Institutions and Public Sector Performance: Empirical Analyses of Revenue Forecasting and Spatial Administrative Structures

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Chapter 1

Introduction

"Don't make the tax figures seem better than they are", the president of the German Court of Auditors remarked in apprehension of budget imbalances – he was concerned about too optimistic revenue forecasts. The performance of agents in the public sector, such as revenue forecasters, depends on the design of institutions. Also local politicians react to incentives originated in institutions: "Certainly, this is local-business-tax cannibalism" claimed the head of the economics department of the city of Frankfurt. He finds his city exposed to increased tax competition, induced by tax cuts of surrounding municipalities. The question arises which institutions cause these statements. Since the institutional framework determines the decision making of politicians and bureaucrats, the performance of the public sector hinges crucially on the respective environment.

This book looks at two aspects where the effects of institutions on the performance of political agents are particularly relevant – one on the country and the other on the local level. The first concerns the environment in which revenue forecasters work. Since revenue forecasts are the basis of every budgetary process, they feature prominently in decisions

Figure 1.1: Standard Deviation of Next-Fiscal-Year Forecast Error (in %)

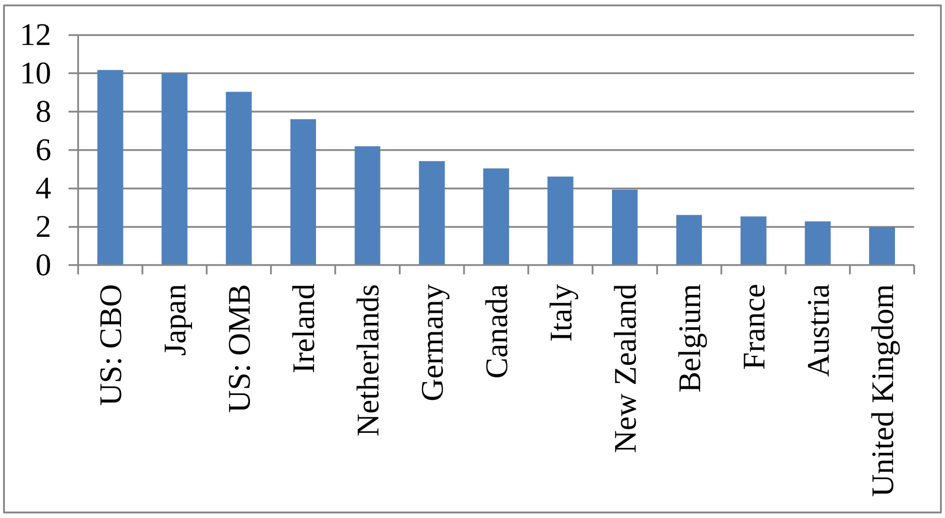


Figure covers the period from 1995 to 2009 (fewer observations for some countries). CBO – Congressional Budget Office. OMB – Office of Management and Budget.

regarding economic policy. But as Figure 1.1 shows, their quality differs notably across countries and institutions. This gives rise to the question why this is, and calls for an investigation into the determinants of forecasting precision. Among these determinants may be the assignment of the task: While in some countries revenue forecasting is performed by independent institutions, others produce the figures in their ministries. But also further conditions differ that can affect the performance of forecasters and hence the policies implemented, such as the structure of the tax system or the timing of forecasts. Exploiting variation in these differences allows for identifying institutions that lead to superior forecasts. Also the information that is provided by the government has to be mentioned in this context. It enables (superior members of) a government to "optimize" the figures strategically in order to influence the forecasts in the preferred direction. The question of whether such manipulation or other biases exist and whether forecasters use all the relevant information available at the time of the forecast calls for further empirical assessment.

Figure 1.2: Relevance of Local Business Tax

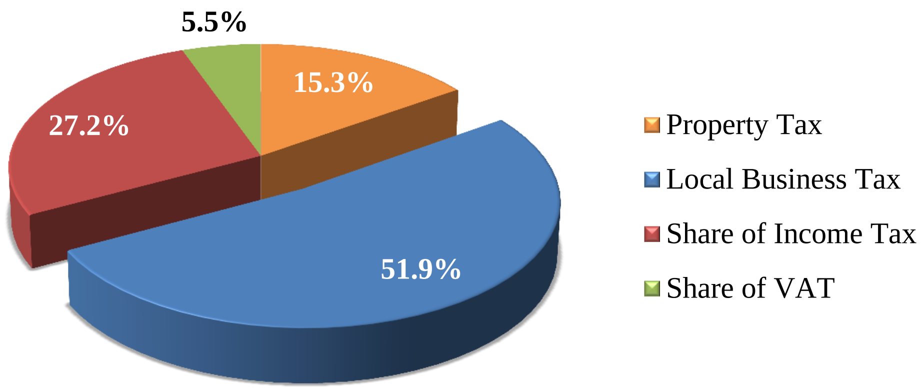


Diagram shows revenue shares of municipal taxes in the average county-independent city ("Kreisfreie Stadt") in 2007. Property Tax comprises Property Taxes A and B. Local Business Tax net of contributions to higher administrative levels ("Gewerbesteuerumlage").

But also local politicians react in one way or another to structures they are working in and upper-level decisions they are "exposed to". This is particularly relevant in the case of the local business tax. As is shown in Figure 1.2, this is the most important tax for German municipalities and – since local politicians decide upon its level – of highest relevance. Since the tax level influences location decisions of firms and, via the resulting tax revenues, also the scope for designing local policies, factors that affect the tax-setting behavior of politicians have to be identified. This leads to the spatial administrative structure of municipalities in the sense of the shape of their borders. Depending on the position of a municipality in its region and on the level of fragmentation of such a region, different tax regimes can result. But borders of municipalities are also subject to reforms. Which dimension they can have is shown in Table 1.1, which refers to reforms in Germany that are also utilized in this book. The way politicians react to incorporations of adjacent municipalities and the hereby induced incentives offered to potentially moving citizens is a further aspect of interest.

Table 1.1: Cities with the Highest Intensity of Incorporation

	Pop. 2008	Area 1961	Area 2008	Area Growth
Ansbach	40,454	9.65	99.91	935.34 %
Hamm	182,459	24.80	226.24	812.26 %
Neustadt an der Weinstraße	53,658	17.68	117.10	562.33 %
Wolfsburg	120,538	31.20	204.03	553.94 %
Bielefeld	323,615	46.84	257.91	450.62 %
Bonn	317,949	31.30	141.22	351.18 %
Memmingen	41,050	15.89	70.20	341.79 %
Münster	273,875	73.84	302.93	310.25 %
Straubing	44,496	19.31	67.58	249.97 %
Passau	50,717	19.94	69.55	248.80 %

Only "county-independent" cities are displayed. The mean intensity of incorporation among all county-independent cities amounts to 112.46 % (area growth), and only a few cities have experienced no incorporations. Area in square kilometers.

Accordingly, this book is concerned with institutions and public sector performance, applied to the cases of revenue forecasting and the spatial administrative structure of municipalities. What do these two issues have in common? Regardless of whether one considers a revenue forecaster or a local politician, both are exposed to circumstances associated with institutions that are designed and implemented by upper-level politicians. These institutions entail incentives that crucially influence the behavior of politicians or political agents. A revenue forecaster may act differently when employed by an independent research institute rather than by a Federal Ministry of Finance. The resulting difference in the revenue forecast in turn has an impact on the budget process of the government and hence on the policies implemented (see Chapters 2 and 3). Similarly, a local politician who decides upon tax policy will behave according to the institutions in which he is employed. As already mentioned, the politician will presumably set lower taxes when there are more institutions (here: municipalities) surrounding his home city (Chapter 4). In the case of reforms of spatial administrative structures it is the scope of institutions that is affected, which particularly influences the expenditure decisions of politicians that in turn have an impact on the residence decisions of households (Chapter 5). Hence, all chapters emphasize the link between institutions and the policies resulting from them. Moreover, they are all concerned with implications for budgets. While revenue forecasting tries to assess future results of current laws, adjustments of the local business tax rate aim to change the law to directly affect revenues. Incorporations of surrounding municipalities almost always result in larger budgets – not least because of more firms being subject to taxation and higher revenue shares from income and value added taxes.

The book is structured into five more self-contained chapters. The next two chapters analyze revenue forecasting, Chapters 4 and 5 are dedicated to spatial administrative structures, and the book ends with some concluding remarks given in Chapter 6.

We start in Chapter 2 by reviewing the practice and performance of revenue forecasting in selected OECD countries. While the mean forecast error turns out to be small in most countries, the standard deviation of the forecast error points to substantial differences in the forecasting performance across countries. In analyzing whether these differences are associated with differences in the conditions of revenue forecasting, it shows that they are first of all explained with the uncertainty about the macroeconomic fundamentals. To some extent they are also driven by country characteristics such as the importance of corporate and (personal) income taxes. Furthermore, differences in the timing of the forecasts prove important. To account for differences in the assignment of forecasting, we come up with an index of the independence from possible government manipulation, which comprises information on whether private institutes and/or external experts are involved, and on the provider of the underlying macroeconomic forecast. While controlling for the other differences, we find that the independence of revenue forecasting from possible government manipulation exerts a robust, significantly positive effect on the accuracy of revenue forecasts. Moreover, for the European countries there are signs that forecasting precision has increased with the establishment of fiscal surveillance by the European institutions. The results are confirmed when distinguishing between four groups of taxes. It shows that the forecasting precision is particularly low for income and corporation taxes. For these we find that the precision strongly depends on the timing of the forecast.

These results motivate to look explicitly into one country. Since independence has proven to be an important factor in explaining the quality of forecasts, valuable insights can be gained from considering a number of further variables describing the environment of forecasting. This is done by analyzing the performance of revenue forecasting in Germany in the third chapter. Tools provided by the literature on rational forecasting are used to investigate both unbiasedness and efficiency of the forecasts, but also whether indications for the relevance of politics can be found. Employing data from 1971 until 2009, we find forecasts to be unbiased and widely efficient; only with regard to tax law changes there are signs that available information at the time of the forecast is not utilized in an efficient way. We find evidence that the effects of tax law changes that are known to the forecaster are overestimated. When law changes are not yet taken into account by the forecaster, however, the forecast errors go in the expected direction. While a substantial part of revenue forecast errors can be explained by GDP forecast errors, there is no evidence that using the GDP forecast of the German Council of Economic Advisors leads to more efficient results. The analysis of possible influences of politics on revenue forecasting shows some room for improvement of the forecasts with respect to the term of office. We find the tax revenue forecast error to become smaller the longer the government is in office. This might reflect larger overestimations at the beginning of the rule, in order to convey the impression that political programs are sufficiently funded.

The second part of this book analyzes the role of institutions on the local level. Chapter 4 considers the impact of the shape of municipalities' borders on local business tax policy in core cities of agglomerations. First, a model is presented that shows the dependence of the level of taxation on the spatial administrative structure. Afterwards, data from Germany are employed to discover the effects of the number and size of municipalities within agglomerations. In the definition of agglomerations we rely on the one hand on a distance-based approach, but further develop a method that is based on cumulative population densities. The results show that the spatial administrative structure matters for the level of local business taxation. On the one hand, the core city's tax rate in a metropolitan area is lower the more municipalities are situated around the city. The effect is confirmed when we focus on the average population size of neighboring municipalities rather than on the sheer number of them, since smaller municipalities imply more competitors. On the other hand, the tax of the core city is higher the larger its population share in the agglomeration. Thereby, the result has more power for regions defined wider, since a given share in a large region is associated with a more powerful position of this city than the same share in a small region. These empirical results coincide with those results from the theoretical analysis. Furthermore, they hold irrespective of whether one defines agglomerations based on distances of surrounding municipalities, or based on cumulative population densities in the agglomeration.

The fifth chapter looks at adjustments of spatial administrative structures. During the 1960s and 1970s, the number of municipalities in Germany was notably reduced. Many municipalities located on the outskirts of a city lost their independent status and became a district of the adjacent core city. This chapter analyzes the consequences of such a reform on the population development in these city districts. In comparing incorporated municipalities with those that remained independent, the former are found to perform better in terms of population growth. This effect is confirmed when differences in states' population growth rates are taken into account, and becomes stronger for municipalities that were incorporated later and for smaller municipalities. Among the arguments for the effects found may be improvements in the infrastructure between city and incorporated municipality or a more efficient provision of public goods in general. Also the location of past housing programs may play a role. To avoid selection bias by possibly comparing two groups with different properties, a propensity score matching approach is employed. This allows us to compare incorporated with still independent communities that had a similar propensity to be incorporated and, hence, similar characteristics. We find that the propensity score is basically driven by the distance of the community to the core city, the size in terms of population and area, and the state to which it belongs. Several methods of propensity score matching are employed, all of them prove able to reduce the difference between the group of incorporated and the group of still independent municipalities to a large and sufficient extent.

Following the four main chapters, this book – as already announced – wraps up with some concluding remarks provided in Chapter 6.

Chapter 2

Revenue Forecasting Practices: Differences across Countries and Consequences for Forecasting Performance

Abstract*

This chapter reviews the practice and performance of revenue forecasting in selected OECD countries. It turns out that the cross-country differences in the performance of revenue forecasting are first of all associated with uncertainty about the macroeconomic fundamentals. To some extent, they are also driven by country characteristics such as the importance of corporate and (personal) income taxes. Also, differences in the timing of the forecasts prove important. However, controlling for these differences, we find that the independence of revenue forecasting from possible government manipulation exerts a robust, significantly positive effect on the accuracy of revenue forecasts.

2.1 Introduction

When the financial crisis hit the economy of OECD countries in 2008, the fiscal outlook for most OECD countries deteriorated substantially. On the revenue side, tax receipts turned out to be much lower than officially predicted. In the US, for instance, the 2008 federal government revenues turned out to be 7.8 % and 5.5 % below official revenue forecasts by the Congressional Budget Office from January 2007 and the Office of Management and Budget from February 2007. For Ireland, the 2008 revenue figure issued by the Department of Finance in December 2008 turned out to be 13.4 % lower than was predicted a year earlier. It seems straightforward to relate these forecast errors to the severe recession that hardly anyone predicted in the first half of 2007 when these forecasts were made. However, given that these forecasts play an important role in setting up the budget, it seems interesting to compare forecasting performance across countries and to discuss its

*This chapter is joint work with Thiess Buettner. It is based on our paper "Revenue Forecasting Practices: Differences across Countries and Consequences for Forecasting Performance," *Fiscal Studies*, 31 (3), 2010.

relationship with different forecasting practices.

Since revenue forecasting is an essential part of budgeting in the public sector, all countries make efforts to obtain reliable figures for the expected revenues – which is a difficult task. Preparing revenue forecasts involves not only predictions about macroeconomic development but also predictions about the functioning of tax law and its enforcement. Furthermore, there are changes in tax laws and structural changes in the economy that make forecasting even more difficult. Another possible uncertainty lies in the repercussions of revenue developments on public spending and the associated macroeconomic consequences. While these challenges are faced by forecasters in all countries, there are significant differences in the practice of revenue forecasting.

In particular, institutional aspects of revenue forecasting differ. In several countries, the executive branch of the government is directly in charge; other countries delegate the forecasting task to independent research institutes and emphasize the independence of forecasting. This raises the question of whether forecasting performance is affected by the different practices involved. Given the efforts that some countries devote to ensuring independence from possible government manipulation, it is particularly interesting to explore whether this independence has a noticeable impact on the quality of the forecasts.

The performance of revenue forecasting and possible determinants including institutional aspects have been explored in the literature in different directions.[1] Revenue forecasting has received most attention in the context of US states' revenue forecasts. Feenberg et al. (1989), for instance, provide evidence that state revenue forecasts are biased downwards. More recently, Boylan (2008) finds evidence for biases associated with the electoral cycle. Bretschneider et al. (1989) focus on the accuracy of revenue forecasts and find that accuracy is higher in US states with competing forecasts from executive and legislative branches.

[1] For a recent survey, see Leal et al. (2008).

Moreover, Krause, Lewis, and Douglas (2006) provide some evidence that the accuracy of states' revenue fund estimates depends systematically on the staffing of the revenue forecasting teams. As Bretschneider et al. (1989) note, the design of US state governments has specific features such as balanced–budget rules and a rivalry between executive and legislative branches of government which may explain some of these results.

International comparisons have mainly centered on forecasts of the budget balance. Recently, the relative performance of deficit forecasts among European countries has been examined in the context of the European Union's Stability and Growth Pact. Strauch, Hallerberg, and von Hagen (2004) consider forecast errors associated with the so-called "stability programmes" of EU member states, Jonung and Larch (2006) discuss political biases of the output forecasts and Pina and Vedes (2007) are concerned with institutional and political determinants of forecast errors for the budget balance. With regard to the narrower issue of revenue forecasting, international comparisons of practice and performance are mainly concerned with developing countries,[2] where institutions relevant for revenue forecasting are underdeveloped.[3]

Against this background, this chapter provides an analysis of the performance of official revenue forecasts and its determinants among 12 OECD countries. The selection of countries aims to capture the seven largest OECD economies (the US, Japan, Germany, Italy, the UK, France, and Canada). Some further countries were added where detailed information about revenue forecasting was available – selected countries in Western Europe (Austria, Belgium, Ireland, and the Netherlands) and New Zealand.

It turns out that the cross-country differences in the performance of revenue forecasting are first of all associated with uncertainty about the macroeconomic fundamentals. To

[2] For example, Kyobe and Danninger (2005).

[3] See Danninger (2005).

some extent, they are also driven by country characteristics such as the importance of corporate and (personal) income taxes. Also, differences in the timing of the forecasts prove important. However, controlling for these differences, we find that the accuracy of revenue forecasting increases with the independence of forecasts from possible government manipulation.

The following section presents descriptive statistics on the performance of revenue forecasting among our sample of OECD countries. Section 2.3 provides an overview of the different conditions that forecasters face in these countries. Section 2.4 discusses institutional aspects of the forecasting task among the selected OECD countries and sets up a simple indicator of the independence of revenue forecasting from possible government manipulation. Section 2.5 presents empirical evidence on the determinants of forecasting performance. Section 2.6 provides a short summary.

2.2 Forecasting Performance

A common way to assess the quality of revenue forecasts is to consider the forecast error defined as the percentage difference between forecasted and realized revenues. A smaller forecast error is then usually regarded as a better forecast quality. However, it should be noted that official revenue forecasts are basically used to indicate the revenue constraint that needs to be taken into account in the preparation of the public budget. Often, the budget will include expenditures that have a direct or indirect effect on tax revenues. While foreseeing these effects might result in a smaller forecast error, it is not clear whether this constitutes an improvement of a forecast that basically aims to provide the policymaker with information about the revenue constraint before actions are taken. In the discussion of the revisions of US revenue forecasts, therefore, policy changes are distinguished from (macro)economic and so-called technical sources (Auerbach, 1999) of forecast errors, where the latter may refer to tax administration or evasion, for instance. However, for most countries, a decomposition is not available. Therefore the quantitative analysis presented below is based on the overall forecast error associated with the revenue forecast.

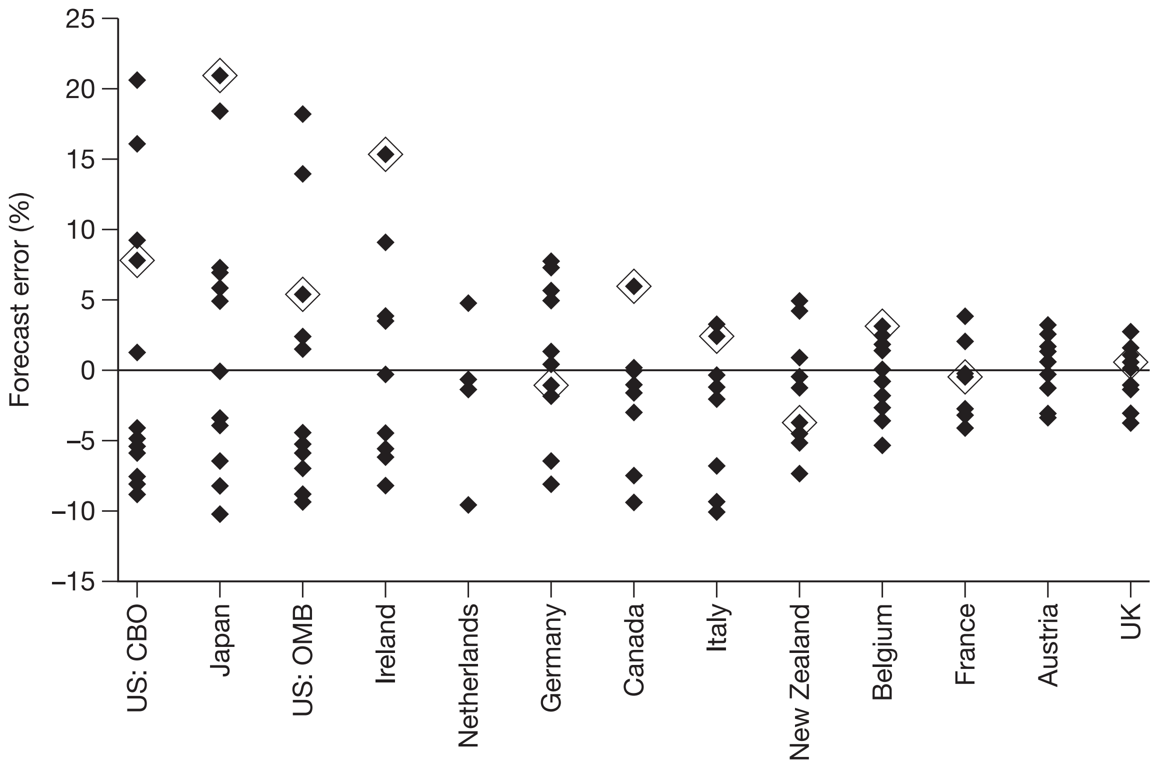
We focus on the official tax-revenue forecasts used for setting up budgets, *i.e.* we deal with revenue forecasts for the next fiscal year. In most cases, this implies that we consider a one-year-ahead forecast error for tax revenues. In some cases, in particular if the fiscal year differs from the calendar year, the forecast is sometimes issued in the same year as the fiscal year begins. Since, ultimately, the forecast should indicate the revenue constraint to the current budget, we define forecast errors as the deviation of the forecasts from the final revenues reported for the corresponding fiscal year.[4] With regard to the time period covered, note that we include forecasts issued from 1995 until 2009, but for several countries revenue forecasts were not available for some years and most forecasts were issued in the period from 1996 until 2007.[5] The forecast errors are depicted in Figures 2.1 and 2.2, where each point represents a single forecast error. Note that in Figure 2.1 the forecast errors are arranged in descending order of the respective standard deviation and that in Figure 2.2 they are arranged according to the year in which the forecasts were issued.

At first sight, Figure 2.1 seems to suggest that in most cases there is some underestimation going on. But there are also instances of large overestimations. For instance, the US Congressional Budget Office (CBO) issued a revenue forecast in January 2001 for the 2001-02 fiscal year, which started on October 1, 2001, amounting to USD 2,236 billion. Two years later, revenues turned out to be only USD 1,853 billion. Hence the forecast was about 20.6 % higher than realized revenues. A revenue forecast by the Japanese Ministry of

[4]Only for the most recent Canadian forecast, final revenues were not available.

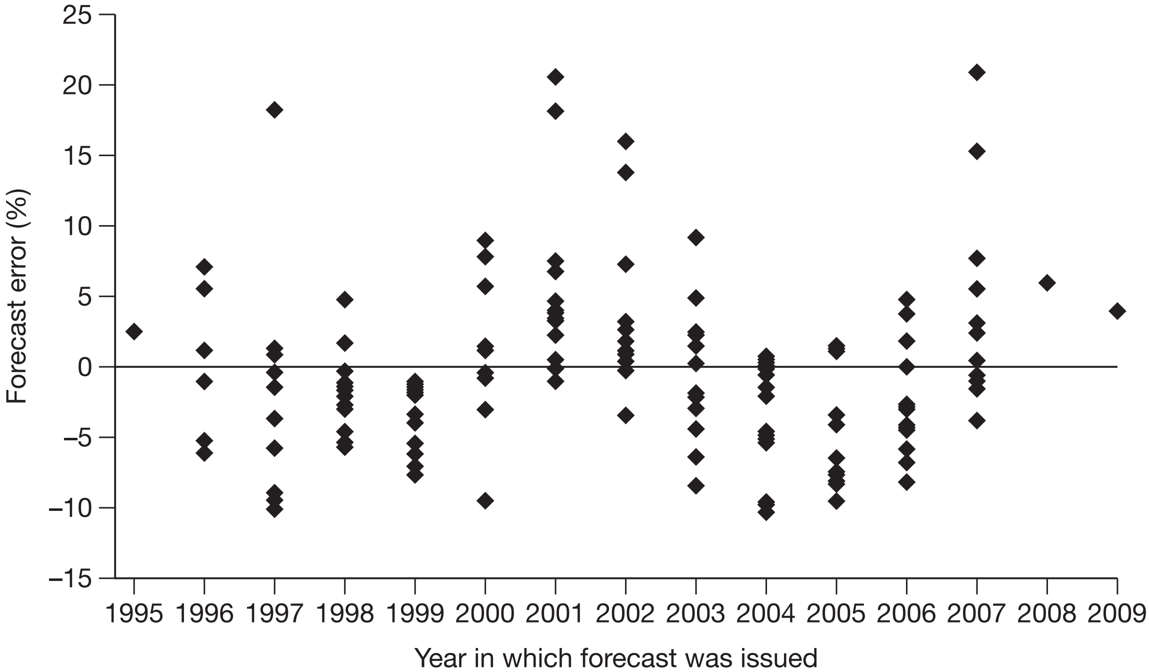
[5]See Table 2.7 in the appendix for an overview of the actual forecasts used. In the case of the Netherlands, due to structural breaks, just five years are considered.

Figure 2.1: Forecast Errors by Country/Institution



CBO – Congressional Budget Office. OMB – Office of Management and Budget.
The figure displays the forecast errors for total tax revenues in percent for up to 13 years in each country, each point representing one forecast. Forecast errors for 2008 are highlighted with a rhombus. A positive (negative) value denotes overestimation (underestimation). The forecasts are arranged in descending order of the standard deviation of the respective forecast errors. The two US forecasts only refer to federal taxes.

Figure 2.2: Forecast Errors by Year



The figure displays the forecast errors for total tax revenues in percent for 13 revenue forecasts in 12 countries. A positive (negative) value denotes overestimation (underestimation).

Finance from December 2007 for the fiscal year 2008-09 turned out to overestimate actual revenues by as much as 21.0 %. While several other forecasts associated with 2008 (marked with a rhombus) also turned out to be overoptimistic, errors of this magnitude are rare. According to Figure 2.2, the forecast errors show a marked cyclical pattern.

Table 2.1 provides figures for the mean forecast error. A positive sign indicates an overestimation of revenues, a negative sign an underestimation. In all cases except Germany, Japan, and the CBO forecast in the US, there is a slight underestimation of revenues. The largest difference from zero is found for the Netherlands, which shows an underestimation of 3.4 % on average. However, given the large standard deviations, statistically the means are not significantly different from zero.

The large differences in the standard deviation of the forecast errors (SDFE) point to substantial differences in the precision of forecasts. As can be seen in Column (3) of Table 2.1 the highest precision is achieved in the UK and Austria, while we find the lowest precision in the US and Japan.

Table 2.1 also reports the root mean squared forecast error (RMSFE), which is a common summary measure of forecasting accuracy, based on a quadratic loss function regarding forecast errors.[6] Note that the RMSFE is equivalent to a combination of the standard deviation of the forecast error and the mean forecast error.[7] However, as documented by Table 2.1, the standard deviation of the forecast error and the RMSFE of revenue forecasts do not show large differences.

[6]See, for example, Clements and Hendry (2002) and Wallis (2008).

[7]The mean squared forecast error (MSFE) can be decomposed into the square of the mean of the forecast error (MNFE) and the square of the standard deviation of the forecast error (SDFE) (for example, Clements and Hendry (1998)). Formally, ignoring adjustments for the degrees of freedom, we have

$$\widehat{MSFE} \simeq \widehat{MNFE}^2 + \widehat{SDFE}^2.$$

Taking the square root yields the root mean squared forecast error: $RMSFE \equiv \sqrt{MSFE}$.

Table 2.1: Descriptive Statistics of Forecast Errors

Country	MNFE[a] (1)	MNAFE[b] (2)	SDFE[c] (3)	RMSFE[d] (4)	Obs. (5)	(Fiscal-) Years (6)
Austria	-0.037	1.880	2.279	2.162	10	1997-2006
Belgium	-0.432	2.179	2.611	2.545	13	1996-2008
Canada	-2.711	4.278	5.044	5.553	13	1997/98-2009/10
France	-1.151	2.290	2.542	2.672	10	1999-2008
Germany	1.308	4.458	5.419	5.351	12	1997-2008
Ireland	-0.536	6.271	7.608	7.274	11	1998-2008
Italy	-2.297	3.716	4.626	4.973	11	1998-2008
Japan	2.578	8.076	10.003	9.918	12	1997/98-2008/09
Netherlands	-3.403	5.265	6.203	6.509	5	2000-2002, 2005-2006
New Zealand	-1.535	3.465	3.939	4.058	11	1997/98-2007/08
United Kingdom	-0.213	1.516	1.977	1.897	11	1997/98-2007/08
USA: CBO	0.807	8.361	10.175	9.775	12	1996/97-2007/08
USA: OMB	-0.472	7.347	9.031	8.659	12	1996/97-2007/08
Average	-0.623	4.546	5.497	5.488	11	

CBO – Congressional Budget Office. OMB – Office of Management and Budget.

[a]Mean of the one-year-ahead forecast error for total revenues in percent. A positive (negative) value denotes overestimation (underestimation).

[b]Mean absolute forecast error.

[c]Standard deviation of the forecast error.

[d]Root mean squared forecast error.

2.3 Conditions Faced by Forecasters

An assessment of the considerable differences in the accuracy of forecasts needs to take account of the different conditions faced by the forecasters. First of all, this is an issue of the point in time when the forecast is made. Across countries, there are important differences in the time span between the official forecast and the beginning of the forecasted period, *i.e.* the beginning of the forecasted fiscal year (see Column (1) of Table 2.2). Actually, the median varies between less than 1 month and 9.5 months.

An important source of differences lies in countries' tax structures. In particular, the degree of differentiation of the tax system might matter. Rather than relying on a few large taxes, a country might employ a variety of smaller tax instruments. Provided that the different tax instruments relate to tax bases that are not closely correlated, this might reduce the revenue risks associated with the tax system. Therefore, forecasting the revenues of a large variety of small taxes might be easier than predicting the revenues in a system that relies on a small number of large taxes. To capture the differentiation of the tax structure, we use an indicator of the number of taxes based on *OECD Revenue Statistics*. More specifically, we employ the most detailed classification of taxes and, starting with the smallest taxes, count the number of taxes needed to account for 50 % of all tax revenues.[8] Of course, this measure is only informative if the individual taxes are really different in the above sense. Moreover, comparing the number of taxes across countries raises difficult problems of classifying taxes and the OECD classification matches the various tax systems to different extents. Nevertheless, relying on this classification, Column (2) of Table 2.2 indicates that there are large differences across countries.

Some types of taxes might be more difficult to predict than others. For instance, we might

[8] While this measure is concerned with 50 % of all tax revenues, note that the results are found to be robust against choosing other fractions of tax revenues.

Table 2.2: Forecasting Conditions

Country	Time Span (Median)[a] (1)	Taxes for 50 % rev.[b] (2)	GDP Forecast error MNFE[c] (3)	SDFE[d] (4)	RMSFE[e] (5)
Austria	3.5	71.1	-0.209	1.134	1.096
Belgium	2.5	53.4	0.072	1.249	1.202
Canada	1.5	34.3	0.114	1.837	1.768
France	3.5	103.3	0.185	0.910	0.883
Germany	7.5	38.3	0.284	0.987	0.987
Ireland	0.5	20.3	-0.387	2.628	2.536
Italy	5.5	48.1	0.518	1.122	1.189
Japan	3.5	36.0	0.393	1.688	1.664
Netherlands	9.5	41.1	0.252	1.605	1.458
New Zealand	1.5	19.6	-0.334	1.757	1.708
United Kingdom	0.5	41.4	-0.544	0.915	1.028
US: CBO	8.5	22.0	-0.280	1.365	1.336
US: OMB	8				
Average[f]	4.1	44.1	0.000	1.433	1.409

CBO – Congressional Budget Office. OMB – Office of Management and Budget.

[a]Median time period between the forecast and the beginning of the forecasted period in months, taken from the various national sources listed in the appendix.

[b]Number of taxes needed to account for 50 % of revenues in the respective country, based on *OECD Revenue Statistics*.

[c]Mean of the one-year-ahead forecast error for gross domestic product in percent. A positive (negative) value denotes overestimation (underestimation).

[d]Standard deviation of the forecast error.

[e]Root mean squared forecast error.

[f]Median time span and statistics for the GDP forecast error are weighted by number of observations.

expect that there are significant differences in the forecast accuracy between forecasting corporation or personal income taxes and forecasting sales and value added taxes. This calls for a separate analysis of forecast errors according to the type of tax. The empirical analysis below therefore distinguishes four groups of taxes: personal income, corporation, value added and sales, and other taxes. This decomposition is also useful since the revenue forecasts are usually prepared for aggregates of individual taxes, especially if these taxes share the same source or taxpayer. This partly reflects the need to employ up-to-date information on current revenues, which is available usually on a source basis.

Another potentially important reason for differences in the forecast errors is related to uncertainty about the business cycle and macroeconomic development. This uncertainty is of particular importance not only because almost all taxes are affected by the macroeconomic environment. A typical feature of revenue forecasting is that taxes that are strongly driven by macroeconomic developments, such as corporation taxes or wage and income taxes, are forecasted using indirect methods. Predominantly, the elasticity method is employed, where some previously estimated elasticity is used to predict revenue growth based on the predicted development of GDP or its components.[9]

Columns (3) to (5) of Table 2.2 provide some statistics for macroeconomic uncertainty for each of the different countries. Note that, as with the revenue forecasts, we are relying on the relative forecast error in percentage points. For instance, the mean forecast error of -0.544 for the UK indicates that, on average, predicted GDP was about half a percentage point lower than actual GDP.[10] Note that the GDP forecasts are not taken from the same source as the above official revenue forecasts. This is important since in some cases the macroeconomic predictions used by the forecasters are based on their own assessment, while in other cases the macroeconomic forecasts of the government are used (see Sec-

[9] For an overview of methods of revenue forecasting, see King (1993).

[10] As in the case of revenue forecasts, the forecast error is computed relative to final figures.

tion 2.4). Conditioning on these predictions would not allow us to capture the impact of possible government manipulation. Therefore, we resort to the German Council of Economic Advisors, an independent body which annually issues forecasts of macroeconomic developments including GDP for a large group of countries.[11]

Uncertainty about revenues also stems from changes in tax law. The immediate "mechanical" effects of tax law changes are often difficult to estimate. In addition, changes in tax law exert all sorts of behavioral effects with revenue consequences that are hard to quantify.[12] This implies that revenue forecasts tend to be much more difficult in the presence of tax law changes. While this may suggest attempting to capture revenue effects of major tax reforms, we have not been able to collect data on revenue estimates for tax reforms. But we should note that there is also uncertainty about which tax law changes will actually be implemented. In some countries, it is common practice not only to include in the revenue forecasts those tax law changes that are already enacted but also to include changes that are agreed within the government (Austria, the Netherlands) or noted in the budget plan (Ireland). If these changes are postponed, amended, or withdrawn, large forecast errors may occur even if the revenue estimate of the reform that was initially intended was correct.

2.4 Institutions and Independence

A basic institutional aspect of revenue forecasting is the assignment of the forecasting task to specific institutions. Interestingly, forecasting is not always assigned to a department of the government or, more precisely, to the executive branch of the government. Only in

[11] An advantage of these forecasts is that the one-year-ahead forecasts are issued every year in November, so there are no timing differences across countries and time.

[12] For a discussion of "dynamic scoring" in revenue estimation, see Adam and Bozio (2009) and Auerbach (2005).

about half of the 13 forecasts surveyed in this chapter is it the Ministry of Finance (Belgium, France, Ireland, Italy, Japan) or the Treasury (New Zealand, the UK) that is responsible.[13] In most other cases, forecasting is assigned to a group representing different institutions, not only the executive branch. Some countries even assign the primary responsibility for revenue forecasting to independent research institutes (the Netherlands) and limit the influence of the executive branch such that it merely consults forecasters. In the other countries, even if the Ministry of Finance or another part of the executive is responsible, external experts from academia or forecasting agencies are often included in the forecasting group.

The efforts to involve institutions that are not part of the government or external experts are usually justified as a means to raise the independence of revenue forecasting from possible manipulation by and strategic influence of the government. Several countries explicitly produce *consensus forecasts*, where all institutions and experts involved have to agree on the forecast (for example, Austria and Germany). However, the extent to which forecasting is independent from government manipulation depends not only on the assignment of forecasting responsibility but also on whether revenue forecasting is based on government predictions for macroeconomic development, as is the case with the official German forecast.

Table 2.3 presents information about how revenue forecasting differs with respect to these issues. The first column indicates whether the government (= 0), research institutes (= 1) or both jointly (= 0.5) are responsible for the forecast. In some cases, no research institutes are involved but, in order to preserve a certain degree of independence, external experts are consulted (see Column (2)). This is the case for the US forecasts of the Congressional Budget Office (CBO) and the Office of Management and Budget (OMB). In the case of the UK, a value of 0.5 is entered, in order to take account of the reported partial consultation of

[13]For a detailed list of sources for the various forecasts covered, see the appendix.

experts.[14] A figure of 0.5 is also entered for Germany, in order to account for the additional participation of the German central bank. For the Netherlands, a figure of -1 is entered to take account of the consulting participation of the Ministry of Finance, which may tend to reduce independence. The third column of the table provides information about the source of the macroeconomic forecast. A value of 1 indicates that an external forecast is used.

By summing across the first three columns of Table 2.3, we obtain a simple indicator of the independence of revenue forecasting. The first column is weighted by 1; the second and third columns are weighted by 0.25. The rationale behind this weighting is the following: a revenue forecast that is conducted by a research institute without any government experts involved would display the maximum level of independence (= 1). A government forecast that includes external experts and employs an external macroeconomic forecast would obtain a medium level of independence (= 0.5). A government forecast without any external experts and without an external macroeconomic forecast would be assigned the lowest level of independence (= 0).

While the indicator varies from zero (= no independence) to unity (= full independence), in our sample of countries the highest degree of independence is 0.75. As can be seen, the indicator is highest for the Netherlands and Austria, followed by Germany. A small, but positive, level of independence can be found in Canada, New Zealand, Belgium and the UK. The US case is somewhat special since here two separate forecasts exist. One is conducted by the OMB, which assists the executive branch; the other is conducted by the CBO, which is assigned to the legislative branch. While their incentives to manipulate forecasts strategically might differ, our indicator of independence, which is simply assessing the institutional conditions, assigns a low value of independence to both of them.[15]

[14]Interestingly, the UK government has recently established the Office for Budget Responsibility to "make an independent assessment of the public finances and the economy for each Budget and Pre-Budget Report" (see www.hm-treasury.gov.uk/data_obr_index.htm).

[15]Bretschneider et al. (1989) argue that the existence of two separate forecasts by the legislative and

Table 2.3: Institutional Characteristics and Independence

Country	Research institutes[a]	Ext./Gov. experts[b]	Macroecon. forecast[c]	Independence[d]
Austria	0.5	0	1	0.75
Netherlands	1	-1	0	0.75
Germany	0.5	0.5	0	0.625
Belgium	0	0	1	0.25
Canada	0	0	1	0.25
New Zealand	0	1	0	0.25
US: CBO	0	1	0	0.25
US: OMB	0	1	0	0.25
United Kingdom	0	0.5	0	0.125
France	0	0	0	0.00
Ireland	0	0	0	0.00
Italy	0	0	0	0.00
Japan	0	0	0	0.00

[a]This column indicates whether the government (= 0), research institutes (= 1) or both jointly (= 0.5) are responsible for the forecast.

[b]This column indicates whether external experts (= 1) or government experts (= -1) are involved. For the UK, a value of 0.5 is entered in order to take account of the reported partial consultation of experts. In Germany, a figure of 0.5 is entered in order to account for the participation of the central bank.

[c]This column provides information about whether an external macroeconomic forecast is used. (The appendix contains a list of various national sources providing this information.)

[d]The degree of independence is obtained as a weighted sum of the first three columns. The first column is weighted by 1 and the second and third columns are weighted by 0.25 (see text).

The general composition of the index, with its emphasis on research institutes, external experts, and the source of the macroeconomic forecast, reflects key institutional characteristics of revenue forecasting. Yet the weights used to aggregate the information about these institutional aspects are somewhat arbitrary. Therefore we conducted some robustness checks where the weights for external experts and external macroeconomic forecasts were increased or decreased. With regard to the ranking, however, only minor changes were found. We will come back to this issue in Section 2.5, where we explore whether the index of independence has sufficient informational content to help explain the observed forecasting performance.

Though we include several European countries, the index does not take account of the fiscal surveillance by EU institutions. Since 1999, due to the Stability and Growth Pact (SGP), EU member states are required to submit budgetary projections including revenue forecasts every year to the European Commission and the Ecofin Council. The forecasts also play a role in the Excessive Deficit Procedure, which defines sanctions for member states that continuously violate the agreed fiscal rules. It should be noted, however, that the purpose of the corresponding revenue forecasts is different: they are not issued to set up and justify the budget plan. Rather, these projections provide the European Commission and the Ecofin Council with necessary information for the purpose of surveillance of budgetary positions and economic policies. Nevertheless, the existence of a supranational body discussing and standardizing the member states' revenue forecasts might well have implications for the national governments' revenue forecasts. By including indicators for EU countries in the time period starting in 1999, the empirical analysis in Section 2.5 tests for a possible impact on the performance of revenue forecasts.

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executive branches exerts a positive effect on forecasting accuracy, in particular when both forecasts are forced into a consensus. This is, however, not the case with the OMB and the CBO.

2.5 Determinants of Forecasting Performance

Having outlined differences in forecasting conditions and practices, let us finally turn to the question of to what extent these are associated with the large differences in forecasting performance noted in Section 2.2. In a first step, we consider the level of the revenue forecast error and test for the presence of forecast biases. Table 2.4 provides the results.

Column (1) indicates that the overall mean or average forecast error is not significantly different from zero. The specification in Column (2) takes account of the panel structure of the data and allows for institution-specific differences in a potential bias – which prove not significant, however. To take account of the difficulties in predicting the macroeconomic environment, Columns (3) and (4) condition on the one-year-ahead GDP forecast error for each country. It shows a strongly significant impact indicating that an unpredicted increase in GDP by 1 % results in an increase of revenues by almost 2 %. According to Column (3), the average conditional forecast is not significantly different from zero. When we allow the average forecast error to differ between forecasting institutions (Column (4)), we find that only the forecasts for Canada and Italy show significant biases. In both cases, conditional on the forecast error associated with the GDP forecast, the estimation indicates that, on average, forecasts have been too pessimistic.

In order to explore whether differences in the forecast errors can be assigned to the forecasting institutions, in Columns (5) and (6) we replace the dummies with a set of institution-specific indicators, most of which are timeinvariant. The set of indicators includes the time span between the forecast and the forecasted period, the indicator of the differentiation of the tax structure and the indicator of the independence of forecasting institutions. However, none of these indicators is significant. While not shown, note that we also tested for some specific effect for European countries, which are required from 1999 onwards to report revenue forecasts to European institutions. Even if we allow the coefficients for the Eu-

Table 2.4: Determinants of Forecast Error

	(1)	(2)	(3)	(4)	(5)	(6)
Constant	-.486		-.441		.171	2.65
	(.517)		(.463)		(4.02)	(3.62)
GDP forecast error			1.89 ***	1.99 ***		1.92 ***
			(.316)	(.323)		(.322)
Time span					.091	.000
					(.167)	(.150)
log(No. of taxes					-.166	-.879
for 50 % of revenue)					(1.12)	(1.01)
Independence					-.308	.326
					(2.29)	(2.06)
Austria		-.037		.380		
		(1.98)		(1.75)		
Belgium		-.432		-.576		
		(1.73)		(1.53)		
Canada		-2.71		-2.94 *		
		(1.73)		(1.53)		
France		-1.15		-1.52		
		(1.98)		(1.75)		
Germany		1.31		.743		
		(1.81)		(1.60)		
Ireland		-.536		.234		
		(1.88)		(1.67)		
Italy		-2.30		-3.33 **		
		(1.88)		(1.67)		
Japan		2.58		1.80		
		(1.80)		(1.60)		
Netherlands		-3.40		-3.90		
		(2.80)		(2.47)		
New Zealand		-1.54		-.871		
		(1.88)		(1.67)		
United Kingdom		-.213		.869		
		(1.88)		(1.67)		
US: CBO		.807		1.36		
		(1.80)		(1.60)		
US: OMB		-.472		0.84		
		(1.80)		(1.60)		
R^2	0.000	0.067	0.202	0.279	0.003	0.207
Observations	143	143	143	143	143	143

Dependent variable: One-year-ahead forecast error for total tax revenues. Standard errors in parentheses. A single star denotes significance at the 10 % level, two stars at the 5 % level, and three stars at the 1 % level.

ropean countries to differ in the time period from 1999 onwards, no significant differences are found.

The failure to find significant effects of institutional characteristics and country characteristics on the mean forecast error does not necessarily indicate that they do not exert any effect on revenue forecasts. Certainly, in the process of setting up the budget, a government or parliament is tempted to manipulate the revenue forecast and to underestimate or overestimate revenues. Yet a sustained manipulation in one direction, which would show up in a significant bias of the forecasts, hardly affects rational agents' beliefs and merely undermines the credibility of the official forecast.[16]

In a second step of the analysis, we explore the differences in forecasting performance using measures of forecast precision and accuracy. More precisely, we consider the standard deviation of the forecast error, which is an indicator of the precision of forecasts, and the root mean squared forecast error, which is a common summary statistic of forecast accuracy. The first two specifications in Table 2.5 explore whether differences in forecasting conditions show some significant effects on the precision of the forecasts, measured by the SDFE for total tax revenues. Column (1) just includes indicators of macroeconomic uncertainty and of the time span between the forecast and the beginning of the forecasted period. The results confirm a strong impact of macroeconomic uncertainty measured by the standard deviation of the GDP forecast error. They also indicate that precision decreases considerably with the time span: every additional month increases the standard deviation by three-quarters of a percentage point. In Column (2), we include our indicator for the differentiation of the tax system into single taxes. The negative sign indicates that forecasting is more precise in countries where the number of taxes is larger. However, the

[16]Consistent with this view, the literature developing models of *rational* forecast bias relies on settings not with one but with multiple forecasting agents, where individual forecasters have incentives to differentiate their forecasts from those of other forecasters (see, for example, Laster, Bennett, and Geoum (1999)).

Table 2.5: Determinants of Forecasting Precision and Accuracy: Total Revenues

Dependent variable	SDFE								RMSFE	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Constant	-.669	7.04	.892	7.16	.665	.905	2.74	3.66	2.22	-.372
	(1.78)	(7.47)	(1.67)	(6.38)	(1.56)	(1.81)	(1.68)	(6.22)	(1.53)	(5.72)
Time span	.734 ***	.635 ***	.861 ***	.773 ***	.887 ***	.830 ***	.772 ***	.762 ***	.755 ***	.785 ***
	(.190)	(.210)	(.171)	(.192)	(.167)	(.177)	(.152)	(.175)	(.130)	(.151)
log(No. of taxes for 50 % of revenue)		-1.63		-1.34				-.216		.584
		(1.54)		(1.32)				(1.39)		(1.24)
Independence[a]			-4.40 *	-4.19 *	*-4.88* **	*-3.61* *	-3.89 *	-3.88 *	-3.44 **	-3.42 *
			(2.00)	(2.01)	*(2.00)*	*(1.94)*	(1.72)	(1.84)	(1.47)	(1.55)
EU-SGP							-2.15 *	-2.05	-2.13 **	-2.39 *
							(1.03)	(1.27)	(.873)	(1.07)
SDFE for GDP	4.36 **	3.08	4.03 **	3.00 *	4.08 **	4.02 **	3.34 **	3.21 **		
	(1.20)	(1.69)	(1.03)	(1.44)	(.984)	(1.09)	(.937)	(1.32)		
RMSFE for GDP									3.66 ***	4.10 **
									(.878)	(1.30)
R^2	0.677	0.713	0.790	0.814	0.805	0.767	0.864	0.864	0.894	0.897
adj. R^2	0.612	0.617	0.720	0.721	0.741	0.689	0.796	0.767	0.841	0.824
Observations	13	13	13	13	13	13	13	13	13	13

Dependent variable in Columns (1)-(8) is the standard deviation of one-year-ahead forecast error for total tax revenues. Columns (9) and (10) focus on the root mean squared forecast error. Weighted least squares estimates taking account of the number of forecasts considered in the computation of the standard deviation. Robust standard errors in parentheses. A single star denotes significance at the 10 % level, two stars at the 5 % level, and three stars at the 1 % level. [a]) Column (5) provides results from a specification where the index of independence uses a higher weight for external macroeconomic forecasts and external experts. Column (6) refers to a specification where the index uses a lower weight for external macroeconomic forecasts and external experts.

effect is not significant.

Columns (3) and (4) show the same specifications augmented with the indicator of the independence of revenue forecasting. While the results from Columns (1) and (2) are confirmed, we find that the precision of the forecast is positively associated with the independence from possible government manipulation. The coefficient of determination (R^2) for the specification in Column (3) indicates that about 80 % of the variation in the precision of the forecasts can be associated with the time span, macroeconomic uncertainty and the degree of independence.

Since the indicator of independence rests on a weighted sum of three institutional characteristics, we conducted some robustness tests using different weights. However, the results do not indicate major differences. If the weights for external experts and external macroeconomic forecasts are increased or decreased by 0.1, for instance, all effects are confirmed (see Columns (5) and (6)).

To capture separate fiscal forecasting requirements according to the Stability and Growth Pact, Columns (7) and (8) include an indicator for EU countries (EU-SGP). It captures the share of forecasts for European countries that were issued in the time period from 1999 onwards, when regular reports have to be filed for European institutions. Interestingly, EU-SGP shows a significantly negative effect, suggesting that the precision of revenue forecasting has generally increased in the presence of budgetary surveillance by the European Union. Yet a causal interpretation seems problematic, since the formation of the European monetary union might have exerted separate effects on the forecasting task.

Columns (9) and (10) report results of specifications where we replace the standard deviation of the forecast error with the root mean squared forecast error. While the set of explanatory variables is the same as above, for reasons of consistency macroeconomic uncertainty is also captured by the root mean squared error of the GDP forecast. It turns out that the results are very similar to the results in Columns (7) and (8). Since the RMSFE combines the standard deviation and the mean of the forecast error (see Footnote 7), this similarity reflects the finding in Section 2.2 that differences in the standard deviation of the forecast error are much more pronounced than differences in the means.[17]

Table 2.6 provides results for the precision of forecasts decomposed into four different types of taxes: (personal) income taxes, corporation taxes, value added and sales taxes, and other taxes. Thus, for each group of taxes, we compute separate indicators of forecast precision and forecast accuracy.[18] A first specification uses a similar set of variables to Column (3) of Table 2.5. In addition, it includes dummy variables for each group of taxes. The coefficients of these variables indicate that corporation taxes show a much larger standard deviation of the forecast error. As documented by the R^2 in Column (1) about 86 % of the differences in the precision of the forecasts can be assigned to tax structure, timing, and independence. In Column (2), the number of taxes needed to account for 50 % of revenues is included. While it is not significant, note that in the specifications reported in Table 2.6 this indicator refers to the corresponding group of taxes.

To test whether the time span has different effects across types of taxes, Columns (3) and (4) allow for possible differences in the effect of timing among the different groups of taxes. As can be seen, the time span is relevant, particularly for corporation taxes but also for income taxes.

All specifications support a negative significant effect on the forecast error for the independence of revenue forecasts. Columns (5) and (6) include indicators for the share of forecasts where reporting requirements to EU institutions existed (EU-SGP). Again, we

[17] Note also that an analysis based on the mean absolute error yields qualitatively similar results.

[18] Missing values are encountered since detailed information was not available for all countries.

Table 2.6: Determinants of Forecasting Precision and Accuracy: Disaggregated Revenues

Dependent variable	SDFE						RMSFE	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Time span	.985 ***	1.08 ***	-.014	.055	-.153	.015	-.172	.035
	(.130)	(.243)	(.284)	(.351)	(.345)	(.382)	(.338)	(.367)
Time span × Tax type 1			1.54 ***	1.45 **	1.54 ***	1.28 **	1.51 ***	1.21 **
			(.484)	(.555)	(.491)	(.522)	(.441)	(.473)
Time span × Tax type 2			1.95 **	1.88 **	1.95 **	1.75 **	1.83 **	1.59 **
			(.730)	(.765)	(.742)	(.729)	(.733)	(.707)
Time span × Tax type 3			.512	.469	.512	.391	.604 *	.462
			(.339)	(.360)	(.344)	(.360)	(.335)	(.336)
SDFE for GDP	3.68 **	5.38 **	3.68 ***	4.02 ***	3.01 ***	3.84 **		
	(1.06)	(1.82)	(1.10)	(1.25)	(.610)	(1.32)		
RMSFE for GDP							3.09 ***	4.23 ***
							(.484)	(1.31)
log(No. of taxes		2.09		.419		1.18		1.39
for 50 % of revenue)		(1.41)		(.950)		(1.10)		(1.06)
Independence	-4.87 **	-4.89 *	-4.87 *	-4.87 *	-3.57 **	-3.33 *	-2.82 **	-2.46 *
	(2.15)	(2.60)	(2.23)	(2.32)	(1.48)	(1.58)	(1.34)	(1.34)
EU-SGP					-2.65 ***	-3.17 ***	-2.68 ***	-3.25 ***
					(.641)	(.958)	(.509)	(.812)
Tax type 1	2.45	-2.66	2.44	1.42	4.21 **	1.67	4.00 ***	.763
(Income taxes)	(2.15)	(3.87)	(2.16)	(2.63)	(1.38)	(3.38)	(1.12)	(3.24)
Tax type 2	11.0 ***	6.13	11.0 ***	10.0 ***	12.7 ***	10.3 ***	12.5 ***	9.38 **
(Corporation taxes)	(2.68)	(3.43)	(2.61)	(2.50)	(2.04)	(3.19)	(1.85)	(3.11)
Tax type 3	.782	-8.21	.788	-1.01	2.56 *	-2.18	2.50 **	-3.32
(Value added and sales taxes)	(2.07)	(6.59)	(2.08)	(3.99)	(1.20)	(5.13)	(.939)	(5.04)
Tax type 4	.868	-6.04	.880	-.504	2.65 **	-.911	2.41 **	-2.02
(Other taxes)	(2.41)	(5.50)	(2.25)	(3.30)	(1.07)	(3.98)	(.933)	(3.94)
R^2	0.862	0.874	0.916	0.916	0.925	0.928	0.928	0.932
Observations	48	48	48	48	48	48	48	48

Dependent variable in Columns (1)-(6) is the standard deviation of the one-year-ahead forecast error for tax revenues grouped into four types of taxes. Columns (7) and (8) focus on the root mean squared forecast error. Robust standard errors in parentheses take account of possible correlation between the forecasts for different groups of taxes. A single star denotes significance at the 10 % level, two stars at the 5 % level, and three stars at the 1 % level.

find significantly negative effects, suggesting that the quality of revenue forecasts is increased as a result. The final two columns of Table 2.6 report results of specifications that focus on the root mean squared forecast error. As with Table 2.5, the results are very similar, qualitatively.

2.6 Summary

In this chapter, we have compared revenue forecasting practice and performance across selected OECD countries. While the mean forecast error is small in most countries, the standard deviation of the forecast error and also summary statistics of forecast accuracy, such as the root mean squared forecast error, point to substantial differences in forecasting performance across countries. This raises the question of whether differences in performance are associated with differences in the conditions and practices of revenue forecasting in these countries.

First of all, it seems likely that important conditions for revenue forecasting are different. This refers to uncertainty about the macroeconomic fundamentals as well as to country characteristics such as the tax structure both in terms of the differentiation into different taxes and with regard to the importance of corporate and (personal) income taxes. But also institutional arrangements vary between countries. This refers not only to the timing of revenue forecasts. While in some countries the Ministry of Finance or the Treasury is responsible, other countries delegate the forecasting task to research institutes. Further differences arise with regard to the inclusion of external experts and with regard to the source of macroeconomic forecasts. To summarize these differences, we came up with an index of independence from possible government manipulation. According to this index, the revenue forecasts are most independent in Austria and the Netherlands.

The quantitative analysis shows that the cross-country differences in the performance of revenue forecasting are first of all related to uncertainty about macroeconomic development: the GDP forecast error exerts a strong effect on the error of revenue forecasts; also, the precision of the revenue forecasts, measured by the standard deviation of the forecast error, is found to be driven by macroeconomic uncertainty. Controlling also for differences in the timing of forecasts, we find that the precision of revenue forecasts increases with the independence of forecasts from possible government manipulation. About 80 % of the differences in forecasting precision concerning total revenues can be explained by differences in macroeconomic uncertainty, in timing, and in the degree of independence. For the European countries, we find some evidence that forecasting precision has increased with the establishment of fiscal surveillance by the European institutions. But it seems difficult to interpret this finding as a causal effect, since the creation of the monetary union might also have exerted direct effects on the difficulties of the forecasting task.

The results are confirmed when distinguishing between four groups of taxes – (personal) income taxes, corporation taxes, value added and sales taxes, and a residual category. This analysis further shows that forecasting precision is particularly low for income and corporation taxes. For these taxes, we find that precision depends strongly on the time span between the forecast and the beginning of the forecasted period.

Our finding of a significant impact of institutional conditions on forecasting performance proves robust against alternative measures of forecasting accuracy. Employing the root mean squared forecast error as a summary measure of forecasting accuracy, we obtain very similar results.

While we have provided robust evidence for a beneficial effect of independence on forecast accuracy, an analysis of governments' incentives to exert an influence on forecasts and the consequences of this influence is beyond the scope of the current chapter and is left for future research. However, given the weak evidence for significant biases, our analysis suggests that government influence tends to show up in temporary deviations of forecasts from the expected values.

Appendix: Sources of Information

Austria

The official revenue forecast for Austria is documented/discussed in:

- Bundesministerium für Finanzen (2007): Budget 2007-2008, Zahlen – Hintergründe – Zusammenhänge. Online: www.bmf.gv.at
- Homepage of the Ministry of Finance: www.bmf.gv.at
- Homepage of the Wifo (Österreichisches Institut für Wirtschaftsforschung): www.wifo.ac.at
- Leibrecht (2004)

Belgium

The revenue forecast of the federal government is documented/discussed in:

- Chambre des représentants de Belgique: Budgets des Recettes et des Dépenses pour l'année budgétaire 1996, ... pour l'année budgétaire 2007, Brussels

- Hertveldt, Bart et al. (2003)
- Lenoir, Thierry and Christian Valenduc (2006): Révision de la méthode macro-économique d'estimation des recettes fiscales. *Service Public Fédéral Finances,* Brussels

Canada

The spring revenue forecast of the Canadian Department of Finance is documented/discussed in:

- Mühleisen et al. (2005)
- O'Neill, Tim (2005): Review of Canadian Federal Fiscal Forecasting: Processes and Systems. *O'Neill Strategic Economics*. Online: www.fin.gc.ca/toce/2005/oneil_e.html
- Homepage of the Department of Finance Canada: www.fin.gc.ca

France

The revenue forecast of the French government is documented/discussed in:

- Homepage of the Juridictions financières: www.ccomptes.fr
- Homepage of the Ministère des Finances: www.minefi.gouv.fr
- Homepage of the Ministère du Budget, des Comptes Publics et de la fonction publique: www.budget.gouv.fr
- Les déterminants des ressources de l'État: www.vie-publique.fr

Germany

The official, centralized forecast of the consensus forecasting group is documented/discussed in:

- Bundesministerium der Finanzen: Finanzbericht 1997-2008
- Bundesministerium der Finanzen (2005): 50 Jahre Arbeitskreis "Steuerschätzungen"
- Gebhardt (2001)
- Homepage of the Ministry of Finance: www.bundesfinanzministerium.de

Ireland

The revenue forecast of the Irish Government is documented/discussed in:

- Budgets of the Department of Finance: www.budget.gov.ie
- Homepage of the Ministry of Finance: www.finance.gov.ie
- Homepage of the Revenue Commissioners: www.revenue.ie
- Minutes of the "Committee of Public Accounts" of the Irish Parliament on January 23, 2003: www.irlgov.ie/committees-29/c-publicaccounts/20030123/Page1.htm
- Report of the Tax Forecasting Methodology Review Group, 2008: www.finance.gov.ie.
- The Tax Forecasting Methodology Group, 1999: www.finance.gov.ie.

Italy

The revenue forecast of the Italian government is documented/discussed in:

- Ministero dell'Economia e delle Finanze, Rome: Documento di Programmazione Economico e Finanziaria per gli anni 1998-2000, ... per gli anni 2006-2009
- Istituto Nazionale di Statistica (2007), Rome: Conti e aggregati economici delle Amministrazioni pubbliche. Statistiche in breve, anni 1980-2006

Japan

The revenue forecast of the Japanese government is documented/discussed in:

- Adachi (2006), (in Japanese), PRI Discussion Paper Series, No. 06A-07
- Homepage of the Cabinett Office: www.cao.go.jp
- Homepage of the Ministry of Finance: www.mof.go.jp

Netherlands

The official revenue forecast of the Netherlands Bureau for Economic Policy Analysis (CPB) is documented/discussed in:

- Bos (2007).

- CPB Netherlands Bureau for Economic Policy Analysis (2005): *Forecasting Tax Revenue.* CPB Presentation
- European Commission (2006): European Economy No 3 / 2006, Public finances in EMU – 2006
- Homepage of the CPB Netherlands Bureau for Economic Policy Analysis: www.cpb.nl
- Ministry of Finance (2007): Stability Programme of the Netherlands, November 2007 Update: http://ec.europa.eu/economy_finance/about/activities/sgp/country/countryfiles/dec_2007/nl_2007_en.pdf
- Teulings, Coen (2006): *Forecasting, Policy Evaluation and the Budgetary Process. Lessons from the Netherlands.* CPB Presentation

New Zealand

The revenue forecast of the Treasury is documented/discussed in:

- Homepage of the Treasury: www.treasury.govt.nz
- Keene and Thomson (2007)
- New Zealand Treasury (2002): Treasury's Forecasting Process. Presentation Online: www.treasury.govt.nz
- New Zealand Treasury (2007): Budget Economic and Fiscal Update 2007. Online: www.treasury.govt.nz

United Kingdom

The revenue forecast of the Treasury is documented/discussed in:

- HM Treasury (2007a): Budget 2007, *HM Treasury, London*
- HM Treasury (2007b): Meeting the aspirations of the British people: 2007 Pre-Budget Report and Comprehensive Spending Review, *HM Treasury, London*
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- Pike and Savage (1998)

United States

The forecasts for federal revenues by the Congressional Budget Office and the Office of Management and Budget are documented/discussed in:

- Auerbach (1999).
- Congressional Budget Office (1995): *Budget Estimates: Current Practices and Alternative Approaches.* CBO Papers Series, Washington D.C.
- Congressional Budget Office (1998): *Projecting Federal Tax Revenues and the Effect of Changes in Tax Law.* Memorandum, Washington D.C.
- Congressional Budget Office (2006): *CBO's Policies for Preparing and Distributing Its Estimates and Analyses*, Washington D.C.
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- Homepage of the Office of Management and Budget (OMB): www.whitehouse.gov/omb
- Joint Committee on Taxation (JCT) (1992): *Discussion of Revenue Estimation Methodology and Process.* US Government Printing Office, Washington D.C.

Other Sources

- GDP forecast errors for all countries are based on the reports of the German Council of Economic Advisors, an independent body that issues annual reports including GDP forecasts for a large group of developed countries; see www.sachverstaendigenrat-wirtschaft.de/en/index.php
- *OECD Revenue Statistics*, various issues.

Table 2.7: Timing of Forecasts and Time Span

Austria	Forecast	Time Span	Belgium	Forecast	Time Span
2006	Sep 05	3.5	2008	Mar 08	-2.5
2005	Sep 04	3.5	2007	Nov 06	1.5
2004	May 03	7.5	2006	Oct 05	2.5
2003	May 03	-4.5	2005	Oct 04	2.5
2002	Sep 01	3.5	2004	Oct 03	2.5
2001	Oct 00	2.5	2003	Oct 02	2.5
2000	Mar 00	-2.5	2002	Oct 01	2.5
1999	Sep 98	3.5	2001	Oct 00	2.5
1998	Sep 97	3.5	2000	Oct 99	2.5
1997	May 96	7.5	1999	Oct 98	2.5
Median		3.5	1998	Oct 97	2.5
			1997	Oct 96	2.5
			1996	Oct 95	2.5
			Median		2.5

Canada	Forecast	Time Span	France	Forecast	Time Span
2009-2010	Jan 09	2.5	2008	Sep 07	3.5
2008-2009	Feb 08	1.5	2007	Sep 06	3.5
2007-2008	Mar 07	0.5	2006	Sep 05	3.5
2006-2007	May 06	-1.5	2005	Sep 04	3.5
2005-2006	Feb 05	1.5	2004	Sep 03	3.5
2004-2005	Mar 04	0.5	2003	Sep 02	3.5
2003-2004	Feb 03	1.5	2002	Sep 01	3.5
2002-2003	Dec 01	3.5	2001	Sep 00	3.5
2001-2002	Feb 00	13.5	2000	Sep 99	3.5
2000-2001	Feb 00	1.5	1999	Sep 98	3.5
1999-2000	Feb 99	1.5	Median		3.5
1998-1999	Feb 98	1.5			
1997-1998	Feb 97	1.5			
Median		1.5			

Timing of Forecasts and Time Span, continued

Germany	Forecast	Time Span	Ireland	Forecast	Time Span
2008	May 07	7.5	2008	Dec 07	0.5
2007	May 06	7.5	2007	Dec 06	0.5
2006	May 05	7.5	2006	Dec 05	0.5
2005	May 04	7.5	2005	Dec 04	0.5
2004	May 03	7.5	2004	Dec 03	0.5
2003	May 02	7.5	2003	Dec 02	0.5
2002	May 01	7.5	2002	Dec 01	0.5
2001	May 00	7.5	2001	Dec 00	0.5
2000	May 99	7.5	2000	Dec 99	0.5
1999	May 98	7.5	1999	Dec 98	0.5
1998	May 97	7.5	1998	Dec 97	0.5
1997	May 96	7.5	Median		0.5
Median		7.5			

Italy	Forecast	Time Span	Japan	Forecast	Time Span
2008	Jun 07	6.5	2008-2009	Dec 07	3.5
2007	Jul 06	5.5	2007-2008	Dec 06	3.5
2006	Jul 05	5.5	2006-2007	Dec 05	3.5
2005	Jul 04	5.5	2005-2006	Dec 04	3.5
2004	Jul 03	5.5	2004-2005	Dec 03	3.5
2003	Jul 02	5.5	2003-2004	Dec 02	3.5
2002	Jul 01	5.5	2002-2003	Dec 01	3.5
2001	Jun 00	6.5	2001-2002	Dec 00	3.5
2000	Jun 99	6.5	2000-2001	Dec 99	3.5
1999	Apr 98	8.5	1999-2000	Dec 98	3.5
1998	May 97	7.5	1998-1999	Dec 97	3.5
Median		5.5	1997-1998	Dec 96	3.5
			Median		3.5

Timing of Forecasts and Time Span, continued

Netherlands	Forecast	Time Span	New Zealand	Forecast	Time Span
2006	Jun 05	6.5	2007-2008	May 07	1.5
2005	Mar 04	9.5	2006-2007	May 06	1.5
2002	Feb 01	10.5	2005-2006	May 05	1.5
2001	Mar 00	9.5	2004-2005	May 04	1.5
2000	Mar 99	9.5	2003-2004	May 03	1.5
Median		9.5	2002-2003	May 02	1.5
			2001-2002	May 01	1.5
			2000-2001	May 00	1.5
			1999-2000	Apr 99	2.5
			1998-1999	Apr 98	2.5
			1997-1998	May 97	1.5
			Median		1.5

UK	Forecast	Time Span	US: CBO	Forecast	Time Span
2007-2008	Mar 07	0.5	2007-2008	Jan 07	8.5
2006-2007	Mar 06	0.5	2006-2007	Jan 06	8.5
2005-2006	Mar 05	0.5	2005-2006	Jan 05	8.5
2004-2005	Mar 04	0.5	2004-2005	Jan 04	8.5
2003-2004	Apr 03	-0.5	2003-2004	Jan 03	8.5
2002-2003	Apr 02	-0.5	2002-2003	Jan 02	8.5
2001-2002	Mar 01	0.5	2001-2002	Jan 01	8.5
2000-2001	Mar 00	0.5	2000-2001	Jan 00	8.5
1999-2000	Mar 99	0.5	1999-2000	Jan 99	8.5
1998-1999	Mar 98	0.5	1998-1999	Jan 98	8.5
1997-1998	Jul 97	-3.5	1997-1998	Jan 97	8.5
Median		0.5	1996-1997	Jan 96	8.5
			Median		8.5

Timing of Forecasts and Time Span, continued

US: OMB	Forecast	Time Span	
2007-2008	Feb 07	8	
2006-2007	Feb 06	8	
2005-2006	Feb 05	8	
2004-2005	Feb 04	8	
2003-2004	Feb 03	8	
2002-2003	Feb 02	8	
2001-2002	Feb 01	8	
2000-2001	Feb 00	8	
1999-2000	Feb 99	8	
1998-1999	Feb 98	8	
1997-1998	Feb 97	8	
1996-1997	Feb 96	8	
Median		8	

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Chapter 3

Revenue Forecasting in Germany: On Unbiasedness, Efficiency, and Politics

Abstract*

This chapter analyzes the performance of revenue forecasting in Germany. Tools provided by the literature on rational forecasting are used to investigate both unbiasedness and efficiency of the forecasts, but also whether indications for the relevance of politics can be found. Employing data from 1971 until 2009, we find forecasts to be unbiased and widely efficient. Only with regard to tax law changes there are signs for possible improvements. While a substantial part of revenue forecast errors can be explained by GDP forecast errors, there is no evidence that using the GDP forecast of the German Council of Economic Advisors leads to more efficient results. The analysis of possible influences of politics on revenue forecasting shows some room for improvement of the forecasts with respect to the term of office.

3.1 Introduction

The results of revenue forecasting provide the basis for the budgeting of virtually every country. This is in particular the case for Germany, where the widely independent "Arbeitskreis Steuerschätzungen"[1] is officially assigned with the task of forecasting. Even though public expenditure does not follow the revenue expectations perfectly, the link between forecasted revenues and expenditure decisions is obvious. In this context the quality of revenue forecasts shows its particular importance. Any forecast error results in an inefficient design of budgets, and can, if revenues fall short of expectations, be associated with corresponding consequences for the level of public debt.

*This chapter is joint work with Thiess Buettner.

[1]This work group is formed by members of the German Federal Ministry of Finance, the German Federal Ministry of Economics and Technology, five leading institutes for economic research, the German Federal Statistical Office, the German central bank, the Council of Economic Advisors ("Sachverständigenrat zur Begutachtung der gesamtwirtschaftlichen Entwicklung"), the State Ministries of Finance and representatives from the municipal level.

It was most notably the emergence of (large) budget deficits during the period from 1996 to 2004 that brought the German Court of Auditors to the scene in fall 2006. The court's president remarked that forecasts had been too optimistic in most cases leading to implications for the budget, since the pressure to cut expenses was reduced. Accordingly, he noted, net borrowing developed mirror-imaged to the deviations of forecasted to actual revenues. Moreover, he specifically referred to the forecasts of GDP growth and the estimated effects of tax law changes, that were characterized by a lack of quality from his point of view.[2] These statements call for an assessment of revenue forecasting in Germany, investigating whether evidence for systematic errors can be found.

To our best knowledge the literature has not yet brought out a comprehensive analysis that evaluates the statistical properties of the forecasts in Germany. While Becker and Buettner (2007) test for a general bias of revenue forecasts, Gebhardt (2001) is concerned with the methods and problems of forecasting in Germany. Several contributions to the issue of forecast evaluation can be found in the literature, that have recognized that numerous uncertainties hinder forecasters to come up with zero forecast errors.[3] The question is rather whether improvements of forecasts are possible. In this context, Buettner and Kauder (2010) analyze and compare revenue forecasting internationally and provide evidence that independent forecast institutions produce more precise forecasts.[4] Although Germany is shown to have a rather high level of independence, this could be further increased to raise the quality of forecasts.

Against this background we provide an analysis of revenue forecasting in Germany, trying to more precisely identify determinants of the forecast errors. Focusing on one country

[2]See Frankfurter Allgemeine Zeitung, "Rechnet euch nicht die Steuern schön", November 14, 2006.

[3]See, for instance, Keane and Runkle (1989 and 1990), Laster, Bennett, and Geoum (1999), Buettner and Horn (1993), Feenberg et al. (1989), and McNees (1978).

[4]See Chapter 2.

in detail allows to include in-depth information, especially on factors that drive the level of independence.[5] The fact that independent institutions provide superior forecasts may be due to three main reasons. They comprise indicators for the economic environment, the political environment, as well as technical issues: independent institutions could have access to superior economic information; they could be more independent from political influences; and they may have access to a superior forecast technology. According to this, the aim of this chapter is to address the question of which factors offer potential for improvements of the forecast quality. Covering the period from 1971 until 2009, this is done by analyzing whether the forecasts are rational, *i.e.* unbiased and efficient, inspired by the methodology of Keane and Runkle (1989). Furthermore, we examine the influence of politics. The results show that forecasts are generally unbiased and widely efficient. The forecast errors are basically driven by GDP forecast errors. Only with regard to tax law changes and the term of office improvements of the forecasts seem possible.

This chapter is structured as follows. The next section describes the investigation approach and Section 3.3 presents the data and descriptive statistics. In Section 3.4 the results of the empirical analysis are shown, while Section 3.5 provides our conclusions.

3.2 Investigation Approach

The goal of the analysis is to investigate German revenue forecasts with regards to rationality. According to Keane and Runkle (1989) forecasts are rational when they are both unbiased and efficient. Unbiasedness refers to a forecast error that – on average – does not show a significant deviation from zero. This means that the expected value of revenues

[5]Indeed, the analysis in Buettner and Kauder (2010) focuses mainly on the standard deviation of the forecast error, while here the mean is concerned. However, respective factors prove potentially relevant in the consideration of both measures of forecast quality.

has to be consistent with the forecast. In contrast, efficiency concerns the utilization of all relevant information available at the time of the forecast. The forecast error should not show a significant reaction to information that was available to the forecaster – otherwise the forecast could have been improved.[6]

In the context of revenue forecasting there are two particular aspects to be taken into account. First, tax revenues hinge crucially on the macroeconomic development. A revenue forecast that relies on a given biased GDP forecast cannot be expected to be correct. Hence, uncertainty about the GDP implies uncertainty about the revenues, so that the corresponding part of the resulting revenue forecast error cannot be attributed to the revenue forecaster. Second, politicians often adjust the tax law. Since the effects of tax law changes, in particular behavioral responses, are hard to predict, revenue forecast errors can be higher when the extent of tax law changes is large. This applies especially when behavioral responses and repercussions in the economy are not taken into account in the first place – as is often the case.[7] This holds even more when such bills are passed in the time between the forecast and the (end of the) forecasted period, so that they are completely neglected in the revenue forecast. A forecaster cannot at all be made responsible for errors resulting from this.

Since the analysis of revenue forecasts entails other requirements than the analysis of price forecasts as done by Keane and Runkle, we adjust their framework in some details. We start with an equation that regresses the revenue forecast for period t rev_t^f on the actual revenue in the same period rev_t:

$$rev_t = \alpha_0 + \alpha_1 rev_t^f + \alpha_2 X_t + u_t. \tag{3.1}$$

[6]See Nordhaus (1987).

[7]See Auerbach (1996).

In this equation, X_t denotes a vector of variables that contains any potentially relevant information that was available at the time of the forecast, while u_t is the idiosyncratic error term.

To analyze unbiasedness of forecasts we can rearrange this equation (while omitting X_t), which yields the forecast error:

$$fe_t \equiv rev_t^f - rev_t = -(\alpha_0 + (\alpha_1 - 1)rev_t^f + u_t), \tag{3.2}$$

where a positive fe_t denotes overestimation and a negative term underestimation. The forecasts are called unbiased when α_0 can be restricted to 0 and α_1 to 1. Unbiasedness, however, has to be rejected when α_0 or α_1 or both show to be significant.

To analyze efficiency we do the same but again take the information vector X_t into account, that contains information available at time t. This yields:

$$fe_t \equiv rev_t^f - rev_t = -(\alpha_0 + (\alpha_1 - 1)rev_t^f + \alpha_2 X_t + u_t). \tag{3.3}$$

Now, forecasts are called efficient when α_2 can be restricted to 0. If, however, α_2 proves to be significantly different from 0, we can conclude that some relevant information was not taken into account by the forecaster.

The analysis of possible influences of politics on revenue forecasting relies on a setting similar to that for efficiency. We only augment the vector X_t in order to describe political circumstances.

As announced above, we slightly adjust the setup for all three steps, unbiasedness, efficiency, and politics. First, we rely on trend variables rather than the forecast itself on the right hand side of most specifications. The trend can capture developments in the structure of the economy during recent decades. Trend and forecast are related, since nominal forecasts are typically an increasing function of time. Second, we use the *relative* forecast error (in percent) on the left hand side $(100 * (rev_t^f - rev_t)/rev_t)$, for consistency reasons.

After having outlined our investigation approach, we proceed with describing the data set.

3.3 Data and Descriptive Statistics

Revenue forecasts are prepared by the “Arbeitskreis Steuerschätzungen” twice a year. The spring forecast is typically issued in May and contains forecasts for the current and the upcoming four years.[8] The fall forecast is typically issued in November and forecasts revenues only for the current and the next year. Both comprise a separate forecast for each tax. The fact that taxes accrue to different administrative levels allows to perform the analysis for federal taxes, state taxes, and municipality taxes separately, or for the sum of them. We focus on the federal tax revenues, for which we will analyze the period from 1971 to 2009. This is done for the spring forecasts for the current and the next year as well as for the fall forecast concerning the next year.

The revenue forecasts and the underlying GDP growth rate forecasts are gathered from the press releases of the Ministry of Finance, while the source for the actual revenues and the actual GDP growth rates is the German Federal Statistical Office. The GDP growth rate forecasts of the German Council of Economic Advisors come from their annual reports. All figures are in nominal values. Descriptive statistics on all relevant variables are displayed in Table 3.1.

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[8] The German fiscal year is equal to the calendar year.

Table 3.1: Descriptive Statistics

Variable		Obs.	Mean	Std.Dev.	Min	Max
Tax Revenue Forecast Error (in %)	May CY	38	-.035	1.74	-4.90	3.40
	May NY	38	1.21	6.04	-15.3	16.2
	Nov. NY	38	-.023	3.92	-7.62	8.29
Federal Tax Revenue Forecast (Bill. €)	May CY	38	142	56.3	51.1	238
	May NY	38	148	56.8	56.1	249
	Nov. NY	38	146	56.1	56.1	247
Actual Federal Tax Revenue (Bill. €)		38	142	56.4	52.0	239
GDP Forecast Error (in %)		38	.487	1.67	-2.66	5.15
GDP Forecast Difference (in %)	Nov. NY	36	.092	.610	-1.34	.976
GDP Growth Rate Forecast Council of Adv. (in %)		38	5.02	2.42	1.50	12.0
GDP Growth Rate Forecast Government (in %)	Nov. NY	36	5.13	2.34	1.80	10.5
Actual GDP Growth Rate (in %)		38	4.54	2.94	-3.47	11.4
Election Year (Dummy)		38	.289	.460	0	1
Tax Law Changes (in %)		38	-.637	2.41	-7.06	4.08
Term of Office (in Days)	May CY	38	645	399	35	1297
	May NY	38	629	385	35	1297
	Nov. NY	38	586	406	1	1379

Data set covers the period from 1971 to 2009. CY – Current Year, NY – Next Year. GDP Forecast Error is based on the relative difference between the Council of Economic Advisors' level forecast and the actual level of the GDP. GDP Forecast Difference measures the relative difference between the GDP level forecast of the Ministry and the one of the Council of Economic Advisors.

The forecast error that we will focus on is a relative forecast error, *i.e.* the difference between the forecasted and the actual revenue divided by the actual revenue. A positive sign of the forecast error means that the forecast was higher than actual revenue, and a negative one that it was lower. As Table 3.1 shows, there is an average overestimation of 1.21 percent in next-year forecasts from May, while both other forecasts show a very small underestimation.

Graphically, the results can be pictured as shown in Figures 3.1 to 3.3. Obviously, the relative forecast error in next-year forecasts shows a larger amplitude in May than in November, while the forecast for the current year shows the smallest amplitude. This had to be expected since a shrinking time span between the preparation of the forecast and the beginning of the forecasted period lowers the level of uncertainty. In the course of the years, however, there is no obvious change in the amplitude or the direction of the forecast error.

Our analysis of the *unbiasedness* of revenue forecasts is based on three types of variables. First, to account for the uncertainty of economic circumstances, a measure for the GDP forecast error is included. This measure is defined as the relative difference between the independent Council of Economic Advisors' forecast of the GDP *level* and the actual *level* of the GDP: $100*(GDP_t^{f\text{-}Advisors} - GDP_t)/GDP_t$.[9] Note that this GDP forecast error is not based on the GDP forecast that underlies the revenue forecast.[10] It is rather a GDP forecast error that measures sheer economic uncertainty and excludes possible political biases of the underlying official GDP forecast. The latter will come into play in the efficiency analysis below.

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[9]The GDP forecast of the Council of Economic Advisors is always issued in November, with the result that the GDP forecast error represents the level of economic uncertainty always at the same point of time, namely the November prior to the forecasted year, and not at the time where the revenue forecast was prepared.

[10]Revenue forecasters are asked to base their forecast on a GDP forecast provided by the Ministry of Economics and Technology.

Figure 3.1: Relative Forecast Error (in %): Current-Year Forecast May

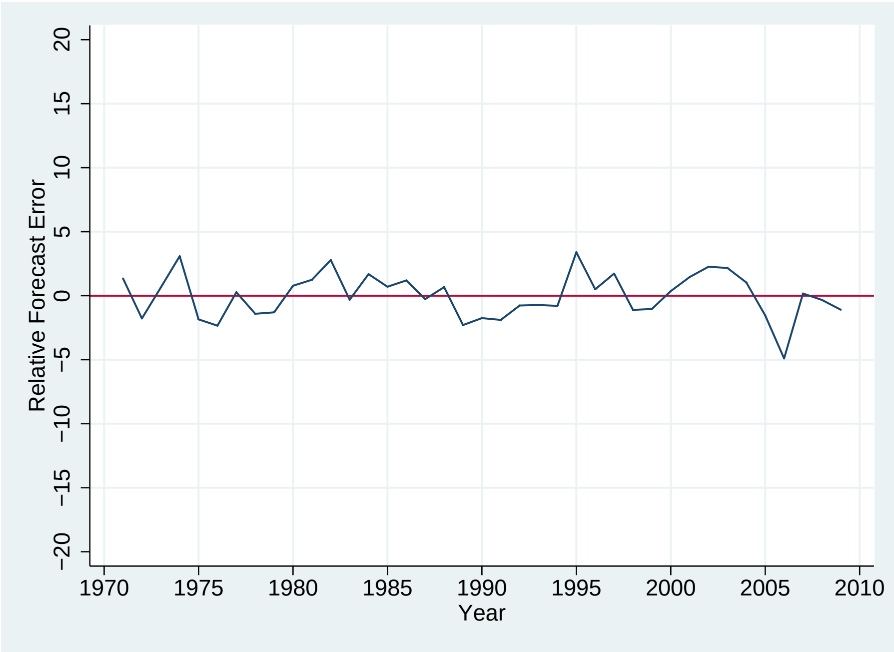


Figure 3.2: Relative Forecast Error (in %): Next-Year Forecast May

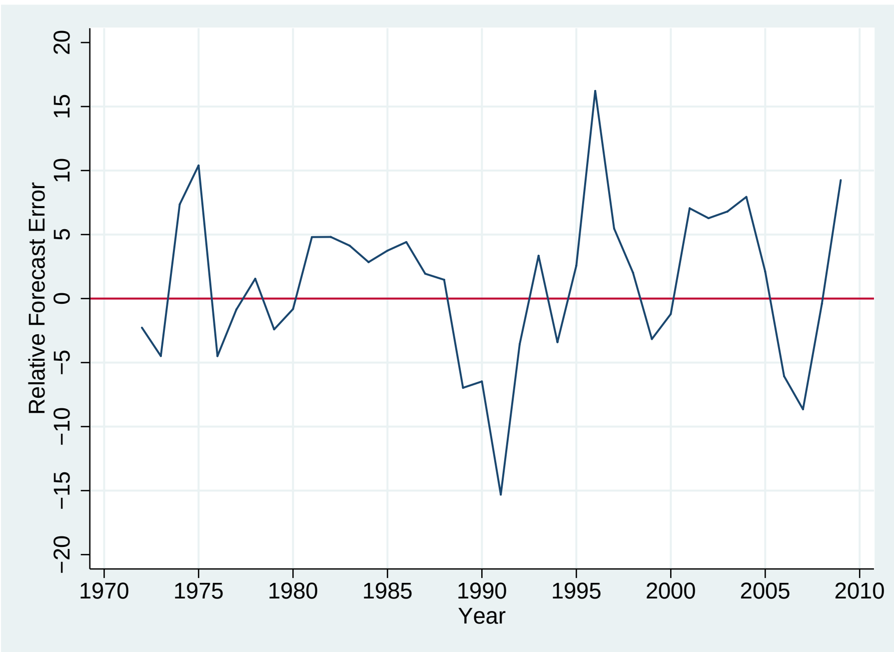
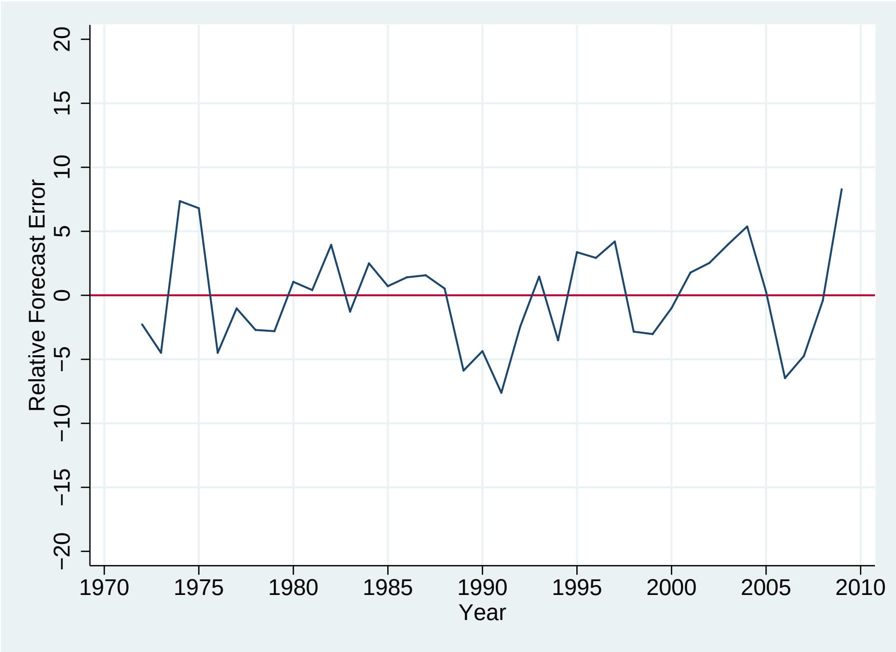


Figure 3.3: Relative Forecast Error (in %): Next-Year Forecast November



Second, the question of whether the revenue forecast error is changing over time is captured by trend variables. To allow for nonlinear developments also the squared and cubed forms of the trend are included. Note that the trend variable is closely related to the level of the forecast that was part of the regression equations derived in the preceding section. Since nominal revenues are – by tendency – an increasing function of time, a large value in the trend variable is associated with a large revenue forecast. Apart from technical reasons for including the trend, this procedure allows to capture structural changes in the economy, such as increasing world-economic integration and the German reunification.

Third, we control for different attitudes of ministers, which may be intrinsically optimistic or pessimistic. Accordingly, minister specific effects are included by adding dummy variables for different ministers of finance. This allows to see whether revenue forecasts prepared under some ministers show a different performance than those prepared under others.

To analyze whether the revenue forecasts are *efficient*, possibly relevant information that was available at the time where the forecast was made is further taken into account. Among this is first of all the revenue forecast error from the last period (fe_{t-1}).[11] It is reasonable to argue that a forecaster reacts to his last forecast error by adjusting his current forecast into the opposite direction.

Since the revenue forecasters have to make use of a GDP forecast that is provided by the Federal Ministry of Economics and Technology it could be that forecast errors arise from strategic goals. To see whether the revenue forecasts could have been improved by taking into account other (independent) GDP forecasts, we include a GDP forecast difference variable. This shows the relative difference between the underlying GDP level forecast of the Ministry and that of the independent Council of Economic Advisors: $100 * (GDP_t^{f\_Ministry} - GDP_t^{f\_Advisors})/GDP_t^{f\_Advisors}$.

Another variable concerns tax law changes. Usually, revenue forecasters are asked to prepare their forecasts based on the current law. Even if a reform proposal will be almost certainly implemented, it is typically not included in the forecast. But since the law can pass legislation between the time the forecast is made and the (end of the) forecasted period, forecast errors may occur that cannot be attributed to the forecaster. To account for this, a variable measuring the extent of law changes on the total tax level (relative to total tax revenues) is included.[12] But even if tax laws have passed the legislator "in time", they can influence the forecast error, since their effects are difficult to assess.[13]

Incentives can differ when a forecast is prepared for an election year. To take this into account a dummy variable is included. This takes on the value 1 if the forecast is prepared for an election year and the value 0 if not.

[11]Since the forecast error from the previous to the current year is not yet available to the forecaster at the time of the compilation of the next-year forecasts, this describes the error of the forecast prepared two periods ago in the respective cases.

[12]Indeed, we only analyze federal taxes, but the major tax law changes occurred on this level.

[13]Note that the estimates of revenue effects of law changes are prepared by the Ministry of Finance.

To investigate whether revenue forecasts are influenced by *politics*, we further control for different effects in the course of terms of office. This is done by including a variable measuring the number of days the government has been in office when a forecast is issued.[14] Political incentives right after having been elected might differ from those when the next election is approaching.

Having discussed the data and the different issues to be addressed, the next section presents our results of the analyses of unbiasedness, efficiency, and politics.

3.4 Empirical Results

We consider three revenue forecasts and investigate whether they are unbiased, efficient, and/or influenced by politics: (i) the May forecast for the current year, (ii) the May forecast for the next year, and (iii) the November forecast for the next year.

Unbiasedness

First, for all three revenue forecasts *unbiasedness* is considered. In our approach unbiasedness requires that neither the constant nor the trend have an impact on the forecast error. The latter is here defined as the difference between the forecast and the actual tax revenue, relative to the actual tax revenue.

The upper part of Table 3.2 presents the results for the current-year forecast prepared in May. While column (1) only includes a constant, in columns (2) to (4) the trend is added. But irrespective of whether just a linear trend or also nonlinear forms are allowed, there are no signs of a biased forecast. None of the respective coefficients are significant.

[14] The term of office ends with federal elections, irrespective of whether the reigning parties remain the same after the election.

Table 3.2: Results on the Forecast Bias: May current / May next / November next Year

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Constant	.001	.213	-.227	.510	2.79	-.205	.073	-.928	-.256	-.136
	(.278)	(.573)	(.895)	(1.26)	(1.71)	(.282)	(.579)	(.953)	(1.46)	(1.14)
GDP Forecast Error						.350 **	.364 **	.395 **	.399 **	.404 **
						(.164)	(.168)	(.168)	(.169)	(.169)
Trend		-.011	.054	-.154			-.014	.122	-.046	
		(.025)	(.103)	(.269)			(.025)	(.107)	(.296)	
Trend Squared			-.002	.011				-.003	.007	
			(.003)	(.015)				(.003)	(.016)	
Trend Cubed				-.000					-.000	
				(.000)					(.000)	
Minister Dummies	–	–	–	–	Yes	–	–	–	–	Yes
R^2	0.000	0.005	0.016	0.036	0.234	0.112	0.120	0.162	0.172	0.349
adj. R^2	0.000	-0.022	-0.039	-0.047	0.030	0.088	0.070	0.089	0.071	0.169
Observations	39	39	39	39	39	38	38	38	38	38

	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)
Constant	1.21	.532	1.36	1.91	4.14	.069	.472	-.189	.970	4.99
	(.979)	(2.10)	(3.52)	(5.46)	(6.62)	(.790)	(1.63)	(2.74)	(4.22)	(4.44)
GDP Forecast Error						2.34 ***	2.36 ***	2.38 ***	2.39 ***	3.00 ***
						(.460)	(.471)	(.482)	(.489)	(.496)
Trend		.033	-.080	-.216			-.020	.070	-.221	
		(.090)	(.394)	(1.11)			(.071)	(.306)	(.855)	
Trend Squared			-.003	.011				-.002	.015	
			(.009)	(.061)				(.007)	(.047)	
Trend Cubed				-.000					-.000	
				(.001)					(.001)	
Minister Dummies	–	–	–	–	Yes	–	–	–	–	Yes
R^2	0.000	0.004	0.006	0.007	0.057	0.418	0.420	0.421	0.424	0.592
adj. R^2	0.000	-0.024	-0.051	-0.081	-0.204	0.402	0.387	0.370	0.354	0.461
Observations	38	38	38	38	38	38	38	38	38	38

	(21)	(22)	(23)	(24)	(25)	(26)	(27)	(28)	(29)	(30)
Constant	-.023	-.495	.928	-.018	-3.03	-.800	-.536	-.111	-.642	-4.66
	(.635)	(1.36)	(2.26)	(3.51)	(4.27)	(.493)	(1.02)	(1.71)	(2.64)	(2.81)
GDP Forecast Error						1.59 ***	1.61 ***	1.59 ***	1.59 ***	1.97 ***
						(.287)	(.294)	(.301)	(.305)	(.312)
Trend		.023	-.171	.065			-.013	-.071	.062	
		(.059)	(.253)	(.710)			(.044)	(.191)	(.534)	
Trend Squared			-.005	-.009				.001	-.006	
			(.006)	(.039)				(.005)	(.030)	
Trend Cubed				.000					.000	
				(.001)					(.000)	
Minister Dummies	–	–	–	–	Yes	–	–	–	–	Yes
R^2	0.000	0.004	0.022	0.025	0.068	0.461	0.463	0.464	0.465	0.614
adj. R^2	0.000	-0.023	-0.034	-0.061	-0.189	0.446	0.432	0.417	0.400	0.490
Observations	38	38	38	38	38	38	38	38	38	38

Dependent variable: Relative Tax Revenue Forecast Error. Standard errors in parentheses. A single star denotes significance at the 10 % level, two stars at the 5 % level, and three stars at the 1 % level (in the case of minister dummies for joint significance).

This also holds when dummy variables for the different ministers of finance are included in column (5), which in fact prove not to be jointly significant. Columns (6) to (10) repeat the exercise but in addition the GDP forecast error is included. This variable is defined as the relative difference between the Council of Economic Advisors' forecast and the actual GDP level.[15] It shows that this measure of macroeconomic uncertainty is positively related to the revenue forecast error and it substantially contributes to the level of R^2. Since the GDP forecasts are not made by the revenue forecasters, however, this cannot be considered as a bias in the revenue forecasts.

When we turn to the forecasts for the next year issued in May and November in columns (11) to (20) and (21) to (30), these results are confirmed. The GDP forecast error even contributes more to the coefficients of determination than before. While the regressions for the May forecast for the current year were only able to explain up to about one third of the variation of the relative forecast error, the R^2 rises to up to 60 % in the case of next-year forecasts. Comparing the coefficients of the GDP forecast error among the three revenue forecasts shows the expected magnitudes. Since the GDP forecast error is based on figures from the November prior to the forecasted year, the coefficients increase with the time span between the revenue forecast and the beginning of the forecasted period.

Efficiency

Turning to the *efficiency* of revenue forecasts brings us to the analysis of whether information was available at the time of the forecast preparation that could have improved the quality of the forecasts. Accordingly, none of the respective variables should show a significant effect for forecasts to be efficient.

[15] One can alternatively make use of the joint forecast of German research institutes ("Gemeinschaftsdiagnose"), rather than of the forecast of the Council of Economic Advisors. The corresponding results are comparable and for the next-year forecast from November shown in the appendix.

Table 3.3: Results on Forecast Efficiency I: May current / May next / November next Year

	(1)	(2)	(3)	(4)
Constant	-.189	.100	.010	.290
	(.284)	(.330)	(1.48)	(1.16)
lag Revenue Forecast Error	.133	.164	.119	-.103
	(.165)	(.162)	(.170)	(.180)
GDP Forecast Error	.309 *	.283	.338 *	.436 *
	(.173)	(.170)	(.180)	(.181)
Trend			-.032	
			(.296)	
Trend Squared			.005	
			(.017)	
Trend Cubed			-.000	
			(.000)	
Election Year		-.959	-.869	-.830
		(.590)	(.608)	(.574)
Minister Dummies	–	–	–	Yes
R^2	0.129	0.191	0.229	0.407
adj. R^2	0.079	0.120	0.080	0.188
Observations	38	38	38	38

	(5)	(6)	(7)	(8)
Constant	-.012	.210	1.87	6.07
	(.856)	(.979)	(7.00)	(4.82)
lag Revenue Forecast Error	.040	.053	.047	-.084
	(.134)	(.138)	(.147)	(.143)
GDP Forecast Error	2.50 ***	2.52 ***	2.53 ***	2.92 ***
	(.499)	(.505)	(.566)	(.507)
Trend			-.305	
			(1.26)	
Trend Squared			.018	
			(.065)	
Trend Cubed			-.000	
			(.001)	
Election Year		-.878	-.747	-.695
		(1.81)	(1.91)	(1.80)
Minister Dummies	–	–	–	Yes
R^2	0.438	0.442	0.448	0.614
adj. R^2	0.404	0.390	0.334	0.460
Observations	36	36	36	36

	(9)	(10)	(11)	(12)
Constant	-.805	-.605	.703	-4.60 *
	(.512)	(.597)	(4.25)	(2.69)
lag Revenue Forecast Error	-.011	.001	-.004	-.001
	(.133)	(.135)	(.143)	(.139)
GDP Forecast Error	1.68 ***	1.69 ***	1.65 ***	1.90 ***
	(.308)	(.311)	(.347)	(.297)
Trend			-.103	
			(.768)	
Trend Squared			.000	
			(.039)	
Trend Cubed			.000	
			(.000)	
Election Year		-.728	-.715	-.606
		(1.09)	(1.15)	(1.00)
Minister Dummies	–	–	–	Yes *
R^2	0.489	0.496	0.505	0.687
adj. R^2	0.458	0.449	0.402	0.562
Observations	36	36	36	36

Dependent variable: Relative Tax Revenue Forecast Error. Standard errors in parentheses. A single star denotes significance at the 10 % level, two stars at the 5 % level, and three stars at the 1 % level (in the case of minister dummies for joint significance).

Table 3.4: Results on Forecast Efficiency II: November next Year

	(1)	(2)	(3)	(4)
Constant	-.594	-.369	-.955	3.88
	(.516)	(.580)	(4.28)	(2.54)
lag Revenue Forecast Error	-.119	-.099	-.090	-.035
	(.135)	(.137)	(.147)	(.139)
GDP Forecast Error	1.43 ***	1.47 ***	1.50 ***	1.82 ***
	(.324)	(.328)	(.359)	(.325)
GDP Forecast Difference	1.05	1.05	1.09	.825
	(.879)	(.883)	(.972)	(.869)
Trend			.192	
			(.771)	
Trend Squared			-.012	
			(.040)	
Trend Cubed			.000	
			(.001)	
Election Year		-.937	-.991	-1.09
		(1.09)	(1.17)	(1.00)
Minister Dummies	–	–	–	Yes
R^2	0.514	0.526	0.531	0.709
adj. R^2	0.465	0.461	0.405	0.563
Observations	34	34	34	34

Dependent variable: Relative Tax Revenue Forecast Error. Standard errors in parentheses. A single star denotes significance at the 10 % level, two stars at the 5 % level, and three stars at the 1 % level (in the case of minister dummies for joint significance).

Table 3.5: Results on Forecast Efficiency III: May current / May next / November next Year

	(1)	(2)	(3)	(4)	(5)	(6)
Constant	-.205	-.084	.140	-.392	-.042	.429
	(.282)	(.279)	(.321)	(1.02)	(1.04)	(2.10)
GDP Forecast Error	.350 **	.376 **	.361 **	.442 ***	.434 ***	.352 *
	(.164)	(.159)	(.157)	(.151)	(.149)	(.177)
Trend						-.431
						(.808)
Trend Squared						.025
						(.037)
Trend Cubed						-.000
						(.001)
Election Year			-.781		-.682	
			(.572)		(.511)	
Tax Law Changes		.211 *	.195 *	.304 ***	.286 ***	.306 ***
		(.110)	(.109)	(.104)	(.104)	(.109)
Minister Dummies	–	–	–	Yes **	Yes **	Yes *
R^2	0.112	0.197	0.238	0.501	0.532	0.535
adj. R^2	0.088	0.151	0.171	0.341	0.358	0.312
Observations	38	38	38	38	38	38

	(7)	(8)	(9)	(10)	(11)	(12)
Constant	.069	-.392	-.232	4.18	5.44	3.04
	(.790)	(.748)	(.881)	(4.16)	(4.49)	(6.28)
GDP Forecast Error	2.34 ***	2.24 ***	2.23 ***	2.83 ***	2.83 ***	2.71 ***
	(.460)	(.426)	(.432)	(.469)	(.473)	(.471)
Trend						.714
						(2.57)
Trend Squared						.039
						(.115)
Trend Cubed						-.001
						(.002)
Election Year			-.560		-1.28	
			(1.57)		(1.64)	
Tax Law Changes		-.799 ***	-.810 **	-.753 **	-.778 **	-.948 **
		(.295)	(.300)	(.334)	(.337)	(.342)
Minister Dummies	–	–	–	Yes	Yes	Yes
R^2	0.418	0.520	0.521	0.657	0.665	0.707
adj. R^2	0.402	0.492	0.479	0.530	0.523	0.548
Observations	38	38	38	38	38	38

	(13)	(14)	(15)	(16)	(17)	(18)
Constant	-.800	-.976 *	-.855	-4.10	-4.09	-8.71
	(.493)	(.496)	(.584)	(2.81)	(2.84)	(15.4)
GDP Forecast Error	1.59 ***	1.56 ***	1.55 ***	1.90 ***	1.90 ***	1.90 ***
	(.287)	(.283)	(.287)	(.313)	(.316)	(.336)
Trend						.562
						(1.84)
Trend Squared						-.006
						(.085)
Trend Cubed						.000
						(.001)
Election Year			-.423		-.718	
			(1.04)		(1.06)	
Tax Law Changes		-.306	-.314	-.290	-.301	-.340
		(.196)	(.199)	(.224)	(.227)	(.243)
Minister Dummies	–	–	–	Yes	Yes	Yes
R^2	0.461	0.496	0.499	0.637	0.643	0.647
adj. R^2	0.446	0.468	0.455	0.502	0.492	0.455
Observations	38	38	38	38	38	38

Dependent variable: Relative Tax Revenue Forecast Error. Standard errors in parentheses. A single star denotes significance at the 10 % level, two stars at the 5 % level, and three stars at the 1 % level (in the case of minister dummies for joint significance).

We start in Table 3.3 with considering the revenue forecast error from the last available forecast (lag Revenue Forecast Error). It shows for all forecasts that the hypothesis of efficiency cannot be rejected at this time. Since the last forecast error does not show an impact on the current forecast error, we can conclude that this information is taken into account by the forecaster. Also trend variables do not affect the forecast error; the same holds for the election year dummy. Again, the GDP forecast error is the only variable with a significant effect. Only in the case of the next-year forecast from November, there is a single significant constant and jointly significant minister dummies.

Concerning the November forecast for the next year, we can also add the GDP forecast difference as a piece of information that was available to the forecaster.[16] This GDP forecast difference measures the (relative) difference between the official GDP forecast that underlies the revenue forecast and the independent one from the Council of Economic Advisors. But as Table 3.4 shows, this measure does not significantly affect the revenue forecast error. Thus, there is no measurable influence of the government via the GDP forecast. However, the coefficients have the expected positive sign, meaning that revenues *tend* to be overestimated when the underlying GDP forecast is more optimistic than the independent one. The remaining variables show a similar pattern as before. Correspondingly, the November forecast for the next year still shows no signs of inefficiency.

One factor that shows a significant effect is the variable describing tax law changes. In the May forecast for the current year, the forecast is based on a widely "correct" legal situation. Hence, there is hardly any uncertainty left regarding tax law changes in the forecasted horizon.[17] Given that all law changes have already been passed and the forecast is based on the new law, the positive sign in the upper part of Table 3.5 indicates a too strong assessment of law changes. When a revenue-enhancing law was implemented, the forecasters expected too positive effects, and when a revenue-reducing law was imple-

[16]In the cases of the other forecasts this was not possible since respective forecasts are issued only in November.

[17]Tax law changes can also be passed in the course of the year. The argumentation, however, assumes all laws to be passed before May, which is in fact the case for most relevant laws.

mented, forecasters expected too negative effects. This can be explained by the fact that behavioral responses of tax payers are generally not taken into account by the revenue forecasters. Since such responses have to be assumed it follows that the true effects of tax law changes are smaller in absolute values than estimated. Apart from the effect of tax law changes, we find again the minister dummies to be jointly significant.

The situation is completely different when the next-year forecasts are considered. Here, the forecasters come up with forecasts that are based on a legal situation which deviates from the one that will be applied in the forecasted year. Given a law change that increases revenues, an underestimation has to result, since this law was not taken into account. In the case of a law that decreases revenues, an overestimation follows, since the negative effect was not incorporated. Accordingly, the coefficients in the lower parts of Table 3.5 are negative. Since in November there are already more law changes passed for the next year than in May, the effect has to be weaker in the November forecast, as the magnitudes of the (now insignificant) coefficients confirm.

Politics

The final step of the analysis looks at possible influences of *politics*. We raise the question whether political factors affect the revenue forecasts. The results are presented in Table 3.6. First of all, the variables that have already been considered in the previous steps still show the same pattern as before. Throughout all the specifications the GDP forecast error proves significant, while the trend does not and the election year dummy only does in one case. Also minister dummies are jointly significant only in single cases. The effect of the tax law changes is as large as before, and now also proves mostly significant in the November forecast.

Table 3.6: Results on Politics: May current / May next / November next Year

	(1)	(2)	(3)	(4)	(5)	(6)
Constant	-.205	.005	-.063	-.624	-.571	.343
	(.282)	(.519)	(.513)	(1.14)	(1.09)	(2.17)
GDP Forecast Error	.350 **	.375 **	.361 **	.442 ***	.429 ***	.359 *
	(.164)	(.161)	(.159)	(.153)	(.147)	(.182)
Trend						-.445
						(.825)
Trend Squared						.026
						(.038)
Trend Cubed						-.000
						(.001)
Election Year			-.945		-1.11 *	
			(.661)		(.586)	
Tax Law Changes		.204 *	.211 *	.325 ***	.341 ***	.315 **
		(.117)	(.115)	(.114)	(.109)	(.118)
Term of Office		-.000	.000	.000	.001	.000
		(.001)	(.001)	(.001)	(.001)	(.001)
Minister Dummies	–	–	–	Yes	Yes **	Yes
R^2	0.112	0.198	0.244	0.505	0.565	0.536
adj. R^2	0.088	0.127	0.153	0.322	0.381	0.285
Observations	38	38	38	38	38	38

	(7)	(8)	(9)	(10)	(11)	(12)
Constant	.069	1.63	1.62	6.91 *	6.98	6.27
	(.790)	(1.42)	(1.44)	(4.03)	(4.27)	(5.97)
GDP Forecast Error	2.34 ***	2.33 ***	2.34 ***	3.01 ***	3.01 ***	2.87 ***
	(.460)	(.418)	(.427)	(.442)	(.451)	(.440)
Trend						.763
						(2.37)
Trend Squared						.024
						(.107)
Trend Cubed						-.001
						(.002)
Election Year			.292		-.105	
			(1.62)		(1.63)	
Tax Law Changes		-.653 **	-.642 **	-.543	-.547	-.715 **
		(.301)	(.311)	(.322)	(.334)	(.332)
Term of Office		-.003	-.003	-.004 **	-.004 **	-.004 **
		(.002)	(.002)	(.002)	(.002)	(.002)
Minister Dummies	–	–	–	Yes	Yes	Yes
R^2	0.418	0.556	0.556	0.716	0.717	0.761
adj. R^2	0.402	0.516	0.502	0.597	0.580	0.615
Observations	38	38	38	38	38	38

	(13)	(14)	(15)	(16)	(17)	(18)
Constant	-.800	.169	.174	-3.82	-3.80	-9.13
	(.493)	(.829)	(.840)	(2.55)	(2.58)	(13.9)
GDP Forecast Error	1.59 ***	1.51 ***	1.51 ***	1.81 ***	1.79 ***	1.79 ***
	(.287)	(.277)	(.280)	(.286)	(.291)	(.308)
Trend						.758
						(1.67)
Trend Squared						-.021
						(.077)
Trend Cubed						.000
						(.001)
Election Year			.418		.575	
			(1.13)		(1.09)	
Tax Law Changes		-.320	-.313	-.383 *	-.384 *	-.423 *
		(.191)	(.194)	(.206)	(.209)	(.223)
Term of Office		-.002 *	-.002	-.003 **	-.003 **	-.003 **
		(.001)	(.001)	(.001)	(.001)	(.001)
Minister Dummies	–	–	–	Yes	Yes *	Yes
R^2	0.461	0.536	0.538	0.713	0.716	0.721
adj. R^2	0.446	0.495	0.482	0.591	0.580	0.550
Observations	38	38	38	38	38	38

Dependent variable: Relative Tax Revenue Forecast Error. Standard errors in parentheses. A single star denotes significance at the 10 % level, two stars at the 5 % level, and three stars at the 1 % level (in the case of minister dummies for joint significance).

The results for the current-year forecast are shown in the upper part of the table. In addition we now control for the time the government has been in office at the time of the forecast. However, no effect of the term of office can be found for this forecast.

Turning to the next-year forecasts in May and November displayed in the lower parts of Table 3.6 provides further insights. When we now control for the duration the government is in office, we find potential to improve the forecasts. The longer the government is in office, the smaller the relative forecast error becomes. This is significant throughout most specifications concerning the next-year forecasts. A possible explanation could be increasing GDP stimuli by the government at the end of the rule that are not taken into account by the revenue forecaster and compensate for earlier overestimations.[18] However, because of the coefficients of the election year variable indicating the opposite, this political business cycle explanation is ruled out. An alternative explanation is that the forecast error is larger in the beginning of a legislative period, meaning that forecasters provide more optimistic figures. This could be based on overoptimistic expectations of forecasters regarding a new government or on pressure of this government to let political programs appear fundable.[19]

3.5 Conclusions

This chapter has analyzed the rationality of official revenue forecasts in Germany. Since these forecasts are essential for the budget process, the Court of Auditors had criticized the occurrence of overestimation in recent years. We investigate whether evidence for (upward) biased forecasts can be found, and whether there is room to improve the forecasts in terms of efficiency.

[18] See Becker and Buettner (2007).

[19] The effect of the term of office is weaker and insignificant when total tax revenues rather than only federal ones are considered. Hence, the effect seems to occur only from federal politicians regarding their own revenues.

Based on the literature on rational forecasting we use the framework developed by Keane and Runkle to derive regression equations that try to tackle three questions. The first one concerns unbiasedness of forecasts. A forecast is considered to be unbiased when the expected value of revenues is consistent with the forecast. The second one asks whether the forecasts are efficient. This is the case when there was no relevant information available to the forecaster at the time of the forecast that could have improved the outcome. Finally, we look at possible influences of politics.

The empirical assessment focuses on the spring forecasts for the current and the next year as well as on the fall forecast for the next year. Employing data for federal tax revenues covering the period from 1971 to 2009 we are able to widely "acquit" the forecasters. The analysis concerning unbiasedness shows that the hypothesis of unbiasedness cannot be rejected. For none of the three forecasts there are signs for biasedness. Rather the GDP forecast error explains a notable part of the revenue forecast error. Turning to the question of efficiency yields to including some pieces of information that appear to have a possible impact on the forecast error. Neither the last revenue forecast error, nor the difference between the official and an independent GDP forecast, nor a dummy variable for election years shows any significance. However, we find the estimated effects of tax law changes to be too strong, when they are taken into account. If, however, they have been passed by the legislator after the preparation of the forecast, the expected effects of over- or underestimation are found. Moreover, the GDP forecast error still proves relevant. Considering variables describing political circumstances we find some room to improve the forecasts; the forecast error is affected by the term of office. The longer the government is in office, the smaller the forecast error becomes. This might reflect larger overestimations at the beginning of the rule, either because forecasters are overoptimistic regarding a new government, or due to political pressure to convey the impression that political programs are sufficiently funded.

Appendix

Table 3.7: Results on the Forecast Bias: November Forecast for the next Year (GDP Forecast Error based on Research Institutes)

	(1)	(2)	(3)	(4)	(5)
Constant	-.023	-.495	.928	-.018	-3.03
	(.635)	(1.36)	(2.26)	(3.51)	(4.27)
GDP Forecast Error					
Trend		.023	-.171	.065	
		(.059)	(.253)	(.710)	
Trend Squared			-.005	-.009	
			(.006)	(.039)	
Trend Cubed				.000	
				(.001)	
Minister Dummies	–	–	–	–	Yes
R^2	0.000	0.004	0.022	0.025	0.068
adj. R^2	0.000	-0.023	-0.034	-0.061	-0.189
Observations	38	38	38	38	38

	(6)	(7)	(8)	(9)	(10)
Constant	-.855 *	-.493	-.305	-.932	-4.84 *
	(.465)	(9.55)	(1.62)	(2.49)	(2.59)
GDP Forecast Error	1.53 ***	1.55 ***	1.54 ***	1.54 ***	1.85 ***
	(.244)	(.250)	(.257)	(.261)	(.258)
Trend		-.018	-.044	.113	
		(.042)	(.181)	(.503)	
Trend Squared			.001	-.009	
			(.004)	(.028)	
Trend Cubed				.000	
				(.000)	
Minister Dummies	–	–	–	–	Yes
R^2	0.521	0.524	0.524	0.526	0.672
adj. R^2	0.508	0.497	0.482	0.468	0.566
Observations	38	38	38	38	38

Dependent variable: Relative Tax Revenue Forecast Error. Standard errors in parentheses. A single star denotes significance at the 10 % level, two stars at the 5 % level, and three stars at the 1 % level (in the case of minister dummies for joint significance).

Table 3.8: Results on Forecast Efficiency I: November Forecast for the next Year (GDP Forecast Error based on Research Institutes)

	(1)	(2)	(3)	(4)
Constant	-.880 *	-.681	-1.56	3.72
	(.493)	(.574)	(4.22)	(2.54)
lag Revenue Forecast Error	.037	.049	.057	.102
	(.129)	(.131)	(.140)	(.135)
GDP Forecast Error	1.60 ***	1.61 ***	1.64 ***	1.82 ***
	(.269)	(.271)	(.314)	(.262)
Trend			.275	
			(.757)	
Trend Squared			-.017	
			(.039)	
Trend Cubed			.000	
			(.001)	
Election Year		-.726	-.773	-.742
		(1.05)	(1.11)	(.953)
Minister Dummies				Yes **
R^2	0.531	0.537	0.545	0.717
adj. R^2	0.502	0.494	0.450	0.604
Observations	36	36	36	36

Dependent variable: Relative Tax Revenue Forecast Error. Standard errors in parentheses. A single star denotes significance at the 10 % level, two stars at the 5 % level, and three stars at the 1 % level (in the case of minister dummies for joint significance).

Table 3.9: Results on Forecast Efficiency II: November Forecast for the next Year (GDP Forecast Error based on Research Institutes)

	(1)	(2)	(3)	(4)
Constant	-.593	-.396	-1.36	3.76
	(.512)	(.580)	(4.20)	(2.55)
lag Revenue Forecast Error	-.080	-.069	-.059	.014
	(.145)	(.147)	(.157)	(.152)
GDP Forecast Error	1.43 ***	1.44 ***	1.49 ***	1.71 ***
	(.278)	(.281)	(.324)	(.280)
GDP Forecast Difference	.454	.603	.629	.663
	(1.20)	(1.22)	(1.29)	(1.13)
Trend			.248	
			(.758)	
Trend Squared			-.014	
			(.039)	
Trend Cubed			.000	
			(.001)	
Election Year		-.817	-.874	-.974
		(1.09)	(1.18)	(1.02)
Minister Dummies				Yes
R^2	0.521	0.530	0.536	0.707
adj. R^2	0.473	0.465	0.411	0.560
Observations	34	34	34	34

Dependent variable: Relative Tax Revenue Forecast Error. Standard errors in parentheses. A single star denotes significance at the 10 % level, two stars at the 5 % level, and three stars at the 1 % level (in the case of minister dummies for joint significance). The GDP Forecast Difference is based on the forecasts of the government and the research institutes.

Table 3.10: Results on Forecast Efficiency III: November Forecast for the next Year (GDP Forecast Error based on Research Institutes)

	(1)	(2)	(3)	(4)	(5)	(6)
Constant	-.855 *	-.974 **	-.859	-4.40	-4.38	-7.69
	(.465)	(.473)	(.557)	(2.62)	(2.64)	(14.4)
GDP Forecast Error	1.53 ***	1.49 ***	1.48 ***	1.80 ***	1.80 ***	1.78 ***
	(.244)	(.246)	(.249)	(.263)	(.266)	(.282)
Trend						.361
						(1.72)
Trend Squared						-.003
						(.079)
Trend Cubed						-.000
						(.001)
Election Year			-.400		-.690	
			(.996)		(.983)	
Tax Law Changes		-.225	-.233	-.221	-.232	-.258
		(.188)	(.192)	(.211)	(.213)	(.230)
Minister Dummies	–	–	–	Yes	Yes	Yes
R^2	0.521	0.540	0.542	0.685	0.691	0.690
adj. R^2	0.508	0.514	0.502	0.568	0.560	0.522
Observations	38	38	38	38	38	38

Dependent variable: Relative Tax Revenue Forecast Error. Standard errors in parentheses. A single star denotes significance at the 10 % level, two stars at the 5 % level, and three stars at the 1 % level (in the case of minister dummies for joint significance).

Table 3.11: Results on Politics: November Forecast for the next Year (GDP Forecast Error based on Research Institutes)

	(1)	(2)	(3)	(4)	(5)	(6)
Constant	-.855 *	.144	.150	-4.11 *	-4.09 *	-8.20
	(.465)	(.790)	(.800)	(2.30)	(2.33)	(12.6)
GDP Forecast Error	1.53 ***	1.44 ***	1.44 ***	1.72 ***	1.71 ***	1.70 ***
	(.244)	(.240)	(.243)	(.232)	(.236)	(.249)
Trend						.592
						(1.51)
Trend Squared						-.019
						(.070)
Trend Cubed						.000
						(.001)
Election Year			.425		.652	
			(1.08)		(.978)	
Tax Law Changes		-.241	-.234	-.318	-.319	-.346
		(.183)	(.187)	(.188)	(.190)	(.203)
Term of Office		-.002 *	-.002 *	-.003 ***	-.003 ***	-.003 ***
		(.001)	(.001)	(.001)	(.001)	(.001)
Minister Dummies	–	–	–	Yes **	Yes **	Yes **
R^2	0.521	0.578	0.580	0.766	0.770	0.772
adj. R^2	0.508	0.541	0.529	0.667	0.660	0.633
Observations	38	38	38	38	38	38

Dependent variable: Relative Tax Revenue Forecast Error. Standard errors in parentheses. A single star denotes significance at the 10 % level, two stars at the 5 % level, and three stars at the 1 % level (in the case of minister dummies for joint significance).

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Chapter 4

Spatial Administrative Structure and Inner-Metropolitan Tax Competition

Abstract

This chapter considers the impact of the shape of municipalities' borders on local business tax policy. First, a model is presented that shows the dependence of the level of taxation on the spatial administrative structure. Afterwards, data from Germany are employed to discover the effects of the number and size of municipalities within agglomerations. In the definition of agglomerations we rely on the one hand on a distance-based approach, but further develop a method that is based on cumulative population densities. The results show that the spatial administrative structure matters for the level of local business taxation. On the one hand, the core city's tax rate in a metropolitan area is lower the more municipalities are situated around the city. The effect is confirmed when we focus on the average population size of neighboring municipalities rather than on the sheer number of them. On the other hand, the tax of the core city is higher the larger its population share in the agglomeration. Thereby, the result has more power for regions which are more widely defined. These empirical results coincide with those results from the theoretical analysis.

4.1 Introduction

Analyzing the role of borders has always been an important research topic in economics, not only since the significant increase in global trade in recent years and decades. Typically, the questions being discussed deal with the trade of goods and services between countries. The literature has found a notable impact of borders on the intensity of trade between regions. Helliwell (1997) for instance has shown, that trade between provinces of Canada is much more intensive than trade between these provinces and states of the U.S. Borders do not just impose a restriction on the intensity of trade, however, they also define the number of players in the competition of institutions. This chapter aims to look at a smaller administrative level than states, namely metropolitan areas. Within this context, borders have also been widely subject to empirical analyses. Hoxby (2000) studied the impact of the concentration of public school districts in metropolitan areas on school outcomes, where rivers acting as natural borders serve as an instrument for concentration. She found a positive effect of the number of school districts on the quality of schooling. Even closer to the main issue of this chapter is the study of Hatfield and Kosec (2009). They examine the administrative structure of metropolitan statistical areas in the U.S. and their impacts on income growth. They find the latter to be higher in areas that host many county governments.

From this regional perspective borders are the crucial determinant of the scope of local governments' actions. They define which firms are subject to taxation, to fees for public services, but also the area of authority of public administration in general. However, one cannot assume that borders have been designed mainly according to economic considerations. While in rare cases borders follow natural conditions, as in the case of islands, in most cases their design is accompanied by a certain degree of arbitrariness. This holds in particular for agglomerations. While the arbitrariness in cases of clear-cut villages is not of major importance, as long as all inhabitants of the settlement are covered by the village area, the situation proves different when borders cut populations. When this is the case, the location of business can be strongly driven by borders. Firms that decided not to locate their business in the core of the city, but rather at the border, can choose between two or more municipalities, which makes them subject to different administrative bodies. Given that the proximity to customers or relevant public infrastructure is the same at both sides of the border, the firm will choose the municipality that provides a more appealing bundle of taxes and administrative services. When it is assumed that the core of a city is naturally more attractive to firms (as long as we keep public policy issues to one side), tax competition should be the fiercer the closer the border of a city is drawn around its core, since then the surrounding municipality can also participate in the core's benefits (such as the higher density of consumers). With a wide border, however, the city can tax its (exclusive) benefits of agglomeration away, which means less tax competition.[1]

In this chapter we analyze the extent to which municipal borders exert effects on local business taxation, relying on German data. Since the population density of Germany is rather high, there is a notable number of agglomerations[2] that are characterized by a settlement that crosses borders. The arbitrariness of municipal borders mentioned above compels us to take a closer look at the influences of borders on local policy. Since the local business tax ("Gewerbesteuer") is one of the most effective economic measures employed by local politicians it is the impact of the borders' design on this instrument that seems to be particularly relevant. Due to the fact that German municipalities have a high degree of freedom with respect to choosing their local rate of business taxation, it is of interest to see in which way the policy of highly segmented metropolitan areas differs from that of those regions that consist only of a few municipalities. Apart from the sheer number of municipalities, the population share of the city in its agglomeration is another indicator of potential importance in this context. In fact, it turns out that both the segmentation of the region and the share of the city are important covariates in explaining the city's level of taxation. This holds for two different approaches to define regions – one that is based on distances and another that relies on cumulative population densities. The analysis could eventually allow to infer changes in tax policy that could follow after reforms of the borderlines. In doing so, however, the impacts on the city and those on the region would have to be distinguished.

The chapter is organized as follows: The next section presents results from a theoretical model, Section 4.3 provides some background information on German municipalities and institutions. While Section 4.4 describes the definition of agglomerations and the empirical strategy, Section 4.5 shows the results. Section 4.6 concludes.

[1] See Buettner and Kauder (2010) for an analysis of the locational competition between city and surrounding area in the case of Frankfurt, with the focus on the local business tax.

[2] Note that the terms "agglomeration", "metropolitan area", and "region" are used interchangeably in the following.

4.2 Theoretical Model

To analyze the impacts of different administrative structures we start by considering a theoretical model.[3] Here, the analysis follows the framework of the standard tax-competition model established by Zodrow and Mieszkowski (1986).

We consider a region that consists of $i = 1, 2, ..., n$ municipalities, where one municipality is the core city, and the others form the surrounding area. In municipality i there are L^i households, each providing one unit of labor. Firms produce a single good with a linear homogenous production function $F^i(K^i, L^i)$, where K^i denotes capital.[4] We assume capital to be perfectly mobile, so that the marginal product of capital equals the rate of return r. Since all municipalities face the same rate of return in equilibrium, we can write:

$$F_K^i(K^i, L^i) = r.$$

Household income consists of both labor (wL^i, where w denotes the wage rate) and capital income (rs^i, with s^i denoting savings). Their utility depends on private consumption x^i and public good consumption z^i.

To finance public goods, the government raises a distortionary tax on capital, τ^i. Hence, the government budget constraint reads

$$\tau^i K^i = z^i,$$

where the costs of providing public goods are assumed to equal their size, z^i.[5]

[3] In the following we will use the term *administrative structure* as the short form of *spatial administrative structure*.

[4] This implies: $F(K, L) = F_K K + F_L L$. Furthermore, we assume: $\frac{\partial F}{\partial K} \equiv F_K > 0$, $\frac{\partial F}{\partial L} \equiv F_L > 0$, $\frac{\partial^2 F}{\partial K^2} \equiv F_{KK} < 0$, $\frac{\partial^2 F}{\partial L^2} \equiv F_{LL} < 0$, $\frac{\partial^2 F}{\partial K \partial L} \equiv F_{KL} > 0$, and $\frac{\partial^3 F}{\partial K^2 \partial L} \equiv F_{KKL} > 0$.

[5] In reality there is typically also a residence-based capital tax. This, however, can be neglected here,

Each firm will maximize its profits

$$\pi^i = F^i(K^i, L^i) - (r + \tau^i)K^i - wL^i,$$

that consist of output net of costs of capital (including taxes) and labor.[6] The resulting first-order conditions are

$$F^i_K = r + \tau^i \tag{4.1}$$

and

$$F^i_L = w.$$

Differentiating (4.1) yields:

$$F^i_{KK}\partial K^i = \frac{\partial r}{\partial \tau^i}\partial \tau^i + \partial \tau^i.$$

Arranging this equation yields the effect of a tax increase in i on the capital employed in i:

$$\frac{\partial K^i}{\partial \tau^i} = \frac{\frac{\partial r}{\partial \tau^i} + 1}{F^i_{KK}}. \tag{4.2}$$

Following an analogous procedure, the impact of a tax increase in i on the capital in j reads:

$$\frac{\partial K^j}{\partial \tau^i} = \frac{\frac{\partial r}{\partial \tau^i}}{F^j_{KK}}. \tag{4.3}$$

In equilibrium, the employed capital in all municipalities equals the total amount of capital in the region:[7]

$$K^i + \sum_{j \neq i}^{n} K^j = K.$$

since it is – in Germany – determined at the national level.

[6]Here, the firm's profits are independent of the level of public goods. The assumption is that public goods exert perfect spillovers for firms, so that the benefit from them is everywhere the same within an agglomeration. Then firms are just sensitive to tax rates, and not to public goods.

[7]Since differences in the administrative structure may result in high tax and low tax regions, one could consider the total amount of capital in the region to depend on the level of taxation. We neglect such *inter*regional aspects in the following, since our focus is on *intra*regional aspects.

Differentiating this with respect to τ^i yields:

$$\frac{\partial K^i}{\partial \tau^i} + \sum_{j \neq i}^{n} \frac{\partial K^j}{\partial \tau^i} = 0.$$

Since all municipalities (apart from the core) are symmetric, and assuming i to be the core, this can be simplified to:

$$\frac{\partial K^i}{\partial \tau^i} + (n-1)\frac{\partial K^j}{\partial \tau^i} = 0.$$

Using (4.2) and (4.3) yields:

$$\frac{\frac{\partial r}{\partial \tau^i} + 1}{F^i_{KK}} + (n-1)\frac{\frac{\partial r}{\partial \tau^i}}{F^j_{KK}} = 0. \tag{4.4}$$

In contrast to the original framework by Zodrow and Mieszkowski, here the second derivatives of the production function with respect to capital do not cancel. Because the city can have a higher capital stock (and more labor) than the other municipalities, these derivatives may differ, even though the marginal products of capital have to be the same in equilibrium.

Before we can find out about the effects of different administrative structures, we have to solve the municipal-government problem. The benevolent mayor will maximize the utility of the representative private household. Since private consumption can be written as $c^i = F^i(K^i) - F^i_K K^i + rs^i L^i$ and the public good as $z^i = \tau^i K^i$, the maximization problem reads:

$$max_{\tau^i}\ u^i(F^i(K^i) - F^i_K K^i + rs^i L^i, \tau^i K^i).$$

The FOC then becomes:

$$u^i_c(-F^i_{KK}\frac{\partial K^i}{\partial \tau^i}K^i + \frac{\partial r}{\partial \tau^i}s^i L^i) + u^i_z(K^i + \tau^i\frac{\partial K^i}{\partial \tau^i}) = 0.$$

Given a balanced capital account ($s^i L^i = K^i$) and using (4.2) this can be rearranged to

$$\frac{u_z^i}{u_c^i} = \frac{K^i}{K^i + \tau^i \frac{\partial K^i}{\partial \tau^i}}, \tag{4.5}$$

which is the marginal cost of public funds (MCPF).

Proposition 1. When the core city faces $(n-1)$ competitors, the chosen tax rate in the core will be declining in n. Hence, the level of underprovision of public goods is increasing in the number of municipalities.

Proof: Solving equation (4.4) for $\frac{\partial r}{\partial \tau^i}$ yields:

$$\frac{\partial r}{\partial \tau^i} = -\frac{1}{1 + (n-1)\frac{F_{KK}^i}{F_{KK}^j}}. \tag{4.6}$$

Differentiating this with respect to the number of municipalities n yields:

$$\frac{\partial \frac{\partial r}{\partial \tau^i}}{\partial n} = \frac{\frac{F_{KK}^i}{F_{KK}^j}}{(1 + (n-1)\frac{F_{KK}^i}{F_{KK}^j})^2} > 0.$$

Hence, the effect of a tax increase on the return to capital r becomes less negative when the number of municipalities is rising. Plugging this into (4.2), the RHS of that becomes more negative and the MCPF (4.5) increases. Thus, the underprovision of public goods becomes more severe and the tax rate is declining.

Proposition 2. The core city will set a tax rate that is higher, the larger the core city's share of total labor in the region.

Proof: Starting from (4.6), the impact of an increase in the share of labor of the core city

$(\frac{L^i}{L})$, holding everything else constant, becomes:

$$\frac{\partial \frac{\partial r}{\partial \tau^i}}{\partial \frac{L^i}{L}} = \frac{(n-1)\frac{\partial \frac{F^i_{KK}}{F^j_{KK}}}{\partial \frac{L^i}{L}}}{(1+(n-1)\frac{F^i_{KK}}{F^j_{KK}})^2} < 0.$$

Since the derivative in the numerator is negative, the impact of a tax increase on the return to capital will be stronger (more negative) when the labor share of the city is higher. Plugging this into (4.2) yields a smaller impact of a tax increase on the stock of capital in the core. Accordingly, the MCPF (4.5) will approach one from above when the labor share of the core increases. Hence, there will be less underprovision of public goods and therefore a higher tax rate in the core when its share of total labor is larger.

4.3 German Municipalities and Institutions

Having shown the results that are predicted by standard tax-competition theory, we try to test whether they can be confirmed in an empirical analysis. In doing so we rely on German data from 2007. First, it is reasonable to have a look at the German institutions, since their design might influence the results.

The tax system in Germany allows both the Federal Government and the municipalities to tax business income. While municipalities take the rate of the *corporate tax* as given, they decide upon the rate of the *local business tax*. To be chosen is the “rate of assessment”, which has a lower bound of 200 points but no upper bound. Since this rate currently is multiplied by 3.5 % to obtain the percentage being taxed away, it equals a minimal burden of 7 %. The lower bound was introduced in 2004 to avoid zero-tax strategies in single municipalities.[8] The highest rate in 2007 was 490 points (in the city of Munich, for example).[9] The local business tax is the most important autonomously determined source of revenue for German municipalities. In the average city it generated about 52 % of tax revenues in 2007 (see Figure 1.2).

Altogether there were 12263 municipalities in Germany in 2007. The trend of recent years shows a notable decline in the number of municipalities in eastern Germany, while the figure is rather constant in other parts of the country.[10] There are significant differences in the municipal structure among the German states ("Bundesländer"), as can be seen in Figure 4.1. Here, the number of municipalities per 100 square kilometers is shown. The state of Rhineland-Palatinate, for instance, comprises 2306 municipalities but the state of North Rhine-Westphalia just 396, though the latter is characterized by a larger area. This indicates a high degree of variation in the data used.

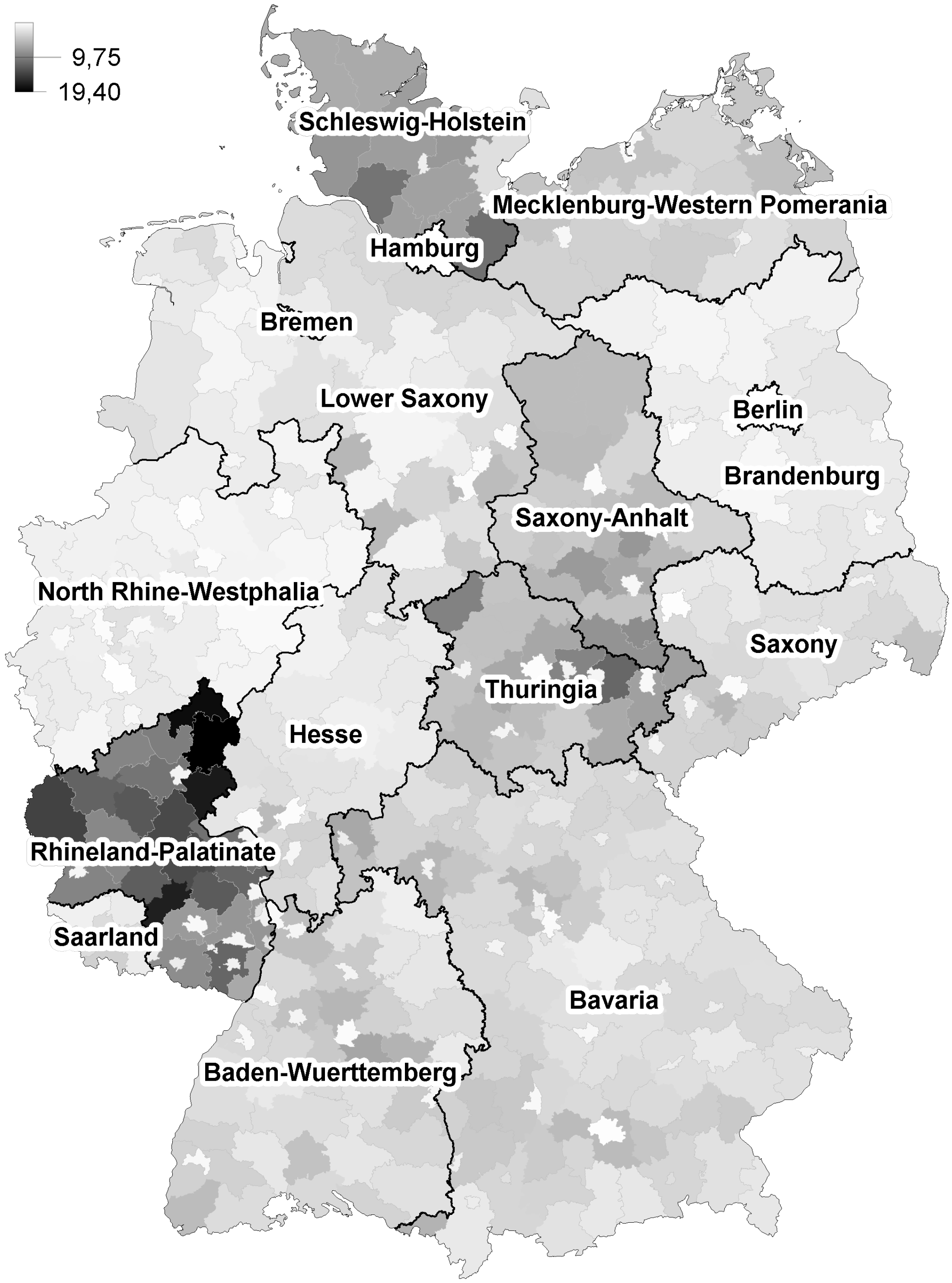
A data set with all German municipalities is employed, where information on the population, the area, the local business tax rate and some further derived measures is exploited. To measure the (linear) distance between municipalities, we rely on the geographical coordinates of their official central points. The latter are well-defined in Germany and are typically some very central places in the centers of the municipalities.

[8] The municipality of Norderfriedrichskoog (about 40 inhabitants) in northern Germany had attracted some hundred firms until 2004 with a zero tax rate. The lower bound aims at preventing the emergence of such tax havens with phantom companies.

[9] To be precise, three municipalities charged an even higher rate, but all of them have less than 100 inhabitants. The "number one" was Dierfeld (8 inhabitants) with a rate of 900.

[10] Compare Chapter 5.

Figure 4.1: Number of Municipalities per 100 Square Kilometers



4.4 Agglomerations and Empirical Strategy

We seek to explain the local business tax rate of the core city of an agglomeration. To identify differences in tax policies among cities as a function of the administrative structure we have to give some thought to how agglomerations can be defined and for which cities this should be done. The answer to the second question is that we will define agglomerations around all cities that are independent of counties ("Kreisfreie Städte").[11] In 2007 there existed 118 of these cities. Since this status typically correlates with size, the units considered comprise most of Germany's large cities.[12]

To answer the first question concerning how agglomerations can be defined, one needs to engage in some more fundamental thinking. Obviously, there exists no universal definition of agglomerations. The United Nations defines *urban agglomerations* as "the de facto population contained within the contours of a contiguous territory inhabited at urban density levels without regard to administrative boundaries. It usually incorporates the population in a city or town plus that in the sub-urban areas lying outside of but being adjacent to the city boundaries." While this definition obviously focuses on population densities and a pretty narrow geographical expanse, the UN definition of *metropolitan areas* also focuses on economic links and captures a broader area. According to this, a metropolitan area "includes both the contiguous territory inhabited at urban levels of residential density and additional surrounding areas of lower settlement density that are also under the direct influence of the city (e.g., through frequent transport, road linkages, commuting facilities etc.)."[13]

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[11] These cities are not associated with a county, but are directly subordinate to their state. In contrast, a county-*dependent* city is part of a county, which is at an intermediate stage between city and state.

[12] The cities of Hanover and Saarbrücken are also considered "Kreisfreie Städte", even though their legal status deviates slightly ("Kommunalverband besonderer Art").

[13] See the UN website: http://esa.un.org/wup2009/unup/index.asp?panel=6.

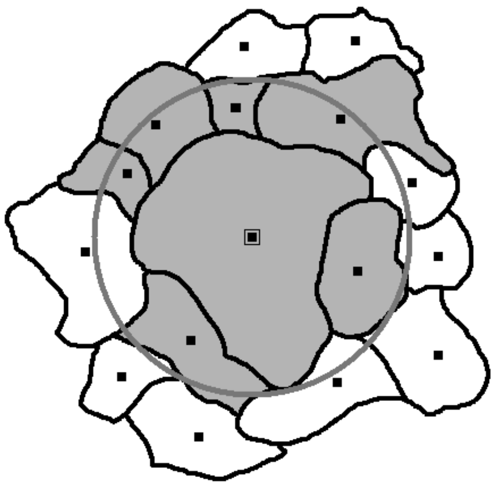
Since these two definitions, with the first being a subset of the second, underpin the difficulties in finding an appropriate definition of agglomerations, we will employ two different approaches. These try to capture the different aspects stated by the UN. While our first approach is based on the sheer proximity of municipalities to a core city, our second approach covers the idea of focusing on population densities.

The first approach defines an agglomeration as consisting of all municipalities that have their central point within a certain distance (of 15, 25, or 50 km) to the core city (see Figure 4.2). In contrast, the second approach sorts the surrounding municipalities precisely according to their distance to the core. Starting from the core, we add municipalities – with increasing distances – until the *cumulative* population density from the core to the respective municipality no longer changes significantly. The resulting area is then considered the agglomeration (see Figure 4.3; a more detailed explanation follows below). It has to be emphasized that agglomerations' size differs from city to city in the second approach, while it is the same for all cities in the first approach. In both approaches it can happen that agglomerations overlap.[14]

To describe the different structures within agglomerations, we establish basically two kinds of indicators: the position of the core city relative to the surrounding municipalities on the one hand, and the picture of the surrounding municipalities among themselves on the other hand. Accordingly, as the first independent variable the population of the core city relative to the total population in the agglomeration is calculated. This "share of core" aims at measuring the "power" of the core in the region. Table 4.1 displays the ten largest

[14] As an alternative approach, also the so-called "BIK regions" have been considered. The concept of BIK regions (provided by *BIK ASCHPURWIS+BEHRENS*) is based on Boustedt (1953), who in turn took the US concept of "Standard Metropolitan Areas" (SMA) as a model. According to the BIK-regions concept agglomerations are formed by those surrounding municipalities in which a certain share of the population is commuting to the core city. Thus, it is pretty close to the UN definition of metropolitan areas. Since the empirical results with this approach are both qualitatively and quantitatively comparable to those in the two other approaches, we do not present these results.

Figure 4.2: The Definition of Agglomerations (First Approach)



In the first approach all municipalities that have their central points within a certain distance to the core are part of the region and are here illustrated by shadowed areas and a circle. The agglomeration shown here comprises six municipalities plus the core.

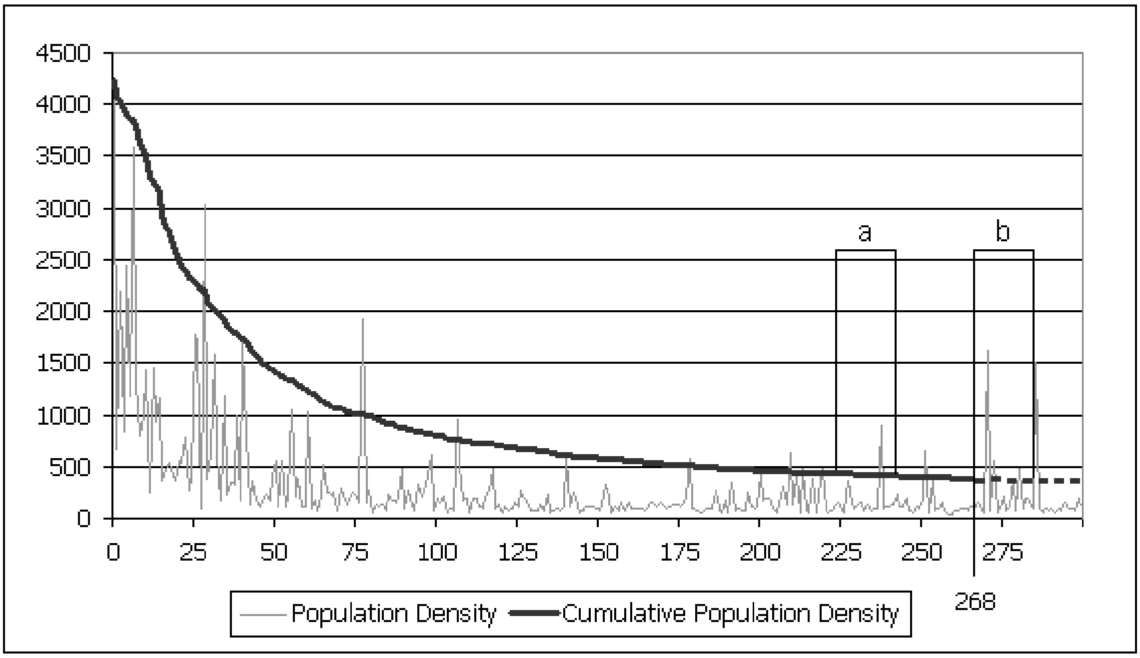
German cities and the population shares in their agglomerations, defined with a radius of 50 km around the city, respectively.

To measure the intensity of competition the city is exposed to, we consider two alternative variables: On the one hand we look at the sheer number of municipalities within the region. On the other hand, the average population of the surrounding municipalities is used, whereby the size of the neighboring municipalities is typically negatively correlated with their number.

In this context, it seems reasonable to assume that close-to-the-core municipalities play a different role than those that are located farther away.[15] The consideration of regions defined with different radii aims at capturing this. The larger the region we are looking at, the smaller we expect the impact of the fragmentation on the level of taxation in the core to be.

[15]For an analysis of the role of different locations of administrative bodies in the case of commodity tax competition, see Ohsawa (1999).

Figure 4.3: The Definition of Agglomerations (Second Approach)



In the second approach municipalities (displayed at the abscissa with increasing distance to the core) are added to the region until the cumulative population density (from the core to the respective municipality) remains "roughly" constant (dashed line). While the decline in cumulative population density at *a* is still too large, it is sufficiently small at *b*. The agglomeration shown here (Munich) comprises 268 municipalities.

To isolate the effects of our variables, we add a set of control variables to account for other than administrative aspects that may drive local business taxation. The first aspect to be mentioned concerns population density figures. Brecht (1932) postulated a positive relationship between population density and public expenditures, based on size disadvantages of large cities. They would result in higher tax rates. Nowadays, the literature discusses a u-shaped relationship with decreasing per-capita expenditures up to an optimal municipality size or density (due to economies of scale) and increasing per-capita expenditures afterwards (due to congestion externalities).[16] Another reason for a larger tax burden would be

[16]See Seitz (2002) for empirical evidence for a u-shaped relationship regarding population densities in

Table 4.1: The Largest German Cities and their Regions (of 50 km)

City	Pop. City	Pop. Region	Pop. Share	No. of Munic.	Pop. per Munic.
Berlin	3,416,255	4,575,679	74.66 %	100	11,594
Hamburg	1,770,629	3,358,761	52.72 %	451	3,521
Munich	1,311,573	3,084,994	42.51 %	255	6,955
Cologne	995,397	6,264,709	15.89 %	141	37,371
Frankfurt	659,021	4,283,820	15.38 %	268	13,525
Stuttgart	597,176	4,213,761	14.17 %	321	11,267
Dortmund	586,909	6,801,063	8.63 %	91	68,287
Essen	582,140	8,545,506	6.81 %	104	76,571
Düsseldorf	581,122	8,775,278	6.62 %	103	79,555
Bremen	547,769	1,601,718	34.20 %	147	7,170

The first column shows the population figure of the respective city, the second column the population of the municipalities within a circle of 50 km (including the core), the third column displays the population share of the core in its region, the fourth column shows the number of municipalities within a circle of 50 km (excluding the core), and the fifth column the population per surrounding municipality.

"agglomeration rents" as formulated in the New-Economic-Geography literature.[17] Here, firms located in agglomerations can enjoy higher profits because of lower transport costs to their customers. These agglomeration rents can be taxed away.

One can argue in a similar way concerning sheer population figures, as done by Popitz (1932). In his argumentation public expenditures (per capita) are higher in large cities because of a higher demand for public goods of people living in such cities. Even though large population figures and large population densities are different things, we can anticipate a similar impact of both on expenditures. Accordingly, we can expect per-capita

Germany. Note that we can assume to be above the optimal point since we only choose to analyze core cities of metropolitan areas.

[17]See, for instance, Baldwin and Krugman (2004) for a theoretical analysis of agglomeration rents and taxation.

expenditures to be higher the larger either the population or the population density of the core or the whole region becomes. This, in turn, can induce local business tax rates to be higher. Since congestion externalities in the city are not only caused by inhabitants of the city, but also by those of the surrounding area, we will focus on the whole region (which includes the core) when controlling for the population.[18] Because the data indicate a high correlation between population and population density, we concentrate on the population figures to isolate the administrative effects.

A further important factor is the affiliation of a municipality to a state. Figure 4.4 shows that the level of local business taxation depends crucially on the state the municipality belongs to. One reason for this may be the different designs of fiscal equalization schemes in the states, which are associated with different degrees of redistribution of fiscal resources between the municipalities. In each state a fiscal equalization scheme partly balances the difference between “fiscal need” and “fiscal capacity” of a municipality, if positive. While the state of Hesse, for instance, balances just 50 % of this difference, the state of North Rhine-Westphalia does so to 90 %. Accordingly, there are significant differences in the share of an additional Euro of revenue that the municipality can keep.[19] Due to the resulting different incentives in setting tax rates,[20] state fixed effects are included to control for this aspect.

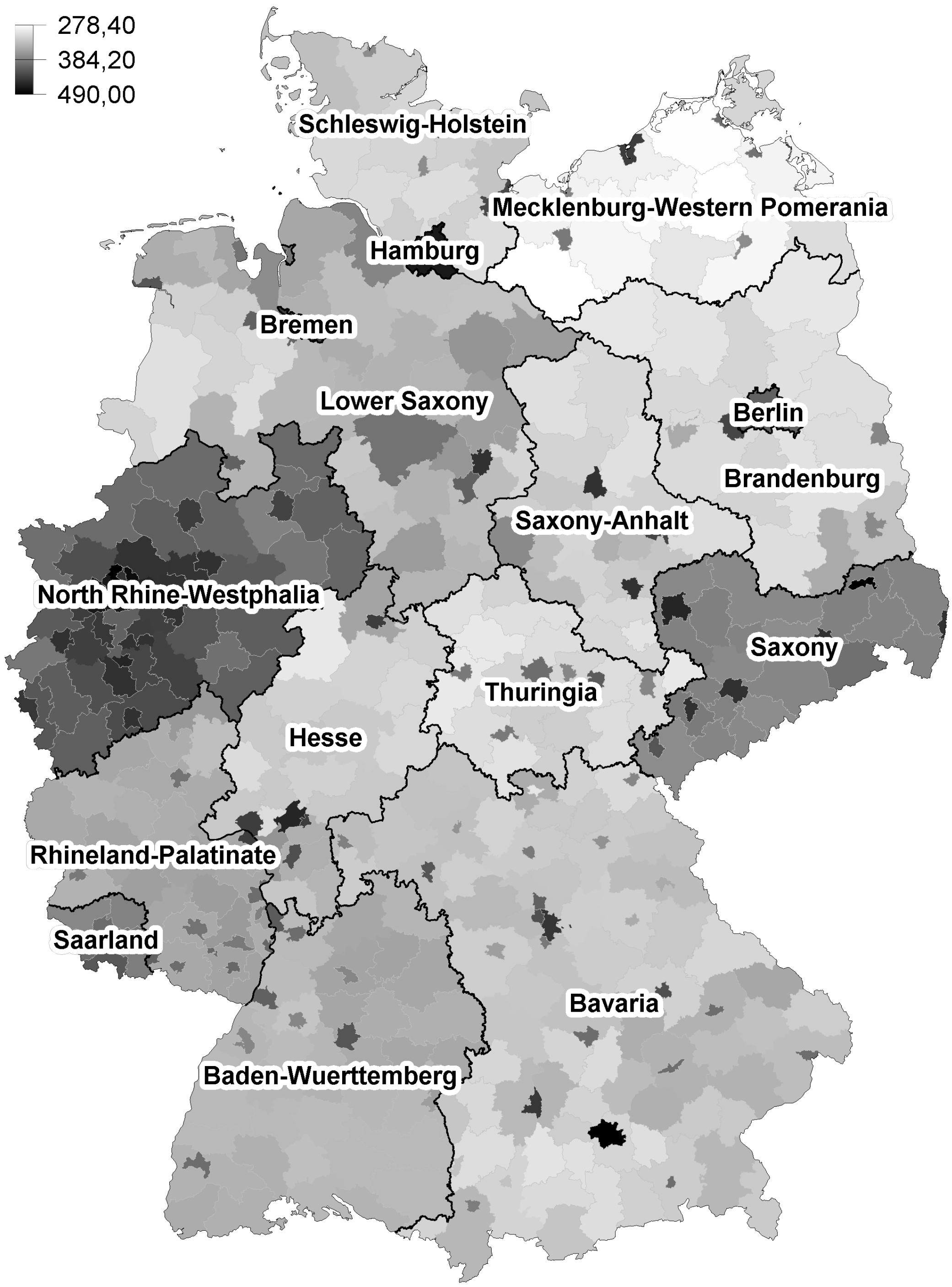
Furthermore, the distance to the next county-independent city is included as an additional covariate. Since this variable aims at capturing the intensity of interregional tax competition, we expect the tax rate in a city to be higher the farther away the next city is

[18]The population of the surrounding area is co-using the infrastructure and hence can also cause taxes in the city to be higher.

[19]Note that this share is also affected by other factors, like the rate that is applied to normalize revenues.

[20]See Buettner (2006) for a theoretical and empirical analysis of this aspect. See Bucovetsky and Smart (2006) and Koethenbuerger (2002) for further discussions.

Figure 4.4: Local Business Taxation in Germany ("Rate of Assessment")



situated.[21]

Tables 4.7 through 4.11 (see appendix) provide descriptive statistics on the most relevant figures. Furthermore, Tables 4.12 through 4.15 show the correlation coefficients of the four main independent variables for both approaches in their different settings. As can be seen when looking at the coefficients for the number of surrounding municipalities and the share of the core city, there is only a small correlation between both approaches. This indicates that they actually cover different areas, and hence, similarities of the empirical results can hardly be attributed to similar definitions of agglomerations. Looking at the coefficients of the two other main independent variables indicates higher correlations. All variables, however, concern figures that can be similar even if the municipalities covered by an agglomeration are considerably different. Thus, we can consider the two approaches as robustness checks for one another.

4.5 Empirical Results

Having discussed the empirical strategy, we go on with considering the results of the first approach, which defines regions by drawing circles of some radius around core cities. For each radius (15, 25, and 50 km) five specifications are shown.

In Table 4.2, regions with a radius of 15 km are considered.[22] As the first column shows,

[21]We also tried some other variables, that, however, have failed to contribute to the explanation of the core's tax rate. Among them were the per-capita expenditures for recipients of "Unemployment Benefit II (SGB II)" to try to capture the "expenditure needs" of a municipality, and the number of passengers of airports within an agglomeration as a possibility of taxing location-specific rents away. Moreover, including the quadratic form of the population of the region does not affect the results. In contrast, controlling for the per-capita level of the core city's debt shows a (positive) significant effect; without, however, affecting the variables of interest.

[22]The effects of single basic variables on the coefficient of determination can be seen in Table 4.16 in the appendix for the case of regions of 50 km.

there is a highly significant impact of the population share of the core in the region. If the share of the core city increases from 0 to 100 %, the rate of assessment in the core will increase by 34 points. The third column shows that the average size of the surrounding municipalities is also an important determinant. If the average size increases by 1 %, the rate of assessment increases by 12 points. This results from the declining number of competitors due to the larger size of the surrounding municipalities. The effect of an increase in the share of the core now becomes even more important. Going from one extreme to the other makes the level of taxation 57 points higher. When we look directly at the number of municipalities instead of the size, the results are confirmed. An increase in the number of municipalities and hence the number of competitors of 1 % results in a rate of assessment that is about 11 points lower (see columns (4) and (5)). The impact of the share of the core city proves important again, though with a somewhat smaller magnitude. In all specifications the population of the region as a baseline control variable proves very important. An increase in the population of 1 % coincides with a rate of assessment that is 15 to 26 points higher. The impact of the distance of the core city to the next county-independent city only proves important in specifications without the share of the core city.[23] Here, the next city to be 1 km farther away results in a rate of assessment that is roughly a quarter of a percentage point higher.

When we turn to regions of 25 km in Table 4.3, the effect of the share of the core city becomes more important. Now, an increase in the population share of the core from 0 to 100 % results in a rate of assessment that is 63 points higher. This higher magnitude is confirmed also in the other specifications (see columns (3) and (5)), and suggests that a large share in a widely defined region is associated with a stronger position of the core than a large share in a narrowly defined region. The effects of the size or number of surrounding municipalities still prove statistically significant in most cases. They are,

[23]This may be due to collinearity, since the next city to be sufficiently far away implies there to be no other city in the agglomeration, and hence a large share of the core.

Table 4.2: Local Business Taxation in Metropolitan Areas of 15 km

	(1)	(2)	(3)	(4)	(5)
Constant	237 ***	253 ***	273 ***	291 ***	274 ***
	(25.0)	(31.2)	(30.7)	(32.6)	(32.8)
log(Pop. per Surround. Mun.)		.248	12.4 *		
		(5.11)	(6.31)		
log(No. of Surround. Munic.)				-11.4 *	-10.5 *
				(6.27)	(6.16)
Pop. Share of Core City	33.9 **		56.9 ***		32.3 **
	(14.6)		(18.5)		(14.5)
log(Pop. Region)	25.7 ***	25.5 ***	14.7 **	26.4 ***	26.3 ***
	(3.46)	(5.72)	(6.51)	(3.52)	(3.45)
Distance to next City	.134	.279 *	.102	.243 *	.109
	(.150)	(.143)	(.149)	(.139)	(.149)
R^2	0.719	0.703	0.729	0.713	0.727
adj. R^2	0.667	0.649	0.677	0.661	0.674
Observations	118	118	118	118	118

Dependent variable: Rate of assessment of the core city of an agglomeration. Fixed-effects estimation with state fixed effects. Standard errors in parentheses. A single star denotes significance at the 10 % level, two stars at the 5 % level, and three stars at the 1 % level.

however, somewhat larger. A rise in the population per surrounding municipality of 1 % or a decline in the number of municipalities of 1 % leads to an increase in the rate of assessment of roughly 16 to 17 points. The impact of the population of the whole region is as important as before, both in terms of economic and statistical significance. An increase of the population of 1 % implies the rate of assessment in the core to be 14 to 32 points higher. Again, the proximity of the core to the next county-independent city is important in our specifications that exclude the share of the core city. The effect has become more important though, with an increase in the core's tax of 0.4 points when the next city is 1 km farther away.

Table 4.3: Local Business Taxation in Metropolitan Areas of 25 km

	(1)	(2)	(3)	(4)	(5)
Constant	210 ***	219 ***	272 ***	275 ***	272 ***
	(29.7)	(41.0)	40.7	43.6	(41.7)
log(Pop. per Surround. Mun.)		1.50	16.7 **		
		(7.12)	(7.72)		
log(No. of Surround. Munic.)				-15.9 **	-15.7 **
				(7.96)	(7.60)
Pop. Share of Core City	62.5 ***		87.1 ***		62.1 ***
	(19.6)		(22.3)		(19.3)
log(Pop. Region)	27.2 ***	28.4 ***	13.5 *	32.0 ***	29.6 ***
	(4.21)	(6.93)	(7.52)	(4.44)	(4.31)
Distance to next City	.187	.446 ***	.161	.412 ***	.161
	(.167)	(.158)	(.165)	(.152)	(.165)
R^2	0.720	0.691	0.733	0.703	0.731
adj. R^2	0.669	0.635	0.681	0.649	0.679
Observations	118	118	118	118	118

Dependent variable: Rate of assessment of the core city of an agglomeration. Fixed-effects estimation with state fixed effects. Standard errors in parentheses. A single star denotes significance at the 10 % level, two stars at the 5 % level, and three stars at the 1 % level.

Table 4.4 shows the results for agglomerations that are defined as the municipalities within a radius of 50 km around the city. Both the size or number of the surrounding municipalities and the share of the core still prove important. The magnitude of the fragmentation effect remains at the same level as before. Thus, our conjecture that this effect becomes smaller with an increasing radius of the region cannot be confirmed.[24] Compared to the preceding definitions of regions the effect of the core city's share in the region, however, has become more important. The (hypothetical) difference between a city that covers the whole region

[24]We can see, however, that the coefficients of determination in those specifications that include only a fragmentation variable, but not the share-of-core variable (columns (2) and (4)), become smaller, the larger the region we consider.

of a radius of 50 km and a city of infinitesimal size would imply a 194 to 225 points higher rate of assessment. The impact of the population of the whole region has become smaller. This is because the broader definition of regions covers also population from farther away. A population that is larger by 1 % is accompanied by an increase in the rate of assessment of 6 to 29 points. The distance to the next county-independent city proves to be somewhat less important than before, both in terms of economic and statistical significance.

As the coefficients of determination in all definitions of regions show, the administrative structure, population figures, and the proximity to the next county-independent city can explain about 60 to 73 % of the variation in the level of local business taxation.

After having discussed the results for the distance-based approach, we go on with the second approach, that defines regions in a different fashion. Rather than simply relying on radii around core cities that are the same for each city, we now focus for two different parameters on population densities in the space. We start with ranking surrounding municipalities according to their distance to the core. Then, so many municipalities from this list are added to the core until the cumulative population density of the resulting region remains roughly constant. The definition of the region is completed when the cumulative population density from the core to some surrounding municipality n exceeds that from the core to the surrounding municipality $n + 20$ by no more than q % (with $q = 1, 10$).[25] This approach captures the "agglomeration mountain" that exposes itself from the flat country (see again Figure 4.3).[26]

Following this idea for $q = 10$, we obtain the results displayed in Table 4.5. The first

[25] We also tried a different span ($n + 10$) and different values for q $(2, 5)$. The larger the span and the smaller q, the larger the agglomerations. The empirical results went in the anticipated directions.

[26] Since in few cases cities are part of a plateau of population density, a surrounding area of zero municipalities can result. These observations are dropped, so that we are left with only 112 and 116 out of 118 observations, respectively.

Table 4.4: Local Business Taxation in Metropolitan Areas of 50 km

	(1)	(2)	(3)	(4)	(5)
Constant	218 ***	216 ***	313 ***	282 ***	313 ***
	(44.8)	(65.6)	(56.3)	(67.0)	(56.6)
log(Pop. per Surround. Mun.)		1.71	19.6 ***		
		(8.34)	(7.41)		
log(No. of Surround. Munic.)				-15.1 *	-19.4 ***
				(8.73)	(7.39)
Pop. Share of Core City	194 ***		225 ***		201 ***
	(32.0)		(33.3)		(31.2)
log(Pop. Region)	22.0 ***	24.5 ***	5.99	28.7 ***	25.4 ***
	(5.66)	(9.27)	(8.16)	(6.68)	(5.65)
Distance to next City	.030	.374 *	-.059	.318	-.059
	(.173)	(.192)	(.172)	(.192)	(.172)
R^2	0.714	0.609	0.733	0.620	0.733
adj. R^2	0.662	0.537	0.681	0.551	0.681
Observations	118	118	118	118	118

Dependent variable: Rate of assessment of the core city of an agglomeration. Fixed-effects estimation with state fixed effects. Standard errors in parentheses. A single star denotes significance at the 10 % level, two stars at the 5 % level, and three stars at the 1 % level.

column shows the rate of assessment of the core city to be 85 points higher when the share of the core increases from 0 to 100 %. This result is roughly confirmed in columns (3) and (5), and the magnitude resembles the figures from the agglomerations of a radius of 25 km in approach 1. Turning to columns (2) and (3) we see a rise in the size of surrounding municipalities of 1 % to increase the core's tax rate by 11 to 14 points. An increase in the number of municipalities of 1 % implies a 13 to 20 points lower tax rate (see columns (4) and (5)). Again, the magnitudes are similar to those in approach 1. Also the relevance of the population of the region is about as high as before, with an increase of 12 to 33 points resulting from a population increase of 1 %. If the next county-independent city is located

Table 4.5: Local Business Taxation in Metropolitan Areas with Parameter of 10 %

	(1)	(2)	(3)	(4)	(5)
Constant	214 ***	328 ***	224 ***	294 ***	227 ***
	(28.0)	(22.5)	(27.3)	(20.5)	(27.6)
log(Pop. per Surround. Mun.)		11.1 **	14.0 ***		
		(5.79)	(5.08)		
log(No. of Surround. Munic.)				-20.2 ***	-12.6 **
				(4.56)	(4.87)
Pop. Share of Core City	84.8 ***		89.4 ***		62.9 ***
	(16.8)		(16.3)		(18.4)
log(Pop. Region)	25.1 ***	11.8 ***	20.4 ***	33.1 ***	33.4 ***
	(3.18)	(3.61)	(3.52)	(4.70)	(4.45)
Distance to next City	.059	.098	.204	.246 *	.202
	(.133)	(.157)	(.139)	(.148)	(.140)
R^2	0.720	0.658	0.742	0.706	0.739
adj. R^2	0.666	0.591	0.688	0.649	0.686
Observations	112	112	112	112	112

Dependent variable: Rate of assessment of the core city of an agglomeration. Fixed-effects estimation with state fixed effects. Standard errors in parentheses. A single star denotes significance at the 10 % level, two stars at the 5 % level, and three stars at the 1 % level.

1 km farther away, the tax in the core will be up to a quarter of a point higher.

When we alternatively employ this second approach with a different parameter, $q = 1$, we get the results in Table 4.6. This parameter ensures that the agglomerations are larger, because now a smaller decline in the cumulative population density is required. The importance of the core city's share is thus a bit larger than before. An increase in the share from 0 to 100 % goes along with an increase in the rate of assessment of 70 to 102 points (see specifications (1), (3) and (5)). The impact of the fragmentation of the region proves to be as important as before. When the size of the surrounding municipalities

Table 4.6: Local Business Taxation in Metropolitan Areas with Parameter of 1 %

	(1)	(2)	(3)	(4)	(5)
Constant	220 ***	347 ***	218 ***	294 ***	225 ***
	(28.4)	(19.3)	(27.1)	(20.3)	(27.5)
log(Pop. per Surround. Mun.)		14.5 **	16.2 ***		
		(5.95)	(5.11)		
log(No. of Surround. Munic.)				-23.2 ***	-14.1 ***
				(4.57)	(5.03)
Pop. Share of Core City	98.8 ***		102 ***		70.3 ***
	(17.7)		(17.0)		(19.9)
log(Pop. Region)	23.2 ***	8.09 ***	21.0 ***	35.9 ***	35.0 ***
	(3.07)	(2.47)	(3.02)	(5.44)	(5.15)
Distance to next City	.110	.095	.276 **	.285 *	.267 *
	(.133)	(.157)	(.138)	(.148)	(.140)
R^2	0.712	0.641	0.739	0.699	0.734
adj. R^2	0.659	0.575	0.688	0.643	0.681
Observations	116	116	116	116	116

Dependent variable: Rate of assessment of the core city of an agglomeration. Fixed-effects estimation with state fixed effects. Standard errors in parentheses. A single star denotes significance at the 10 % level, two stars at the 5 % level, and three stars at the 1 % level.

increases by 1 % or the number of municipalities declines by 1 %, the tax rate will be 14 to 23 points higher. Also the impacts of the total population of the region and the proximity to the next city are similar to the first parameter setting. A rise in the population of the region of 1 % implies the rate of assessment in the core to be 8 to 36 points higher. If the next city is located 1 km farther away, a rate of assessment results that is about up to a quarter of a point higher.

As we have seen, the results from the first approach are confirmed by the second approach (with even higher levels of R^2 of 64 to 74 %). Both variables that aim at describing the administrative structure within agglomerations, the number or size of the surrounding municipalities on the one hand and the share of the core city in the agglomeration on the other hand, have proven important throughout the majority of specifications presented in this section.[27] Hence, we can conclude that the administrative structure is an important factor in explaining the level of local business taxation in core cities of metropolitan areas.

4.6 Conclusions

This chapter has considered the question of whether the spatial administrative structure of municipalities affects the level of local business taxation. We examined the tax policy of core cities of metropolitan areas, in which the design of borders can be considered to be rather arbitrary. Due to the differences in the border design among agglomerations, the respective core cities are facing different circumstances in deciding upon local tax policy. On the one hand, cities could be affected by their position relative to the surrounding area, and on the other hand they may be affected by the administrative structure of the surrounding area itself.

Initially we considered the standard tax-competition model established by Zodrow and Mieszkowski. Here, the two aspects just mentioned are analyzed theoretically. As a first result, it is shown that an increasing number of surrounding municipalities (and hence competitors) lowers the level of taxation in the core city. The second result states that having a larger share in the agglomeration goes along with a higher tax rate of the core city.

[27]When the analysis is restricted to the old West German states the effect of the share shows to be stronger, while the effect of the surrounding area is somewhat weaker. The opposite holds when only the newly formed German states are considered.

Before empirical analysis can be applied, one has to think about how to define agglomerations. Since different concepts appear to be reasonable, we establish two approaches that try to represent different ideas. The first approach simply considers all municipalities that are located within a certain distance to the core city to be part of an agglomeration. The second approach is more subtle. It sorts surrounding municipalities according to their distance to the core, and adds them to the agglomeration until the cumulative population density from the core to the respective municipality is approaching a constant value. This allows the area of an agglomeration to differ from city to city.

Based on these two approaches, each of them performed with different parameters, we apply German data and in doing so focus on county-independent cities as cores of agglomerations. To capture differences in the administrative structure we rely basically on two kinds of indicators. The position of the core relative to the surrounding area is measured by the core's population share in the agglomeration. The design of borders in the surrounding area is described by focusing on two numbers. Both the number of municipalities and their average size are used to describe the degree of fragmentation.

It is shown that the theoretical predictions can be confirmed empirically. In metropolitan areas a rise in the number of surrounding municipalities lowers the level of taxation in the core. The result is confirmed when we use the average size of surrounding municipalities instead. Since smaller surrounding municipalities imply a larger number of competitors, the level of taxation will decrease. Furthermore, the relative position of the core in the agglomeration proves important. The larger the share of the core in its agglomeration the higher its tax rate. Hence, not only the competitors prove important, but also the position of the core city itself. The effect of the share of the core city becomes more important when the region that is considered becomes larger, since a given share in a large region is associated with a more powerful position of this city than the same share in a small region. All these results hold true for both concepts of defining agglomerations, for the distance-based as well as for the density-based approach. They explain up to three quarters of the variation in local business taxation. This leads to the conclusion that the administrative structure of a metropolitan area is a crucial determinant of tax policy in the core. We can expect that both consolidations of municipalities including the core and those just affecting the surrounding area can strengthen the core in its power to charge taxes. This includes a reduction in the level of that part of tax competition that results from positive externalities of core cities.

It is left for future research to look into the surrounding areas of core cities to trace the determinants of tax policy in the suburbs. Furthermore, whether reverse impacts exist could be analyzed: One could raise the question of whether the level of business taxation exerts effects on the administrative structure. Also, the reaction of firms could be considered. On the one hand, where firms locate *within* an agglomeration as a reaction to different designs of borders is a subject of interest. On the other hand, the question remains as to the benefits to a region which redesigns its borders (in terms of attractions of firms), and hence further interregional issues could be analyzed.

Appendix

Table 4.7: Descriptive Statistics (Regions of 15 km)

Variable	Obs.	Mean	Standard Dev.	Min	Max
Tax Rate Core	118	415	37.2	300	490
Pop. Core	118	229382	383510	34719	3416255
Pop. Region	118	533841	560748	72125	3439531
Pop. Share of Core City	118	.466	.214	.056	.993
Pop. per Surr. Munic.	118	27993	51894	954	267950
No. of Surr. Munic.	118	22.0	15.3	2	80
Distance to next City	118	30.4	18.1	2.36	89.9

Tax Rate Core: “rate of assessment”. Region comprises core and surrounding municipalities. Distance in kilometers.

Table 4.8: Descriptive Statistics (Regions of 25 km)

Variable	Obs.	Mean	Standard Dev.	Min	Max
Tax Rate Core	118	415	37.2	300	490
Pop. Core	118	229382	383510	34719	3416255
Pop. Region	118	1006343	987922	129565	4053493
Pop. Share of Core City	118	.266	.171	.032	.889
Pop. per Surr. Munic.	118	22508	36806	890	160605
No. of Surr. Munic.	118	64.2	41.5	11	247
Distance to next City	118	30.4	18.1	2.36	89.9

Tax Rate Core: “rate of assessment”. Region comprises core and surrounding municipalities. Distance in kilometers.

Table 4.9: Descriptive Statistics (Regions of 50 km)

Variable	Obs.	Mean	Standard Dev.	Min	Max
Tax Rate Core	118	415	37.2	300	490
Pop. Core	118	229382	383510	34719	3416255
Pop. Region	118	2815419	2533411	304597	10038362
Pop. Share of Core City	118	.102	.107	.012	.747
Pop. per Surr. Munic.	118	17934	26361	1366	89780
No. of Surr. Munic.	118	254	154	48	911
Distance to next City	118	30.4	18.1	2.36	89.9

Tax Rate Core: “rate of assessment”. Region comprises core and surrounding municipalities. Distance in kilometers.

Table 4.10: Descriptive Statistics (Regions 10 %)

Variable	Obs.	Mean	Standard Dev.	Min	Max
Tax Rate Core	112	415	37.5	300	490
Pop. Core	112	236045	392392	34719	3416255
Pop. Region	112	794969	1120851	43506	5866000
Pop. Share of Core City	112	.410	.189	.029	.887
Pop. per Surr. Munic.	112	18357	32004	632	146789
No. of Surr. Munic.	112	43.4	31.5	1	151
Distance to next City	112	31.4	18.0	2.36	89.9

Tax Rate Core: "rate of assessment". Region comprises core and surrounding municipalities. Distance in kilometers.

Table 4.11: Descriptive Statistics (Regions 1 %)

Variable	Obs.	Mean	Standard Dev.	Min	Max
Tax Rate Core	116	415	37.3	300	490
Pop. Core	116	231949	386298	34719	3416255
Pop. Region	116	1435340	2569809	43879	14746017
Pop. Share of Core City	116	.330	.202	.017	.911
Pop. per Surr. Munic.	116	14196	22581	728	132404
No. of Surr. Munic.	116	102	106	1	520
Distance to next City	116	30.8	18.0	2.36	89.9

Tax Rate Core: "rate of assessment". Region comprises core and surrounding municipalities. Distance in kilometers.

Table 4.12: Correlation of the Number of Surrounding Municipalities

Variable	Reg. 15 km	Reg. 25 km	Reg. 50 km	Reg. 10 %	Reg. 1 %
Regions of 15 km	1.000				
Regions of 25 km	0.911	1.000			
Regions of 50 km	0.786	0.925	1.000		
Regions 10 %	0.185	0.216	0.144	1.000	
Regions 1 %	0.037	0.192	0.228	0.583	1.000

Table 4.13: Correlation of the Population per Surrounding Municipality

Variable	Reg. 15 km	Reg. 25 km	Reg. 50 km	Reg. 10 %	Reg. 1 %
Regions of 15 km	1.000				
Regions of 25 km	0.942	1.000			
Regions of 50 km	0.815	0.935	1.000		
Regions 10 %	0.952	0.966	0.891	1.000	
Regions 1 %	0.926	0.938	0.917	0.932	1.000

Table 4.14: Correlation of the Population Share of the Core City

Variable	Reg. 15 km	Reg. 25 km	Reg. 50 km	Reg. 10 %	Reg. 1 %
Regions of 15 km	1.000				
Regions of 25 km	0.899	1.000			
Regions of 50 km	0.718	0.864	1.000		
Regions 10 %	0.508	0.370	0.286	1.000	
Regions 1 %	0.524	0.364	0.244	0.853	1.000

Table 4.15: Correlation of the Population of the Region

Variable	Reg. 15 km	Reg. 25 km	Reg. 50 km	Reg. 10 %	Reg. 1 %
Regions of 15 km	1.000				
Regions of 25 km	0.910	1.000			
Regions of 50 km	0.726	0.900	1.000		
Regions 10 %	0.871	0.844	0.653	1.000	
Regions 1 %	0.764	0.800	0.664	0.861	1.000

Table 4.16: Local Business Taxation in Metropolitan Areas of 50 km (extended)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Constant	415 ***	403 ***	407 ***	208 ***	218 ***	216 ***	313 ***	282 ***	313 ***
	(3.42)	(13.4)	(15.2)	(52.2)	(44.8)	(65.6)	(56.3)	(67.0)	(56.6)
log(Pop. per Surround. Mun.)						1.71	19.6 ***		
						(8.34)	(7.41)		
log(No. of Surround. Munic.)								-15.1 *	-19.4 ***
								(8.73)	(7.39)
Pop. Share of Core City					194 ***		225 ***		201 ***
					(32.0)		(33.3)		(31.2)
log(Pop. Region)				25.9 ***	22.0 ***	24.5 ***	5.99	28.7 ***	25.4 ***
				(6.54)	(5.66)	(9.27)	(8.16)	(6.68)	(5.65)
Distance to next City			-.092	.376 *	.030	.374 *	-.059	.318	-.059
			(.160)	(.191)	(.173)	(.192)	(.172)	(.192)	(.172)
State Dummies	–	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.000	0.546	0.547	0.608	0.714	0.609	0.733	0.620	0.733
adj. R^2	0.000	0.479	0.475	0.542	0.662	0.537	0.681	0.551	0.681
Observations	118	118	118	118	118	118	118	118	118

Dependent variable: Rate of assessment of the core city of an agglomeration. Standard errors in parentheses. A single star denotes significance at the 10 % level, two stars at the 5 % level, and three stars at the 1 % level.

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Chapter 5

Consolidation of Municipalities and Impact on Population Growth – A Propensity Score Matching Approach

Abstract

During the 1960s and 1970s, the number of municipalities in Germany was notably reduced. Many municipalities located on the outskirts of a city lost their independent status and became a district of the adjacent core city. This chapter analyzes the consequences of such a reform on the population development in these city districts. In comparing incorporated municipalities with those that remained independent, the former are found to perform better in terms of population growth. This effect is confirmed when differences in states' population growth rates are taken into account, and becomes stronger for municipalities that were incorporated later and for smaller municipalities. To avoid selection bias by possibly comparing two groups with different properties, a propensity score matching approach is employed. This allows us to compare incorporated with still independent communities that had a similar propensity to be incorporated and, hence, similar characteristics. We find that the propensity score is basically driven by the distance of the community to the core city, the size in terms of population and area, and the state to which it belongs.

5.1 Introduction

The spatial structure of administrative units is an ongoing political issue. Although consolidations or divisions at the state or even country level are rare, local-level reforms are a common occurrence. These reforms, which typically reduce the number of municipalities, are often controversial in those municipalities that are going to lose their independent status by being merged into a larger municipality. Since identification with one's home town is often very strong, broad resistance is not unexpected. However, changing settlement patterns in the course of (sub-)urbanization and changing demands for public services necessitate adjustments of administrative structures.[1]

[1] As in Chapter 4, we use the short version *administrative structure* instead of *spatial administrative structure*.

The literature discusses several aspects of designing and/or reforming administrative structures, at both the regional and country level. Alesina et al. (1995) provide an overview of articles that focus on the normative perspective. Among the core arguments in favor of larger jurisdictions are economies of scale in the production of public goods and services, the internalization of externalities, and possible insurance against revenue shocks. Arguments in opposition highlight the advantages of decentralization as including an increase in political influence for each citizen and the possibility of serving different tastes in the provision of public goods.[2]

Another strand of literature is concerned with the positive perspective, and analyzes the determinants of (voluntary) mergers or secessions at the municipality level. Brink (2004) provides evidence that within-municipality imbalances of wealth are relevant in the occurrence of split-ups in Sweden. Sørensen (2006) looks at consolidations and lists several hindering factors. These include that the common grant for merged municipalities might be lower than the sum of individual grants to unmerged communities; that high-revenue municipalities will have to share their revenues when consolidated with a poorer neighbor; and that the composition of political preferences may affect the policies implemented. By employing Norwegian data, he finds consolidations to be more likely when efficiency gains are expected to be large and differences in revenues and political preferences are small. Filer and Kenny (1980) find evidence that the income distribution between city and county explains approval in consolidation referenda in the United States, but that the relevance of economies of scale cannot be confirmed. Austin (1999) also considers incorporations in the United States, some of which could have realized on the city's own right. He finds "unfavorable" migration between city and surrounding area to be an important factor in explaining incorporations because by expanding city boundaries, the composition of voters can be manipulated to politicians' advantage. Exploiting tax bases by incorporation, on

[2]Theoretical analyses focusing on the trade-off between efficiency and heterogeneous populations or political influence are provided by Alesina and Spolaore (1997) and Bolton and Roland (1997).

the contrary, shows not to be relevant.

The consequences of incorporation are also examined in the literature. Blume (2009) analyzes the effects of the German reforms implemented in the 1960s and 1970s on the performance of both regions and cities. He finds that counties with an incorporated core have less debt than regions with an independent core city. This is explained as due to politico-economic incentives: municipalities' politicians force counties to use efficiency gains from the incorporation of the core city to reduce debt, with the consequence that transfers paid by municipalities to the county can be lowered. Furthermore, cities are shown to grow faster (in terms of gross value added) the higher their intensity of incorporation. The explanation here focuses on politicians' interest in being reelected. Since voters are assumed to appreciate public expenditures more than they do debt reduction, efficiency gains from consolidations are used for further public investment, which results in higher growth rates.

These German reforms of the 1960s and 1970s are also the basis of this chapter. However, instead of looking at the effects of consolidation on the core city that is expanded the focus here is on the municipalities that were incorporated. Specifically, the interest is in changes in population as a measure of the increased or decreased attractiveness of a community. A community can be more or less attractive to citizens for several reasons, some of which involve, as mentioned above, differences in wealth or political influence. The big reforms that reduced the number of municipalities in Germany from roughly 24,000 to 8,500 occurred between 1967 and 1978. Hence, a sufficiently long time has passed to evaluate the effects on a rather persistent factor like the population. Municipalities that remained independent are compared with those that became a city district. Population statistics from the pre-reform period of the early 1960s are employed, as well as current population numbers at the municipality and city-district level.

We employ a propensity score matching approach that links incorporated to still independent municipalities on the basis of similar probabilities of incorporation. It is found that these probabilities are basically driven by location, population, area, and state affiliation. Subsequently, the estimated effect of incorporation is calculated and shows that municipalities that became a city district have grown faster in terms of population. This effect is stronger for later incorporated and for small municipalities.

The chapter is organized as follows. Section 5.2 discusses the reforms and the current situation in Germany. Section 5.3 develops the empirical strategy and provides information on the data. Section 5.4 contains the results; Section 5.5 concludes.

5.2 Institutional Background

The number of municipalities in Germany is constantly changing or, to be more precise, constantly declining. In 1900, Germany had approximately 77,000 municipalities; by 2008, there were only 12,000. Of course, the loss of territory associated with the world wars explains a significant part of the decline. However, even within the country's current territory, the number of municipalities in 1900 was about 45,000, remarkably more than exist today. In the mid-1960s, the number (in West Germany) was around 24,000, with about 95 % of the municipalities having less than 5,000 inhabitants.

Between 1967 and 1978, large reforms in the administrative structure of municipalities were implemented, in the course of which the number of municipalities was reduced to 8,500.[3] Initially, even more reductions were planned; however, in some cases municipalities successfully resisted merger, both before and after it occurred.

[3] The current number of about 12,000 municipalities is explained solely by the 1990 reunification of Germany.

Table 5.1: Reforms of the Administrative Structure

State	No. Municipal. Before Reforms	No. Municipal. After Reforms	Reduction in %	No. Munic. in 2008
Saarland	347	50	85.59	52
Hesse	2684	427	84.09	426
North Rhine-Westphalia	2277	396	82.61	396
Lower Saxony	4231	1029	75.68	1024
Bavaria	7077	2048	71.06	2056
Baden-Württemberg	3379	1111	67.12	1102
Rhineland-Palatinate	2905	2303	20.72	2306
Schleswig-Holstein	1378	1132	17.85	1119
Berlin	1	1	0.00	1
Hamburg	1	1	0.00	1
Bremen	2	2	0.00	2
Total	24282	8500	64.99	8485

Number of municipalities before and after the reforms according to the German Federal Statistical Office, quoted according to Püttner (1983).

According to Wagener (1969), the aims of the reforms can be grouped into five categories: promoting equal conditions of life in both urban and rural areas, strengthening the efficiency of municipalities in rural areas, ensuring a well-arranged development and function of urban areas, restructuring areas with respect to aspects of spatial planning and infrastructure, and improving general-interest services. Accordingly, the reforms were characterized by a change from historically established units to ones focused on efficient public structures.

The mergers were realized both by voluntary agreements of municipalities and via enforcement. In the state of Baden-Württemberg, for instance, the number of municipalities was reduced from 3,379 to 1,111, with the smallest municipalities inhabited by at least

8,000 people. Municipalities that merged voluntarily were granted funds. The reforms in Bavaria were of similar intensity and reduced the number of municipalities in that state from 7,077 to 2,048. As can be seen in Table 5.1, the reforms in Saarland, Hesse, North Rhine-Westphalia, and Lower Saxony were even more intense. In all these states, the number of municipalities declined by more than 75 %. The intensity of the reforms was lower in Rhineland-Palatinate and Schleswig-Holstein, with reductions of only about 20 %. In the federal city states of Berlin, Hamburg, and Bremen, no changes have been realized. After these reforms, there have been just a few further adjustments, as the last column shows. The number of municipalities has remained almost constant over the past 30 years.

5.3 Identification Strategy and Data

The aim of this chapter is to discover whether the incorporation of a municipality into a core city is a causal factor in either faster or slower growth in the incorporated unit in terms of population. For this purpose, municipalities that were consolidated with a core city are compared with ones that remained independent. This is done by estimating the counterfactual outcome, which tells how fast an incorporated municipality would have grown if it had not been incorporated (or vice versa). Comparing these counterfactuals with the actual growth rates gives the effect of incorporation.

The sample includes all cities that were county-independent in 1961 and remained so until 2008 ("Kreisfreie Städte"), restricted to the territory of former West Germany. Two groups of (surrounding) communities of these core cities are looked at. The first group comprises municipalities that were incorporated into a city in the course of the reforms (the treatment group). For these, the population figures from when they were still independent in 1961 are compared with their population as a city district in 2008. Since appropriate data are not available for all cities, particularly when a city district is not identical to the former municipality, this group contains 124 observations. The second group comprises those municipalities in the surrounding area of a core city that have remained independent until the present day (the control group). The surrounding area is restricted to a circle of 12.5 km around the center of the core city.[4] Furthermore, only those (in 1961 *and* 2008 county-*dependent*) surrounding municipalities are considered that have for their part experienced no mergers or break-ups. Based on these restrictions, 447 observations remain.

To compare the treatment and the control group without further adjustments is not a valid identification strategy. Employing standard OLS techniques would likely entail a selection bias. The municipalities that lost their independent status were certainly not selected randomly; rather, certain characteristics are likely to have influenced the probability of being incorporated. However, when, for instance, small surrounding municipalities were more likely to be incorporated, higher population growth might have occurred not because of the incorporation itself, but because of an increased preference of potential citizens for smaller communities. The same argument holds for other factors, like the distance from the community to the center of the core city.

Since such factors notably affect the probability of being merged with the respective core city, the likely occurrence of a selection bias has to be bypassed. A strategy has to be developed that allows comparing treated and untreated municipalities that have similar (pre-treatment) characteristics. Optimally, municipalities should differ only with respect to whether or not they have been treated, all other characteristics being equal. Since it is hardly possible to match on every covariate, however, such characteristics should be condensed into one number. This is done by the propensity score matching approach suggested by Rosenbaum and Rubin (1983). According to this approach, a probability of incorporation is calculated in a first step. In doing so, factors likely to affect the probability

[4] This distance equals the longest distance of an incorporated municipality in the data set.

are used in a probit regression as covariates X, with a binary variable (treated: $T = 1$, untreated: $T = 0$) as the dependent variable:[5]

$$Prob(T = 1|X) = \phi(X'\beta).$$

After having found such propensity scores for both members of the treatment group and the control group, and if the scores of both groups overlap (common support), actual matching of appropriate partners is possible. This procedure is intended to make the groups comparable by possibly omitting such observations that are causing the groups to be different. Four different versions of propensity score matching are employed in the following.

One-to-one matching is the simplest way to match treated with untreated observations. In this version, every treated municipality is matched with the untreated neighbor (in the probability space) that has the closest propensity score. Several treated municipalities can be matched with the same untreated one (with replacement). To avoid imprecise matches, a caliper is introduced. This restricts matches to those having a sufficiently small difference in propensity scores between treated and untreated case.

In a more general approach, one can match each treated case with an unweighted average of k nearest neighbors, which is called *k-nearest-neighbors matching*. We run this method with 10 neighbors. Since a caliper is also used here, matching will be conducted with less than 10 neighbors when there is not a sufficient number of neighbors available within the caliper. A higher number of neighbors to compare with provides the advantage of exploiting more information and being less subject to outliers (smaller variance). However, this is at the expense of a less precise matching (larger bias). The one-to-one matching is a special case of k-nearest-neighbors matching; that is, it uses only one neighbor.

[5] Alternatively, a logit model could be used.

A different approach based on a similar idea is *radius matching*. Here, the treated observation is matched to an unweighted average of those untreated observations with a propensity score within a certain radius, e.g., all untreated cases that have a propensity score up to 1.5 percentage points higher or lower than the treated case. This is not the same as k-nearest-neighbors matching with a caliper. Radius matching captures all neighbors within a certain radius, no matter how many there are. K-nearest-neighbors matching with a caliper, in contrast, captures a certain number of neighbors, no matter how many there are available within the caliper. Only if k is greater than the number of observations within the caliper, and hence all neighbors within the caliper are captured, radius and k-nearest-neighbors matching can produce identical matches (given $radius = caliper$).

As a fourth and final method, *kernel matching* is considered. This method resembles radius matching, but it compares the treated case with a *weighted* average of untreated cases given a certain bandwidth. The weights depend on the difference in propensity score. The farther away the matching partner in terms of propensity scores, the smaller the weight it is given. Accordingly, this approach puts more weight on near neighbors than on those farther away in the probability space. When kernel matching is employed with a uniform kernel function, it is the same as radius matching. However, we use the common epanechnikov kernel with decreasing weights.

In all matching approaches, calipers – or a bandwidth in the case of kernel matching – of 1.5 % are used. This means that the propensities of the treated and the matched untreated case(s) are not allowed to differ by more than 1.5 percentage points, which is intended to avoid imprecise matches. This is done at the expense of a smaller number of observations exploited (larger variance), but does ensure against comparing treated and untreated municipalities that are too different from one another in terms of propensity score (smaller bias). Accordingly, setting up calipers circumvents the investigation of the overlap and common support issue.[6]

Given the matching of observations and the resulting appraisals for counterfactual outcomes, the average treatment effect on the treated (ATT) can be calculated:[7]

$$ATT = \frac{1}{n}\left(\sum_{i=1}^{n}\frac{Pop\_2008_i - Pop\_1961_i}{Pop\_1961_i} - \sum_{j=1}^{m}\alpha_j\frac{Pop\_2008_j - Pop\_1961_j}{Pop\_1961_j}\right).$$

This entails nothing more than calculating the extent to which population growth of comparable incorporated (i) and still independent municipalities (j) differs. The significance of this treatment effect can be tested with the usual t-statistics.

Finally, to ensure the validity of the results, it must be proven that the treatment and the control group have become sufficiently similar. Gadd et al. (2008), for instance, ensure that this "balancing property" holds by looking at every variable used to explain the propensity score. Only if it is certain that the differences between the average value of each variable in the treatment and the control group are insignificant, can it be concluded that the groups are comparable.[8]

The data set contains information on the population from both the pre-reform period and latest figures available currently. The first is taken from the population census 1961; the

[6] Without calipers, one would have to use another method of ensuring that only those observations in the control group that are similar to the treated observations are used.

[7] The number of (exploited) observations in the treatment group is denoted n, the number of (exploited) observations in the control group is denoted m, and α_j indicates the different weights for the untreated observations that can follow from all matching methods.

[8] Another issue regarding the validity of the results concerns unconfoundedness. According to this and with T as treatment, Y_0 and Y_1 as potential outcomes, X as pre-treatment characteristics, and $p(X)$ as propensity score, assignment to the treatment has to fulfill $T \perp Y_0, Y_1|X$, which can be shown to imply $T \perp Y_0, Y_1|p(X)$. This requires that "systematic differences in outcomes between treated and comparison individuals [municipalities] with the same values for covariates are attributable to treatment" (Caliendo and Kopeinig (2008)). However, since unconfoundedness cannot be tested directly, we only assume the condition to hold. For further details, see Rosenbaum and Rubin (1983).

recent data are derived from the local statistics of the German Federal Statistical Office from 2008 and from publications of the respective cities from about the same time.

The variables used are basically needed to explain the incorporation probabilities of municipalities. Since these are driven by conditions prevailing in the period prior to the reforms, only variables from the pre-reform period or time-invariant variables come into play.[9] As to covariates that might be relevant, location of the respective municipality is an obvious choice. It is reasonable to assume that the farther away from the center of a core city a municipality is located, the more likely it is to remain independent. Using the geographic coordinates of each municipality in both the treatment and the control group provides the distance of each municipality to the center of the respective core city.[10]

Another potentially important factor is the size of the surrounding municipality, both in terms of population and area. It is reasonable to expect that a surrounding municipality is more likely to lose its independent status the smaller it is. Also the size of the core city might matter. Large cities could incorporate more or less intensively than smaller ones. For this reason, the population and the area of the core city are included as well.

Numerous other statistics are available at the county level only. However, attributing the county figure to a municipality can provide some relevant information. For example, the 1966 per-capita debt level for both the core city and the (county of the) surrounding municipality is of interest, since the public finances situation should influence whether a municipality can remain independent.[11] Furthermore, political variables are included.

[9] In a narrow sense, variables not only must originate in the pre-reform period, but also must be unaffected by a possible anticipation of the reforms.

[10] In the case of overlapping surrounding areas, a non-incorporated municipality is related to the nearest core city. Official central points are used as coordinates of core cities, while the coordinates of surrounding municipalities are based on centroid considerations.

[11] Debt at the county level comprises both the debt of the county itself and of the respective municipalities.

Table 5.2: Descriptive Statistics

Variable	Obs.	Mean	Standard Dev.	Min	Max
Population Growth Rate	571	.747	.904	-.363	10.0
Pop. Growth Rate, State-Corr.	571	.508	.886	-.560	9.68
Absolute Population Growth	571	1470	2963	-1236	43100
Treatment	571	.217	.413	0	1
Year of Incorporation	124	1972	2.17	1969	1978
Distance to Core City	571	8.42	2.59	1.76	12.5
Population 1961 Surr. Munic.	571	2254	3881	91	50906
Population 1961 Core City	571	119080	162310	28725	1832346
Area 1961 Surr. Munic.	571	10.5	9.53	.970	119
Area 1961 Core City	571	62.0	54.7	9.65	747
Debt 1966 Surr. Munic. (p.c.)	571	197	65.0	101	441
Debt 1966 Core City (p.c.)	571	518	220	115	1271
Political Diff. Surr. Munic.	571	5.45	17.5	-51.8	49.1
Political Diff. Core City	571	-10.0	10.5	-65.0	20.7
Religious Difference 1961	571	8.69	23.3	-103	79.4
State Capital	571	.102	.302	0	1

The number of core cities in the data set amounts to 67. Distance in kilometers, area in square kilometers, debt in Euro.

One of them measures the percentage difference between the conservative (CDU/CSU) and the social-democratic party (SPD) in the county elections of the mid-1960s. The other variable describes the same for the city council elections in the core cities.[12] Finally, a variable measuring the 1961 percentage difference between Catholics and Protestants in the (county of the) surrounding municipality net of this difference in the respective core city is included to capture differences in the religious attitude between surrounding area and core.

[12] The elections were held in Lower Saxony, North Rhine-Westphalia, Hesse, Rhineland-Palatinate, and Saarland in 1964, in Baden-Württemberg in 1965, and in Schleswig-Holstein and Bavaria in 1966.

The state the municipality belongs to may also have an impact on the probability of a merger. Since some states have reduced the number of municipalities to a greater extent than have others (see Section 5.2), state dummies are added as additional explanatory variables. Moreover, a dummy variable indicating whether the respective core city is a state capital is included.

Descriptive statistics on all relevant variables are provided in Table 5.2.

5.4 Empirical Results

As set out above the identification strategy involves four steps: (1) explaining the probability of incorporation, (2) matching observations, (3) calculating the treatment effect, and (4) ensuring validity by checking the balancing property.

We start by running a probit model to explain the probability of incorporation (propensity score). This yields the results shown in the left part of Table 5.3.[13] As the table reveals, the distance of the municipality from the center of the core city is the most significant factor in creating the propensity score. The farther from the core city's center a municipality is, the less likely it is to be incorporated. The size of the municipality is important also. The bigger the municipality's population, the less likely it is to be incorporated. The opposite holds for an increase in area, however. A larger area is associated with a significantly higher propensity score. These two effects together mean that being incorporated is more likely the lower the population density. This is in line with the argument that incorporation is done to strengthen the efficiency of administrations, since municipalities with low population density are hardly able to exploit economies of scale in the provision of public

[13]The results of an identical logit model are shown in the right part of the table for comparison. However, in the following, we restrict the analysis to the probit results.

goods and services. The size of the core city shows opposite (and insignificant) effects. A higher population and a smaller area increase the probability of incorporating surrounding municipalities. This can be explained by the fact that such cities need space for further population development.

Among the county-level variables, a higher per-capita debt level of the (county of a) surrounding municipality increases its probability of incorporation. Moreover, a core city is more likely to be enlarged when its debt level is higher. Both effects, however, are insignificant. A conservative municipality is more likely to remain independent than a social-democratic one. This effect, which may result from conservative communities holding more traditional attitudes, is almost significant. Furthermore, a conservative core city will incorporate more intensively, but this effect is not significant: surrounding municipalities that are considered to be richer than the core city, and consequently more conservative politically, may be a motivation for conservative politicians to enhance their electoral base by an act of incorporation. Concerning religiosity, a significant effect can be found that shows incorporation is more likely the more Catholic the surrounding municipality and the less Catholic the core city.

Looking at the state dummies provides clues as to the lower intensity of consolidations in Schleswig-Holstein and Rhineland-Palatinate, as mentioned in Section 5.2. On the contrary, Lower Saxony shows a higher intensity of reforms. Regarding state capitals, an (insignificant) effect of a higher propensity of incorporations is found: when cities are considered to be willing to be enhanced, this could result from a bias of state politicians towards the city in which their parliament is located. All the variables contribute to a pseudo R^2 of about 45 %.[14]

[14]The results of the analysis below are very similar when all variables with z-values below 1 are excluded from the probit regression. For a discussion regarding the specification of propensity score models, see Caliendo and Kopeinig (2008).

Table 5.3: Treatment Probability

	Probit			Logit		
Variable	Coeff.	Std.E.	Sign.	Coeff.	Std.E.	Sign.
Constant	1.85	3.73		3.21	7.24	
log(Distance to Core City)	-2.78	.314	***	-5.05	.632	***
log(Pop. 1961 Surr. Munic.)	-.530	.159	***	-.980	.281	***
log(Pop. 1961 Core City)	.489	.365		.911	.701	
log(Area 1961 Surr. Munic.)	.298	.161	***	.577	.273	**
log(Area 1961 Core City)	-.130	.371		-.177	.708	
log(Debt 1966 Surr. Munic. (p.c.))	.256	.393		.447	.703	
log(Debt 1966 Core City (p.c.))	.325	.301		.543	.567	
Political Diff. Surr. Munic.	-.016	.010		-.027	.019	
Political Diff. Core City	.010	.013		.019	.024	
Religious Difference 1961	.014	.008	*	.027	.014	*
Dummy Schleswig-Holstein	-3.10	.642	***	-5.60	1.15	***
Dummy Lower Saxony	-.460	.513		-.844	.878	
Dummy Hesse	-2.33	.687	***	-4.02	1.17	***
Dummy Rhineland-Palatinate	-2.89	.657	***	-5.14	1.23	***
Dummy Baden-Württemberg	-1.86	.553	***	-3.33	.962	***
Dummy Bavaria	-2.84	.577	***	-5.06	1.02	***
Dummy State Capital	.351	.353		.793	.671	
Pseudo R^2		0.454			0.454	
Log pseudolikelihood		-163			-163	
Observations		571			571	

Dependent variable: Dummy variable indicating merger (1 = treated, 0 = untreated). Standard errors clustered on the level of the core city. A single star denotes significance at the 10 % level, two stars at the 5 % level, and three stars at the 1 % level. Distance in kilometers, area in square kilometers, debt in Euro. The states of North Rhine-Westphalia and Saarland are missing due to collinearity or a lack of observations, just as the federal city states.

After using the estimated coefficients of the probit model to predict the propensity scores, we proceed to the matching procedure and the resulting treatment effects. The left part of Table 5.4 shows the results for the four matching methods outlined in the preceding section. All four methods estimate a large positive and significant effect of incorporation on population growth. Accordingly, a municipality that became a city district grows by about 29 percentage points more than it would have if it had not been merged with the core city. Municipalities that remained independent grew on average by only about 77 %; those that were incorporated grew by about 106 % (not shown in the table). The significance of the effects is not affected when bootstrapped standard errors, which attempt to take into account that the propensity score is estimated, are considered.[15]

The treatment effects just discussed are based on comparisons of incorporated and still independent municipalities that may be located in different states. Since the population growth rates among the states differed notably in the period considered – ranging from -3.9 % in Saarland to +38.5 % in Baden-Württemberg – it would seem relevant to take this fact into account.[16] The treatment effect can be adjusted for the different state growth rates by subtracting this difference in the calculation of the figures of every single match. Hence, it is no longer the population growth rate of a municipality itself that is considered, but the extent to which the population growth rate exceeds that of the state in which the municipality is located. The results are displayed in the right part of Table 5.4. As can be seen, the treatment effect still amounts to about 29 percentage points and is again significant. Municipalities that have been incorporated grew 80 percentage points more than their respective state; those that remained independent grew only about 51 percentage points more.

[15]Although widely used in the literature, it is unclear whether bootstrapped standard errors are valid in the context of propensity score matching (see Abadie and Imbens (2006)).

[16]Indeed, state differences have already been taken into account in the estimation of propensity scores, but now states' differences in population growth, rather than in the intensity of incorporation, are concerned.

Table 5.4: Average Treatment Effect on the Treated

	Population Growth Rate	
		State-Corrected
Matching Method	Coeff. (Std.E.)	Coeff. (Std.E.)
One-to-one *(Caliper: 1.5 %)*		
Average Treatment Effect on the Treated	.296 (.169)⋆	.292 (.167)⋆
(Bootstrapped Standard Errors)	(.148)⋆⋆	(.144)⋆⋆
10-Nearest-Neighbors *(Caliper: 1.5 %)*		
Average Treatment Effect on the Treated	.294 (.152)⋆	.297 (.149)⋆⋆
(Bootstrapped Standard Errors)	(.144)⋆⋆	(.143)⋆⋆
Radius *(Caliper: 1.5 %)*		
Average Treatment Effect on the Treated	.282 (.153)⋆	.288 (.150)⋆
(Bootstrapped Standard Errors)	(.149)⋆	(.146)⋆⋆
Kernel *(Epanechnikov, Bandwidth: 1.5 %)*		
Average Treatment Effect on the Treated	.291 (.155)⋆	.294 (.152)⋆
(Bootstrapped Standard Errors)	(.159)⋆	(.157)⋆

Number of observations: 133 to 524 (treated: 80, untreated: 53 to 444). Outcome variable: Difference of average population growth rates between incorporated and still independent municipalities. The right column is based on growth rates that take differences in state growth rates into account. A single star denotes significance at the 10 % level, two stars at the 5 % level, and three stars at the 1 % level.

As discussed in Section 5.2, some of the mergers aimed at reducing the number of municipalities in Germany were voluntary, but others were not. This raises the question of whether the two ways of merging gave rise to different consequences. However, the distinction between voluntary and involuntary mergers is not clear-cut in many cases. Some mergers may have been passed off as though they were voluntary, even though they were the result of negotiations involving concessions offered to the municipality as an inducement to incorporation. To overcome this problem of different (unobservable) degrees of

Table 5.5: Average Treatment Effect on the Treated (Different Time Periods)

Matching Method	Population Growth Rate *Incorp. '69-'72* Coeff. (Std.E.)	 *Incorp. '73-'78* Coeff. (Std.E.)	*Difference* Coeff. (Std.E.)
One-to-one *(Caliper: 1.5 %)*			
Avg. Treatment Effect on the Treated	.259 (.151)⋆	.363 (.335)	-.103 (.070)
(Bootstrapped Standard Errors)	(.142)⋆	(.363)	(.075)
10-Nearest-Neighbors *(Caliper: 1.5 %)*			
Avg. Treatment Effect on the Treated	.193 (.153)	.527 (.269)⋆	-.335 (.058)⋆⋆⋆
(Bootstrapped Standard Errors)	(.149)	(.270)⋆	(.058)⋆⋆⋆
Radius *(Caliper: 1.5 %)*			
Avg. Treatment Effect on the Treated	.189 (.144)	.505 (.294)⋆	-.315 (.062)⋆⋆⋆
(Bootstrapped Standard Errors)	(.122)	(.257)⋆⋆	(.054)⋆⋆⋆
Kernel *(Epanechnikov, Bandwidth: 1.5 %)*			
Avg. Treatment Effect on the Treated	.193 (.145)	.515 (.297)⋆	-.322 (.062)⋆⋆⋆
(Bootstrapped Standard Errors)	(.134)	(.308)⋆	(.064)⋆⋆⋆

Number of observations: 99 to 475 / 45 to 215 (treated: 55 / 25, untreated: 44 to 420 / 20 to 190). Outcome variable: Difference of average population growth rates between incorporated and still independent municipalities. Growth rates corrected for state differences (see above). A single star denotes significance at the 10 % level, two stars at the 5 % level, and three stars at the 1 % level.

voluntariness, we separate the data set into two subsamples. The first one comprises the incorporations from 1969 to 1972, the second one those from 1973 to 1978.[17] The idea behind this is that mergers realized earlier should feature a higher degree of voluntariness. However, this voluntariness in turn might be based on certain characteristics that could result in different population growth effects.

[17]In fact, the reforms started in 1967, but there are no incorporations earlier than 1969 in our data set. Descriptive statistics on the two data sets are provided in the appendix.

Table 5.5 shows the results for earlier and later incorporations. Obviously, early incorporations result in weaker population growth effects than do later ones. The first group shows a mostly insignificant effect of only 19 to 26 percentage points; the second group shows a mostly significant one of 36 to 53 percentage points.[18] The resulting difference of about 32 percentage points is significant; only in the case of one-to-one matching is the difference smaller and insignificant.[19] The result is not too surprising, since municipalities that were incorporated earlier, and hence on more of a voluntary basis, can be expected to have been in a weaker position. In contrast, municipalities that were incorporated later, and hence less voluntarily, should have done better before. The result of a smaller treatment effect for early incorporated municipalities is thus merely a product of both their poorer initial position and the fact that we always compare with the whole pool of remained-independent municipalities, and not with only those that were in a poor or strong position before the reforms. Accordingly, the effect of the early integrated is underestimated, and that of the later integrated overestimated. If, however, we could compare with initially poor or strong remained-independent municipalities, we would possibly find similar treatment effects for both groups. Even though the different magnitudes of the treatment effects shown do not necessarily imply different effects for the two groups, they do hint at an earlier incorporation of weak municipalities.[20]

Another issue concerns incorporations of municipalities of different size. Since large mu-

[18] The effect of 19 to 26 (36 to 53) percentage points is a result of a growth rate that exceeds the respective state growth rate by 70 (102) percentage points in the incorporated, and 44 to 51 (49 to 65) percentage points in the independent municipalities.

[19] The standard error of the difference follows from $se(x-y) = \sqrt{\frac{var(x)}{n_x} + \frac{var(y)}{n_y}}$, with n indicating the respective number of observations, and is based on the assumption of two independent samples. See Kendall (1952), p. 226.

[20] If both weak and strong municipalities had been incorporated to the same extents at all points in time, one would expect similar effects of early and late incorporation, or even a stronger effect for early ones, because they have profited for a longer time from the positive effects of incorporation. That the early incorporations show a weaker effect can thus only be explained by the fact that these municipalities have always been weaker.

nicipalities might be more efficient (economies of scale) than small ones in the first place, they could possibly benefit less from incorporation. Thus, their treatment effect would be smaller. To discover if this is indeed the case, we again separate the data set. The first subsample now contains municipalities with less than or exactly 1,500 inhabitants in 1961, and the second one those with more than 1,500 inhabitants.[21] Table 5.6 presents the results. The treatment effect is much larger for small municipalities than for large ones. The treatment significantly increases the population growth rate by 41 to 60 percentage points in small municipalities; the effect amounts only to insignificant 6 to 12 percentage points in the group of large municipalities.[22] The difference of about 35 to 48 percentage points is highly significant across all four matching methods. Accordingly, only small municipalities gain noticeably from incorporation in terms of population growth, while municipalities that were already relatively large before their incorporation benefit to a lesser extent.[23]

The validity of all these results remains to be confirmed by looking at the balancing property. Table 5.7 displays the results for the case of radius matching with state-corrected growth rates.[24] It shows that radius matching notably reduced the bias between the treated and the control group for most variables. More important, the last two columns demonstrate that all variables show a very insignificant difference between treated and untreated municipalities. In most cases, the t-value is far less than one; only for the Bavaria dummy it is somewhat larger. Thus, it can be concluded that the balancing property is fulfilled and the differences between the treatment and control group have been reduced sufficiently

[21]Descriptive statistics on the two data sets are provided in the appendix.

[22]The effect of 41 to 60 (6 to 12) percentage points is a result of a growth rate that exceeds the respective state growth rate by 115 (29) percentage points in the incorporated, and 55 to 74 (17 to 23) percentage points in the independent municipalities.

[23]Note that Tables 5.5 and 5.6 do not show mirror-image results. On the one hand, the early incorporated municipalities are even smaller than the late ones and, on the other hand, the timing of incorporations of small and large municipalities is very similar (see the descriptive statistics in the appendix).

[24]The tables for one-to-one, 10-nearest-neighbors, and kernel matching are very similar and can be found in the appendix. The tables for the cases without state correction, different time periods, and different sizes of municipalities are also comparable.

Table 5.6: Average Treatment Effect on the Treated (Different Sizes)

Matching Method	Population Growth Rate *Pop. <= 1500* Coeff. (Std.E.)	*Pop. > 1500* Coeff. (Std.E.)	*Difference* Coeff. (Std.E.)
One-to-one *(Caliper: 1.5 %)*			
Avg. Treatment Effect on the Treated	.599 (.259)**	.123 (.123)	.475 (.047)***
(Bootstrapped Standard Errors)	(.245)**	(.117)	(.044)***
10-Nearest-Neighbors *(Caliper: 1.5 %)*			
Avg. Treatment Effect on the Treated	.443 (.232)*	.074 (.131)	.369 (.044)***
(Bootstrapped Standard Errors)	(.273)	(.106)	(.047)***
Radius *(Caliper: 1.5 %)*			
Avg. Treatment Effect on the Treated	.413 (.221)*	.060 (.141)	.353 (.043)***
(Bootstrapped Standard Errors)	(.239)*	(.118)	(.043)***
Kernel *(Epanechnikov, Bandwidth: 1.5 %)*			
Avg. Treatment Effect on the Treated	.460 (.223)**	.072 (.142)	.388 (.044)***
(Bootstrapped Standard Errors)	(.231)**	(.125)	(.043)***

Number of observations: 69 to 254 / 53 to 172 (treated: 40 / 30, untreated: 29 to 214 / 23 to 142). Outcome variable: Difference of average population growth rates between incorporated and still independent municipalities. Growth rates corrected for state differences (see above). A single star denotes significance at the 10 % level, two stars at the 5 % level, and three stars at the 1 % level.

by using propensity score matching. Due to this convincing circumvention of the selection-bias issue, the average treatment effects on the treated as displayed in Tables 5.4 to 5.6 are based on solid ground.

What explains the higher population growth rates in incorporated municipalities compared to those that remained independent? One reason could have to do with hard location factors, like improvements in the provision of public goods and infrastructure. The city

Table 5.7: Balancing Property (Radius Matching)

Variable	Mean Treated	Mean Control	% Bias	% Bias Reduct.	t	$p > \|t\|$
log(Distance to Core City)	1.80	1.75	15.1	88.2	0.80	0.423
log(Pop. 1961 Surr. Munic.)	7.29	7.30	-1.5	87.5	-0.09	0.929
log(Pop. 1961 Core City)	11.4	11.3	13.4	62.8	0.92	0.358
log(Area 1961 Surr. Munic.)	2.05	1.97	10.8	57.1	0.68	0.495
log(Area 1961 Core City)	3.97	3.90	10.5	-36.0	0.67	0.506
log(Debt 1966 Surr. Munic. (p.c.))	5.27	5.25	8.4	61.7	0.58	0.562
log(Debt 1966 Core City (p.c.))	6.17	6.16	1.0	95.1	0.06	0.955
Political Diff. Surr. Munic.	6.19	6.57	-2.1	89.7	-0.13	0.894
Political Diff. Core City	-10.3	-9.81	-5.1	70.3	-0.36	0.719
Religious Difference 1961	9.00	10.0	-4.6	62.2	-0.30	0.767
Dummy Schleswig-Holstein	.075	.051	8.3	73.7	0.62	0.539
Dummy Lower Saxony	.1	.088	4.0	93.9	0.27	0.788
Dummy North Rhine-Westphalia	.038	.030	3.1	93.1	0.25	0.803
Dummy Hesse	.038	.028	6.5	-137.3	0.34	0.734
Dummy Rhineland-Palatinate	.325	.313	2.6	95.0	0.17	0.868
Dummy Baden-Württemberg	.275	.251	6.4	63.6	0.35	0.730
Dummy Bavaria	.15	.240	-25.2	-131.3	-1.43	0.154
Dummy State Capital	.125	.084	13.9	-150.8	0.85	0.397

Distance in kilometers, area in square kilometers, debt in Euro. The state of Saarland is missing due to a lack of observations.

will probably be more willing to extend a subway or streetcar network to a city district than to a municipality that is the same distance as the district from the city's core, for example. But other public goods and services may also contribute to the greater attractiveness of an incorporated unit. This is the case when the public sector becomes more efficient by exploiting economies of scale and is consequently able to increase the provision of goods and services. Furthermore, mergers of municipalities can reduce the volatility in the provision of public goods by absorbing revenue shocks due to an insurance effect. If these effects caused higher population growth rates, such would signal that the incorporation policy is a success. Then municipalities would have lost their independent statuses that have actually had potentials for increasing efficiency.

The location of housing programs is also potentially relevant. In the 1960s and 1970s, commuter towns were built within city borders to relieve pressure on the city's core. Typically, these were constructed in the outlying city districts, which may very well have included some newly incorporated districts. Since these commuter towns were built in previously sparsely populated areas, a high population growth rate could have resulted.

5.5 Conclusions

This chapter analyzes the impact of incorporations of city's surrounding municipalities on population growth in these units. To this end, the large reforms of the spatial administrative structure in Germany during the 1960s and 1970s are considered. To isolate the effects on population growth, municipalities in the surrounding area of cities that remained independent are compared with those that have merged with the adjacent core city.

To reduce a possible selection bias, a propensity score matching approach is used. We run a probit regression that explains the probability of being incorporated. The distance of the municipality to the center of the core city, its population, the geographic size of the municipality, and state dummies are significant covariates that explain a notable part of the variation. Given the predicted probability of consolidation, different methods of propensity score matching are employed to match treated and untreated municipalities, with the intent of making the two groups as similar as possible. Each of the methods employed proves able to reduce the differences between treated and control municipalities to a large and sufficient extent. The average treatment effect on the treated that is subsequently estimated, shows a significantly higher population growth rate for the incorporated municipalities. This holds for all matching approaches, throughout which the effect remains constant. Adjusting the calculations for different state growth rates confirms the results. Furthermore, the effect appears to be larger for municipalities that were incorporated during the second part of the reform period, which may be causal in this group to consist of stronger municipalities. Moreover, the effect proves to be stronger for small municipalities.

The results contradict some theoretical predictions from the literature, according to which citizens are opposed to mergers for political or fiscal reasons. These reasons may not only result in no merger, but could also make the municipality less likely to attract new residents. Other arguments seem more relevant, however. Among these may be improvements in the connection between city and incorporated municipality (infrastructure) or a more efficient provision of public goods in general. Also the location of past housing programs may play a role.

It is left for future research to identify the effects of incorporation (or its absence) on different economic indicators. In particular, location decisions of businesses may be influenced by whether a possible location is situated within the borders of the core city or is rather part of an independent surrounding municipality (driven by the level of taxation, for example). Furthermore, the impact of consolidations on the political environment could be analyzed, both as to election outcomes (due to a different composition of voters) and political commitment.

Appendix

Table 5.8: Descriptive Statistics (Incorporation <= 1972)

Variable	Obs.	Mean	Standard Dev.	Min	Max
Population Growth Rate	521	.699	.874	-.339	10.0
Pop. Growth Rate, State-Corr.	521	.460	.855	-.518	9.68
Absolute Population Growth	521	1337	2450	-1236	26764
Treatment	521	.142	.349	0	1
Year of Incorporation	74	1971	1.38	1969	1972
Distance to Core City	521	8.59	2.59	1.76	12.5
Population 1961 Surr. Munic.	521	2164	3374	91	33290
Population 1961 Core City	521	111031	161740	28725	1832346
Area 1961 Surr. Munic.	521	10.5	9.32	.970	119
Area 1961 Core City	521	59.6	54.5	9.65	747
Debt 1966 Surr. Munic. (p.c.)	521	198	63.8	117	441
Debt 1966 Core City (p.c.)	521	520	219	115	1271
Political Diff. Surr. Munic.	521	6.02	17.1	-51.8	49.1
Political Diff. Core City	521	-9.74	10.4	-65.0	20.7
Religious Difference 1961	521	9.19	23.8	-103	79.4
State Capital	521	.106	.308	0	1

Table shows also municipalities that have remained independent. The number of core cities in this data set amounts to 62. Distance in kilometers, area in square kilometers, debt in Euro.

Table 5.9: Descriptive Statistics (Incorporation > 1972)

Variable	Obs.	Mean	Standard Dev.	Min	Max
Population Growth Rate	497	.713	.898	-.363	10.0
Pop. Growth Rate, State-Corr.	497	.473	.877	-.560	9.68
Absolute Population Growth	497	1465	3070	-1236	43100
Treatment	497	.101	.301	0	1
Year of Incorporation	50	1974	1.19	1973	1978
Distance to Core City	497	8.84	2.36	2.49	12.5
Population 1961 Surr. Munic.	497	2329	4112	91	50906
Population 1961 Core City	497	123744	171829	28725	1832346
Area 1961 Surr. Munic.	497	10.8	9.99	.970	119
Area 1961 Core City	497	64.1	57.4	9.65	747
Debt 1966 Surr. Munic. (p.c.)	497	198	66.9	101	441
Debt 1966 Core City (p.c.)	497	520	212	115	1271
Political Diff. Surr. Munic.	497	5.58	17.4	-51.8	49.1
Political Diff. Core City	497	-9.98	10.8	-65.0	20.7
Religious Difference 1961	497	8.71	23.3	-103	79.4
State Capital	497	.101	.301	0	1

Table shows also municipalities that have remained independent. The number of core cities in this data set amounts to 66. Distance in kilometers, area in square kilometers, debt in Euro.

Table 5.10: Descriptive Statistics (Population <= 1500)

Variable	Obs.	Mean	Standard Dev.	Min	Max
Population Growth Rate	356	.785	.925	-.363	6.36
Pop. Growth Rate, State-Corr.	356	.555	.898	-.560	6.05
Absolute Population Growth	356	658	875	-467	5193
Treatment	356	.194	.396	0	1
Year of Incorporation	69	1972	2.10	1969	1978
Distance to Core City	356	8.68	2.49	2.68	12.5
Population 1961 Surr. Munic.	356	787	378	91	1496
Population 1961 Core City	356	96027	92032	28725	1085014
Area 1961 Surr. Munic.	356	7.54	4.97	.970	34.0
Area 1961 Core City	356	54.3	37.5	9.65	310
Debt 1966 Surr. Munic. (p.c.)	356	201	68.1	109	441
Debt 1966 Core City (p.c.)	356	500	213	115	1271
Political Diff. Surr. Munic.	356	6.25	17.3	-51.8	46.0
Political Diff. Core City	356	-10.1	10.0	-65.0	20.7
Religious Difference 1961	356	7.04	23.2	-97.5	62.4
State Capital	356	.101	.302	0	1

The number of core cities in this data set amounts to 50. Distance in kilometers, area in square kilometers, debt in Euro.

Table 5.11: Descriptive Statistics (Population > 1500)

Variable	Obs.	Mean	Standard Dev.	Min	Max
Population Growth Rate	215	.685	.865	-.251	10.0
Pop. Growth Rate, State-Corr.	215	.429	.862	-.429	9.68
Absolute Population Growth	215	2816	4381	-1236	43100
Treatment	215	.256	.437	0	1
Year of Incorporation	55	1972	2.27	1969	1977
Distance to Core City	215	7.99	2.69	1.76	12.4
Population 1961 Surr. Munic.	215	4683	5512	1509	50906
Population 1961 Core City	215	157252	231886	28725	1832346
Area 1961 Surr. Munic.	215	15.4	12.7	2.33	119
Area 1961 Core City	215	74.7	73.4	12.6	747
Debt 1966 Surr. Munic. (p.c.)	215	191	59.2	101	441
Debt 1966 Core City (p.c.)	215	548	228	115	1271
Political Diff. Surr. Munic.	215	4.12	17.8	-39.2	49.1
Political Diff. Core City	215	-9.87	11.3	-32.3	18.3
Religious Difference 1961	215	11.4	23.3	-103	79.4
State Capital	215	.102	.304	0	1

The number of core cities in this data set amounts to 54. Distance in kilometers, area in square kilometers, debt in Euro.

Table 5.12: Balancing Property (One-to-one Matching)

Variable	Mean Treated	Mean Control	% Bias	% Bias Reduct.	t	$p > \|t\|$
log(Distance to Core City)	1.80	1.78	7.4	94.2	0.39	0.698
log(Pop. 1961 Surr. Munic.)	7.29	7.35	-6.8	42.2	-0.39	0.693
log(Pop. 1961 Core City)	11.4	11.4	-0.4	99.0	-0.02	0.980
log(Area 1961 Surr. Munic.)	2.05	1.99	7.8	69.0	0.46	0.644
log(Area 1961 Core City)	3.97	3.96	0.5	93.4	0.03	0.975
log(Debt 1966 Surr. Munic. (p.c.))	5.27	5.26	4.7	78.7	0.33	0.740
log(Debt 1966 Core City (p.c.))	6.17	6.19	-3.9	80.2	-0.23	0.818
Political Diff. Surr. Munic.	6.19	8.69	-13.9	33.1	-0.87	0.387
Political Diff. Core City	-10.3	-10.2	-1.5	91.4	-0.11	0.913
Religious Difference 1961	9.00	9.87	-3.8	68.6	-0.23	0.819
Dummy Schleswig-Holstein	.075	.088	-4.3	86.2	-0.29	0.774
Dummy Lower Saxony	.1	.113	-4.0	93.9	-0.25	0.799
Dummy North Rhine-Westphalia	.038	.038	0.0	100.0	-0.00	1.000
Dummy Hesse	.038	.05	-8.5	-207.9	-0.38	0.701
Dummy Rhineland-Palatinate	.325	.313	2.7	95.0	0.17	0.866
Dummy Baden-Württemberg	.275	.238	9.8	43.6	0.54	0.590
Dummy Bavaria	.15	.163	-3.5	67.7	-0.22	0.829
Dummy State Capital	.125	.138	-4.2	23.9	-0.23	0.816

Distance in kilometers, area in square kilometers, debt in Euro. The state of Saarland is missing due to a lack of observations.

Table 5.13: Balancing Property (10-Nearest-Neighbors Matching)

Variable	Mean Treated	Mean Control	% Bias	% Bias Reduct.	t	$p > \|t\|$
log(Distance to Core City)	1.80	1.75	15.5	87.9	0.83	0.411
log(Pop. 1961 Surr. Munic.)	7.29	7.29	-0.0	99.9	-0.00	0.999
log(Pop. 1961 Core City)	11.4	11.3	12.0	66.5	0.83	0.407
log(Area 1961 Surr. Munic.)	2.05	1.96	12.2	51.6	0.76	0.445
log(Area 1961 Core City)	3.97	3.90	10.3	-33.5	0.66	0.510
log(Debt 1966 Surr. Munic. (p.c.))	5.27	5.25	6.4	70.8	0.44	0.663
log(Debt 1966 Core City (p.c.))	6.17	6.17	-1.0	94.9	-0.06	0.954
Political Diff. Surr. Munic.	6.19	6.51	-1.8	91.5	-0.11	0.912
Political Diff. Core City	-10.3	-9.91	-4.1	76.0	-0.29	0.771
Religious Difference 1961	9.00	9.71	-3.2	74.1	-0.21	0.836
Dummy Schleswig-Holstein	.075	.059	5.6	82.1	0.41	0.685
Dummy Lower Saxony	.1	.087	4.2	93.5	0.28	0.777
Dummy North Rhine-Westphalia	.038	.031	3.0	93.3	0.24	0.811
Dummy Hesse	.038	.027	7.1	-156.6	0.37	0.711
Dummy Rhineland-Palatinate	.325	.321	0.9	98.4	0.05	0.956
Dummy Baden-Württemberg	.275	.244	8.1	53.6	0.44	0.659
Dummy Bavaria	.15	.232	-22.9	-110.4	-1.31	0.192
Dummy State Capital	.125	.097	9.5	-71.7	0.57	0.573

Distance in kilometers, area in square kilometers, debt in Euro. The state of Saarland is missing due to a lack of observations.

Table 5.14: Balancing Property (Kernel Matching)

Variable	Mean Treated	Mean Control	% Bias	% Bias Reduct.	t	$p > \|t\|$
log(Distance to Core City)	1.80	1.75	14.3	88.8	0.76	0.450
log(Pop. 1961 Surr. Munic.)	7.29	7.30	-0.9	92.1	-0.06	0.955
log(Pop. 1961 Core City)	11.4	11.3	13.1	63.6	0.90	0.371
log(Area 1961 Surr. Munic.)	2.05	1.98	10.1	60.0	0.63	0.532
log(Area 1961 Core City)	3.97	3.91	9.8	-26.9	0.62	0.537
log(Debt 1966 Surr. Munic. (p.c.))	5.27	5.25	8.4	61.6	0.59	0.557
log(Debt 1966 Core City (p.c.))	6.17	6.16	1.4	92.7	0.08	0.933
Political Diff. Surr. Munic.	6.19	7.76	-8.7	57.9	-0.55	0.582
Political Diff. Core City	-10.3	-9.54	-7.8	54.6	-0.55	0.581
Religious Difference 1961	9.00	11.0	-8.7	29.0	-0.56	0.578
Dummy Schleswig-Holstein	.075	.054	7.3	76.6	0.54	0.588
Dummy Lower Saxony	.1	.090	3.2	95.0	0.22	0.827
Dummy North Rhine-Westphalia	.038	.035	1.1	97.4	0.09	0.929
Dummy Hesse	.038	.024	9.0	-228.4	0.48	0.628
Dummy Rhineland-Palatinate	.325	.320	1.1	97.9	0.07	0.943
Dummy Baden-Württemberg	.275	.244	8.3	52.7	0.45	0.652
Dummy Bavaria	.15	.234	-23.6	-116.9	-1.35	0.179
Dummy State Capital	.125	.083	14.0	-152.8	0.86	0.393

Distance in kilometers, area in square kilometers, debt in Euro. The state of Saarland is missing due to a lack of observations.

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Chapter 6

Concluding Remarks

This book has analyzed the role of institutions in public finance, focusing on the issues of revenue forecasting and the spatial administrative structure of municipalities. Chapter 2 analyzes the international differences in forecasting practices and shows that forecasting performance depends on the institutional arrangement. The performance turns out to improve with the degree of independence, but also hinges on the timing of the forecast and the structure of the tax system. Chapter 3 looks into revenue forecasting in Germany and widely confirms the unbiasedness and efficiency of the forecasts. Only with regard to tax law changes and the term of office there appears to be some room to improve the forecasts. In Chapter 4 we turn to local policies and provide evidence how the design of borders impacts on local tax policy. Both the number of competitors and the size of a core city in its agglomeration prove important. Chapter 5 analyzes the effects of reforms of spatial administrative structures. Considering the reforms in Germany in the 1960s and 1970s, we show that incorporated surrounding municipalities of core cities perform better in terms of population growth than comparable municipalities that have remained independent.

What follows from the insights given in this book? And what is left for future research? The purpose of this book is first of all to discover effects of institutions on the performance of the public sector. Evidence is provided for the relevance of a number of institutions. There is, however, still space left to analyze which political recommendations may follow. While the main result of Chapter 2, that independent revenue forecasts are superior to public ones, is clear in its implications, adjustments with respect to the other results cannot be easily performed. The timing of forecasts has to fulfill the needs of the budgetary process and is hence subject to this. This is even more the case with regard to the structure of the tax system. One can hardly recommend that the importance of simple-to-forecast taxes should be increased simply in order to be able to come up with more precise forecasts. However, the results allow revenue forecasters to attribute forecast errors to certain circumstances that they are exposed to. This may prevent replacing them with different forecasting structures, a result that also follows from Chapter 3. The German Court of Auditors had criticized the revenue forecasters in 2006 for being too optimistic in recent years. While this may have been the case for a certain period, our analysis of forecasts over a period of almost 40 years shows no significant bias. In this way the results take the pressure off the forecasters.

Implications are somewhat different when local policies are considered. The results that are derived in Chapter 4 explain a notable part of the variation in the level of local business taxation in core cities of agglomerations. It seems natural to incorporate surrounding municipalities into these cities or to consolidate them among themselves to improve the fiscal position of the cities. However, analyzing the elasticity of tax bases regarding tax rate changes and estimating the resulting revenue effects is beyond the scope of this book and is left for future research. Furthermore, there are aspects besides the power to tax businesses that also have to be mentioned where consolidations are considered. These comprise aspects of political influence and tastes in public goods provision, for instance, which can affect the attraction of citizens. This leads directly to the implications of Chapter

5. From this we learn that the incorporation into a city exerts a positive population growth effect on the incorporated unit. However, it is again unclear whether this is an argument in favor of incorporations. The interests of "long-established" citizens may differ from those of potential new ones, as evidence in the literature on voting behavior in the case of consolidations shows. To answer the question why these communes become more attractive to citizens, a more in-depth analysis of this aspect has to be performed. Further research must also be undertaken to identify effects of incorporations on other economic variables, such as income levels or real estate prices.

Curriculum Vitae

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