5989)	(0.5970)
	$\beta+\gamma$	0.8115	-0.2978	-0.1491	-0.1828
		(0.2024)***	(0.5330)	(0.5291)	(0.5295)
Degree of polynomial		None	Third	Fourth	Fifth

Notes: Expenditure variables are expressed in constant 2005 prices. The tax rate multipliers are expressed in percentage points. Standard errors are clustered at the municipality level. There are 397 clusters. The sample includes only municipalities with a relevant population size below 25000 in the time period 1985-2000. There are 6308 observations. All regressions include year-fixed effects and the proportion of people aged below 15 and above 65 as control variables. *Significant at the 10 percent level, **Significant at the 5 percent level, ***Significant at the 1 percent level.

the public administration has no discretion over an expenditure category and can accordingly not influence it even after the reform, a large(r) council is simply not needed to monitor the public administration. In this case, the number of legislators would not have any effect on fiscal policies. As argued in section 2, the public administration should have considerable discretion over personnel expenditures and material spending. Table 2.4 has shown that the public administration indeed strongly increases spending in these areas once it has become more independent from the council after the introduction of elections. Thus, there is suggestive evidence that the public administration is budget-maximizing.

Using the estimation approach (3), for total expenditures I find a council size effect that is at the border of statistical significance when the manager is appointed: For the third order polynomial, the council size effect is marginally insignificant and for the fourth and fifth order polynomial marginally significant (p-values: 0.096 and 0.099). Thus, there is only weak evidence for a council size effect when the manager is appointed. After the introduction of elections, the estimates suggest that there is a highly significant negative council size effect. This is some preliminary weak evidence that the council size effects could not be generalizable to all political systems. However, for stronger evidence in favor of this hypothesis, as explained above I will consider different types of expenditures because total expenditures also include expenditure types over which the manager has no discretion.

I indeed find highly significant negative council size effects for material spending and personnel expenditures for the period with an elected manager, but not for the period with an appointed manager. The estimates for the period with an elected manager are of a larger magnitude for material spending than for total expenditures. To put the estimates in numerical magnitude, my baseline results suggest that an additional councilor decreases personnel expenditures and material spending after the reform by approximately 0.9% and 1.4%, respectively, while the corresponding estimates for the period with an appointed manager suggest that an additional councilor reduces both expenditure types by only approximately 0.4%.

For the elected manager the results therefore correspond to those from Pettersson-Lidbom (2012). The estimated effects are very robust across polynomial specifications. This insensitivity to the choice of the polynomial order suggests that the control function is not misspecified. Thus, a directly elected manager wants to increase spending according to the results in section 2.4.1, while this increase is smaller the larger the council is. This is highly suggestive for the interpretation that a larger council has more possibilities to restrict the spending preferences of the manager. The manager can use the increasing independence from the council after the reform better if he cannot be monitored by many councilors.

For investment expenditures over which the manager has no discretion, I do not find any significant effect for either period. Again, the council size effects are very robust across polynomial specifications. Other outcome variables over which the public administration has no discretion are the multipliers of the three local tax rates that municipalities are free to set. Therefore, I should not find negative council size effects after the change in the nomination scheme for these outcome variables. Indeed, using the three local tax rates reveals no significant council size effects before and after the reform: The tax rate effects are small and close to zero (note again that I have not transformed the tax rates into logarithms) and there is no clear pattern of whether the council size effects before or after the reform are larger.

The channel through which the reform affects fiscal policy is important to evaluate the welfare consequences of the interaction between the switch in the nomination scheme and government fragmentation. If agency problems are responsible for the results, then more fragmentation is positive from a welfare point of view, because more legislators are better able to monitor a budget-maximizing administration. This is especially true after the switch in the nomination scheme as agency problems have presumably become stronger after the reform. By contrast, pork-barrel type models as put forward by Weingast et al. (1981) and Primo and Snyder (2008) would suggest that any effect of the number of legislators on expenditures (i.e. over- or underspending) leads to inefficiency. In this case, the introduction of elected managers has created inefficiency through underspending.

If I relate the findings for different outcome variables using the empirical model (3)) to the findings of the direct effects of the change in the nomination scheme as shown in table 2.4, my results suggest that the number of legislators only plays a role for those expenditure categories for which a powerful administrator had the opportunity to increase expenditures after the reform due to his growing independence from the council. This finding indicates that indeed agency problems could be responsible for the results: As mentioned above, monitoring the public administration to restrict a possible spending expansion is only necessary if the administrator has some discretionary power to decide on expenditures and if the administrator chooses spending levels that diverge from the spending preferences of the council. It seems to be the case that more legislators are indeed better able to curb down the spending expansion that has resulted from the change in the nomination scheme.

As an alternative explanation one might argue that the change in the council size effect could be due to political selection rather than due to the direct effect of a different institutional setting. For example, it may be possible that when the manager is appointed by the council, strong ties to the party system are necessary for a candidate to hold office. By contrast, when the manager is elected by the voters more independent candidates could have a greater chance. Therefore, I have compared the party affiliations of the appointed managers before the switch in the system with the party affiliations of the elected managers after the switch.

Unfortunately, municipalities recorded the party affiliations (and other personal characteristics of the city manager) only after the change of the nomination scheme. For the time before the change, I only have cross-sectional information on the party affiliations for the year 1991. Comparing the structure of the party affiliations of the city managers in the year 1991 to the structure in 1999 (after all municipalities have switched to the new system) only points to minor changes in the partisan identities of the city managers. Especially the number of independent candidates only rose marginally.

Moreover, political selection might play a role in the sense that the average quality of the managers changes after the reform. This may be the case if, for example, the position of the manager has become politically more attractive. As noted above, detailed characteristics about the city managers as proxies for their quality are not available before the reform. However, there are good arguments for why the quality of the managers did not change through the reform (at least in the short-run period after the reform that I consider).

First, neither pay nor pension conditions have changed through the reform. Second, the city manager has also not become politically more important as he has not gained any competencies. Third, Hesse was the first German state that has reformed the position of the city manager and moreover, the local government system of Hesse is unique in Germany. Thus, political actors in Hesse could not have any precise information about how the attractiveness of the position could change after the reform. It is likely that for the first manager election the same types of candidates were nominated that would have also been considered under the old system. In fact, many of the politicians that had been city manager shortly before the reform (in 1991) stood also for election after the system had been changed (and were often elected). Thus, the average quality of the city manager has likely not changed at least in the short run. To summarize, it seems to be unlikely that political selection is driving the results.

As a specification check of the results in table 2.5, the council size effect is also estimated using only observations that are close to the population thresholds. Tables 2.6-2.8 report the results from the ±25%, ±20% and ±10% window widths around the thresholds, respectively. These results are very similar to the estimation results obtained with the full sample. Most of the results stay significant and the magnitude of the point estimates roughly stays the same. This is further evidence that the results are very robust against misspecification.

2.4.3 Robustness checks

In this section, I discuss the robustness of the results. A concern that may render the RDD invalid is when political units strategically manipulate the assignment variable in order to self-select into the treatment or control group. While I argue that local governments do not have the possibility to misreport their population size, it may be - at least theoretically - possible that citizens strategically migrate into or out of municipalities to influence council size. Evidence for this kind of sorting would be given if there is a discontinuity in the distribution of the assignment variable at the threshold (see Lee and Lemieux, 2011). In Figure 2.4, I show histograms of the assignment variable at the three thresholds. These histograms do not provide any evidence for sorting around the thresholds as the number of local governments slightly below each threshold is always nearly equal to the number of local governments slightly above each threshold.

More formally, I can test the null hypothesis of no discontinuity with a McCrary-test (see McCrary, 2008). Figure 2.5 reports the results of the McCrary-test at

TABLE 2.6: Window size ±25% around the threshold (equation (3))

		OLS	RDD	RDD	RDD
Log(Total Expenditures)	β	0.0025	-0.0068	-0.0070	-0.0066
		(0.0018)	(0.0040)*	(0.0041)*	(0.0042)
	γ	-0.0040	-0.0034	-0.0028	-0.0016
		(0.0017)**	(0.0057)	(0.0058)	(0.0061)
	$\beta+\gamma$	-0.0014	-0.0103	-0.0099	-0.0082
		(0.0017)	(0.0051)**	(0.0054)*	(0.0056)
Log(Personnel exp.)	β	0.0046	-0.0030	-0.0028	-0.0030
		(0.0017)***	(0.0037)	(0.0037)	(0.0039)
	γ	-0.0027	-0.0081	-0.0083	-0.0089
		(0.0016)*	(0.0061)	(0.0062)	(0.0065)
	$\beta+\gamma$	0.0019	-0.0111	-0.0111	-0.0119
		(0.0020)	(0.0054)**	(0.0056)**	(0.0057)**
Log(Material spending)	β	0.0000	-0.0042	-0.0037	-0.0038
		(0.0022)	(0.0046)	(0.0046)	(0.0046)
	γ	-0.0031	-0.0126	-0.0113	-0.0125
		(0.0018)*	(0.0070)*	(0.0072)	(0.0074)*
	$\beta+\gamma$	-0.0030	-0.0168	-0.0150	-0.0162
		(0.0021)	(0.0062)***	(0.0064)**	(0.0066)**
Log(Capital expenditures)	β	-0.0045	0.0004	0.0007	0.0009
		(0.0030)	(0.0067)	(0.0068)	(0.0074)
	γ	-0.0041	-0.0004	0.0007	0.0064
		(0.0039)	(0.0131)	(0.0136)	(0.0144)
	$\beta+\gamma$	-0.0087	0.0000	0.0014	0.0074
		(0.0040)**	(0.0122)	(0.0127)	(0.0134)
Multiplier Property Tax A	β	-0.9219	-0.2472	-0.3104	-0.1111
		(0.3736)**	(0.8949)	(0.9076)	(0.9414)
	γ	0.1205	1.0453	0.7994	0.5415
		(0.3090)	(1.3200)	(1.3283)	(1.3676)
	$\beta+\gamma$	-0.8015	0.7981	0.4890	0.4304
		(0.3641)**	(1.0379)	(1.0720)	(1.0767)
Multiplier Property Tax B	β	-0.4545	0.0605	0.0504	0.1921
		-0.3555	-0.8367	-0.8450	-0.8855
	γ	0.0999	-0.5019	0.3609	-0.7062
		-0.2811	-1.1479	-1.1449	-1.1961
	$\beta+\gamma$	-0.3546	-0.4414	-0.3105	-0.5141
		-0.3452	-0.9037	-0.9051	-0.9494
Multiplier Business Tax	β	0.3672	-0.5301	-0.5293	-0.4191
		(0.2109)*	(0.4398)	(0.4415)	(0.4595)
	γ	-0.0477	0.5876	0.5434	0.3192
		(0.2125)	(0.7073)	(0.7106)	(0.7487)
	$\beta+\gamma$	0.3194	0.0576	0.0141	-0.0999
		(0.2473)	(0.5298)	(0.5391)	(0.5521)
Degree of polynomial		None	Third	Fourth	Fifth

Notes: Expenditure variables are expressed in constant 2005 prices. The tax rate multipliers are expressed in percentage points. Standard errors are clustered at the municipality level. There are 298 clusters. The sample includes all municipalities with a population size in the ±25% window around the three thresholds. There are 4204 observations. All regressions include year-fixed effects and the proportion of people aged below 15 and above 65 as control variables. *Significant at the 10 percent level, **Significant at the 5 percent level, ***Significant at the 1 percent level.

TABLE 2.7: Window size ±20% around the threshold (equation (3))

		OLS	RDD	RDD	RDD
Log(Total Expenditures)	β	0.0029	-0.0067	-0.0067	-0.0064
		(0.0019)	(0.0040)*	(0.0041)*	(0.0042)
	γ	-0.0040	-0.0026	-0.0027	-0.0013
		(0.0018)**	(0.0060)	(0.0060)	(0.0061)
	$\beta+\gamma$	-0.0011	-0.0093	-0.0094	-0.0077
		(0.0018)	(0.0053)*	(0.0054)*	(0.0056)
Log(Personnel exp.)	β	0.0053	-0.0031	-0.0030	-0.0026
		(0.0018)***	(0.0037)	(0.0037)	(0.0039)
	γ	-0.0037	-0.0086	-0.0092	-0.0100
		(0.0018)**	(0.0064)	(0.0064)	(0.0066)
	$\beta+\gamma$	0.0016	-0.0117	-0.0122	-0.0125
		(0.0021)	(0.0056)**	(0.0056)**	(0.0057)**
Log(Material spending)	β	0.0006	-0.0028	-0.0027	-0.0037
		(0.0023)	(0.0046)	(0.0046)	(0.0046)
	γ	-0.0034	-0.0131	-0.0120	-0.0111
		(0.0020)*	(0.0073)*	(0.0073)*	(0.0073)
	$\beta+\gamma$	-0.0027	-0.0159	-0.0147	-0.0148
		(0.0021)	(0.0064)**	(0.0065)**	(0.0065)**
Log(Capital expenditures)	β	-0.0034	0.0034	0.0036	0.0022
		(0.0032)	(0.0069)	(0.0069)	(0.0074)
	γ	-0.0027	-0.0008	-0.0004	0.0072
		(0.0041)	(0.0136)	(0.0139)	(0.0146)
	$\beta+\gamma$	-0.0060	0.0027	0.0032	0.0094
		(0.0041)	(0.0126)	(0.0129)	(0.0135)
Multiplier Property Tax A	β	-0.7479	-0.4319	-0.4231	-0.2153
		(0.3883)*	(0.9033)	(0.9071)	(0.9309)
	γ	0.1947	1.2487	1.1849	0.6368
		(0.3557)	(1.3724)	(1.3705)	(1.3757)
	$\beta+\gamma$	-0.5532	0.8168	0.7617	0.4215
		(0.3836)	(1.0611)	(1.0723)	(1.0759)
Multiplier Property Tax B	β	-0.5839	0.0165	0.0291	0.1827
		(0.3729)	(0.8539)	(0.8568)	(0.8809)
	γ	0.0682	-0.3733	-0.3553	-0.8657
		(0.3270)	(1.1858)	(1.1681)	(1.1692)
	$\beta+\gamma$	-0.5157	-0.3568	-0.3262	-0.683
		(0.3632)	(0.9232)	(0.9163)	(0.9329)
Multiplier Business Tax	β	0.2920	-0.6594	-0.6631	-0.5010
		(0.2287)	(0.4390)	(0.4393)	(0.4510)
	γ	-0.0158	0.6360	0.6899	0.3698
		(0.2395)	(0.7337)	(0.7260)	(0.7458)
	$\beta+\gamma$	0.2763	-0.0235	0.0268	-0.1312
		(0.2654)	(0.5413)	(0.5378)	(0.5526)
Degree of polynomial		None	Third	Fourth	Fifth

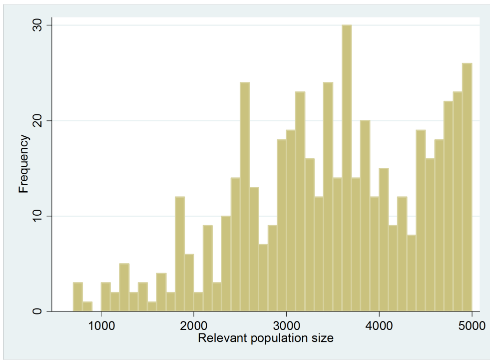
Notes: Expenditure variables are expressed in constant 2005 prices. The tax rate multipliers are expressed in percentage points. Standard errors are clustered at the municipality level. There are 269 clusters. The sample includes all municipalities with a population size in the ±20% window around the three thresholds. There are 3460 observations. All regressions include year-fixed effects and the proportion of people aged below 15 and above 65 as control variables. *Significant at the 10 percent level, **Significant at the 5 percent level, ***Significant at the 1 percent level.

Table 2.8: Window size ±10% around the threshold (equation (3))

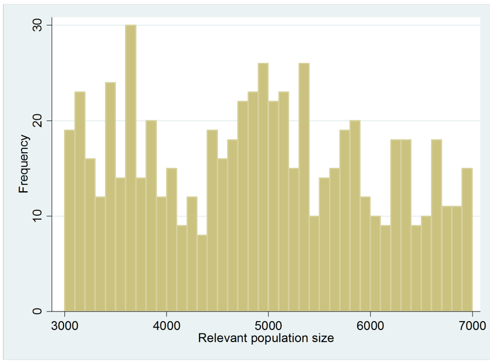
		OLS	RDD	RDD	RDD
Log(Total Expenditures)	β	0.0019 (0.0025)	-0.0052 (0.0041)	-0.0060 (0.0043)	-0.0060 (0.0043)
	γ	-0.0054 (0.0030)*	-0.0009 (0.0067)	-0.0009 (0.0073)	-0.0008 (0.0073)
	$\beta+\gamma$	-0.0035 (0.0025)	-0.0061 (0.0057)	-0.0069 (0.0059)	-0.0068 (0.0058)
Log(Personnel exp.)	β	0.0049 (0.0024)**	-0.0019 (0.0041)	-0.0015 (0.0041)	-0.0013 (0.0042)
	γ	-0.0058 (0.0033)*	-0.0090 (0.0077)	-0.0097 (0.0081)	-0.0098 (0.0082)
	$\beta+\gamma$	-0.0009 (0.0034)	-0.0109 (0.0064)*	-0.0112 (0.0068)*	-0.0112 (0.0068)
Log(Material spending)	β	0.0026 (0.0029)	-0.0004 (0.0048)	0.0013 (0.0047)	0.0013 (0.0047)
	γ	-0.0041 (0.0034)	-0.0126 (0.0081)	-0.0161 (0.0081)**	-0.0158 (0.0081)*
	$\beta+\gamma$	-0.0015 (0.0029)	-0.0130 (0.0069)*	-0.0147 (0.0067)**	-0.0146 (0.0067)**
Log(Capital expenditures)	β	-0.0031 (0.0047)	0.0008 (0.0074)	-0.0002 (0.0077)	-0.0003 (0.0077)
	γ	-0.0033 (0.0070)	0.0099 (0.0172)	0.0043 (0.0169)	0.0045 (0.0170)
	$\beta+\gamma$	-0.0064 (0.0067)	0.0107 (0.0154)	0.0041 (0.0152)	0.0042 (0.0152)
Multiplier Property Tax A	β	-1.2784 (0.5180)**	-0.6249 (0.9809)	-0.7592 (0.9442)	-0.7567 (0.9446)
	γ	0.8601 (0.6047)	1.1362 (1.6055)	1.5534 (1.6370)	1.523 (1.6262)
	$\beta+\gamma$	-0.4182 (0.5720)	0.5113 (1.1640)	0.7942 (1.2041)	0.7663 (1.1914)
Multiplier Property Tax B	β	-0.6996 (0.5233)	0.2624 (0.9493)	0.4290 (0.9194)	0.4781 (0.9084)
	γ	0.3213 (0.5348)	-1.2911 (1.3526)	-1.4974 (1.3739)	-1.5765 (1.3425)
	$\beta+\gamma$	-0.3784 (0.4916)	-1.0288 (0.9693)	-1.0684 (1.0024)	-1.0984 (0.9891)
Multiplier Business Tax	β	-0.0657 (0.3010)	-0.7949 (0.4549)*	-0.8391 (0.4565)*	-0.8418 (0.4594)*
	γ	0.365 (0.3376)	1.2704 (0.8247)	1.4131 (0.8790)	1.3959 (0.8822)
	$\beta+\gamma$	0.2993 (0.3076)	0.4755 (0.5993)	0.5739 (0.6586)	0.5541 (0.6533)
Degree of polynomial		None	Third	Fourth	Fifth

Notes: Expenditure variables are expressed in constant 2005 prices. The tax rate multipliers are expressed in percentage points. Standard errors are clustered at the municipality level. There are 175 clusters. The sample includes all municipalities with a population size in the ±10% window around the three thresholds. There are 1816 observations. All regressions include year-fixed effects and the proportion of people aged below 15 and above 65 as control variables. *Significant at the 10 percent level, **Significant at the 5 percent level, ***Significant at the 1 percent level.

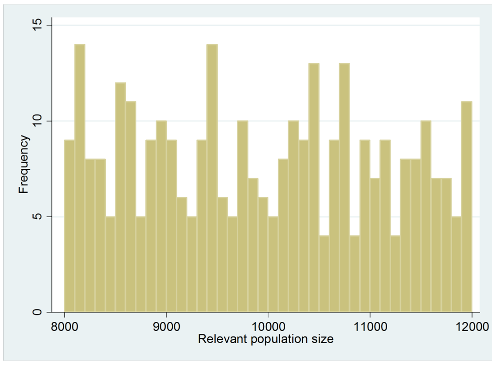
FIGURE 2.4: Histogram of relevant population size around the thresholds



(A) 3000-threshold



(B) 5000-threshold



(C) 10000-threshold

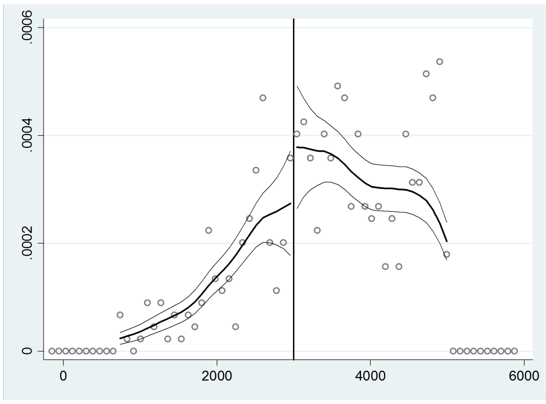
the three thresholds before the four elections. At none of these thresholds, the null hypothesis of no discontinuity can be rejected. Moreover, I conducted the McCrary-test separately for the relevant population size before and after the reform. It might be possible that the potential benefits of a specific council size differ between the two possible nomination schemes such that sorting of political units might only occur before or after the reform. However, I also do not find any evidence for a discontinuity at the thresholds if I consider both time periods separately. Results are omitted here, but are available upon request.

A second concern with the RDD is that council size might not be randomly assigned at the threshold. To test for this, I use council size as dependent variable and run a regression on the observable covariates and the control function (see Pettersson-Lidbom, 2008), i.e. I estimate

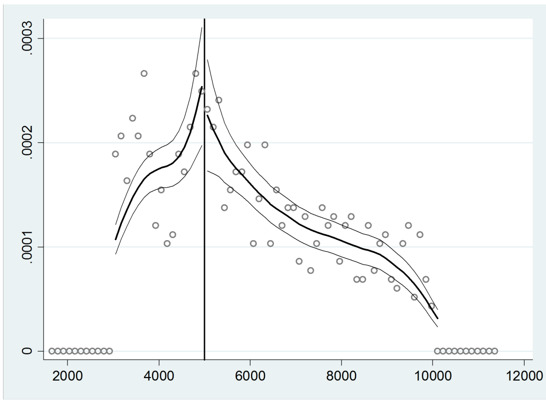
$$C_{it} = \alpha + \delta' \mathbf{X}_{it} + f(Pop_{it}) + \lambda_t + \epsilon_{it} \quad (5)$$

I run separate regressions for the periods with either nomination scheme of the

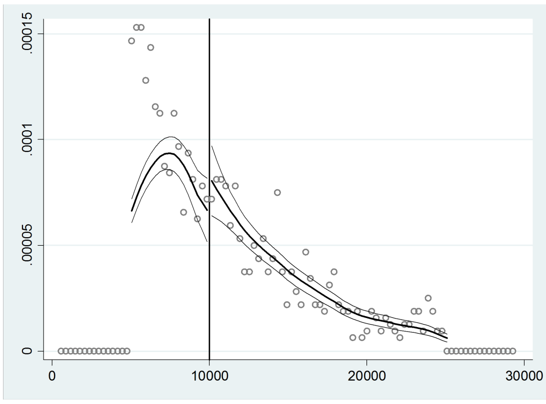
FIGURE 2.5: Histogram of relevant population size



(A) Threshold of 3000 inhabitants. Point estimate of the discontinuity: 0.3349, se: 0.2662



(B) Threshold of 5000 inhabitants. Point estimate of the discontinuity: -0.1356, se: 0.1821



(C) Threshold of 10000 inhabitants. Point estimate of the discontinuity: 0.24, se: 0.1675

Notes: Weighted kernel estimation of the log density of the assignment variable, performed separately on either side of each of the three population thresholds. The optimal bandwidth is computed as in McCrary (2008).

Table 2.9: Council size as dependent variable before change in nomination scheme (eq. 5))

	(1) Council Size	(2) Council Size	(3) Council Size
Proportion of old, 65+	-0.0431	-0.0416	-0.0463
	(0.0425)	(0.0413)	(0.0410)
Proportion of young, 0-15	-0.1040	-0.0988	-0.1017
	(0.0649)	(0.0648)	(0.0647)
Dummy: Absolute majority	-0.0856	-0.0229	-0.0056
	(0.2029)	(0.2075)	(0.2109)
Seat share CDU	-0.1172	-0.3019	-0.3869
	(0.8907)	(0.9425)	(0.9779)
Seat share SPD	-0.4416	-0.6637	-0.7309
	(0.9096)	(0.9873)	(1.0345)
Observations	4238	4238	4238
Degree of polynomial	Third	Fourth	Fifth
p-value F-Test	0.5584	0.6151	0.58

Notes: Standard errors are clustered at the municipality level. There are 397 clusters. The sample includes all municipalities before the switch in the nomination scheme of the manager with a relevant population size below 25000. All regressions include year-fixed effects. *Significant at the 10 percent level, **Significant at the 5 percent level, ***Significant at the 1 percent level

manager. If the coefficients of the covariates were significantly different from zero, this would indicate that council size depends on some covariates and that therefore council size is not randomly assigned. Tables 2.9 and 2.10 show the results for different polynomial orders in the control function. Neither are any of the individual coefficients significantly different from zero for either period nor are the coefficients jointly significantly different from zero as indicated by the F-tests. Another strategy to test whether council size is randomly assigned is to look for a discontinuity in the distribution of a covariate at the threshold, i.e. to employ the baseline covariates as dependent variable in (3). However, I do not find any significant treatment effect no matter which covariate I use as dependent variable. Results are available upon request.

A concern with the empirical strategy might be that the estimates could be driven by the large variation in the assignment variable because my estimation approach pools data from three different population thresholds. A strategy to rule out such concerns is to include discontinuity fixed effects into the specifications within specific window sizes around the three thresholds (i.e. the specifications in tables 2.6-2.8). Precisely, I include a dummy variable equal to

TABLE 2.10: Council size as dependent variable after change in nomination scheme (eq. 5))

	(1) Council Size	(2) Council Size	(3) Council Size
Proportion of old, 65+	-0.0564	-0.0669	-0.0693
	(0.0578)	(0.0571)	(0.0574)
Proportion of young, 0-15	-0.0109	-0.0213	-0.0164
	(0.0859)	(0.0856)	(0.0856)
Dummy: Absolute majority	-0.1136	-0.0284	-0.0295
	(0.2766)	(0.2814)	(0.2845)
Seat share CDU	-0.4797	-0.5515	-0.6082
	(1.1319)	(1.1525)	(1.1806)
Seat share SPD	1.0521	0.8932	0.8989
	(1.2058)	(1.2929)	(1.3377)
Observations	2070	2070	2070
Degree of polynomial	Third	Fourth	Fifth
p-value F-Test	0.7936	0.7453	0.7063

Notes: Standard errors are clustered at the municipality level. There are 393 clusters. The sample includes all municipalities after the switch in the nomination scheme of the manager with a relevant population size below 25000. All regressions include year-fixed effects. *Significant at the 10 percent level, **Significant at the 5 percent level, ***Significant at the 1 percent level

one if an observation is close to the first threshold and zero otherwise. Analogously, I include a second dummy variable for whether an observation is close to the second threshold (the third threshold serves as the reference category). Moreover, I interact the running variable with these discontinuity fixed effects to estimate separate functions of the assignment variable within each of the three discontinuity samples. With this procedure, I allow the estimates to be identified only by the variation within each of the three discontinuity samples.

In tables 2.11-2.13, I present the results of specifications without polynomials and with polynomials of first and second degree. To economize on space, I only present the estimates for those outcome variables with significant results in the baseline specification.[19] As can be seen, the results are qualitatively very similar to those obtained by the baseline model. For the larger bandwidths $\pm 25\%$ and $\pm 20\%$, a second order polynomial seems to be more appropriate than a first order polynomial (this is also suggested by model selection criteria like the AIC). For the smallest bandwidth, specifications with first and second order polynomials give quantitatively very similar results. In contrast to the

[19] The results for the other outcome variables are never significant and in line with what I have found in the baseline specification.

TABLE 2.11: Specification (3) with discontinuity fixed effects - Window size ±25% around the threshold

		RDD	RDD	RDD
Log(Total Expenditures)	β	-0.0033 (0.0028)	-0.0049 (0.0037)	-0.0046 (0.0039)
	γ	-0.0039 (0.0017)**	0.0008 (0.0041)	-0.0021 (0.0047)
	$\beta+\gamma$	-0.0073 (0.0027)***	-0.0041 (0.0044)	-0.0066 (0.0046)
Log(Personnel exp.)	β	-0.0019 (0.0025)	-0.0035 (0.0037)	-0.0025 (0.0037)
	γ	-0.0027 (0.0016)**	-0.0023 (0.0045)	-0.0053 (0.0051)
	$\beta+\gamma$	-0.0046 (0.0027)*	-0.0058 (0.0047)	-0.0078 (0.0052)
Log(Material spending)	β	-0.0077 (0.0031)**	-0.0005 (0.0044)	0.0024 (0.0045)
	γ	-0.0031 (0.0018)*	-0.0036 (0.0046)	-0.0107 (0.0057)*
	$\beta+\gamma$	-0.0107 (0.0030)***	-0.0041 (0.0051)	-0.0084 (0.0056)
Degree of polynomial		None	First	Second

Notes: Standard errors are clustered at the municipality level. The table shows results obtained by estimating table 2.6 with discontinuity fixed-effects. The same control variables as in Table 2.6 are included. *Significant at the 10 percent level, **Significant at the 5 percent level, ***Significant at the 1 percent level.

baseline model, however, some estimates are now slightly insignificant because of an increase in the standard errors. Most interestingly, the estimates with no polynomial included are also close to the results of the baseline specification in table 2.5: This suggests that there is no evidence for the initial concern that the results could be driven by the (high-order) population controls.[20]

A further concern with the change of the nomination scheme might be that appointed managers who know to get treated (i.e. elected) in the near future

[20] A second strategy to test whether the large between-variation of population controls is able to drive the results is to include municipality-fixed effects. As discussed by Pettersson-Lidbom (2012), in this case the estimates are only identified by the within-variation of population size, and can thus not be due to the large variation in the running variable between different municipalities at presumably different population thresholds. In non-reported robustness checks, I have also performed these fixed-effects regressions. The results were qualitatively similar to the baseline specification such that I omit them here.

TABLE 2.12: Specification (3) with discontinuity fixed effects - Window size ±20% around the threshold

		RDD	RDD	RDD
Log(Total Expenditures)	β	-0.0034 (0.0028)	-0.0038 (0.0038)	-0.0031 (0.0040)
	γ	-0.0039 (0.0018)**	0.0010 (0.0043)	-0.0021 (0.0049)
	$\beta + \gamma$	-0.0073 (0.0027)***	-0.0028 (0.0046)	-0.0052 (0.0048)
Log(Personnel exp.)	β	-0.0016 (0.0025)	-0.0036 (0.0036)	-0.0031 (0.0037)
	γ	-0.0034 (0.0018)*	-0.0038 (0.0046)	-0.0054 (0.0054)
	$\beta + \gamma$	-0.0050 (0.0028)*	-0.0074 (0.0050)	-0.0085 (0.0054)*
Log(Material spending)	β	-0.0063 (0.0033)*	0.0011 (0.0044)	0.0033 (0.0045)
	γ	-0.0034 (0.0020)*	-0.0057 (0.0049)	-0.0118 (0.0061)*
	$\beta + \gamma$	-0.0097 (0.0031)***	-0.0046 (0.0055)	-0.0086 (0.0059)
Degree of polynomial		None	First	Second

Notes: Standard errors are clustered at the municipality level. The table shows results obtained by estimating table 2.7 with discontinuity fixed-effects. The same control variables as in table 2.7 are included. *Significant at the 10 percent level, **Significant at the 5 percent level, ***Significant at the 1 percent level.

will already start to pursue different policies before the switch takes place.[21] For example, they might feel the need to please important party members to be nominated as an official party candidate for the upcoming elections. To rule out such concerns, I investigate whether the time until the switch of the nomination scheme influences the behavior of the managers by including anticipation terms into specification (4). Moreover, I also investigate whether the time since the switch has occurred affects the policies pursued by the (then elected) managers. Specifically, I estimate

$$y_{it} = \mu_i + \rho D_{it} + \lambda_t + \sum_{l=-3}^{3} \tau_{ilt} + \delta' \mathbf{X}_{it} + \epsilon_{it} \tag{6}$$

with τ_{ilt} as dummy variables that indicate the time until/since the first election of the city manager (i.e. $l = -3$ indicates three years before the first election

[21] I thank an anonymous reviewer for this suggestion.

TABLE 2.13: Specification (3) with discontinuity fixed effects - Window size ±10% around the threshold

		RDD	RDD	RDD
Log(Total Expenditures)	β	-0.0027	-0.0065	-0.0059
		(0.0032)	(0.0043)	(0.0046)
	γ	-0.0054	-0.002	-0.0038
		(0.0030)*	(0.0058)	(0.0065)
	$\beta+\gamma$	-0.0081	-0.0084	-0.0097
		(0.0034)***	(0.0065)	(0.0067)
Log(Personnel exp.)	β	-0.0028	-0.0022	-0.0018
		(0.0029)	(0.0042)	(0.0045)
	γ	-0.0059	-0.0065	-0.0068
		(0.0031)*	(0.0069)	(0.0074)
	$\beta+\gamma$	-0.0086	-0.0087	-0.0086
		(0.0039)**	(0.0075)	(0.0077)
Log(Material spending)	β	-0.0025	0.0001	0.0015
		(0.0038)	(0.0049)	(0.0051)
	γ	-0.0041	-0.0084	-0.0113
		(0.0034)	(0.0072)	(0.0084)
	$\beta+\gamma$	-0.0066	-0.0083	-0.0098
		(0.0038)*	(0.0072)	(0.0078)
Degree of polynomial		None	First	Second

Notes: Standard errors are clustered at the municipality level. The table shows results obtained by estimating table 2.8 with discontinuity fixed-effects. The same control variables as in table 2.8 are included. *Significant at the 10 percent level, **Significant at the 5 percent level, ***Significant at the 1 percent level.

and $l = 3$ three years after the first election). Specification (6) includes the same control variables as (4).

Table 2.14 shows the results. Only for the multiplier of the business tax in the third period *after* the switch I find an effect that is marginally significant at the 10%-level. To further probe whether my results could be sensitive to adjustment or anticipation effects, I have re-run my baseline model (3) excluding the year of the first election as well as the three years before and after the first election for each municipality. However, my baseline results did not change. Based on these two robustness checks, it is very unlikely that my results are driven by adjustment or anticipation effects.

Although the order in which municipalities switch to an elected manager appears to be quasi-random, another robustness check is to test whether the timing of the transition to the new system is really independent of observable covariates. Therefore, I perform a duration analysis for the transition from the

TABLE 2.14: Dynamic effects (equation 6)

	τ_{-3}	τ_{-2}	τ_{-1}	τ_0	τ_1	τ_2	τ_3
Log(Total Expenditures)	-0.0102	-0.0002	-0.0149	0.0109	0.0036	-0.0073	-0.0137
	(0.0105)	(0.0141)	(0.0182)	(0.0235)	(0.0191)	(0.0152)	(0.0132)
Log(Personnel exp.)	0.0024	-0.0024	-0.0103	0.0114	0.0109	0.0034	0.0088
	(0.0052)	(0.0074)	(0.0107)	(0.0191)	(0.0183)	(0.0122)	(0.0087)
Log(Material spending)	-0.0043	-0.0076	-0.0149	0.0277	0.0278	0.0136	0.0019
	(0.0096)	(0.0135)	(0.0183)	(0.0260)	(0.0216)	(0.0171)	(0.0131)
Log(Capital expenditures)	-0.0106	0.0211	0.0175	0.0100	-0.0432	-0.0391	-0.0057
	(0.0291)	(0.0390)	(0.0459)	(0.0703)	(0.0588)	(0.0465)	(0.0375)
Multiplier Property Tax A	-0.6759	-0.9898	-1.1000	-2.0074	-0.8991	-0.3301	-0.8260
	(0.6162)	(0.9076)	(1.2846)	(2.9253)	(2.4699)	(1.9801)	(1.5870)
Multiplier Property Tax B	0.2232	-0.0322	0.6459	-2.3626	-1.7416	-0.8664	-0.5353
	(0.6461)	(0.9720)	(1.4575)	(3.1574)	(2.6432)	(2.1083)	(1.5447)
Multiplier Business Tax	0.0466	-0.1494	-0.1947	1.9184	2.2041	1.9565	1.6113
	(0.5829)	(0.8289)	(1.2134)	(2.2512)	(1.8135)	(1.3742)	(0.9332)*

Notes: Expenditure variables are expressed in constant 2005 prices. The tax rate multipliers are expressed in percentage points. Standard errors are clustered at the municipality level. There are 397 clusters. The sample includes all municipalities with a relevant population size below 25000 in the time period 1985-2000. There are 6308 observations. All regressions include year-fixed effects, municipality-fixed effects and the same control variables as in table 2.4. *Significant at the 10 percent level, **Significant at the 5 percent level, ***Significant at the 1 percent level.

TABLE 2.15: Results from the discrete complementary log-log model (7)

	(1) D_{it}
Proportion old	0.0082
	(0.0233)
Proportion young	0.0517
	(0.0406)
Seat share SPD	0.7379
	(0.5188)
Seat share CDU	-0.0336
	(0.5274)
Abs. majority	-0.1392
	(0.1250)
Population	-0.000
	(0.000)
Observations	1446

Notes: Standard errors are clustered at the municipality level. There are 394 clusters. The sample includes all municipalities since 1993 that either have an appointed manager or have $D_{it} = 1$ for the first time. The baseline hazard is fully nonparametric. *Significant at the 10 percent level, **Significant at the 5 percent level, ***Significant at the 1 percent level

year 1993 on. Because I have grouped the timing of the switch into yearly data, I estimate a discrete duration model. I estimate the discrete complementary log-log model with a fully nonparametric baseline hazard (Allison, 1982) which is the discrete time representation of a continuous time proportional hazard model. To estimate this model, I construct a panel data set in which the dependent variable is 0 for a municipality if it has not switched yet to the new system and 1 in the year of the transition (it is then the last observation for this municipality).[22] Formally, using the newly constructed panel data set I estimate

$$D_{it} = \alpha + \delta' \mathbf{X}_{it} + \lambda_t + \epsilon_{it} \tag{7}$$

The effects of the observable covariates on the hazard of switching to the new system are shown in table 2.15. In short, I do not find any significant effect of neither the population variables nor the political variables on the hazard of the transition from the system of an appointed manager to the system with

[22] Practically, estimating the discrete complementary log-log model is equivalent to estimating a logit model on the newly constructed panel data set.

an elected manager. Although I can of course not perform such a test for unobservable covariates, the fact that the observable covariates do not have any influence on the timing of the first manager election suggests that the order of the switch was indeed quasi-random.

In section 2.3, I have discussed that also the population weight to calculate the fiscal need of a municipality in the fiscal equalization law in Hesse as well as the salary of the manager are discontinuous functions of population size, but that the weight as well as the salary do not depend on the relevant population size, but on the current population size and the population size lagged by one year, respectively. Although the relevant population size is not perfectly correlated with the assignment variables for the weights in the fiscal equalization law and for the salary of the manager[23], the correlation between these assignment variables might take up some effects that I have attributed to the council size.

To further investigate this issue, I first include the corresponding population weights from the fiscal equalization law into the baseline regression (3) and investigate whether this inclusion alters the council size effect. The fiscal equalization law is depicted in table 2.16. Here, I can use that the thresholds

TABLE 2.16: Population weights in the fiscal equalization law

Current Population (Pop_cur)	Weight
≤5000	107
5000<Pop_cur≤7500	114
7500<Pop_cur≤10000	121
10000<Pop_cur≤15000	124
15000<Pop_cur≤20000	126
20000<Pop_cur≤30000	127
30000<Pop_cur≤50000	129
Pop_cur>50000	130

in this law are not perfectly collinear with the council size law. There are more thresholds than in the council size law and not at all thresholds of the council size law, there is also a change of the population weights in the fiscal equalization law. Second, I investigate whether the change in the salary of the

[23]For example, the correlation between a dummy variable indicating whether the relevant population size has crossed the threshold 10000 and a dummy variable indicating whether the current population size has crossed the threshold 10000, i.e., $Cor(\mathbb{1}_{\{Pop>1000\}}, \mathbb{1}_{\{Pop\_cur>1000\}})$, is only 0.5314 in a 5%-interval around the 10000-threshold of the relevant population size.

manager at the 10000-threshold of the lagged population size captures some effects that I have attributed to the council size effect. To be precise, I include a dummy variable indicating whether the lagged population size is above 10000 into the regression. Again, I can use that the change in the salary of the manager is not perfectly collinear with the change in the council size.

Table 2.17 shows the corresponding results. I restrict the presentation to the case of a third-order polynomial of the relevant population size in the control function as other polynomial orders give very similar results. Column 1 presents the results obtained by including the population weights from the fiscal equalization law into the regression. The general conclusions do not change: The council size effect after the change in the nomination scheme is much larger than the council size effect before the change in the nomination scheme. Column 2 shows the results obtained by including the dummy variable for the salary of the manager. Again, the results do not change. The point estimates stay very similar to the ones before inclusion of the dummy variable and the general conclusions stay the same. Moreover, the coefficients on the population weights are never significant, while the coefficients on the salary of the manager are sometimes significant.[24] Nevertheless, the inclusion does not change the magnitude and significance of the baseline results. Thus, I can be confident that the effects that I have estimated are really due to the council size effect.

2.5 Conclusion

Empirical results on the effects of government fragmentation are diverse. One reason for this diversity might be that empirical studies are typically settled in totally different institutional environments and that some studies even conflate data from different environments. In this chapter, I investigate whether a different institutional setting can indeed change the relationship between fragmentation and fiscal policies. To this end, I compare the council size effect in two different local political settings: when the head of the public administration is elected by the voters and when he is appointed by the municipal council. In contrast to the scarce existing empirical studies on factors that might shape

[24] I do not report these coefficients here due to space considerations.

Table 2.17: Results when controlling for confounding factors in equation (3)

		(1)	(2)
Log(Total Expenditures)	β	-0.0052 (0.0036)	-0.0050 (0.0037)
	$\beta+\gamma$	-0.0076 (0.0042)*	-0.0074 (0.0042)*
Log(Personnel exp.)	β	-0.0042 (0.0037)	-0.0041 (0.0037)
	$\beta+\gamma$	-0.0092 (0.0050)*	-0.0089 (0.0050)*
Log(Material spending)	β	-0.0045 (0.0045)	-0.0028 (0.0046)
	$\beta+\gamma$	-0.0136 (0.0055)**	-0.0112 (0.0055)**
Log(Capital expenditures)	β	-0.0016 (0.0061)	0.0006 (0.0063)
	$\beta+\gamma$	-0.0092 (0.0104)	-0.0059 (0.0105)
Multiplier Property Tax A	β	0.0517 (0.7567)	-0.1517 (0.7647)
	$\beta+\gamma$	0.3652 (0.9051)	0.0986 (0.8893)
Multiplier Property Tax B	β	1.0425 (0.7341)	0.9853 (0.7455)
	$\beta+\gamma$	0.2624 (0.7375)	0.1769 (0.7286)
Multiplier Business Tax	β	-0.4279 (0.3908)	-0.5705 (0.3908)
	$\beta+\gamma$	-0.2862 (0.5393)	-0.4923 (0.5297)
Degree of polynomial		Third	Third

Notes: Standard errors are clustered at the municipality level. There are 397 clusters. Column 1 includes the population weight taken from the fiscal equalization law. Column 2 includes a dummy variable for different salary categories of the city manager. There are 6308 observations.*Significant at the 10 percent level, **Significant at the 5 percent level, ***Significant at the 1 percent level fragmentation effects, in my setting there are two natural experiments. This allows giving the results a causal interpretation.

I indeed find that the institutional background matters for the effect of fragmentation on spending: When the manager is appointed by the council, there is no robust relationship between fragmentation and spending, while there is a significantly negative council size effect when the manager is elected by the voters. Moreover, the most probable explanation for these effects are agency problems between the public administration and the municipal council as I find significant effects for expenditure categories over which the public administration has the most discretion, but not for capital expenditures or for the three local tax rates that the council is free to set. My results are robust to a battery of sensitivity checks.

To conclude, the findings thus suggest that the negative effects of fragmentation found in the recent literature do not need to be a universal phenomenon. It seems to be the case that the sign and significance of the effects of fragmentation depend on the underlying (local) political system and the incentives created from it. The results therefore provide evidence against the validity of empirical studies (in particular cross-country studies) in which data from different political systems are pooled together.

Regarding fiscal policy determination in different forms of local governments, it might be interesting in future research to further study the interplay between the local council and the local executive. Both theoretically and empirically, such kind of evidence is up to now almost non-existent. However, this chapter suggests that already a simple change in the nomination scheme of the public administration (without changing any competencies) is able to change the policies pursued by the council. It is likely that due to the small change in the institutional setting studied in this chapter, my results provide a lower bound of the effects that the institutional framework has on the council size effect. Further investigating the interplay between these local institutions might uncover other interesting relationships. For example, in future work I consider investigating whether a strong manager is able to curb down party politics in the city council. Importantly, the large number of local government reforms (such as the one used in this chapter) in the last decades in Germany allow to give these estimates a causal interpretation.

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Chapter 3

Do government ideology and fragmentation matter for reducing CO2-emissions? Empirical evidence from OECD countries

3.1 Introduction

Given the high stakes involved, climate policy and, more specifically, the reduction of CO2-emissions as the main cause of global warming should rank high on the political agenda of the 21^{st} century. Global climate change is arguably the most important environmental problem that governments have to solve. A particularly problematic feature that significantly impedes international cooperation is that the reduction of CO2-emissions can be regarded as a global common-pool problem with temporal as well as spatial dimensions (see Stavins, 2011), in which it is individually rational to free-ride, given that a sufficient number of other countries cooperate at some point in time to avoid the worst-case scenario. Where the impulse for countries to take a leading position in the emission reduction process should come from, given that it is individually irrational to do so, is thus an important question. Why do some countries invest high efforts into CO2-emission reduction while others behave more like free-riders? Why do some countries make the necessary adjustments earlier while others free-ride at the expense of future generations?

A possible explanation that has received only limited attention so far is government ideology. Political groups might simply favor the reduction of CO2-emissions because they prefer to do so. The emergence of Green parties in several Western democracies suggests that there is indeed a political market for such preferences. For example, the German Greens have promoted strong increases of taxes on fuels once they were for the first time part of a German national government. On the other hand, ideology might also hinder the emission reduction process. It is interesting to observe that the United States has never ratified the Kyoto Protocol, while Canada abandoned it in 2011. Both countries are often argued to be politically more right-wing than most European democracies. Moreover, other political factors might also slow down the reduction process. Government fragmentation might lead to less reduction of emissions because decision costs increase with the number of decision-makers in governments (for example, the number of parties in government), leading to policy inertia.

So far, there is only limited evidence on the political economy of emission reductions. Most research dealing with political economy considerations of emission reductions studies the political economy of international negotiations (see, e.g., DeCanio, 2009) rather than political economy forces in individual countries. There are some noteworthy exceptions. For example, some studies (see Bättig and Bernauer, 2009; Policardo, 2010) examine how democracy can affect climate policy. In a recent contribution, Fredriksson and Neumayer (2013) argue that it is the initial stock of democratic capital rather than the current level of democracy that affects the emission reduction process. Steves and Teytelboym (2013) investigate how several factors like democracy, public awareness, and strength of the carbon-intensive industry lobby influence climate policy.

In this study, I examine how political factors like government ideology and fragmentation influenced the emission reduction process in OECD countries in the time period 1992-2008. A few studies have considered whether government ideology is associated with climate policy (see, e.g., King and Borchardt, 1994; Jahn, 1998; Scruggs, 1999; Neumayer, 2003, 2004), but overall the existing evidence is inconclusive. Econometric methods and data availability have considerably improved in recent years, making a re-evaluation of the effects of government ideology on climate policy particularly attractive. Moreover, I am not aware of any study that has investigated how political fragmentation represented by the number of parties in government influences climate policy.

Using rigorous panel data estimation methods, my findings indicate that (1) right-wing governments are associated with reduced emissions to a smaller extent than center and left-wing governments, (2) emissions rise with the number of parties in government, and (3) minority governments (compared to majority governments) do not have a significant effect on emission reductions. These results indicate that, first, political factors are important determinants of the strength of the reduction efforts in OECD countries and, second, there is an association between political factors and the emission reduction process even in the short run. I also have investigated whether the influence of political factors changes over time and find that political variables have become less important in the 21st century than in the 1990s, which suggests that political platforms have converged over time. Possible reasons could be that electoral competition has increased over time or that the emergence of Green parties has forced mainstream parties to adopt some of their policy positions.

The remainder of this chapter is organized as follows: Section 2 describes the theoretical background of the political economy concepts and their possible application to the field of climate policy, while section 3 presents the data set and my empirical model. Section 4 presents the results and evaluates their robustness. Finally, section 5 concludes the study.

3.2 Partisan theory and government fragmentation

The standard model of electoral competition, the median voter model (see Downs, 1957), crucially assumes that political candidates are opportunistic and thus only care about holding office. Under this assumption, electoral competition leads policies to converge to the position of the median-voter, and the conclusion would therefore be that partisan differences have no effect on emissions. However, an important part of the existing literature on partisan differences assumes that policy outcomes directly motivate political candidates. For example, politicians could be intrinsically motivated by concerns for environmental quality and the fate of future generations. A growing body of literature has examined how personal characteristics of politicians shape policy outcomes (see, e.g., Moessinger, 2014). Whether politicians can make binding commitments to policies announced before an election is an issue when they have ideological preferences over outcomes. If this is not possible, political candidates may choose divergent policies (see Persson and Tabellini, 2000).

Ideological differences between politicians directly lead to the partisan theory. Burke (1770) describes a party as "a body of men united, for promoting by their joint endeavours the national interest, upon some particular principle in which they all are agreed." Thus, parties are a collection of politicians with similar ideological preferences, and policy can at least partly be shaped by these partisan preferences because of the possibility of divergent political platforms.

Accordingly, the classical partisan theory (Hibbs, 1977) as well as the rational expectations (Alesina, 1987) partisan theory has been developed to predict government interventions as the result of which party/parties forms/form the government. Left-wing parties are closer to the labor base and are thus more likely to conduct expansionary policies. Generally, while right-wing parties are more concerned about inflation, left-wing parties focus more on preventing unemployment. As ecological policies might threaten jobs, for example, in heavily-polluting industry sectors, left-wing parties might not promote emission reduction (see Neumayer, 2004). On the other hand, left-wing governments are also known to be more in favor of government interventions. Left-wing parties may therefore promote the restructuring of the economy in the course of climate change to expand control and regulation in the economy (see Buttel and Flinn, 1976). Additionally, the typical clientele of left-wing parties is often more likely to be negatively affected by air pollution, thus providing an additional rationale for why left-wing governments could be in favor of ecological policies.

By contrast, right-wing governments are traditionally closer to capital owners. On the one hand, capital owners may dislike increased taxes and adjustment costs because of emission reduction requirements. In this case, right-wing governments will promote less emission reduction. On the other hand, capital owners might be successful in passing these higher costs onto the consumers through higher prices. This might be especially true in the energy sector because energy demand could be quite inelastic (see Espey, 1996; Belke et al., 2012). Under the assumption that the energy consumer sector constitutes the labor base, reducing emissions might also hit the typical clientele of left-wing parties. Moreover, there is also the possibility that the government does not tax all polluters at the same rate, but resorts to discriminatory taxes. In practice, it is often observed that the household sector is taxed more than the industry sector. Richter and Schneider (2003) show that this can be an efficient policy if labor supply exerts market power.

In addition, rising costs of energy and adjustment processes to change to renewable energy sources might lead to a recession in the national economy. This might hurt both the labor base and capital owners, the former more than the latter to the extent that capital is more internationally mobile than labor. Right-wing voters are more likely to be able to escape from the negative consequences of climate change as well as from economic downturns. To summarize, whether government ideology matters for climate policy and, if so, which type of government tends more strongly to reduce emissions is an open empirical question. Moreover, it is so far not clear whether emission reduction policy can be placed on the typical left-right scale of the political spectrum.

Theoretical research also suggests that the form of government matters for policy. The weak government hypothesis states that weaker, i.e., more fragmented, governments tend to have larger spending and larger budget deficits. Two theoretical foundations of the weak government hypothesis are particularly interesting for the case of the emission reduction policy. First, Alesina and Drazen (1991) present a "war of attrition" model that explains government inaction as a function of the number of decision-makers in the decision-making process. With more than one decision-maker in government, decisions are delayed until the cost of delay becomes too high for one of the decision-makers. This decision-maker in turn has then to bear the largest burden of the negative effects resulting from the decision. A second and somewhat related theoretical argument is the veto player theory proposed by Tsebelis (1995). In this theory, each decision-maker is viewed as a veto player, and a decision would not be possible without his agreement. Thus, decision costs increase with the number of decision-makers, leading to more difficulties to change the status quo. Both theories thus propose that fragmented governments have a tendency to postpone changes in policy.

Some studies have tested whether the number of veto-players in a country matters for environmental policy (see, e.g., Stadelmann-Steffen, 2011; Madden, 2014). However, I am not aware of any study that explicitly examines the effect of the number of parties on climate change policies.

Obviously, from a theoretical standpoint, in the field of climate policy, more fragmented governments are therefore likely to not lower emissions at least in the short run. However, the validity of the weak government hypothesis, and thus the effect of the number of decision-makers in government on CO2-emission reductions, is an open empirical question. While some earlier studies have found the theoretically suggested effects of fragmentation on fiscal variables (see, e.g., Roubini and Sachs, 1989; Perotti and Kontopoulos, 2002), some recent studies (see Pettersson-Lidbom, 2012; Freier and Odendahl, 2012; Garmann, forthcoming) find the reverse relationship, thus making the effect of fragmentation on fiscal variables empirically ambiguous. Moreover, while the existence of policy inertia might predict less adjustment in climate policy (and thus lower reductions), accountability in coalition governments could be lower if voters have more precise knowledge about who is responsible for presumably unpopular policies under single-party than under coalition governments. This may well lead to higher emission reductions with coalition governments.

It also makes sense to include a dummy for minority governments into the analysis, because minority governments might differ in important ways from majority governments. For example, minority governments have to work with varying partners and find compromises that are then often supported by a broad majority in parliament. Often, these policies are more pragmatic and less ideology induced than those proposed by majority governments.

The presented theories assume that governments target policies towards certain voters in order to increase their chances of re-election. An issue that has been neglected so far is that these theories require voters to be informed about the actions of the government, i.e. voters need to know the policy fields that governments can influence and which party can be hold accountable for the chosen policies. As already described by Downs (1957, p. 248) a government that knows that specific voters cannot trace back the effects of policy to the government that has chosen this policy will ignore these voters. The literature on voter ignorance strongly suggests that the immense size and scope of government makes it impossible for most voters to be fully informed about government actions and their consequences (see Somin, 1998; 2006; 2010). This is especially true for policy fields like climate policy that are not major motivators in elections. Thus, there could not be a straightforward connection between party orientation and policy as proposed by the theories presented above as governments may well ignore a large part of the electorate. This would make the effect of government ideology and fragmentation on climate policy even more an empirical issue.

3.3 Data and empirical strategy

3.3.1 Data

I combine CO2-emissions data obtained from Marland et al. (2008) with GDP data from the World Development Indicators (WDI) database of the World Bank to calculate yearly data on CO2-emissions per unit of GDP (kg per 2005 PPP $). Thus, I use emission intensity data (see Camarero et al., 2013) in my analysis because CO2-emissions may well change with the economic situation.[1] I thereby eliminate the possibility that the true effect stems from the economic policy of the political sector and not from climate policy, considering that the general economic situation also depends on the influence of political factors.

I use the yearly growth rates in CO2-emissions per unit GDP as the outcome variable. Panel unit roots strongly indicate that the growth rates are stationary while the results for the raw series are ambiguous. The test results for the raw series strongly depend on how the panel unit root tests are specified. Only when no lags are included, but a deterministic time trend, are the panel unit root tests able to reject the null hypothesis of a unit root at conventional significance levels. By contrast, the null hypothesis can always be rejected for the growth rates, independent of the assumptions made in the panel unit root tests. Note that these results are similar irrespective of which of the commonly used panel unit root tests is chosen. See table 3.1 for test results obtained by using the Im-Pesaran-Shin test (Im et al., 2003).

One might argue that it would be desirable to use more direct measures of climate policy, for example, the amount of taxes imposed on energy use. However, this seems infeasible, as there is a strong heterogeneity in the mix of policy instruments across countries as well as even across different sectors in one country. As already noted above, it is not uncommon to tax the household sector more than the industry sector. Measuring the extent of government

[1]One potential explanation could be the environmental Kuznets curve. However, it is also important to note that the environmental Kuznets curve is highly contested and its general validity not established (see Dinda, 2004; Stern, 2004).

TABLE 3.1: Panel unit root tests

H_0: Unit Root	Deterministic Trend	Lags	stat.	Prob.
(a) Raw series				
	No	0	1.411	0.921
	Yes	0	-5.276	0.000
	Yes	1	-1.279	0.104
	Yes	2	-1.045	0.148
	No	1	3.062	0.999
	No	2	3.000	0.999
(b) Growth rates				
H_0: Unit Root	Deterministic Trend	Lags	stat.	Prob.
	No	0	-10.42	0.000
	Yes	0	-10.55	0.000
	Yes	1	-8.22	0.000
	Yes	2	-3.91	0.000
	No	1	-9.53	0.000
	No	2	-5.58	0.000

Notes: Results are obtained by performing the Im-Pesaran-Shin Test. Other unit root tests give quite similar results.

interventions in the presence of multiple policy instruments and different implementations across different sectors seems difficult if not impossible. Moreover, for example, taxes on energy are not always raised to influence behavior and reduce emissions, but sometimes simply to generate revenues. Note that using the climate policy indicator developed by Steves and Teytelboym (2013), and also adopted by Fredriksson and Neumayer (2013), in my panel data analysis is also not possible as this indicator is available only for a single cross-section. Therefore, I use the extent to which emissions actually have been reduced as a more easily measurable outcome variable (see López and Palacios, 2014).

The countries included in my empirical analysis are the 20 founding members of the OECD except Ireland. Ireland is left out because emission data are available only since 2001. By excluding Ireland, my panel data set is nearly balanced.[2] The founding members of the OECD are characterized by good data coverage and, even more importantly, should follow the same trajectory of climate policy over time. By contrast, the countries that have joined the OECD after its founding are mostly former communist states whose trajectories

[2] As can be seen in table 3.2, for the change in the employment shares of the secondary and tertiary sectors, one data point is missing. Thus, in the regressions with control variables, I lose one observation.

of environmental policy probably differ markedly from those of other OECD countries because of the regime change (see, e.g., Hwang, 2007; Kahn, 2003). Thus, the countries included are Austria, Belgium, Canada, Denmark, France, Germany, Greece, Iceland, Italy, Luxembourg, the Netherlands, Norway, Portugal, Spain, Sweden, Switzerland, Turkey, the United Kingdom, and the United States. The observation period runs from 1992 to 2008.[3] A question that arises from the selection of countries in my empirical analysis is to what extent my results can be generalized to other countries. Because of the concentration on OECD founding members, the sample consists mostly of European countries, of which many are members of the EU. The EU has played an important role in enforcing international climate negotiations. Therefore, it is not guaranteed that my results can be generalized to other highly developed countries such as Australia or Japan. Moreover, it would also be interesting to know whether my results could be generalized to emerging economies such as the BRICS countries. The political systems (including the party systems) and cultures of these countries are very dissimilar to those of the included countries. This heterogeneity would be difficult to capture econometrically.

Table 3.2: Summary statistics

	Mean	Std. Deviation	Min	Max	Obs
Δ*Emissions*	-1.964	5.2	-19.515	20.819	323
Government ideology	3.031	0.845	2	4	323
Number of parties	2.183	1.345	1	8	323
Election	0.266	0.443	0	1	323
Minority government	0.198	0.399	0	1	323
Population growth	0.676	0.463	-0.190	2.530	323
Urban population growth	1.003	0.630	-0.065	2.656	323
GDP growth	2.609	2.071	-5.697	9.363	323
Change share aged 0-14	-0.007	0.010	-0.037	0.014	323
Change share aged 15-64	0.000	0.002	-0.005	0.008	323
Electric power consumption growth	0.017	0.033	-0.086	0.306	323
Change in openness	0.022	0.052	-0.281	0.235	323
Change in tertiary sector	0.009	0.018	-0.086	0.135	322
Change in sec. sector	-0.013	0.028	-0.139	0.124	322
KP ratification	0.359	0.480	0	1	323

Data on political variables come from Woldendorp et al. (2000, 2011) as well as from the database on political institutions (see Beck et al., 2001). Data

[3]The observation period was chosen so as to minimize the influence of German reunification on the results. Growth rates of German emissions in 1991 were of course extremely high because of the additional emissions from East Germany.

on the control variables mentioned in the next section come from the WDI database. In table 3.2, I provide summary statistics for the main variables used in this study. As can be seen, the observation period shows a small tendency towards lower emission intensity, with the mean growth rate slightly below zero. However, some country-year pairs show remarkably large growth rates, with the maximum slightly above 20%.

3.3.2 Empirical strategy

In this section, I explain the empirical models that I estimate - a static and a dynamic panel data model. The static empirical model has the following form:

$$\Delta Emissions_{it} = \sum_{j} \alpha_j PoliticalVariable_{ijt} + \sum_{k} \beta_k \Delta \ln X_{ikt} + \eta_i + \lambda_t + u_{it} \quad (1)$$

where $\Delta Emissions_{it}$ denotes the yearly growth rate of emissions per unit GDP in country i and year t, η_i is a country-fixed effect[4] that is able to take up time-invariant effects of not only history, climate, or topography, for example, but also political factors such as the electoral system[5] and λ_t is a year-fixed effect that should take up general global trends in climate policy. u_{it} is an error term further discussed below.

$\sum_{j} \alpha_j PoliticalVariable_{ijt}$ contains four political variables. First, as an indicator for government ideology, I follow the related literature (see, e.g., Chang and Berdiev, 2011; Potrafke, 2012; Klomp and de Haan, 2013) and use an index proposed by Woldendorp et al. (2000) that places government ideology on a left-right scale from 1 to 5, where 1 indicates right-wing dominance, and is

[4]The usage of a fixed-effects model can be motivated by a Hausman test that rejected the null hypothesis that the fixed-effects model does not significantly differ from the random-effects model (p-value: 0.000). In this case, the fixed-effects estimator is consistent while the random-effects estimator is not.

[5]Government fragmentation is closely related to the electoral system, as proportional election systems are typically associated with coalition governments while majoritarian election systems often lead to single-party governments. Hence, it is important to note that the electoral systems of the countries in the sample are almost time-invariant over the sample period (only Italy changed the election system in the time period under investigation; my results are robust to taking this into account by means of a dummy variable). Thus, the country-fixed effects capture the effects of the electoral system such that it is possible to estimate the effects of fragmentation on emissions without conflating it with the effect of the electoral system on emissions.

given if the share of seats of governing right-wing parties in the cabinet and in parliament is greater than 2/3. The value 2 is given if this share is between 1/3 and 2/3, while 3 is given if the share of center parties is larger than 1/2 or if left-wing and right-wing parties form a government that is not dominated by either side. As the index is symmetric, 4 and 5 indicate left-wing governance analogously.[6] However, in the period of investigation, the values 1 and 5 of the index do not occur (see summary statistics in table 3.2). Thus, there have been no forms of extreme right- or left-wing dominance.

If the coefficient on the government ideology index were negative, this would mean that left-wing governments are associated with reduced emissions compared to more right-wing governments. As the second political variable, I include the number of parties in government in the year under consideration. Third, I include a dummy variable that is one in election years and zero otherwise to control for the influence of the electoral cycle on climate policy. Fourth, I use a dummy variable that is equal to one if a minority government is in place and zero otherwise. A minority government is in place if the governing parties do not possess more than 50% of the seats in parliament. In case of a minority government, however, it might obviously be problematic that government ideology could overlap in terms of seats in parliament. I will discuss this problem further in the robustness section and find that my results for government ideology still hold if all minority government observations are excluded.

$\sum_k \beta_k \Delta \ln X_{ikt}$ contains control variables shown in the literature as strong determinants of CO2-emissions (see Friedl and Getzner, 2003: Lantz and Feng, 2006; Halicioglu, 2009; Sharma, 2011; Camarero et al., 2013). I include the growth rate of GDP, the growth of total population, the growth in the percentage of urban population, the change in the share of people aged between 0 and 14 and between 15 and 64, the growth rate of energy consumption (proxied by the electric power consumption per capita as in Sharma, 2011), the change in trade openness (measured as the sum of exports and imports as a percentage

[6]If there is a change of government within a year, I code the year according to the government that was in office for the longer period.

of GDP) to measure the influence of globalization and the change in the employment shares of the secondary and tertiary sectors.[7] Moreover, I include a dummy variable indicating the ratification status of the Kyoto Protocol (KP) in the country-year pair under consideration. Aichele and Felbermayr (forthcoming) have shown that the ratification of the Kyoto Protocol has affected CO2-emissions.[8]

I use standard errors proposed by Driscoll and Kraay (1998) to take serial correlation within countries as well as cross-sectional correlation across countries into account (see Hoechle, 2007).[9] Driscoll-Kraay standard errors are the natural extension of the Newey-West standard errors (see Newey and West, 1987) to the case of cross-sectional dependence.[10] The policy field of climate change is a typical field in which one would predict cross-sectional correlation to be very important because the behavior of all other countries is likely to influence a single country's behavior. Climate change mitigation is a global public good. On the one hand, when everybody else abates emissions, there might be a large incentive to free-ride. On the other hand, if no country restricts its emissions, then there will be also no incentive to abate, because in this case the worst-case scenario will not be avoidable. Moreover, there might also be cross-sectional dependence through the existence of emission reduction agreements. Accordingly, the Pesaran test (see Pesaran, 2004) rejects the null hypothesis of cross-sectional independence at the 5%-level (p-value: 0.026). Thus, taking into account cross-sectional dependence is important. The average value of the off-diagonal elements in the cross-sectional correlation matrix is 0.242, which is a high value. Hoechle (2007) shows that Driscoll-Kraay standard errors clearly outperform other types of commonly used standard errors in the presence of cross-sectional dependence even if the time period of the panel is relatively short.

[7] Note that my results are robust to the inclusion of the urban population percentage and trade openness in level form as in Sharma (2011). Panel unit root tests give no definitive indication of whether these variables are stationary or not.

[8] See also Sauquet (2014) who studies interdependence between countries in the Kyoto ratification process.

[9] By contrast, Newey-West or clustered standard errors would only take serial correlation within a country into account.

[10] The lag length of the MA-term in the calculation of the Discroll-Kraay standard errors is set to 2 by means of a heuristic criterion as presented in Hoechle (2007). Hoechle (2007) notes that this heuristic criterion often sets too short a lag length. I checked whether my results are sensitive to choosing longer lag lengths but found that this is not the case.

Besides the static fixed-effects model, I also estimate a dynamic panel data model, motivated by the idea that the change in emissions might exhibit some persistence over time, for example, because of the possibility of partial adjustments in emission levels. The dynamic panel data model takes the following form[11]:

$$\Delta Emissions_{it} = \sum_j \alpha_j PoliticalVariable_{ijt} + \sum_k \beta_k \Delta \ln X_{ikt} + \gamma \Delta Emissions_{it-1} + \eta_i + \lambda_t + u_{it}$$

The model contains the same political variables as well as the same control variables as the static model specified in (1). It is well known that the common fixed-effects estimator is biased in a dynamic panel data model with a relatively short time dimension as in my case ($T = 17$). In those approaches that consider the bias, one can distinguish between instrumental variables estimators and estimators that directly correct for the bias. Estimators from the first group, for example, the Arellano-Bond (1991) and the Blundell-Bond (1998) estimator, are designed for small-T, large-N panels. Thus, these estimators are likely to be biased in my panel with relatively small $N = 19$. Therefore, I use, from the group of the estimators that directly correct for the bias, the estimator proposed by Bruno (2005a, 2005b), which has been shown to have good properties for a relatively small N.[12]

3.4 Results

3.4.1 Baseline results

Table 3.3 shows the baseline results. First, to evaluate whether my regressions

[11] I have checked the significance of a higher number of lags in the dependent variables using the Arellano-Bond estimator, but the specification with only one lag proved to be optimal. Higher lags were not significant.

[12] I choose the Blundell-Bond estimator as the initial estimator that has to be specified to apply Bruno's estimator. However, using the estimator proposed by Anderson and Hsiao (1981) or the Arellano-Bond estimator gives the same results. Moreover, standard errors have to be bootstrapped. I choose 1000 bootstrap repetitions. Note that the bootstrapped standard errors are not able to account for cross-sectional dependence. However, as noted by Roodman (2009), the problem of cross-sectional dependence in dynamic panel data models can at least be reduced by including year-fixed effects. I am not aware of any estimator for the standard error in dynamic panel data models that considers cross-sectional dependence.

TABLE 3.3: Results from the baseline model

VARIABLES	(1) FE	(2) DYN	(3) FE	(4) DYN
Government ideology	-0.769***	-0.884**	-0.791***	-0.905**
	(-3.325)	(-2.204)	(-3.148)	(-2.178)
Number of parties	0.726*	0.667	0.527	0.469
	(1.985)	(1.548)	(1.615)	(1.039)
Minority government	0.858	0.860	0.401	0.414
	(0.565)	(0.632)	(0.223)	(0.280)
Election	0.228	0.123	0.151	0.061
	(0.288)	(0.192)	(0.196)	(0.093)
Population growth			0.024	-0.225
			(0.011)	(-0.074)
Urban population growth			0.509	0.751
			(0.215)	(0.317)
GDP growth			-0.259	-0.298
			(-1.577)	(-1.492)
Change in share aged 0-14			-45.266	-41.001
			(-1.193)	(-0.669)
Change in share aged 15-64			167.405	198.379
			(1.077)	(0.796)
Electric power consumption growth			19.393	16.555
			(1.604)	(1.468)
Change in openness			-0.584	-2.943
			(-0.115)	(-0.413)
Change in share employed in secondary sector			-5.246	-0.201
			(-0.498)	(-0.016)
Change in share employed in tertiary sector			16.606	15.714
			(1.166)	(0.779)
KP ratification			1.246	1.281
			(0.716)	(0.740)
$\Delta Emissions_{it-1}$		-0.253***		-0.245***
		(-4.479)		(-4.237)
Observations	323	304	322	304
Number of countries	19	19	19	19
p-value F-test model	0.102	0.000	0.000	0.003
p-value F-test controls	-	-	0.000	0.489

Notes: Outcome variable is growth rates of emissions per unit GDP. Driscoll-Kraay standard errors are used in columns (1) and (3). In columns (2) and (4), bootstrapped standard errors are used. t-statistics in parentheses. *Significant at the 10 percent level, **Significant at the 5 percent level, ***Significant at the 1 percent level.

can explain a part of the variation in the change of emission intensity, I have performed F-tests for each regression testing for the joint significance of all included variables. As can be seen, with the exception of the first model where the p-value is 0.102, the F-tests always reject the null hypothesis that there is no joint effect of the chosen explanatory variables. Thus, the chosen right-hand side variables have some explanatory power for the change in emission intensity.

Column 1 presents the static fixed-effects model without control variables. Both the index of government ideology and the number of parties in government are significant at conventional levels. The coefficient of the government ideology index implies that an increase of one unit in the index (i.e., going one step further to the left of the political spectrum) yields a growth rate of emission intensity that is 0.769% lower. By contrast, one additional party in government increases the growth rate of emission intensity by approximately 0.726%. The election year effect is small and insignificant. Minority governments increase growth rates by 0.85%, but this effect is insignificant at conventional levels. Column 2 shows the results from the dynamic model without control variables. The main conclusions from the static model carry over to the dynamic model. Most coefficients have approximately the same size as in the static version. Again, the results point to government ideology and fragmentation as important political determinants of emissions, although the effect of fragmentation is marginally insignificant. Moreover, column 2 shows that the dependent variable indeed exhibits some persistence: the coefficient on the lagged value is highly significant. This suggests that the emission intensity is only partially adjusted.

Column 3 shows the estimates from the static model with control variables. Using an F-test, I first examined whether the included control variables are jointly significant as a group. For each static model this is the case: The null hypothesis can always be rejected at conventional significance levels. Thus, the control variables add some explanatory power to the baseline models. For the dynamic model, however, the control variables are not jointly significant. A potential explanation could be that, if the control variables have a rather low within-variation, much of their effects is captured by the lagged dependent variable that is included.

Of the individual control variables, two seem to be the most important, though marginally insignificant, in explaining the growth rates of emission intensity:

First, the estimates suggest that a one-percent-higher growth rate of GDP decreases the growth rates of emission intensity by 0.26%. Second, emissions rise, as expected, with the amount of electric power consumption. The insignificance of the control variables might be explained by the fact that because of the inclusion of country-fixed effects, the coefficients of the control variables measure the effects of changes in these variables rather than the level effects and within-variation in some of the variables is rather low.

Importantly, the political variables in column 3 lead to the same conclusions as in columns 1 and 2: a more left-leaning government yields significantly lower growth rates of emission intensity while fragmentation represented by the number of parties in government has a positive and marginally insignificant effect. As a last baseline specification, I present the dynamic panel data model with control variables in column 4. Again, the political variables are of a similar magnitude as before. The control variables show that emission growth rates significantly decline with an increase in the GDP growth rate. Taken together, my analysis suggests that two political factors play an important role in explaining the growth rates of emission intensity: Growth rates decrease with a shift to the left of the political spectrum and (to a less significant extent) increase with more parties in government.

3.4.2 Have political platforms converged? Different policy periods

Public attention given to environmental policy in general and the reduction of CO2-emissions in particular has arguably not been constant over the years, but has increased. The best examples for this are the Nobel Peace Prize in 2007 given to Al Gore and the Intergovernmental Panel on Climate Change as well as the massive attention that the Stern Review has gained from the media as well as academia in 2006. Because of this increasing attention, it is possible that the median voter in the average country in my sample has become more informed and now gives the field of climate policy a larger weight in his vote decision. More information about the potential consequences of climate change might also reduce voter polarization regarding this topic. Moreover, electoral competition might have increased over time. Green parties now play an important role in several countries in my sample, and the threat that Green parties might "steal" potentially decisive votes from the traditional parties might have forced the traditional parties to adopt some political positions of the ecological parties. Moreover, in proportional election systems, Green parties are sometimes important coalition partners, causing mainstream parties also to adjust their political platforms. All of these developments might lead to converging political platforms regarding climate policy over time. Political platforms may have converged, as hypothesized by Laurency and Schindler (2011), also because of international climate agreements that might limit the maneuverability of political parties at the national level.

To test for the idea of convergence of political platforms over time, I split the sample into the time periods 1992-1999 and 2000-2008, and investigate how political factors have influenced the growth rates of CO2-emissions in both sub-periods.[13] If my hypothesis that climate policy has lost its polarization across political parties is true, then I should observe that political factors have more influence on emissions in the first time period. Table 3.4 shows the results for the first and second time period, respectively. As can be seen, the influence of government ideology in the first time period is approximately three times as large as in the second time period. The influence of the number of parties is even close to zero in the second time period. Thus, climate policy really seems to have lost its polarization, and political platforms have converged in the average country in my sample. However, it should be noted that there might exist important outliers: In the United States, political platforms seem to have diverged rather than converged over time, presumably because the US has not reached a consensus on the reduction of CO2-emissions.

3.4.3 Robustness checks

In this section, I evaluate the robustness of the results. A first issue that needs to be discussed is that up to now, I have assumed that climate policy can be placed on a left-right scale of the political spectrum and that there exists a linear relationship between reducing emissions and the index of government ideology. The same linearity assumption has also been made for the relationship between

[13] I have checked whether my results are sensitive to the concrete choice of the cut-off point between both time periods by extending and shortening the first time period by one year (i.e., the second time period is then either shortened or lengthened by one year, respectively). However, this does not affect my results.

TABLE 3.4: Results for different time periods

VARIABLES	(1) FE	(2) DYN	(3) FE	(4) DYN
(a) Time period 1992-1999				
Government ideology	-1.109***	-1.108	-0.870*	-1.182
	(-2.956)	(-1.512)	(-1.908)	(-1.409)
Number of parties	1.324**	1.527*	1.557***	1.572
	(2.338)	(1.856)	(3.571)	(1.631)
Minority government	1.634	1.557	2.497	2.807
	(0.818)	(0.581)	(1.107)	(0.939)
Election	-0.148	-0.341	0.133	-0.149
	(-0.189)	(-0.353)	(0.188)	(-0.144)
$\Delta Emissions_{it-1}$		-0.335***		-0.336***
		(-3.862)		(-3.759)
Observations	152	133	151	133
Number of countries	19	19	19	19
Controls	NO	NO	YES	YES
(b) Time period 2000-2008				
Government ideology	-0.411*	-0.432	-0.340	-0.268
	(-1.826)	(-0.681)	(-1.271)	(-0.402)
Number of parties	-0.074	-0.390	0.137	-0.209
	(-0.212)	(-0.516)	(0.327)	(-0.267)
Minority government	-6.140***	-6.313**	-6.671***	-6.659**
	(-4.157)	(-2.228)	(-4.705)	(-2.230)
Election	0.867	0.885	0.744	0.656
	(0.780)	(1.068)	(0.661)	(0.763)
$\Delta Emissions_{it-1}$		-0.182**		-0.220**
		(-2.014)		(-2.497)
Observations	171	152	171	152
Number of countries	19	19	19	19
Controls	NO	NO	YES	YES

Notes: Outcome variable is growth rates of emissions per unit GDP. Driscoll-Kraay standard errors are used in columns (1) and (3). In columns (2) and (4), bootstrapped standard errors are used. t-statistics in parentheses. Columns 3 and 4 contain the same control variables as columns 3 and 4 of Table 3 (coefficients are not shown here). *Significant at the 10 percent level, **Significant at the 5 percent level, ***Significant at the 1 percent level.

the number of parties and the change in emissions. To relax these assumptions, I change the baseline specification by including dummy variables for whether the government ideology index takes the value 3 or 4 (note again that the values 1 and 5 do not appear in my estimation period, while 2 serves as the reference category). Analogously, I employ dummy variables to differentiate between the categories "2 or 3 parties in government" and "4 or more parties in government" (the case of only one party in government serves as the reference category).

As can be seen from table 3.5, center and left-wing governments are associated with reduced emissions to a larger extent than right-wing governments, but, surprisingly, center governments are associated with reduced emissions to a larger extent than left-wing governments. Thus, climate policy can obviously not be placed on the typical left-right scale of the political spectrum (as would, for example, be the case for redistribution and tax rates). Moreover, table 3.5 shows that a government with 2 or 3 parties in a coalition has the same degree of association with the growth rates of emissions as a government with 4 or more parties. Both kinds of governments have higher growth rates than a single-party government. Thus, the decisive factor when it comes to fragmentation does not seem to be the quantitative aspect, i.e., the number of parties in government, but rather the qualitative aspect, i.e., whether the country has a single-party or coalition government. As this less restrictive specification seems to be more appropriate than the baseline specification, the following robustness checks are performed on the less restrictive specification.[14]

A second issue that needs to be discussed is the timing of the effects. It is natural to believe that the emission reduction process is subject to several technological constraints that might not be changeable in the short run. Thus, the climate policy of the political sector might only pay off with some time lag. To consider this possibility, I have estimated a distributed lag model. Specifically, I have included, in addition to the year t effect, the year $t-1$ and year $t-2$ effects of the political factors. However, only the year $t-1$ effect of "2 or 3 parties in government" has also been significant. Thus, there might be some small evidence for lagged responses. However, taking the lagged responses into account, the results show that including lags changes little the overall pattern of the effects of the political variables. Surely, a problem is that due to the rather small number of observations, a distributed lag model is estimated quite

[14]However, the results of robustness checks for the baseline models are similar.

TABLE 3.5: Specification Check

VARIABLES	(1) FE	(2) DYN	(3) FE	(4) DYN
Center government	-1.719***	-1.976**	-3.146***	-3.588***
	(-3.472)	(-1.982)	(-3.089)	(-3.046)
Left-wing government	-1.197**	-1.441*	-1.476***	-1.754**
	(-2.692)	(-1.722)	(-3.114)	(-1.999)
2 or 3 parties	2.820	2.653	1.321	1.048
	(1.624)	(1.643)	(0.590)	(0.589)
4 or more parties	3.146*	2.994	2.630*	2.503
	(2.097)	(1.430)	(1.875)	(1.125)
Minority government	1.101	1.069	0.659	0.670
	(0.641)	(0.768)	(0.348)	(0.449)
Election	0.211	0.108	0.142	0.051
	(0.277)	(0.169)	(0.185)	(0.078)
Population growth			0.407	0.206
			(0.184)	(0.068)
Urban population growth			0.638	0.926
			(0.275)	(0.389)
GDP growth			-0.287	-0.335*
			(-1.628)	(-1.664)
Change in share aged 0-14			-6.959	1.574
			(-0.166)	(0.025)
Change in share aged 15-64			370.628*	428.708
			(1.822)	(1.635)
Electric power consumption growth			21.809*	19.284*
			(1.814)	(1.706)
Change in openness			-1.963	-4.483
			(-0.466)	(-0.623)
Change in share employed in secondary sector			-3.946	1.310
			(-0.395)	(0.105)
Change in share employed in tertiary sector			18.515	17.843
			(1.318)	(0.879)
Kyoto			2.083	2.335
			(0.953)	(1.271)
$\Delta Emissions_{it-1}$		-0.252***		-0.251***
		(-4.435)		(-4.374)
Observations	323	304	322	304
Number of countries	19	19	19	19
p-value F test model	0.000	0.000	0.000	0.000
p-value F test controls	-	-	0.000	0.220

Notes: Outcome variable is growth rates of emissions per unit GDP. Driscoll-Kraay standard errors are used in columns (1) and (3). In columns (2) and (4), bootstrapped standard errors are used. t-statistics in parentheses.*Significant at the 10 percent level, **Significant at the 5 percent level, ***Significant at the 1 percent level.

imprecisely. Therefore, future research on this issue might be important. To summarize, political factors such as government ideology and fragmentation seem to have a short-run association with the development of emissions.

A shortcoming of the proposed government ideology index could exist because of the presence of minority governments in the sample. For example, assume that a center-right government is in office. In the case of a minority government, this governing coalition has no majority in parliament, and it could even be the case that an opposing left-wing party has a larger seat share in parliament. Nonetheless, because the center-right minority government is likely to fill most of the cabinet seats, under the assumption that the governing right-wing party has more than 1/3 of the seats in parliament and cabinet the government ideology index would assume the value 2. However, the opposing left-wing party might have more power in parliament than the governing coalition. It is thus questionable whether political power is adequately captured in the presence of minority governments. To rule out that my results are driven by the presence of minority governments, I also have run all regressions excluding all minority government observations. However, the results for government ideology did not differ.

My results could be dependent on idiosyncratic circumstances in single countries. To rule this out, I test whether the point estimates are sensitive to excluding single countries from the estimation sample. For government ideology, the point estimates are slightly sensitive when I exclude Luxembourg from the sample. However, the point estimates remain at a quantitatively large magnitude: in the full specificaiton, center as well as left-wing governments are associated with more emissions abatement than right-wing governments, to the extent of approximately 1.6% and 1%, respectively, even when Luxembourg is excluded. For the number of parties, the point estimates are slightly sensitive when I exclude France or Iceland (for the effects of 2 or 3 parties) and Sweden (for 4 and more parties in government). The effect of an exclusion of these countries on the results concerning the number of parties is not surprising as these countries have the largest within-variation in the number of parties in my sample. However, even when one of these countries is excluded, the point estimates are still of an economically relevant magnitude.

The change in emission intensity shows a considerable variation. For example, the maximum growth rate of emissions is more than four standard deviations away from the mean of the growth rates. Therefore, it is interesting to investigate how the results would change if these large fluctuations were removed. In non-reported robustness checks, I have re-run the regressions in tables 3.3 and 3.5 using only those changes in emission intensity within two standard deviations from the mean. The government ideology index stays significant and has the same size as before. Thus, conclusions regarding government ideology do not change. However, the effect of the number of parties is sensitive to excluding outliers. For the specification used in table 3.3, the coefficient declines in size and is insignificant. For the less restrictive specification of table 3.5, only the effect of "4 or more parties in government" is significant. This suggests a need for caution regarding the effects of the number of parties on emissions.

Reverse causality and omitted variable bias are natural concerns in any panel data study such as this. However, while I have no valid instrumental variable for the political variables under consideration to rule out such concerns completely, I think that reverse causality is only of minor importance in my setting. Reverse causality would be a problem if the amount of emissions influences which parties are elected. Although the field of climate policy has undoubtedly gained more attention in the last years, climate policy is not likely to have been a significant issue in the electoral campaigns in most countries of my sample. Thus, governments are also unlikely to have been turned out of office because of the climate policy they pursue. For example, List and Sturm (2006) call environmental policy a "secondary policy issue". Surely, policy areas like spending on social affairs or foreign policy still have greater importance in the eyes of the voters. Additionally, as discussed above, ignorance of the voters makes reverse causality unlikely because for reverse causality to be an issue it would be necessary that a large part of the electorate is well informed about the field of climate policy. Moreover, I am not aware of any anecdotal evidence (at least in the time period under consideration) where climate policy has decided the national election of a country in my sample.

Omitted variable bias could be a more relevant concern. Since I include country-fixed effects, time-invariant factors cannot influence the results. However, emissions could likely be influenced by a multitude of time-varying factors that are not always measurable or for which data cannot always be obtained. A good example is the sector composition of a country's economy for which, because of data limitations, I was only able to use rather crude proxies. It may be possible that a specific kind of government influences the speed of structural adjustments in a country's industry and that this in turn affects the amount of emissions. For example, left-wing governments could be more willing to curb down the influence of carbon-intensive industries in the economy, resulting in lower emissions. To better proxy for the sectoral composition of a country, in non-reported robustness checks, I have additionally included the share of manufacturing in GDP as a control variable. However, the results were not affected by this inclusion.

Moreover, the emission intensity depends on how electric power is generated, and a specific government might be more inclined to promote the substitution of natural gas for coal. In this case, lower emission intensity would in fact be driven by substitution processes in the input factors for electric power. As a last example, car ownership could be an important variable that might influence the relationship between emissions and government ideology. Car owners are more likely to be right-wing voters, whereas left-wing governments often support alternative transport systems. As mentioned in the introduction, the German Greens have promoted strong increases in taxes on fuels, presumably because their own clientele tends to more strongly use public transport systems like trains or busses. To summarize, it is not possible to establish causation beyond doubt from the estimated regressions. Neither can causation be ruled out entirely. The statistical influence of government ideology and fragmentation is robust even when a number of important control variables and country-fixed effects are included in the regressions. Since it is important to know which political factors can influence climate policy, it should be evaluated in future research whether the results in this study continue to hold such that there is indeed a causal effect of political factors on the increase in CO2-emissions.

A possibility to overcome endogeneity issues in the investigation of the effects of party differences would be to use a Regression Discontinuity analysis in which close elections can be regarded as similar to random experiments and can thus be used to generate exogenous variation in the party affiliations of governments (see Lee et al., 2004; Pettersson-Lidbom, 2008; Folke, forthcoming). Regression Discontinuity analyses have also been implemented in the environmental policy literature (see Fredriksson et al., 2011; Folke, forthcoming). While desirable, the usage of Regression Discontinuity methods is not possible in the present study for at least two reasons: First, Regression Discontinuity methods require a large number of data points close to the thresholds that are typically not available in cross-country studies. Second, comparable data on the margin of victory in different electoral systems do not exist.

Nonetheless, it might be important to control for the extent of political competition in the regressions as has been done, for example, by List and Sturm (2006). It seems plausible that a government under stiff political competition might be more reluctant to implement potentially costly environmental policies, whereas a government that can easily win the next election can afford to risk losing some votes as a result of presumably unpopular policies. Because appropriate data on the margin of victory across countries do not exist, I use the level of electoral competitiveness measured as one minus a Herfindahl index calculated as the sum of the squared seat shares of all parties in parliament based on the Database of Political Institutions (see Beck et al., 2001). However, the inclusion of this variable did not change my results. Because of space considerations, I omit the results of this robustness check here.

3.5 Conclusion

This study investigated how political factors like government ideology and government fragmentation have influenced climate policy. Specifically, I investigated whether government ideology and government fragmentation have contributed to the growth rates of CO2-emission intensity in OECD countries in the time period 1992-2008. I found that both government ideology and government fragmentation, the latter represented by the number of parties in government, play an important role. Center governments are associated with more emissions abatement than left-wing governments, while the latter are associated with more emissions abatement than right-wing governments. Thus, climate policy cannot be classified on a typical left-right scale of the political spectrum. As for government fragmentation, I found that the qualitative aspect (i.e., whether the government is formed by a single-party or a coalition) is more important than the quantitative aspect (i.e., the number of parties in government).

Moreover, the study found that political platforms have converged in recent years. This might be due to better-informed voters or through increasing electoral competition. Green parties have become more important, and mainstream parties might have adopted some of their positions. Furthermore, international agreements might also have contributed to converging policy platforms over time. As the period under consideration in this study is characterized by a growing awareness of the importance of climate policy, a natural extension of this study would be to investigate how political factors have contributed to the increase in CO2-emissions in earlier time periods. It would be interesting to know whether political factors play any role in an era with little or no awareness about the potential consequences of rising emissions, especially with regard to the rising emission levels in developing countries. It is also important to study whether my results can be generalized to emerging economies such as the BRICS countries, particularly because these countries account for a large share of the global CO2-emissions. Additionally, an interesting extension of this study would be to investigate to what extent political factors have influenced multilateral environmental cooperation. At present, there exist more than 1000 multilateral agreements dealing with not only climate change but also other environmental problems. It would be interesting to observe whether the willingness to sign these agreements depends on government ideology and fragmentation.

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Chapter 4

Does globalization influence protectionism? Empirical evidence from agricultural protection

4.1 Introduction

Globalization is supposed to heavily influence the parameters under which policies are chosen. This is true in many economic sectors, and the food sector is no exception. Already in 1997, Traill (1997) has written that the food sector displays trends towards globalization and its consequences. For example, he observes the tendency of agricultural production to involve more stages, and that these stages increasingly take place in different countries. Food manufacturers assemble ingredients which are now sourced supra-nationally. Moreover, food trade is strongly growing and it is more often of an intra-industry type. This also suggests that another effect of globalization on the food sector could be a convergence in food consumption.

Globalization thus changes incentives for national policy-makers. In many sectors, the resulting import competition might put domestic industries under pressure, leading potentially to unemployment and income losses. In a recent contribution, Autor et al. (2013) provide compelling evidence that increasing import competition from China has indeed led to unemployment and lower wages in the United States. According to Rodrik (2011), people will demand compensation against risk that results from the exposure to international forces due to globalization, and re-election concerned politicians might thus be incentivized to strengthen protectionist policies in those sectors that have a comparative disadvantage (see Hillman, 1982). It is a stylized fact in the food sector that there is a negative relationship between support given to the agricultural sector and the agricultural comparative advantage. Moreover, protection might be especially strong in the food sector because interest groups are often well-organized. As Anderson et al. (2013) note, import competition is thus the most articulated argument for the restriction of food trade.

For the field of food policy, there are additional motivations for protectionist policies than just to insulate domestic markets from import competition in the course of globalization. An often articulated argument for agricultural support given to domestic farmers is that it should ensure a high food quality. It is interesting to observe that the average export ratios of developing countries for milk and meat - two agricultural products that arguably are most subject to quality concerns - have only minimally increased over the last 50 years and are still on a relatively low level (see von Braun and Díaz-Bonilla, 2008). Von Braun and Díaz-Bonilla (2008) note that this is due to trade protection as well as sanitary measures which make these products behave like non-tradables. A further good example is that the European restrictions on imports of beef from the United States were justified by politicians on the ground of health risks from hormones given to US cattle (see Hillman, 2003).

Moreover, a motivation for domestic food market support could be to avoid importing of food price spikes and volatility from international markets (see Gilbert and Morgan, 2010).[1] Food price spikes and volatility could be seen as especially problematic in developing countries in which food is a major consumption item and thus expenditures on food often comprise a large amount of the citizen's budget. Tadesse et al. (2014) show that increased market linkages can contribute to higher food price volatility. Market integration via globalization could enforce the transmission of world prices to local markets. In this case, politicians might want to insulate domestic markets to prevent this transmission.[2]

As a last example for a potential motivation of increasing domestic food market

[1]On the other hand, if price volatility does not originate from the international, but from the domestic market (for example because of region-specific weather shocks), international trade could be a powerful engine to reduce price volatility (see FAO et al., 2011).

[2]It should be noted that if a sufficient number of countries act in this way, such a policy may exacerbate volatility in the residual world market (see Gilbert and Morgan, 2010).

support in the course of globalization, especially in developed countries it is often argued that agricultural support should allow to sustain traditional rural life and to avoid too extreme depopulation of the hinterland (see Bagwell and Staiger, 2001). To the extent to which globalization reinforces urbanization tendencies, policy-makers might also be motivated to support agriculture in the course of globalization to counteract such developments.

However, there are also theoretical arguments for less protection of the domestic food market in the course of globalization. The neo-classical literature shows that free trade can be globally efficient (see, e.g., Krugman et al., 2012). Moreover, agricultural support is often argued to be implemented because it secures a country's self-sufficiency and thus a stable national food supply. Globalization, however, might lower the necessity of self-sufficiency. Self-sufficiency has been mainly important in times in which countries were dependent on a few major trading routes and/or trading partners. In times of globalization characterized by improved transport possibilities, it seems hard to defend this argument. Transportation that is now to a significant degree realized via air clearly reduces the dependence on single trading routes or partners. Because there exist both arguments for more and less agricultural protection in the course of globalization, the effect of globalization on the protection of domestic food markets is an open empirical question.

In this article, I examine the effect of globalization on agricultural protection empirically. Although globalization is heavily studied (see, e.g., Nissanke and Thorbecke, 2006; Dreher et al., 2008; Arribas et al., 2009; Bergh and Nilsson, 2010)[3], this is the first paper that investigates whether globalization, which is a process that has also been reinforced by less trade restrictions (but also by technological improvements), does on the other hand again lead to more protection.[4]

The focus on food policy can be motivated by several considerations. First, as already mentioned above, the agricultural sector is influenced by the process of globalization. Second, a new data set compiled by the World Bank (see

[3]See Potrafke (forthcoming) for a recent survey of the literature.

[4]Aidt and Gassebner (2010) investigate the effect of autocracy on trade flows. In some specifications, they also include the KOF-index of globalization. However, in contrast to my paper, they are interested in explaining realized trade flows rather than trade restrictions set up by the government such as for example agricultural support. Moreover, also in contrast to my paper, they do not deal with the potential endogeneity of globalization.

Anderson and Valenzuela, 2008; Anderson and Nelgen, 2013) allows a very accurate measurement of agricultural protection that moreover is available for a wide range of countries and years. Third, agricultural policy is a very important policy field. Approximately 75% of the world's poorest people depend at least indirectly on agriculture as their main source of income (World Bank, 2007). The Common Agricultural Policy of the European Union accounts for more than 40% of its budget. The political importance of agricultural policy can also be gauged by the fact that the Doha round could until now not come to a definitive agreement because the participants resisted to liberalize the agricultural market. Fourth, the agricultural sector is subject to heavy-handed government interventions not only in single regions, but throughout the world. Not only high-income, but also developing countries have an incentive to insulate their domestic food markets in the course of globalization due to agricultural overproduction and export dumping of the developed countries (see Watkins, 2008).[5] Despite significant market-opening reforms in several sectors, strong anti-trade policies in the agricultural sector persist. Olper and Raimondi (2013) thus call the agricultural sector "an ideal case for studying the political economy of public policies".

To study the effects of globalization on agricultural protection, I use a large cross-country data set and the KOF-index as a proxy for globalization. The KOF-index is widely employed in empirical studies because it has the advantage of taking all possible dimensions of globalization into account. I will estimate a system GMM model that does not only allow for persistence in agricultural support over time, but also holds the promise that it is - if well specified - able to generate internal instruments for globalization that can help to overcome identification issues in the form of endogeneity and measurement error even when external time-varying instruments are not available. As it is reasonable to believe that not only globalization influences trade restrictions as for example domestic food market protection, but that also trade restrictions can influence the globalization process, endogeneity is an important issue that must be addressed.[6] I find that globalization significantly increases agricultural protection. Considering

[5] Historically, developing countries have taxed rather than subsidized agriculture. However, since the beginning of the 1990s, many developing countries have begun to subsidize the agricultural sector (see Anderson et al., 2013).

[6] As Potrafke (forthcoming) writes, the problem of endogeneity was so far not taken too seriously in the existing globalization literature. This might explain why credible external instrumental variables that vary in the time dimension do not exist so far.

different aspects of globalization, my results show that not only economic globalization, but that also political and social globalization increase agricultural support.

My results are not only important for the specific area of domestic industry protection, but might also shed light on the effects of globalization on other institutions. For example, the theoretical literature has argued that due to growing international competition, globalization should lead to more deregulated labor markets, causing deterioration in the working conditions of unskilled workers (see, e.g., Wood, 1998; Stiglitz, 2002; Heine and Thakur, 2011). However, the empirical evidence is far from supportive for this argument (see, e.g., Algan and Cahuc, 2006; Fischer and Somogyi, 2009; Potrafke, 2013). An explanation for this discrepancy would be that governments do not have to deregulate labor markets in the course of globalization because they increasingly tend to insulate their domestic markets.

The remainder of this article is structured as follows. In section 2, I discuss the KOF-index of globalization and the data set on agricultural protection. In section 3, I present my empirical strategy, while in section 4, the results are presented. Finally, section 5 concludes the study.

4.2 Data

4.2.1 Agricultural protection

To measure agricultural protection, I use a new database that has recently been developed by the World Bank (see Anderson and Valenzuela, 2008; Anderson and Nelgen, 2013). It contains annual data on agricultural price distortions for 82 countries over an average number of 45 years per country. This data set thus provides information on agricultural support for a larger number of countries and years than any other existing data set (see Anderson et al., 2013). My main outcome variable is the Nominal Rate of Assistance (NRA) which is computed as the percentage by which government policies have raised (or lowered) gross returns to farmers compared to the case in which governments have not intervened.[7] If governments have raised gross returns, i.e. if agricultural production was subsidized, then $NRA > 0$, while $NRA < 0$ if governments have effectively taxed agriculture. Compared to other possible measures of agricultural protection, as for example score indices as used in Giuliano et al. (2013), the outcome variables used in this article directly measure the policy outcomes of interest, and are therefore less subject to measurement errors and subjective judgments (see Olper and Raimondi, 2013).

In the robustness checks, I will also use the Relative Rate of Assistance (RRA) in which the NRA for agricultural tradables is set in relation to the NRA for non-agricultural tradables. The RRA thus takes into account that farmers are not only affected by prices for their own products, but also compete with non-agricultural producers over the same mobile resources (see Anderson et al., 2013).

A shortcoming of the NRA as the outcome variable is that the NRA focuses on price distortions and does not include decoupled support as for example direct payments to farmers. However, this component of agricultural support is important given that especially developed countries have shifted their policy instruments from price support to decoupled payments in recent years (see Swinnen et al., 2011; Anderson et al., 2013). To deal with this issue, in the robustness checks I additionally have used a variant of the NRA (also available in the dataset of Anderson and Valenzuela, 2008) that includes decoupled support.

A further shortcoming of the employed outcome variables could be that these aggregate indices might give no clear picture about the support of domestic farmers if there is a large heterogeneity in the amount of support for different types of agricultural goods (see Aksoy, 2005). Indeed, agricultural support sometimes shows a large heterogeneity not only across countries, but also across types of commodities within a country (see Anderson et al., 2013). Furthermore, a problem with the aggregate indices could exist because the weights used for aggregation could be measured with error, thus potentially leading to an aggregation bias (see Olper and Raimondi, 2013). To attenuate these issues, in the robustness checks I will also estimate the globalization effect at the product type level.

[7] Note that I use the NRA for covered products. Results change neither quantitatively nor qualitatively if I use the NRA that also contains noncovered products.

4.2.2 KOF-index of globalization

Globalization is a complex process that could affect protectionism through various channels. Globalization does not only influence the economic sphere, but also political and social life. For example, political globalization could influence protection through international trade agreements. Social globalization could influence protection through an increasing concern for poverty in developing countries, for example because of a more intense contact to foreign people through tourism and improved communication possibilities.

While there are no studies that directly investigate the influence of globalization on protectionism by using appropriate estimation methods and dealing with the potential endogeneity of globalization, there are a few studies (see, e.g., Klomp and de Haan, 2013; Olper and Raimondi, 2013) that include economic indicators of globalization as control variables when investigating other determinants of agricultural protection. However, given the above considerations, these single economic indicators like trade openness are neither able to capture all aspects of economic globalization (for example important factors such as foreign direct investment or outsourcing are not captured) nor all possible dimensions of overall globalization due to the multidimensional nature of globalization. Empirical studies thus need a clear definition of what globalization is and how different dimensions of globalization can be quantified.

The KOF-index of globalization (see Dreher, 2006; Dreher et al., 2008) is commonly accepted in the empirical literature to be such an attempt to capture all relevant aspects of globalization. In its most recent version, it is annually available for 207 countries. The KOF-index is based on 24 variables that relate to different dimensions of globalization and are combined into three sub-indices (social, political and economic globalization) and one overall index of globalization with an objective statistical method. It assigns a value from 0 to 100 indicating the level of globalization of a country. Higher values indicate more globalization. In the year 2010, globalization has for example been high in countries such as Ireland (91.79), the Netherlands (91.33) and Austria (89.48). Globalization has for example been low in Sudan (36.19), Ethiopia (37.46) and Tanzania (39.12).

4.2.3 Estimation period and sample

I use data for all countries for which both the KOF-index of globalization and data on agricultural protection provided by the World Bank are available. There are 77 countries that fulfill this criterion. Table 4.1 lists the covered countries.

TABLE 4.1: Countries in the data set

Argentina	Estonia	Madagascar	South Africa
Australia	Ethiopia	Malaysia	Spain
Austria	Finland	Mali	Sri Lanka
Bangladesh	France	Mexiko	Sudan
Benin	Germany	Morocco	Sweden
Brazil	Ghana	Mozambique	Switzerland
Bulgaria	Greece	Netherlands	Tanzania
Burkina Faso	Hungary	New Zealand	Thailand
Cameroon	Iceland	Nicaragua	Togo
Canada	India	Nigeria	Turkey
Chad	Indonesia	Norway	Uganda
Chile	Ireland	Pakistan	Ukraine
China	Israel	Philippines	United Kingdom
Colombia	Italy	Poland	United States
Côte d'Ivoire	Japan	Portugal	Vietnam
Czech Republic	Kazakhstan	Romania	Zambia
Denmark	Kenya	Russia	Zimbabwe
Dominican Republic	Republic Korea	Senegal	
Ecuador	Latvia	Slovakia	
Egypt	Lithuania	Slovenia	

I use annual data for the time period 1991-2010. The start of the time period is motivated by the fact that the fall of the Iron Curtain and the transition of several countries from socialism to capitalism has likely led to a structural break in agricultural support (see Rozelle and Swinnen, 2010) and trade policy in general. It is well documented that these developments have led to more trade liberalization (see Brülhart et al., 2012). Moreover, some post-Soviet states have become independent at the beginning of the 1990s such that the chosen time period maximizes the potential balancedness of the panel data set. Note that there are missing values both for the dependent variable as well as for the explanatory variables. Thus, I will run my estimations on an unbalanced panel data set. I will show in the robustness section, however, that my results do not change when I focus on a balanced panel data set instead. Summary statistics for the main variables used in this chapter can be found in table 4.2.

TABLE 4.2: Summary statistics

	Mean	Std. Deviation	Min	Max	Obs
NRA	0.193	0.495	-0.852	3.73	1482
RRA	0.153	0.505	-0.755	3.763	1317
Overall globalization	59.43	18.41	19.56	91.86	1514
Economic globalization	57.53	19.04	9.76	96.83	1514
Social globalization	49.91	24.35	7.59	91.25	1514
Political globalization	74.45	19.11	1.28	98.43	1514
GDP per capita	8416.2	10896.5	92.1	42450	1520
Population size	7.07e+07	1.89e+08	257000	1.37e+09	1506
GDP growth	3.39	4.57	-32.12	33.63	1506
Share employment in agriculture	17.63	17.86	0.3	89.3	1087
Prop. Rural population	0.435	0.217	0.066	0.887	1506
Polity2-index	5.43	5.62	-8	10	1500
Land area	1286506	2752899	20140	1.64e+07	1525
Prop. Agricultural land	43.89	19.36	2.66	85.64	1516
EU-membership	0.205	0.404	0	1	1525
Number natural disasters	6.03	9.99	0	101	1525
Conflict	0.187	0.39	0	1	1525

Notes: Land area is measured in square km.

Data sources can be found in the appendix.

4.3 Empirical strategy

Following the related literature on the determinants of agricultural support (see Klomp and de Haan, 2013), I will estimate a dynamic panel data model. It has the following form:

$$NRA_{it} = \gamma NRA_{it-1} + \beta Globalization_{ist} + \alpha_i + \lambda_t + u_{it} \tag{1}$$

where the dependent variable denotes the nominal rate of assistance to the agricultural sector for country i in year t. $Globalization_{ist}$ denotes the s-th dimension of globalization for country i in year t ($s = \{Overall, Economic, Social, Political\}$). α_i is a country-fixed effect that is able to take up general time-invariant country-specific characteristics such as geography[8], climate, or history, while λ_t is a

[8]For example, Bates and Block (2010) have shown that landlocked African countries have a lower RRA and in some specifications also a lower NRA than countries with a coastborder. These kinds of time-invariant geographic features of a country are captured by the country-fixed effects.

year-fixed effect that takes up global time trends in agricultural support. u_{it} is an error term with the usual properties.

A vector of time-varying control variables can be included into the model in a straightforward way. In the specifications with control variables, I first include GDP per capita to control for the level of development of a country. Second, I include the growth rate of GDP to control for the effect of the business cycle on agricultural support. Moreover, I include total population size, the percentage of the rural population, membership in the EU, the number of natural disasters in a country-year combination, the Polity2-index from the Polity IV project to account for a country's democratic capital (see Olper, 2001), agricultural employment as a share of total employment (see Olper, 2007) as well as agricultural land as a share of the total land area.[9] Furthermore, I control for the total land area and a dummy indicating whether a country was involved in a conflict in the year under consideration.

It is well known that in a dynamic panel data model, the simple fixed-effects estimator is biased unless the time dimension of the panel is large. This, however, is not the case in my panel with T = 20. In a simulation study, Judson and Owen (1999) show that the resulting bias is not negligible even if T = 30. To account for the bias, I use the system GMM (see Arellano and Bover, 1995; Blundell and Bond, 1998) estimator that combines moment conditions for the model in first differences with moment conditions for the model in levels. The system GMM estimator could be superior to the difference GMM estimator in the present application because the dependent variable turns out to be highly persistent. In this case, the difference GMM estimator might suffer from a weak instruments problem, while the system GMM estimator does not (see Che et al., 2013). However, I will show in the robustness checks that my results do not differ when I use the difference GMM estimator instead.

[9]Klomp and de Haan (2013) also include a control variable indicating whether a country has a proportional electoral system. Analogously, they include a control variable for parliamentary systems (as compared to presidential systems). In my period of investigation, however, there is a rather low within-variation in these variables: Only 8 countries have switched from a parliamentary to a presidential system or vice versa. Moreover, only 9 countries have switched from a majoritarian election system to a proportional electoral system or vice versa. In non-reported robustness checks, I have included dummy variables for the electoral system and for whether a country had a parliamentary or a presidential system. However, this did not affect the results.

I deal with potential reverse causality and omitted variable bias by treating globalization as endogenous in my estimation approach. Thus, my estimation approach exploits lagged values of globalization as instruments. If the system GMM model is properly specified, it thereby holds the promise of overcoming severe identification issues via the use of internal instrumental variables. Reverse causality could be such an identification problem as it is likely that not only does globalization influence agricultural support, but that also trade restrictions as, for example, support given to specific sectors of an economy influence the degree of a country's globalization.

In my estimation approach, I employ the two-step estimator and use Windmeijer's (2005) finite-sample correction of the standard errors. Windmeijer (2005) has shown that the standard errors could else be subject to a downward bias. Moreover, to avoid the problem of instrument proliferation, I collapse the instruments as suggested by Roodman (2009).

4.4 Results

4.4.1 Baseline Results

Before turning to the GMM regressions, tables 4.3 and 4.4 show the results from simple static fixed-effects specifications. In table 4.3, each dimension of globalization has a significantly positive effect on agricultural support. In table 4.4 in which I have included the control variables mentioned above, the effects are smaller and insignificant. The effects of economic and political globalization are close to being significant, while the effect of social globalization on agricultural support is virtually zero. However, these simple fixed-effects specifications are subject to two types of biases: First, given that agricultural support is likely to be highly persistent as shown in many previous studies, the omission of a lagged dependent variable leads to an omitted variable bias because the lagged dependent variable is both correlated with agricultural support as well as with the country-fixed effects. Second, globalization is likely to be endogenous to agricultural support, thus motivating the use of an instrumental variable for globalization. Both issues can be solved in a GMM framework.

Table 4.5 presents the results from the system GMM estimator. Note first that

TABLE 4.3: Static Panel Data Results without Control Variables, Fixed-Effects Estimator

VARIABLES	(1) NRA	(2) NRA	(3) NRA	(4) NRA
Overall globalization	0.018***			
	(3.084)			
Economic globalization		0.011***		
		(2.670)		
Social globalization			0.008*	
			(1.789)	
Political globalization				0.006**
				(2.610)
Observations	1473	1473	1473	1473
Number of clusters	77	77	77	77

Notes: All regressions include, but do not report, country- and year-fixed effects. Robust t-statistics based on clustered standard errors in parentheses. *Significant at the 10 percent level, **Significant at the 5 percent level, ***Significant at the 1 percent level.

the specification tests indicate that the models are well-specified: The null hypothesis of the Hansen J test of the joint validity of the instruments cannot be rejected. Moreover, the null hypothesis of no second-order autocorrelation cannot be rejected at conventional significance levels. Second-order autocorrelation must be absent in order for the estimators to be consistent, while first-order autocorrelation exists by design. Accordingly, the null hypothesis of no first-order autocorrelation can be rejected. The lagged dependent variable has a positive sign and is highly significant in every specification, indicating that there is strong persistence in agricultural support. Thus, a dynamic panel data model is justified, and not accounting for the lagged dependent variable would lead to an omitted variable bias. Additionally, the number of instruments is low and by far smaller than the number of cross-sections such that a bias due to the problem of instrument proliferation is not likely.

As can be seen in column 1, the KOF-index of globalization is positive and highly significant. Thus, this estimate suggests that globalization induces policy-makers to increase agricultural protection. The numerical interpretation of this estimate is that a one-unit increase in the KOF-index of globalization leads to an increase in agricultural protection of 1.6%. In columns 2 to 4, I

TABLE 4.4: Static Panel Data Results with Control Variables, Fixed-Effects Estimator

VARIABLES	(1) NRA	(2) NRA	(3) NRA	(4) NRA
Overall globalization	0.008			
	(1.129)			
Economic globalization		0.007		
		(1.408)		
Social globalization			0.000	
			(0.021)	
Political globalization				0.003
				(1.523)
GDP per capita	-0.000***	-0.000***	-0.000***	-0.000***
	(-4.534)	(-4.805)	(-5.420)	(-5.068)
Population size	0.000*	0.000*	0.000*	0.000*
	(1.832)	(1.781)	(1.857)	(1.885)
GDP growth	0.005*	0.005**	0.006**	0.005**
	(1.751)	(2.048)	(2.562)	(2.221)
Share of employment in agriculture	-0.004	-0.004	-0.005*	-0.005*
	(-1.615)	(-1.537)	(-1.758)	(-1.670)
Proportion of rural population	0.138	0.129	0.102	0.071
	(0.538)	(0.523)	(0.401)	(0.274)
Polity2-index	0.007**	0.005*	0.007**	0.007**
	(2.077)	(1.764)	(2.009)	(2.071)
Land area	-0.000	-0.000	-0.000	-0.000
	(-1.039)	(-1.046)	(-1.062)	(-1.117)
Proportion of agricultural land	-0.007*	-0.006*	-0.008**	-0.008**
	(-1.778)	(-1.978)	(-2.103)	(-2.204)
EU-membership	-0.076	-0.104	-0.033	-0.044
	(-1.004)	(-1.248)	(-0.451)	(-0.567)
Number of natural disasters	-0.003	-0.003	-0.002	-0.002
	(-1.526)	(-1.451)	(-1.255)	(-1.415)
Conflict	0.085*	0.076	0.092*	0.091*
	(1.691)	(1.501)	(1.915)	(1.879)
Observations	1021	1021	1021	1021
Number of clusters	74	74	74	74

Notes: All regressions include, but do not report, country- and year-fixed effects. Robust t-statistics based on clustered standard errors in parentheses. *Significant at the 10 percent level, **Significant at the 5 percent level, ***Significant at the 1 percent level.

Table 4.5: Panel Data Results without Control Variables, System GMM estimator, Globalization endogenous

VARIABLES	(1) NRA	(2) NRA	(3) NRA	(4) NRA
Overall globalization	0.016***			
	(4.026)			
Economic globalization		0.018***		
		(3.635)		
Social globalization			0.010***	
			(4.025)	
Political globalization				0.028***
				(4.235)
NRA_{it-1}	0.752***	0.729***	0.796***	0.789***
	(12.679)	(11.022)	(16.793)	(8.472)
Observations	1459	1459	1459	1459
Number of cross-sections	77	77	77	77
Number of instruments	41	41	41	41
AR(1) test p-value	0.000	0.000	0.000	0.001
AR(2) test p-value	0.252	0.303	0.241	0.139
Hansen J test p-value	0.162	0.152	0.212	0.316

Notes: All regressions include, but do not report, year-fixed effects. Instruments are collapsed as suggested by Roodman (2009). Robust two-step t-statistics based on Windmeijer's (2005) finite sample correction in parentheses. *Significant at the 10 percent level, **Significant at the 5 percent level, ***Significant at the 1 percent level.

investigate which dimension of globalization has the largest influence on agricultural support. Each dimension has a highly significant effect on globalization. Political globalization, however, has the largest effect, followed by economic globalization.

In table 4.6, I include the control variables mentioned above. Of these, three reach statistical significance at least at the 10%-level. First, agricultural support increases in the share of the rural population. Second, agricultural support also increases in the share of agricultural employment in total employment. Third, agricultural support is lower for those countries that became member of the EU. While the first finding is in line with existing studies, the second and third finding stand in contrast to the recent literature. Olper (2007) finds that the share of agricultural employment in total exployment significantly decreases agricultural assistance. However, an explanation for the discrepancy between

TABLE 4.6: Panel Data Results with Control Variables, System GMM estimator, Globalization endogenous

VARIABLES	(1) NRA	(2) NRA	(3) NRA	(4) NRA
Overall globalization	0.049***			
	(3.107)			
Economic globalization		0.030**		
		(2.556)		
Social globalization			0.017*	
			(1.717)	
Political globalization				0.034***
				(3.227)
NRA_{it-1}	0.642***	0.646***	0.771***	0.746***
	(6.086)	(6.938)	(13.32)	(6.843)
GDP per capita	-0.000	0.000	-0.000	-0.000*
	(-1.336)	-0.269	(-0.537)	(-1.699)
Population size	0.000	0.000	0.000	-0.000
	(0.246)	(0.291)	(0.234)	(-1.375)
GDP growth	0.001	-0.003	0.005	0.009*
	(0.174)	(-0.494)	(1.462)	(1.662)
Share of employment in agriculture	0.010*	0.004	0.006	0.001
	(1.826)	(1.137)	(1.403)	(0.233)
Proportion of rural population	0.711**	0.801*	0.258	0.909**
	(1.974)	(1.918)	(0.78)	(1.972)
Polity2-index	-0.018	-0.005	-0.006	-0-000
	(-1.601)	(-0.463)	(-0.808)	(-0.006)
Land area	-0.000	0.000	-0.000	-0.000
	(-0.634)	(0.797)	(-1.145)	(-0.101)
Proportion of agricultural land	-0.003	-0.001	-0.002	-0.000
	(-1.157)	(-0.364)	(-1.478)	(-0.076)
EU-Membership	-0.542***	-0.380***	-0.201*	0.046
	(-3.159)	(-2.610)	(-1.741)	(0.253)
Number of natural disasters	0.000	0.001	0.000	-0.004
	(0.092)	(0.317)	(0.098)	(-0.950)
Conflict	0.031	0.128	0.097	-0.077
	(0.38)	(1.599)	(1.638)	(-0.510)
Observations	1012	1012	1012	1012
Number of cross-sections	74	74	74	74
Number of instruments	52	52	52	52
AR(1) test p-value	0.002	0.001	0.001	0.002
AR(2) test p-value	0.404	0.211	0.224	0.217
Hansen J test p-value	0.602	0.588	0.173	0.522

Notes: All regressions include, but do not report, year-fixed effects. Instruments are collapsed as suggested by Roodman (2009). Robust two-step t-statistics based on Windmeijer's (2005) finite sample correction in parentheses. *Significant at the 10 percent level, **Significant at the 5 percent level, ***Significant at the 1 percent level.

the two studies might exist in the fact that Olper (2007) studies a time period in which many developing countries (i.e. those with a high share of employment in agriculture) have taxed rather than subsidized agriculture, while this has changed in the more recent time period that I study (see also footnote 5).

Klomp and de Haan (2013) find that EU-membership significantly increases support given to the agricultural sector. Note however that because of the use of fixed-effects, the EU-membership variable is determined by those countries in the sample that have switched from non-membership to membership in the period of investigation. As Klomp and de Haan (2013) have used a different time period than I do, the countries that have switched partly differ between their paper and mine. In my period of investigation, many countries that have switched to EU-membership clearly have lowered their agricultural support. Prime examples for this are Austria, Finland and Romania.[10] Note further that as the coefficients of the control variables measure the effects of within-country changes rather than level effects because of the use of a fixed-effects model, the non-significance of some control variables might be explained by a rather low within-variation.

The point estimates of the effect of globalization on agricultural support in the regressions with control variables stay statistically significant and are even of a larger magnitude than before. This might be explained through the effect of the control variables or through the fact that the number of observations is now smaller, since for the control variables, there are some missing values. The bottom line from the dynamic panel data model is that globalization has induced policy-makers to increase agricultural support, and that not only economic globalization, but also other dimensions of globalization influence agricultural support. Thus, only focusing on economic globalization would have missed important insights. Importantly, the effects are obtained by treating globalization as endogenous by means of the system GMM estimator which generates internal instruments for globalization.

[10]The influence of the EU-enlargement on agricultural protection is also discussed by Anderson et al. (2013). They argue that the negative effect of EU-membership on agricultural protection is, beyond others, due to the facts that 1) the new EU-countries brought many poor farmers into the EU which decreased the pressure for the EU to increase agricultural support and 2) many of the poorer countries that became part of the EU were not part of the GATT which caused GATT constraints for the whole EU. This, in turn, led to lower agricultural support.

An interesting extension is to investigate to what extent these results are heterogeneous with regard to the income level of the included countries. To explore whether this is case, I have built dummy variables for high, middle and low income countries according to the classification of the World Bank, and have run regressions in which I have included these dummies and interacted them with the indices of globalization. The results can be found in table 4.7 (without

TABLE 4.7: Heterogeneity of the effects with regard to the income level, System GMM estimator, Globalization endogenous, No control variables included

VARIABLES	(1) NRA	(2) NRA	(3) NRA	(4) NRA
Overall global.*High income	0.014***			
	(2.595)			
Overall global.*Middle income	0.018***			
	(3.17)			
Overall global.*Low income	0.022***			
	(2.865)			
Econ. global.*High income		0.024***		
		(3.166)		
Econ. global.*Middle income		0.024***		
		(3.356)		
Econ. global*Low income		0.031**		
		(2.536)		
Social global.*High income			0.022***	
			(2.768)	
Social global.*Middle income			0.018***	
			(3.069)	
Social global.*Low income			0.031***	
			(3.119)	
Polit. global.*High income				0.011*
				(1.695)
Polit. global.*Middle income				0.013***
				(2.717)
Polit. global.*Low income				0.011***
				(2.877)
NRA_{it-1}	0.759***	0.740***	0.820***	0.823***
	(15.033)	(11.937)	(15.23)	(13.829)
Observations	1459	1459	1459	1459
Number of cross-sections	77	77	77	77
Number of instruments	83	83	83	83
AR(1) test p-value	0.000	0.000	0.000	0.000
AR(2) test p-value	0.237	0.313	0.226	0.202
Hansen J test p-value	0.374	0.388	0.618	0.403

Notes: All regressions include, but do not report, year-fixed effects. Instruments are collapsed as suggested by Roodman (2009). Robust two-step t-statistics based on Windmeijer's (2005) finite sample correction in parentheses. *Significant at the 10 percent level, **Significant at the 5 percent level, ***Significant at the 1 percent level control variables) and table 4.8 (control variables included). There is no clear

TABLE 4.8: Heterogeneity of the effects with regard to the income level, System GMM estimator, Globalization endogenous, Control variables included

VARIABLES	(1) NRA	(2) NRA	(3) NRA	(4) NRA
Overall global.*High income	0.026			
	(0.242)			
Overall global.*Middle income	0.024			
	(0.208)			
Overall global.*Low income	0.027			
	(0.232)			
Econ. global.*High income		0.036**		
		(2.214)		
Econ. global.*Middle income		0.026***		
		(2.982)		
Econ. global*Low income		0.018		
		(1.012)		
Social global.*High income			0.032	
			(1.306)	
Social global.*Middle income			0.017**	
			(2.273)	
Social global.*Low income			0.054	
			(0.999)	
Polit. global.*High income				0.035**
				(2.019)
Polit. global.*Middle income				0.013**
				(2.152)
Polit. global.*Low income				(0.002)
				(-0.036)
NRA_{it-1}	0.742	0.642***	0.789***	0.813***
	(0.985)	(6.558)	(11.359)	(6.301)
Observations	1012	1012	1012	1012
Number of cross-sections	74	74	74	74
Number of instruments	77	77	77	77
AR(1) test p-value	0.328	0.001	0.001	0.003
AR(2) test p-value	0.284	0.196	0.191	0.235
Hansen J test p-value	0.141	0.239	0.216	0.39

Notes: All regressions include, but do not report, year-fixed effects. Instruments are collapsed as suggested by Roodman (2009). Robust two-step t-statistics based on Windmeijer's (2005) finite sample correction in parentheses. *Significant at the 10 percent level, **Significant at the 5 percent level, ***Significant at the 1 percent level.

pattern that the globalization effect is fundamentally different across income groups. In most cases, the coefficients are similar for all three income groups. If anything, there is a slight tendency that social globalization is more and political globalization less important in low-income countries.

4.4.2 Robustness checks

I have checked the robustness of my results in several ways. As a first robustness check, I have estimated the difference GMM model (see Arellano and Bond, 1991) instead of the system GMM model (Table 4.9).[11] Compared to the difference GMM model, the system GMM model uses an additional moment condition that could be violated when the lagged first differenced dependent variable is correlated with the country-fixed effects. Thus, both models have different identification assumptions that have to be fulfilled for the coefficients to be consistent. Moreover, the existing literature (see Che et al., 2013) has provided evidence that results could be different depending on whether the system or the difference GMM estimator is used. Therefore, it is important to check the sensitivity of the results with respect to the estimation method that is used. The difference GMM estimator confirms my results: With the difference GMM model, the point estimates are even larger than in the case of the system GMM model and, except for the effect of social globalization, always highly significant. Interestingly, the effect of social globalization is now non-significant, although the point estimate is much larger than with the system GMM estimator. Thus, the standard errors have increased considerably. This indicates that the instruments in the difference GMM model could indeed be weaker than in the system GMM model in the present application, thus validating the choice of the preferred estimation method.

My results could depend on the fact that my panel data set is not balanced. To investigate whether this is the case, I have re-run my estimations without control variables on a balanced panel data set. For this sake, I have restricted the estimation sample to the time period 1996-2005 for which data on agricultural support is available for all countries in the initial sample. Moreover, I have excluded Sri Lanka from the sample because the KOF-index for Sri Lanka is only available since 1998. However, my results do not change. If anything, the coefficient of interest sometimes grows slightly in size (Table 4.10).

Surprisingly, GDP per capita has been insignificant in the GMM regressions with control variables, whereas it has been found as a major determinant of

[11]For the difference GMM model, I only report the results for the specifications with control variables. The specifications without control variables are in line with Table 5 which is why I omit them here. Results are available upon request.

TABLE 4.9: Panel Data Results with Control Variables, Difference GMM estimator, Globalization endogenous

VARIABLES	(1) NRA	(2) NRA	(3) NRA	(4) NRA
Overall globalization	0.092***			
	(2.900)			
Economic globalization		0.061***		
		(2.893)		
Social globalization			0.057	
			(1.165)	
Political globalization				0.046**
				(2.438)
NRA_{it-1}	0.589***	0.490***	0.627***	0.661***
	(3.918)	(4.136)	(4.614)	(6.400)
GDP per capita	0.000	-0.000	-0.000*	0.000
	(0.487)	(-0.280)	(-1.956)	(0.011)
Population size	0.000	0.000	0.000	0.000
	(0.377)	(0.459)	(0.568)	(1.498)
GDP growth	0.006	0.008*	0.008***	0.007**
	(1.634)	(1.895)	(3.474)	(2.381)
Share of employment in agriculture	-0.001	-0.004	0.002	0.001
	(-0.243)	(-0.755)	(0.421)	(0.258)
Proportion of rural population	0.435	0.457	0.990	0.278
	(0.773)	(1.123)	(1.376)	(0.541)
Polity2-index	0.003	-0.003	0.003	0.009
	(0.273)	(-0.218)	(0.362)	(1.405)
Land area	-0.000***	-0.000***	-0.000	-0.000**
	(-2.594)	(-3.388)	(-0.045)	(-2.559)
Proportion of agricultural land	-0.001	-0.001	-0.001*	0.000
	(-0.386)	(-0.438)	(-1.827)	(0.256)
EU-membership	-0.192	-0.329*	0.062	0.058
	(-1.365)	(-1.751)	(0.478)	(0.629)
Number of natural disasters	-0.001	-0.001	0.001	-0.002
	(-0.401)	(-0.276)	(0.481)	(-0.928)
Conflict	0.077*	-0.019	0.067*	0.100*
	(1.914)	(-0.409)	(1.815)	(1.812)
Observations	904	904	904	904
Number of cross-sections	57	57	57	57
Number of instruments	49	49	49	49
AR(1) test p-value	0.007	0.001	0.016	0.007
AR(2) test p-value	0.34	0.193	0.229	0.197
Hansen J test p-value	0.313	0.418	0.204	0.331

Notes: All regressions include, but do not report, year-fixed effects. Instruments are collapsed as suggested by Roodman (2009). Robust two-step t-statistics based on Windmeijer's (2005) finite sample correction in parentheses. *Significant at the 10 percent level, **Significant at the 5 percent level, ***Significant at the 1 percent level.

TABLE 4.10: Panel Data Results without Control Variables, System GMM estimator, Globalization endogenous, Balanced Panel Data Set 1996-2005

VARIABLES	(1) NRA	(2) NRA	(3) NRA	(4) NRA
NRA_{it-1}	0.409***	0.450***	0.510***	0.514***
	(3.484)	(3.889)	(4.359)	(3.885)
Overall globalization	0.018**			
	(2.227)			
Economic globalization		0.020***		
		(2.802)		
Social globalization			0.014**	
			(2.035)	
Political globalization				0.018**
				(2.282)
Observations	760	760	760	760
Number of cross-sections	76	76	76	76
Number of instruments	31	31	31	31
AR(1) test p-value	0.003	0.002	0.002	0.002
AR(2) test p-value	0.11	0.116	0.114	0.134
Hansen J test p-value	0.076	0.042	0.136	0.166

Notes: All regressions include, but do not report, year-fixed effects. Instruments are collapsed as suggested by Roodman (2009). Robust two-step t-statistics based on Windmeijer's (2005) finite sample correction in parentheses. *Significant at the 10 percent level, **Significant at the 5 percent level, ***Significant at the 1 percent level.

agricultural support in other studies (see, e.g., Swinnen et al., 2012). I have therefore also tried to include GDP per capita in logarithms. However, this does neither affect the baseline results nor the fact that GDP per capita is insignificant. Additionally, I have also included a squared term of GDP per capita. In this case, the linear term is negatively significant, while the squared term is positively significant, thus suggesting a U-shaped relationship between economic development and agricultural protection. However, the globalization effects are not affected. All dimensions of globalization still have a significant effect on agricultural protection.

I have also employed the Relative Rate of Assistance as outcome variable. It may well be the case that some governments do not change the NRA for agricultural products, but might lower assistance for some non-agricultural sectors that are less subject to import competition than agriculture. This would benefit the agricultural sector, but not be captured by the NRA. However, employing the RRA as measure for agricultural protection does not change the results as can be seen in table 4.11.[12] In non-reported robustness checks, I also have used a variant of the NRA that includes decoupled support to domestic farmers. It might be the case that governments do not react to globalization with measures that distort prices but instead for example increase direct payments to domestic farmers. This would not be captured by the conventional NRA used in my previous regressions. However, the results stay completely unchanged when decoupled support is included in the outcome variable which is why I omit them here.

My results could depend on idiosyncratic circumstances in single countries. Therefore, I have checked whether my results are robust to excluding single countries. However, no matter whether the control variables are included or not, the coefficient of interest is always significant at conventional levels. Thus, my results are not sensitive to excluding single countries. I have also checked whether my results could be sensitive to outliers by excluding the most globalized (KOF-index over 85) and the least globalized countries (KOF-index below 40). However, the results do not change.

In non-reported specifications, I also have substituted the contemporaneous KOF-index by lagged values of the KOF-index. Specifically, I have estimated regressions in which I have either included the KOF-index lagged by one year or lagged by two years. The idea is 1) that if the KOF-index is included in lagged form the potential reverse causality problem can be further reduced and 2) that globalization could need time to manifest itself and to have an effect on policies. However, my results stay the same when I employ lagged values of the KOF-index.

I have also estimated more parsimonious specifications in which I have restricted the set of control variables to those that have reached statistical significance at least at the 10%-level in the baseline model in table 4.6. However, the results were virtually the same which is why I omit them here.

As mentioned in the introduction, it is an established fact that there exists a negative correlation between the agricultural comparative advantage and

[12]In table 4.11, I only provide the results for specifications with control variables. The results are robust to excluding the control variables.

TABLE 4.11: Panel Data Results with Control Variables, System GMM estimator, Globalization endogenous, Relative Rate of Assistance as Outcome Variable

VARIABLES	(1) RRA	(2) RRA	(3) RRA	(4) RRA
Overall globalization	0.051***			
	(2.661)			
Economic globalization		0.025***		
		(3.587)		
Social globalization			0.025**	
			(2.124)	
Political globalization				0.033*
				(1.798)
RRA_{it-1}	0.553***	0.617***	0.723***	0.686***
	(4.034)	(7.881)	(9.474)	(3.958)
GDP per capita	-0.000	0.000	-0.000	-0.000
	(-0.837)	(1.551)	(-0.837)	(-0.730)
Population size	0.000	0.000	0.000	0.000
	(1.064)	(0.769)	(0.847)	(0.135)
GDP growth	-0.003	-0.003	0.003	0.006
	(-0.497)	(-0.786)	(0.722)	(1.263)
Share of employment in agriculture	0.011	0.003	0.008	0.000
	(1.430)	(0.832)	(1.322)	(0.028)
Proportion of rural population	0.574	0.700**	0.307	0.578
	(1.628)	(2.028)	(0.971)	(1.212)
Polity2-index	-0.017	-0.009	-0.012	0.014
	(-1.448)	(-1.202)	(-1.178)	(0.512)
Land area	-0.000	-0.000	-0.000*	-0.000
	(-1.210)	(-0.290)	(-1.808)	(-1.236)
Proportion of agricultural land	-0.003	-0.000	-0.003	-0.000
	(-0.903)	(-0.081)	(-1.461)	(-0.055)
EU-membership	-0.580***	-0.369***	-0.341***	-0.249
	(-3.592)	(-3.491)	(-2.741)	(-1.524)
Number of natural disasters	0.003	0.002	0.003	-0.004
	(0.645)	(0.522)	(0.706)	(-1.032)
Conflict	0.058	0.073	0.078	0.073
	(0.637)	(1.035)	(1.186)	(0.521)
Observations	942	942	942	942
Number of cross-sections	68	68	68	68
Number of instruments	52	52	52	52
AR(1) test p-value	0.004	0.002	0.002	0.012
AR(2) test p-value	0.506	0.219	0.31	0.265
Hansen J test p-value	0.747	0.603	0.18	0.645

Notes: All regressions include, but do not report, year-fixed effects. Instruments are collapsed as suggested by Roodman (2009). Robust two-step t-statistics based on Windmeijer's (2005) finite sample correction in parentheses. *Significant at the 10 percent level, **Significant at the 5 percent level, ***Significant at the 1 percent level.

agricultural support. So far, I have not controlled for the comparative advantage of the agricultural sector. In non-reported robustness checks, I have proxied for the comparative advantage first by including the net export share in total agricultural production based on FAO data into the regression (see Swinnen et al., 2012).[13] However, the globalization effects were unchanged. Second, I use agricultural land per capita (instead of agricultural land as a share of the total land area) as a proxy for the comparative advantage of agriculture. However, conclusions did not change. Only social globalization was marginally insignificant. Results are available upon request.

An additional specification test is to compare the results for the simple fixed-effects estimator in a dynamic panel data model (Table 4.12) and for the pooled

TABLE 4.12: Dynamic Panel Data Results without Control Variables, Fixed-Effects Estimator

VARIABLES	(1) NRA	(2) NRA	(3) NRA	(4) NRA
Overall globalization	0.003			
	(1.464)			
Economic globalization		0.002		
		(1.163)		
Social globalization			0.000	
			(0.394)	
Political globalization				0.001*
				(1.828)
NRA_{it-1}	0.701***	0.703***	0.709***	0.705***
	(30.258)	(28.849)	(31.188)	(30.118)
Observations	1459	1459	1459	1459
Number of clusters	77	77	77	77

Notes: All regressions include, but do not report, country- and year-fixed effects. Robust t-statistics based on clustered standard errors in parentheses. *Significant at the 10 percent level, **Significant at the 5 percent level, ***Significant at the 1 percent level.

OLS model (Table 4.13) with the results of the system GMM model. Unbiased estimates for the lagged dependent variable in the system GMM model should lie between the estimates of the lagged dependent variable in the fixed-effects

[13] The net export share in total agricultural production is calculated as (export value − import value)/production value.

TABLE 4.13: Pooled OLS Results without Control Variables

VARIABLES	(1) NRA	(2) NRA	(3) NRA	(4) NRA
Overall globalization	0.000*			
	(1.754)			
Economic globalization		0.000		
		(1.181)		
Social globalization			0.000**	
			(2.034)	
Political globalization				0.000
				(1.251)
NRA_{it-1}	0.893***	0.895***	0.891***	0.898***
	(60.258)	(61.370)	(57.722)	(82.584)
Observations	1459	1459	1459	1459
Number of clusters	77	77	77	77

Notes: All regressions include, but do not report, year-fixed effects. Robust t-statistics based on clustered standard errors in parentheses. *Significant at the 10 percent level, **Significant at the 5 percent level, ***Significant at the 1 percent level.

and the pooled OLS specification. This bracketing range therefore provides a natural specification check of the system GMM model: If the coefficients do not lie in this range, then there would be reason to worry about the model specification (see Bond, 2002). According to column 1 of tables 4.12 and 4.13, for overall globalization the coefficient of the lagged dependent variable must lie between 0.701 and 0.893. As can be seen from table 4.5, these criteria are fulfilled such that the model seems to be well-specified. Moreover, from table 4.12 it becomes evident that the bias of the simple fixed-effects specification compared to the system GMM model that takes the bias into account is enormous. This once more illustrates the importance of estimating a system GMM model.

It can also be easily seen that the coefficients of the lagged dependent variable also lie in this bracketing range when the three different dimensions of globalization are used as explanatory variables instead. The coefficients of the lagged dependent variable also lie in this bracketing range for the specifications with control variables. To economize on space, I have omitted the fixed-effects and pooled OLS specification for the case when control variables are included because the results are not different. Results are available upon request.

As mentioned above, estimates with the aggregate indices as the outcome variable could give an unclear picture about agricultural protection if there were a large heterogeneity of protection across different types of agricultural goods within a country. To investigate whether this could affect my conclusions, I have also run some regressions on the commodity group level. Following Olper and Raimondi (2013), I consider the following four commodity sectors: 1) Grains and Tubers, 2) Oilseeds, 3) Livestock and 4) Tropical crops.[14] In the sector-level regressions, in addition to the controls used in the baseline analysis, I control for each product value share on total agricultural production as well as for product fixed effects (see Olper and Raimondi, 2011).

Table 4.14 shows the results. In principle, the globalization effects are quite

TABLE 4.14: The globalization effect across different types of commodities

	Grains and Tubers	Oilseeds	Livestock	Tropical crops
Overall globalization	0.044	0.015	0.005	0.008
	(3.075)***	(0.644)	(0.381)	(0.347)
Economic globalization	0.021	-0.018	0.012	0.012
	(1.22)	(2.175)**	(1.516)	(0.832)
Social globalization	0.007	-0.009	0.002	-0.012
	(1.272)	(1.233)	(0.267)	(1.219)
Political globalization	0.034	0.032	0.016	0.042
	(1.899)*	(1.300)	(0.736)	(1.743)*
Observations	2894	1097	3968	1219
Controls	YES	YES	YES	YES
Method	System GMM	System GMM	System GMM	System GMM

Notes: All regressions include, but do not report, year-fixed effects and control variables. Instruments are collapsed as suggested by Roodman (2009). Robust two-step t-statistics based on Windmeijer's (2005) finite sample correction in parentheses. *Significant at the 10 percent level, **Significant at the 5 percent level, ***Significant at the 1 percent level.

similar across commodity types. However, there are some exceptions. For oilseeds, economic globalization can even have a significantly negative effect on protection. For overall globalization, protection of grains and tubers seems to be

[14] For these regressions I have additionally included a third lag of the dependent variable because the specification test otherwise showed that autocorrelation was not sufficiently removed.

most affected. As in the aggregate regressions, the effect of social globalization is the smallest. In the disaggregated data, the coefficient is always close to zero and can even be slightly negative. In sum, the positive effect of globalization on protection can also be found in most regressions with commodity level data. However, the coefficients are often very imprecisely measured as indicated by a high standard error. Thus, aggregated data seem to be more efficient. Finally, a note of caution is in order for the results of "Livestock" and "Tropical crops". While for "Grains and Tubers" and "Oilseeds" the models were always very well specified, this was not the case for both other categories. The autocorrelation tests performed well, but the Hansen J test rejected the null hypothesis of instrument exogeneity suggesting a need for caution regarding the effects for these commodity types.

A specific identification problem that should have been solved with the GMM model is reverse causality. To assess the extent to which reverse causality is indeed a problem in my regressions, I use a reverse regression to investigate whether feedback effects from agricultural support to globalization exist (see Olper et al., 2014). I estimate the following reverse regression:

$$Globalization_{ist} = \gamma Globalization_{ist-1} + \beta NRA_{it-1} + \alpha_i + \lambda_t + u_{it} \tag{2}$$

If agricultural support had a significant effect on globalization, this would point to some reverse causality problems. Table 4.15 shows the results.[15] Agricultural support is never significant no matter which dimension of globalization is used. This indicates that reverse causality does not seem to be a major limitation of my analysis.

Nevertheless, an important concern that remains is omitted variable bias although I include country-fixed effects that are able to take up many important time-invariant variables and a lagged dependent variable that can capture time-varying, but slow moving explanatory variables. It is still possible that an omitted shock affects both globalization and agricultural support, and can thus drive the relationship between both variables. Thus, I have found a robust association between globalization and agricultural support. While it appears that an omitted variable must have a large effect on the estimates to explain away the effect of globalization on agricultural support, it can, however, not

[15] Table 4.15 shows the results when control variables are included. Results without control variables are similar and therefore omitted to save space.

TABLE 4.15: Reverse Regressions with Control Variables, System GMM estimator, Agricultural Support Endogenous

VARIABLES	(1) Overall glob.	(2) Economic glob.	(3) Social glob.	(4) Political glob.
NRA_{it-1}	0.070	0.771	-0.508	0.682
	(0.175)	(1.090)	(-1.537)	(0.669)
Overall globalization$_{it-1}$	0.892***			
	(10.799)			
Economic globalization$_{it-1}$		0.810***		
		(7.858)		
Social globalization$_{it-1}$			0.941***	
			(13.612)	
Political globalization$_{it-1}$				0.820***
				(4.880)
Observations	1012	1012	1012	1012
Number of cross-sections	74	74	74	74
Number of instruments	51	51	51	51
AR(1) test p-value	0.000	0.000	0.000	0.010
AR(2) test p-value	0.787	0.236	0.358	0.973
Hansen J test p-value	0.582	0.358	0.643	0.721
Controls	YES	YES	YES	YES

Notes: All regressions include, but do not report, year-fixed effects and control variables. Instruments are collapsed as suggested by Roodman (2009). Robust two-step t-statistics based on Windmeijer's (2005) finite sample correction in parentheses. *Significant at the 10 percent level, **Significant at the 5 percent level, ***Significant at the 1 percent level.

be definitively concluded that the estimated relationship is causal. Thus, it is clearly desirable in future research to check whether my results also hold for alternative, external instruments.

4.5 Conclusion

This is the first paper that has empirically investigated whether globalization affects public support given to the agricultural sector. For this sake, I have employed the KOF-index as a proxy for globalization and data on agricultural support recently developed by the World Bank. By contrast to most of the existing literature on the effects of globalization, I take the potential endogeneity of globalization into account via internal instruments generated by the system GMM estimator.

Theoretically, the link between agricultural support and globalization is ambiguous. There exist both arguments for an increase as well as for a decrease of agricultural support in the course of globalization. For example, agricultural support could increase because of concerns for domestic import competition or food quality. On the other hand, agricultural support could decrease because of the efficiency of free trade as derived in the neo-classical literature.

I find that globalization significantly increases agricultural support. Not only economic globalization, but also social and political globalization have significant effects on agricultural support. Thus, only focusing on economic globalization or single indicators of globalization such as trade openness would have missed important insights. My results are robust to a battery of robustness checks.

Nonetheless, future research should develop external instruments for globalization and evaluate whether my results continue to hold. It is also important in future research to discriminate between the different channels through which globalization might affect agricultural support. Is the concern of national policy-makers, for example, related to import competition, urbanization or potentially decreasing food quality in the course of globalization? Besides being interesting in its own right, knowing which specific channel is responsible for the results presented in this chapter is also important to evaluate the welfare consequences of my findings.

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Appendix: Data sources

Variable	Source
NRA	Anderson and Valenzuela (2008); Anderson and Nelgen (2013)
RRA	Anderson and Valenzuela (2008); Anderson and Nelgen (2013)
Overall globalization	Dreher (2006); Dreher et al. (2008)
Economic globalization	Dreher (2006); Dreher et al. (2008)
Social globalization	Dreher (2006); Dreher et al. (2008)
Political globalization	Dreher (2006); Dreher et al. (2008)
GDP per capita	Anderson and Valenzuela (2008); Anderson and Nelgen (2013)
Population size	Anderson and Valenzuela (2008); Anderson and Nelgen (2013)
GDP growth	Own calculations based on Anderson and Valenzuela (2008); Anderson and Nelgen (2013)
Share employment in agriculture	World Development Indicators
Proportion rural population	Own calculation based on Anderson and Valenzuela (2008); Anderson and Nelgen (2013)
Polity2-index	Polity IV Project
Land area	World Development Indicators
Proportion agricultural land	World Development Indicators
EU-membership	Own calculation
Number of natural disasters	Own calculation based on EM-DAT
Conflict	Own calculation based on the UCDP Conflict Encyclopedia

Part III

Concluding Remarks

Concluding Remarks

This thesis has presented four self-contained essays on the influence of institutions and representation on policy. Chapter 1 and chapter 3 investigate how representation, represented by the form of government and political ideology, affects policies. Chapters 2 and 4 are concerned with the effects of different institutions on policies.

Because all chapters make self-contained contributions to the literature, there are important differences between them. For example, the chapters differ in the kind of data they employ (national-level- versus local level-data), in the specific kind of empirical method they use (macro-versus microeconometric approaches) and in the policies they focus on (fiscal policies, emissions or special-interest policies).

However, while all chapters make independent contributions, there are some common denominators underlying the chapters of this thesis. First, they all have an empirical focus, and use state-of-the-art empirical methods. This is reasonable as for all chapters, theoretical predictions are ambiguous. Second, all estimation approaches must - to a more or lesser extent - deal with a potential endogeneity problem in the estimation approach. Third, all chapters can be categorized into the "New Political Economy" - literature. But most importantly, all chapters have the clear result that institutions and representation matter for (economic) policy, thus motivating further research in this important field.

Eidesstattliche Erklärung

Hiermit erkläre ich an Eides statt, dass ich die vorliegende Arbeit selbständig verfasst habe und alle in Anspruch genommenen Quellen und Hilfen in der Dissertation vermerkt wurden. Diese Dissertation ist weder in der gegenwärtigen noch in einer anderen Fassung oder in Teilen an der Technischen Universität Dortmund oder an einer anderen Hochschule im Zusammenhang mit einer staatlichen oder akademischen Prüfung vorgelegt worden.

Dortmund, September 2014

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