# The Impact of Public Opinion on Public Policy

## A Study of Why, When, and How Agenda Setting Matters

Peter Bjerre Mortensen
Ph.D. Dissertation

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# Acknowledgements

On the face of it the past three years of my life may seem to have been largely unaffected by the writing of this Ph.D. dissertation. I got married, we now have a lovely daughter, many good friends are still around and, although I am certainly not nearly as active a sportsman as I used to be, I still manage to get a little exercise now and then. However, upon closer inspection something has indeed changed during these three years. What began as a vague idea and a sudden impulse soon turned into a project that has required an ever-growing share of my attention. It has often been a dear and stimulating companion, but from time to time it has also been a rather annoying and very possessive acquaintance. Going to the grocery shop, attending a family dinner, or being up with a 5-4 third set lead on the tennis court – you never know when the project suddenly pops into your head and demands your attention. However, many generous people have helped to ensure that things did not go (utterly) awry.

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*Peter B. Mortensen*

# Chapter 1

# Introduction

Responsiveness to citizen preferences is a primary concern in many theories about democracy. Many scholars have argued that policies passed by government should reflect the preferences of the governed (see e.g. Dahl 1956; Key 1961; Luttbeg 1981; Lijphart 1984; Burstein 1998; Glynn et al. 1999; Manza & Cook 2002). Central to this understanding of democracy is the notion that citizens constitute the foundation of all political authority and delegation to elected representatives is only a second-best solution, necessitated by the size of modern societies. On the other hand, some scholars have warned against this responsive rule, arguing that the general public is too uninformed and too ignorant of public affairs to play a role governing the state (see e.g. Lippman 1922 [1997]; Schumpeter 1943 [1994]; Converse 1964).

Most people would probably be dissatisfied with a democracy that did not provide at least some representation of public wants in the policies enacted. And even for those who believe public policy should not be responsive to public opinion, as noticed by Luttbeg (1981, p. 3), the empirical question about degree and conditions of such response should be important. This empirical question of responsiveness is the primary concern in the present study. Just as the number of normative perspectives on democratic responsiveness is vast, there has been no shortage of empirical theorizing either. At least three theoretical positions can be identified.

The first group of empirical theories predicts a relatively high degree of responsiveness to public opinion (Downs 1957, part I; Mayhew 1974; Arnold 1990). Central to these theories is an emphasis on politicians' or political parties' electoral competition for votes. Ultimately, the theories rest on the argument that political elites derive private benefits from policies that correspond to the preferences of the citizens and that they will be punished in regular elections if public policies deviate substantially from the preferences of the electorate.

The second group of theories, on the other hand, stresses the opportunities for political elites and special interests to obstruct the will of the mass public and to pursue their private interests at the expense of the preferences of the public (Lowi 1969, Niskanen 1971). According to this view, specialists, bureaucrats, and political elites can interact and decide on a broad range of political issues within various cozy subsystem structures without interference from a broader public. In such privileged and more or less closed policy-making systems, policy can often deviate substantially from the preferences of the public without politicians necessarily getting punished.

Third, it is possible to identify an intermediate theoretical position that points to both responsiveness and non-responsiveness. A forceful advocate of this view is Schattschneider (1960) in his seminal essay, *The Semi-Sovereign People*. According to Schattschneider (1960, Chapters 2 and 8), a "pressure system" dominated by well-organized special interests is often successful in pursuing their own objectives at the expense of the preferences of ordinary citizens, especially when citizens are unaware of the particular policies. On the other hand, Schattschneider maintains that the public can exert a substantial influence on public policymaking, in particular on issues highly visible to a broader public audience and in times of increasing public attention and mobilization. Later studies within this tradition have elaborated and refined this dynamic view of democratic responsiveness (see Cobb & Elder 1983; Baumgartner 1989; Thurber 1991; Baumgartner & Jones 1993).

What distinguishes the theoretical perspectives is disagreement over degree of responsiveness. As pointed out by Burstein (1998, p. 29), no one would probably argue that public opinion always impacts public policy, and very few that it never does. A central question is when and under what circumstances does public opinion impacts public policy? An important theoretical and empirical challenge in answering this question, it seems, is to place the model of electoral competition in a meaningful relation to models that stress the power of special interests and subsystem actors. Developing and testing such a conditional explanation of responsiveness is the main ambition of this dissertation.

## The argument in brief

Inspired by elements from each of the three views on responsiveness introduced above, this dissertation offers a model of when political decisions respond to citizen preferences. According to this model, many policymakers are not overly concerned with public opinion when they pursue certain policies, just as the average voter is unaware of most public policies. However, some policymakers are deeply concerned with voter reactions, just as some policies are more visible to the average voter than others. The latter is a characteristic of policymakers in the so-called macropolitical venue and of issues placed high on the agenda of this venue. An example of the macropolitical venue is the floor of the national parliament, where political parties are the dominant policymakers and where the activities of these policymakers are visible to a broader public audience. The central implication of this model is that the impact of public opinion on a given issue depends on the issue's share of the macropolitical agenda. Or more to the point, the larger a given issue's share of the agenda of the macropolitical venue, the more likely are popular policies on that issue because policymakers in the macropolitical

venue fear the electoral costs of unpopular policies on visible issues. Conversely, when an issue's share of the macropolitical agenda is low or fading, responsiveness to majority opinions decrease as well.

Underlying this conditional model of responsiveness is the assumption that subsystems are an evident feature of modern government, but so is the macropolitical venue, and national macropolitical policymakers have the power to adjust policies if they so desire. Furthermore, it is assumed that issues can move up and down this hierarchy of policy making venues and that the activities in the macropolitical venue are very visible to the public. Political parties are the dominant players in the macropolitical venue and vote seeking is assumed to be their primary objective. Finally, inspired by literature on voting, the average or median voter is believed to base his vote on an evaluation of the deviation between actual policies and his preferred policies on only the most visible issues.

The recognition of both subsystems and macropolitical venues is not new, but earlier research within this literature has largely focused on the implications for stability and change in public policy, ignoring implications of responsiveness to public opinion (see Baumgartner 1989; Thurber 1991; Baumgartner & Jones 1993). The standard view is that subsystem politics is characterized by stability and status quo policies, but when macropolitical actors intervene, policy is likely to change, sometimes abruptly and in a punctuated manner, sometimes moderately or smoothly. Following the model developed in this dissertation, however, we can say more than this. Knowing the content of the majority public opinion, we can actually predict the direction of policy changes set in motion by a change in the agenda of the macropolitical venue. Policy does not just change when macropolitical actors intervene. It changes in the direction preferred by a majority of the voters.

In the following chapters, the basic model of conditional responsiveness presented above is extended in several ways. Incorporating ideas from the literature on political business cycles (Nordhaus 1975; Tufte 1978), I identify a central condition under which changes in the macropolitical agenda are particularly likely to result in popular policy adjustments. According to this literature, voters weigh recent information higher than information acquired in a more distant past, which implies that electoral costs of deviating from mass opinions on visible issues increase as elections draw near. Consequently, national macropolitical policymakers will adjust policies on visible issues more eagerly the closer the upcoming election is.

Furthermore, it is considered how the introduction of constraints on the behavior of the policymakers in the macropolitical venue alters the main proposition introduced above. Various constraints are reflected on, but most

importantly, it is argued that the effects postulated above are weaker on issues with a high degree of local government involvement.

Looking at public spending attitudes and public spending decisions on seven Danish policy issues from 1980 to 2003, the empirical analysis generally supports the theoretical model. Specifically, it shows that when an issue's share of the macropolitical agenda increases – measured in length of debate in the national Danish parliament – public spending on that given issue is adjusted in the direction preferred by a majority of the electorate. Furthermore, the empirical analysis lends support to the claim that these agenda effects are stronger in election years and weaker on issues with a high degree of local government involvement.

## Structure of the dissertation

Chapter 2 is devoted to a review of earlier empirical research on the impact of public opinion on public policy. The role of public opinion in public policy-making is a core question in political science and nearly all policy studies therefore have something to say about this question, be it directly or indirectly, by demonstrating the power of alternative policy determinants. While in the final chapter of the dissertation, in a discussion of the broader implications of the present study, I include a wider range of relevant policy perspectives, I confine my view in Chapter 2 to the group of empirical studies that directly explore the impact of public opinion on public policy.

Chapter 3 develops the model behind the central claims about conditional responsiveness introduced above. Theoretically, this chapter draws on ideas put forward in a number of the studies referred to in Chapter 2, on insights from rational choice theories about party competition and on voting behavior literature.

In Chapter 4, questions about case selection, translation of the theoretical propositions into observable implications, and measurements of key dependent and explanatory variables are discussed and decided upon. In this chapter, the application of the theoretical model to Danish politics is also considered.

Chapter 5 applies the theoretical model to four policy issues characterized as "popular" in the sense that a majority of the voters prefers increased public spending to decreased public spending. These are law & order, environmental protection, primary education, and health. Using time series regression on annual spending data from 1980 to 2003 it is demonstrated that these four policy domains generally seem to benefit from increased macropolitical attention, measured as annual length of debate in the Danish parliament. The more parliamentary debate on these issues, the more the politicians tend to

spend, although the analysis also reveals some variation in response across issues and between election years and non-election years.

The opposite spending effects are found on the three policy issues analyzed in Chapter 6, that is, defense, aid to developing countries, and cultural affairs. These issues are all characterized as "unpopular" since a majority of the voters always tend to prefer budgetary cuts to budgetary increases, suggesting that the popular policy decision in these particular domains is budgetary cuts. The analyses in Chapter 6 clearly show parliamentary debate to have a negative impact on spending changes on these three issues, implying that public opinion does influence spending decisions in times of increased parliamentary debate. With regard to the election cycle effects, the results are more mixed.

Chapter 7 is an important follow-up on the empirical analyses presented in Chapters 5 and 6. In the first part of Chapter 7, essential questions about direction of causation and validity of the most likely alternative explanations are discussed and evaluated in additional analyses. In the second part, using a panel study design, the seven issues are surveyed again in order to arrive at an estimate of the overall effect of the variables of theoretical interest.

Chapter 8 summarizes the results of the empirical analysis and assesses the evidence on balance. The chapter also provides a discussion of the relevance of the findings beyond the seven Danish issues and points out how these findings either fit or challenge alternative public policy theories. The chapter closes with a reflection on a set of normative questions about the impact of public opinion on public policy in light of the outcome of the empirical study.

# Chapter 2

# The impact of public opinion on public policy. A discussion of earlier research

Many scholars have pondered the relationship between public opinion and public policy, but relatively few have examined it explicitly in empirical research. Over the last couple of decades, however, the number of systematic empirical studies has been on the increase, not least in the United States. To what extent does public policy actually correspond to public opinion, and how much impact does public opinion actually have on public policy? These issues have been the primary concern in many of these studies (see Page & Shapiro 1983; Brooks 1985; 1987; 1990; Wlezien 1995; 1996; Stimson et al. 1995; Monroe 1998; Petry 1999; Petry & Mendelsohn 2004)[1].

This dissertation contributes to this ongoing debate over the impact of public opinion on public policy. Specifically, it focuses on the conditions influencing public opinion's impact on public policy. Given this focus, I deliberately emphasize aspects and findings within the literature related to this conditional understanding of the opinion policy linkage. Though often seen as a deviation from the main research question about extent of correspondence, the opinion policy literature does point to a whole group of such circumstances, including questions about how opinion policy relations may vary with institutional designs, fiscal resources, different types of issues, the ideology of governments, distribution of public opinions, and a given issue's salience to the average voter.

In addition, I include research rooted within the agenda-setting literature, the literature on political party competition, and the literature on policy making venues. Characteristically, this adjacent research has not explicitly modelled and measured the impact of public opinion on public policy, but as I shall argue in this chapter, it does point to some promising extensions to earlier research of the opinion policy relationship. In particular the work by Baumgartner and Jones (1993), but also ideas from the literature on political business cycles (Nordhaus 1975; Tufte 1978) are discussed in this chapter and integrated in the model developed in the next chapter.

Since my dependent variable is public policy, not public opinion, I exclude from this discussion studies primarily concerned with explaining the latter. This does not reflect a naïve belief that voter opinions are fully exogenous to decisions made by politicians. However, using a longitudinal research design I attempt to separate out these opposite causal effects. Hence,

this question will only be addressed in specific assessments of how well my own and other scholars' research designs succeed in this endeavour.

Furthermore, I ignore the many micro-level studies of representation such as the subdiscipline often termed "dyadic representation" (see Miller & Stokes 1963; Bartels 1991; Hill & Hurley 1999). First, with its narrow focus on the relationship between individual legislators' votes in Congress and the opinions of their constituencies, these studies seem to constitute a rather ideographic American research agenda (cf Petry & Mendelsohn 2004, p. 509). Second, since my dependent variable is policy – not the individual voting behaviour of legislators, which may or may not turn into policy – the tradition seems less relevant to the research question guiding the present study.

Finally, a long-standing debate about the nature of public opinion, beginning with Converse (1964) characterizing many survey responses as "non-attitudes", is downplayed in this review. Newer studies convincingly demonstrate that aggregated voter opinions are rather sticky and when they do move they move in quite understandable ways (Page & Shapiro 1992; Stimson et al. 1995; Wlezien 1995; 1996; Kinder 1998; Erikson et al. 2002). This does not reject irrational and random fluctuations at the individual level, but such randomness tends to cancel out at the aggregate level (cf Erikson et al. 2002, Chapter 6).

To follow up on this point, I use a quite aggregated definition of public opinion. Borrowing from Glynn et al. (1999, p. 307) and Brooks (1985, p. 252), public opinion is defined as: *the majority opinion that could be measured on selected issues through responses to questions in opinion polls that ask explicitly about identifiable government policies*. For an overview of alternative definitions of public opinion, see Weisberg (1976, Chapter 2) or Glynn et al. (1999, Chapter 1).

After this delimitation of the perspectives most relevant to this study, we are ready to take a closer look at the literature. In the first section, empirical evidence of the strength of the unconditional opinion policy connection is presented and evaluated. I then move on to a more comprehensive discussion of the circumstances under which public policy is most likely to reflect public opinion. These circumstances are divided into two main categories: one group of explanations emphasizing cross-sectional variation, and one that points to variation across time.

## Does public opinion influence public policy?

### *First generation studies*

The first generation of systematic, quantitative studies of the opinion policy relationship were initiated and formed by Page & Shapiro's (1983) seminal

article *Effects of Public Opinion on Policy.* Theoretically, this and subsequent studies belonging to this first generation research have been characterized by a rather explorative approach. The more or less implicit working assumption behind this research simply seems to be that politicians derive some private benefits from policies that comply with the preferences of the mass public and hence will pay attention to these preferences when they decide on policies. Where the perspectives may differentiate is the extent to which they believe politicians can deviate from mass preferences and get away with it (cf Manza & Cook 2002b, p. 644).

Methodologically, Page & Shapiro (1983) examine democratic responsiveness by looking at over three hundred issues drawn from US surveys conducted between 1935 and 1979. First, they only include issues where a question has been asked more than once in order to measure opinion changes. Second, they correlate these opinion changes with subsequent changes in corresponding policies as coded by the authors. To allow the policymaking process to react to public opinion changes they operate with a lag between opinion changes and policy changes (p. 177). Ignoring the 33 % of the cases where policy did not change at all, they find a congruent change between opinion and policy in 66 % of the cases (p. 178). Noting that a 50 % congruence would be expected by chance, the authors conclude that this finding indicates a "rather substantial congruence between changes in opinion and policy" (p. 179).

The methodological approach in Page & Shapiro's (1983) study has been replicated in a range of studies, which differentiate mainly with regard to whether they look at *congruent changes* or *consistent levels* of opinion and policies. The degree of correspondence – most comparable to Page & Shapiro's findings – varies significantly across these studies. Monroe (1998) found American public policy to be consistent with public opinion in 63 % of the cases from 1961 to 1979, whereas he finds only 55 % correspondence when looking at issues between 1980 and 1993. Brooks (1985; 1987; 1990) finds correspondence to be significantly lower with a 44 % consistency between opinion and policy in Britain, 42 % in Germany, 41 % in the US, and 40 % in France. Looking at Canadian politics, Petry (1999) finds a 60 % correspondence from 1968 to 1993, but looking at 1994 to 2001, Petry & Mendelsohn (2004) find correspondence in only 49 % of the cases.

It is not obvious what the reasons are for these mixed assessments about the degree of responsiveness, but it is a conclusion that reviews including a larger number of the opinion policy literature also arrive at (cf Glynn et al. 1999a; Manza & Cook 2002b; Page 2002). Furthermore, it seems evident that all these quantitative estimates of public opinion's effect on policy are

skewed in an upward direction. First, they only include issues where public opinion has been measured, meaning that they in a sense represent most likely cases compared to a sample including those issues where no surveys are available (Page & Shapiro 1983, p. 189). Second, the lack of control for third variables and the vague assessment of the reciprocal effect going from public policy to public opinion also contribute to an overestimation of the effect of public opinion on policy (cf Monroe 1998, p. 12).

In recent years, a new generation of studies has developed using time series regression to estimate the effect of public opinion on public policy (Hartley & Russett 1992; Stimson et al. 1995; Wlezien 1995; 1996; 2004; Erikson et al. 2002; Soroka & Wlezien 2004; 2005; Eichenberg & Stoll 2003). The methodological approaches of these studies address some of the problems described above, whereas others, as I will point out below, still remain. Two supplementary views dominate these recent developments. Whereas Stimson, Erikson, and McKuen examine what they term "global representation" (Stimson et al. 1995; Erikson et al. 2002), Wlezien and colleagues are primarily concerned with representation within specific policy domains (Wlezien 1995; 1996; 2004; Soroka & Wlezien 2004; 2005; Eichenberg & Stoll 2003).

*Global representation*

According to the model of global representation, public attitudes rarely crystallize on specific issues and it is hence difficult for politicians to obtain reliable information about public attitudes at this level (Stimson et al. 1995, p. 545). This belief is in sharp contrast to the many studies above that tend to assume that policy responsiveness is issue-specific. As an alternative opinion policy link Stimson et al. (1995, p. 545) suggest that:

> For anticipating an uncertain future, we believe trends in global sorts of preferences (e.g., whether more or less government is desirable) offer the greatest promise…Elected politicians, we believe, sense the mood of the moment, assess its trend, and anticipate its consequence for future elections.

The novel contribution of this approach is the measurement of public opinion and public policy. Pooling more than 130 opinion polls and 1610 separate questions into an explorative factor analysis, they find a dominating dimension in these data, a dimension they interpret as reflecting an underlying liberal/conservative policy dimension (Erikson et al. 2002, Chapter 6). As a consequence, changes in the ratio of responses to these repeated items are combined to produce a single liberal-conservative measure of "national mood" that varies over time.

Their measure of policy is no less aggregated and crude. Combining two activity/policy indicators from The US House of Representatives, the US Senate, the US Presidency, and the US Supreme Court, respectively, they establish a measure of "policy liberalism" covering the years 1953 to 1990. By regressing the measure of "national mood" on this policy indicator, they find an impressive effect of public opinion on public policy across these four US institutions. Including the effect of a variable measuring the share of democrats/liberals in the different institutions, an effect they interpret as *indirect representation*, they find "a one-to-one translation of preferences into policy" (Stimson et al. 1995, p. 557).

Compared to the studies mentioned above, a major advantage of Stimson, Erikson, and McKuen's approach is the systematic use of annual time series data, where the time lag of public opinion certainly strengthens the argument that the produced estimates capture effects going from public opinion to public policy. However, as argued by Page (2002, p. 327-332) the impressive statistical estimate of the opinion policy relationship is almost certainly biased upward. First, the construction of the highly aggregated public opinion measure begins by excluding all foreign issues and certain domestic issues that do not fit into a single, liberal-conservative dimension. Second, even after excluding these inconvenient issues, the common liberal-conservative dimension explains only half of the variance in the survey-data (cf Erikson et al. 2002, p. 82). Third, the weighting and standardizing procedures of the remaining items and the exponential smoothing of the final mood construct may add further to the impression of a very selected, refined, and crunched public opinion variable.

This criticism is not intended to belittle the fact that Stimson and colleagues certainly have revealed important tendencies in the opinion policy relationship. However, it does suggest that the highly aggregated study conceals some important opinion policy dynamics at a more specific and disaggregated level of public policy making. This interpretation is corroborated by the work initiated by Christopher Wlezien (1995; 1996).

### *The Thermostat Model*

Contrary to the model of global representation, Wlezien is mainly concerned with "specific representation", i.e. representation within policy domains such as defense, education, health, environment, etc. Public preferences are measured by repeated polls asking respondents whether they think the government spends too much, too little, or about the right amount in a given policy domain (Wlezien 1995, p. 984). Correspondingly, public policy is measured as annual budget appropriations/expenditures in a given policy domain.

Inspired by Easton's (1965) depiction of a political system with built in feedback mechanisms, Wlezien (1995, p. 982) develops a model, where public opinion functions like a thermostat on public policy:

> In effect, the public would behave like a thermostat, where a departure from the favored policy temperature (which itself can change) produces a signal to adjust policy accordingly, and once sufficiently adjusted, the signal stops.

In line with this feedback model, Wlezien is concerned with both the expected negative effect of spending increases on public spending preferences and with the expected positive effect of spending preferences on public spending. To separate out these reciprocal effects, public preferences are generally lagged by one year when examining the effect on public spending (Wlezien 1996, p. 84).

Thus far, Wlezien and Soroka have applied this model to the US, the UK, and Canada. Focusing on public spending as the dependent variable, the empirical support for the model has been mixed. In the US, public spending preferences have a positive significant effect on public spending in 5 out of 9 policy domains in the years 1977 to 1995 (Wlezien 2004, p. 12-15). In the UK they find a positive significant effect in 2 out of 4 domains from 1978 to 1995 (Soroka & Wlezien 2005, p. 677), and in Canada, looking at 1984 to 2001, a significant effect is shown in 2 out of 6 policy domains (Soroka & Wlezien 2004, p. 546). In addition to these studies, Eichenberg and Stoll (2003) have applied the model to defense spending in the US, the UK, France, Germany, and Sweden. Their analyses suggested a significant effect in 4 out of these 5 countries. Wlezien does come up with a clever explanation of the variation in representation across policy domains. However, since in this section we focus narrowly on the extent of public opinion effects, I shall return to that explanation in the next section.

Empirically, these studies support the general impression that public opinion does affect public policy, but again the effect seems to be of medium size and circumstantial. Methodologically, studies of the thermostat model benefit from the systematic use of time series data and they do, as I will return to shortly, bridge part of the gap in the debate about global versus specific representation referred to above. However, they do not escape aforementioned problems such as lack of control variables and exclusion of issues where no surveys have been conducted. The latter may be especially relevant in this case, where the time series approach demands that only issues where surveys have been repeated annually are included (see Petry 1999).

*Summary*

On the whole, the existing body of research within the opinion policy literature reviewed in this section, points to the conclusion that public opinion does have an effect on public policy. Whatever the exact true estimate is, this evidence clearly suggests that public opinion is not just relevant from a normative democratic point of view, but is also an important determinant in public policy analysis. This conclusion invites to a closer look at the relationship between public opinion and public policy. When does public opinion influence public policy and under what circumstances is public policy particularly likely to reflect public opinion? These are obvious questions in light of the evidence discussed in this section. In the remaining of this chapter, we take a closer look at such conditions tempering the relationship between public opinion and public policy.

## When and under what circumstances?

The opinion policy literature introduced above has pointed to a number of contingent explanations, but perhaps because most studies seem to treat this topic as a deviation from the main question about extent of responsiveness (cf Manza & Cook 2002b, p. 652), systematic theorizing and accumulation of empirical knowledge about these effects has been limited. In this section, I divide the conditional explanations into two main categories. The first set of explanations points to cross-sectional variation in representation, while the other set stresses longitudinal variation in responsiveness.

*Cross-sectional variation*

Institutional explanations

The first set of explanations emphasizes the role of institutions. According to the *new institutionalism* approach, institutions constrain and shape the behavior of political actors.[2] Therefore, an institutional approach would expect different institutions to generate different patterns of representation. First, government institutions and electoral systems vary significantly across countries, which, according to the literature on comparative politics, result in different types of democracies (cf Lijphart 1984). As a consequence, degree of responsiveness may reflect whether we are dealing with, for instance, a proportional or majoritarian political system, a parliamentarian versus presidential system, or a unitary state versus a federation. Empirically, however, the opinion policy literature has overwhelmingly focused on US politics only. Therefore, we do not know very much about how national institutions affect representation. Whereas the first comparative studies conducted by Brooks (1985; 1987; 1990) found no systematic country differences, a few recent

comparative studies do suggest some systematic cross-country variation (Eichenberg & Stoll 2003; Hobolt & Klemmensen 2005; Soroka & Wlezien 2004; 2005).

According to Hobolt and Klemmensen's (2005) study, the correlation between voter priorities and government priorities is stronger in political regimes with a proportional electoral system than in regimes with a majoritarian electoral system. The explanation of this difference in responsiveness, they argue, is to be found in a higher degree of party competition and government vulnerability in proportional systems compared to majoritarian systems. Based on their study of the Thermostat Model in the US, the UK, and Canada, Soroka and Wlezien (2004, p. 552) suggest that parliamentary systems are less responsive than presidential systems because the cabinets in the former are more powerful than those in presidential systems with strong legislatures. Eichenberg and Stoll (2003) have studied defense spending in five western countries. They find rather similar effects from public opinion across the five countries, but in some countries, the effect on defense spending is evident after one year, whereas in other countries it is evident after two or three years. According to Eichenberg and Stoll (2003, p. 417), the explanation of these different lag structures should be found in the variation in budgetary systems across countries. Yet, however, none of the other comparative studies has found such cross-country differences in the time of response. On the whole, the empirical evidence from comparative studies of responsiveness is scarce and more research is certainly needed on this important topic.

Institutions also vary within countries. In some policy domains, state and local governments are granted more autonomy than in others, which may both constrain policy actions of national governments and make the locus of policy responsibility less clear to the public (cf Soroka & Wlezien 2004, p. 552). Furthermore, it may matter significantly whether policies are decided for instance within relatively closed subsystem venues or on the floor of national legislatures. Whereas the institutional design of the latter venue should enhance representation, this may not be the case for subsystem venues. Evidence from a comprehensive qualitative case study offers some support for this idea (Sharp 1999), but we need more knowledge about the extent to which venue characteristics temper the opinion policy relationship. The massive subsystem literature on subgovernments, whirlpools, iron-triangles, policy networks, etc. clearly suggests lower responsiveness to public opinion, when policy is conducted within these more or less closed structures (for reviews, see Rhodes & Marsh 1992; Jordan & Maloney 1997). However, to my knowledge this convincing claim has never really been examined systematically in studies explicitly measuring both public opinion and public policy.

A similar conclusion holds when it comes to the impact of public opinion on policy decided in non-subsystem venues, i.e. policy decided in the macro-political venue. This focus on institutional venues is central to the dynamic agenda-setting model developed by Baumgartner and Jones (1993) and I therefore return to this idea in the discussion of dynamic conditions later in the chapter.

### The Thermostat Model

Wlezien has developed another explanation of cross-sectional variation. When we left the Thermostat Model above, I was about to account for his elegant explanation of seemingly deviant policy domains, those where no (specific) effect on policy was recognized. In line with the Thermostat Model, Wlezien (2004, p. 9), in his study of the US claims that:

> Where the public notices and responds specifically to policy in a particular domain, as for defense and welfare, representation would be specific. Where the public responds – in effect, more globally – to policy across a set of domains taken together, as for the nonwelfare social programs, representation would be global. Where the public does not respond to policy, as for crime, foreign aid, and space, representation simply would not occur.

These propositions imply symmetry between public importance of different policy domains measured as public responsiveness and politicians' representation of public attitudes. If the public does not notice it anyway, there is no reason for rational politicians to comply with public opinion. Furthermore, public responsiveness across issues is clearly modeled as time-invariant, whereas only the direction of public opinion is expected to change over time.[3] Finally, the proposition bridges the gap between global and specific representation, elucidating the conditions under which each is most likely to occur.

Wlezien's idea finds some empirical support. In the US the empirical pattern fits almost perfectly with these expectations, as evidenced by the nine policy domains arrayed as stated in the quote above (see Wlezien 2004). Applying the model to the UK, however, the public turns out to be much more responsive than policy makers, which leads to the following conclusion: "In the United States policy representation of preferences is symmetrical to public responsiveness to policy. In Britain, this is not the case" (Soroka & Wlezien 2005, p. 686). In the Canadian case, we saw earlier in this chapter that a specific effect on public spending occurred in only 2 out of 6 policy domains and when it comes to public responsiveness this is also less pronounced in Canada than in the US and more like the UK (Soroka & Wlezien 2004, p. 552).

These different findings may reflect institutional differences across the three countries, just as the variation within and between countries could stem from ignorance of other relevant variables or a misspecification of the dynamic elements of public responsiveness. In that respect, these results are only suggestive. However, they do reveal that though the parsimonious and elegant Thermostat Model does quite a good job, it misses important variation in the opinion policy relationship even in its more sophisticated version described in this section. Inspired by the agenda setting research reviewed below, one likely improvement to the thermostat studies could be a more explicit modeling and measuring of political process variables, including a model that allows political attention to vary over time.

### Other explanations

Another proposition frequently considered in the opinion policy literature is the idea that opinion policy consistency is greater on non-redistributive issues than on redistributive issues (cf Brooks 1985, 1987, 1990; Petry 1999). Since the benefits of redistributive policies are widely distributed and their costs borne chiefly by organized elite groups (cf Lowi 1964; Wilson 1980), mass opinion is assumed to favor policy change on redistributive issues, while organized interest groups tend to oppose such policy change. In such cases, organized interests may work against representation of the popular will on redistributive issues compared to non-redistributive issues, which leads to the proposition of more responsiveness on the latter kind of issues (cf Petry 1999, p. 544).

Within the opinion policy literature Brooks (1985; 1987; 1990) has turned out to be the most persistent advocate of this proposition, and in a set of five simple correlation studies in five different countries, he finds the expected effect in four out of the five countries. However, using a similar coding and categorization procedure as Brooks, Petry (1999, p. 546) finds a significant, but opposite effect in his multivariate analysis of Canadian politics. The (non–)redistributive proposition points to a broader question about the obstructing effect of special interests which, in light of the meager and contradictory evidence presented here, needs much more empirical exploration within the opinion policy literature.

Another proposition common among many opinion policy studies is that foreign policy is less susceptible to pressures of public opinion than domestic policy because foreign policy issues are assumed to be less visible and important to the public than domestic policy issues. However, having somehow survived for so many years, it now seems clear that this proposition has almost never been supported by empirical observations (Page & Shapiro 1983, p. 182; Brooks 1987, p. 475; Brooks 1990, p. 518; Monroe 1998, p. 14;

Petry 1999, p. 546; Page 2002, p. 336). Sometimes responsiveness may be lower on foreign policies and sometimes higher, but this suggests merely dynamic explanations than inherent differences in responsiveness across foreign policy issues and domestic issues.

Finally, fiscal resources is an old determinant in studies of public policy that has generated a well-established literature of its own.[4] Here we narrow the focus to the rather limited attention it has caught in the opinion policy literature. In this context, it may work as a potential constraint on policy responsiveness. Responsiveness may be lower on issues where responsiveness requires significant budgetary expenditures in comparison with issues that do not involve high fiscal costs (cf Manza & Cook 2002b, p. 653). Confirming the general lack of contingent studies, however, these effects have not, to my knowledge, been subject to systematic empirical examination in the opinion policy literature.

Summary

To sum up much more empirical research is needed on the conditional explanations to provide a solid evaluation of their explanatory power. From the available evidence, however, it does seem like the reflection of public opinion on some issues may be genuinely higher than on others. And if not genuinely higher then, at least for several years, such cross-sectional differences may stay rather constant. On the other hand, despite methodological deficiencies and uncertainty stemming from very few systematic empirical studies, such cross-sectional explanations certainly miss important variation in responsiveness. One likely explanation is that the role of public opinion varies over time. Probably, it is not the same issues that people notice and respond to at varies points in time; the impact of special interests likely depends on the venues in which policy is decided and these venue attachments also vary over time; and probably foreign politics is not a constantly low attention issue compared to domestic issues. To explore such ideas further, we now turn to a set of explanations pointing to dynamics in representation over time.

*Dynamic effects on representation*

The opinion policy literature has produced relatively few systematic quantitative studies of the dynamic conditions affecting the influence of public opinion on public policy. Apart from an ongoing debate about a general decline in (US) responsiveness to public opinion over the last decades (cf Monroe 1998; Jacobs & Shapiro 2000; Page 2002; Burstein 2003), the number of studies examining year-to-year variation is rather modest. As I shall further elucidate below, variation in issue salience has been the most

commonly explored condition, but even in this case, as concluded in Burstein's (2003, p. 37) comprehensive review of the literature, only one study assesses statistically whether saliency affects responsiveness.[5] On the other hand, adjacent studies on representation rooted within the literature on agenda setting and the literature on political business cycles do provide some evidence and not least some promising ideas about how representation might vary systematically over time. Consequently, we take our starting point in this literature.

## Agenda setting and policymaking venues

The study of agenda setting is the study of issue salience. As noted in Dearing & Rogers' (1996, p. 8) comprehensive review of research on agenda setting:

> *Salience* is the degree to which an issue on the agenda is perceived as relatively important. The heart of the agenda-setting process is when the salience of an issue changes on the media agenda, the public agenda, or the policy agenda.

Traditionally, agenda setting research has been conducted by communication scholars focusing primarily on the public and the media agenda, while often ignoring implications for the policy agenda (cf Dearing & Rogers 1996, p. 73). Furthermore, most agenda setting studies have narrowly focused on how and why agendas change and have shown much less interest in the systematic policy consequences generated by such changes (ibid. Chapter 5). However, the study of policy agendas has been revitalized in recent years, not least because of Baumgartner and Jones' (1993) work.

According to Baumgartner and Jones (1993), policies are often decided within relatively closed subsystems characterized by low attention from outsiders and a lack of interference by broader political forces. In such settings, it is the preferences of privileged elites and special interests, who often share similar policy views, that dominate policies. However, from time to time such systems of limited participation are disrupted or significantly modified when macropolitical institutions intervene and public attention to the given issue increases. As Baumgartner and Jones (1993, p. 22) write:

> When an issue receives sufficient attention, it often can no longer be confined to subsystems. Then parties may be drawn to it because it has the potential of conveying electoral advantage.

The significant contribution of Baumgartner and Jones' research is the systematic study of these dynamic processes. The stabilizing and destabilizing policy effects resulting from this agenda based description of the policy

making system have been their chief concern (see Baumgartner & Jones 1993; 2002; 2005; True, Baumgartner & Jones 1999). However, this policy making model also has obvious and more direct implications for the impact of public opinion on public policy. First, it supports the institutional explanation put forward above, claiming that it matters in terms of representation whether a policy is decided within a subsystem venue or in the macropolitical venue. Second, it implies that, over time, there may be significant changes as to the venue in which a policy is decided.

Somewhat surprisingly, given its strong impact on many diverse research fields, the representational implications of this agenda based venue model developed by Baumgartner and Jones have never really been tested empirically. Instead, opinion policy studies have concentrated on secondary implications related to the broader concept of public saliency.

First, Baumgartner and Jones (2004) have examined whether the issue or problem priorities of the public, as measured by people's responses to the survey question that asks about the "most important problem facing the nation" (MIP), is correlated with government priorities measured as the annual number of congressional hearings. Studying US politics, Baumgartner & Jones (2004, p. 14) find a positive and significant correlation between MIPs and annual number of congressional hearings in 10 out of 16 policy domains from 1946 to 1998. With regard to policy output, which they measure as the annual number of passed statutes grouped into 16 policy domains from 1946 to 1998, they find a significant correlation with MIPs in 7 out of the 16 domains, the magnitude of these correlations however being much smaller than those for congressional hearings (ibid.).

Applying a similar approach to UK and Danish data on government activities measured as the government's opening speeches in parliament and MIPs lagged one year, Hobolt & Klemmensen (2005, p. 394) find a positive and significant correlation for the period from 1970 to 2002 in 4 out of 10 policy domains in Denmark, and in 2 out of 10 policy domains in the UK. Hence, even when disregarding the direction of policy changes and even when, as in the latter case, looking at policy intentions only and not policy outputs, the correlation between public priorities measured in MIP questions and government priorities is far from impressive.

The same seems to hold when the effect of MIPs is modeled as interacting with the direction of public opinion. The first one to test this model was Jones (1994, Chapter 5). According to his statistical analysis of US defense spending from 1965 to 1990, the product of public defense spending attitudes and the importance people ascribe to defense measured in MIP questions shows a better fit than the model including linear additive effects only (ibid.).

This model, which Jones termed *the preference activation model,* has generated some scholarly debate, however. Wlezien (2005), in his critical essay on the use of the MIP question as a measure of public salience, has not been able to reproduce Jones' findings. Furthermore, the preference activation model does not work in any of the 9 US policy domains explored by Wlezien (2004, p. 16). On the other hand, Soroka (2003, pp. 40-42), when examining defense spending in the US and the UK, does find a significant effect from the preference activation term when altering the salience measure so that it equals one when MIP responses are above the seventy-fifth percentiles and zero otherwise. This implies a threshold effect where governments pay attention to public preferences when public salience exceeds this threshold.

Thus, even though some issues may constantly be more salient to the public than others, it seems evident that the public salience of most issues does vary substantially over time. However, this longitudinal variation is not reflected in the representation of public opinion to the extent expected theoretically, where the concept of public saliency has often been treated as something of a panacea (cf Page 2002; Burstein 2003). This impression is supported by classical opinion policy studies examining the effect of public saliency measured as the inverse number of respondents answering "no opinion" or "don't know" in a given opinion survey (Page & Shapiro 1983; Brooks 1990; Petry 1999; Petry & Mendelsohn 2004). Page & Shapiro (1983, p. 181) did found higher correspondence between opinion and policy on the most salient issues without controlling for other determinants, but the effect seems to disappear in multivariate statistical analyses (Page & Shapiro 1983, p. 184; Petry 1999, p. 546; Petry & Mendelsohn 2004, p. 520).

What should we make of this misfit between high theoretical expectations and empirical observation? First, the misfit may stem from the way public saliency has been measured. As discussed at length in Wlezien (2005), the MIP question may confuse different characteristics of public salience, namely the importance of issues and the extent to which an issue is a problem. Wlezien may be right in this criticism, but a more fundamental flaw in the focus on public saliency may also exist.

A basic, though often implicit idea underlying the opinion policy approach discussed so far, is the assumption that politicians adjust public policies in rational anticipation of the next election. In addition, however, we may expect governments to anticipate and prepare for that handful of most salient issues that will decide the next election. In that case, they may constantly be seeking cues informing them about what might turn into salient issues in the electorate, trying to adjust policies before the issue becomes a major problem in the eyes of the electorate. Some problems do not have a simple cure and

will turn into major problems despite political action, but from a governmental reelection point of view, it may be too late to respond at that time.

This suggestion points to an increased focus on the policy making process and the preferences of the relevant policymakers instead of the preoccupation with measures of voter salience characteristic to many of the empirical studies referred to above. Returning to Baumgartner and Jones' theoretical model of subsystems and macropolitical systems, the immediate implication of this model is that public opinion is a more prevalent concern when macropolitial institutions, and hence macropolitical actors, are drawn into the handling of a given issue. It might be, for instance, that an issue is salient to the general public without being subject to macropolitical intervention, just as macropolitical intervention may anticipate, reduce or perhaps even prevent increased public saliency to that given issue. Yet, no one has systematically tested the representational effects of macropolitical intervention. However, in order to do so, the Baumgartner and Jones model would need a further specification of the characteristics of the macropolitical venue and the preferences of macropolitical actors.

These suggestions may not suffice to remedy the discrepancy between high theoretical expectations and moderate to weak empirical correlations identified above. But I consider them promising enough to deserve further exploration. They will therefore be addressed in much greater detail in the next chapter. In the following section, I introduce the political business cycle model and discuss how this idea may contribute to the opinion policy literature.

## Political business cycles

Inspired by Downs' (1957) model of party competition, Nordhaus (1975) formally developed the model of a political business cycle, which rests on a rather straightforward idea. According to Nordhaus (1975, p. 182):

> …voters do not take simple averages of economic variables over the last electoral period, but have a decaying "memory" of past events. On election day, the memory of recent events is probably more poignant than that of ancient ills.

If this model of myopic voting behavior is correct, or at least believed to be correct in the minds of political leaders, then compliance with public preferences would be stronger in election years than in non-election years because the state of the economy in election years is more important to citizens' voting preferences than the economy is in non-election years.

Developed by scholars in economics, empirical studies have from the outset focused on testing politicians' manipulation of the economy, in particular unemployment levels and inflation (Nordhaus 1975; Tufte 1978). Despite the first proponents' optimistic conclusions about their own empirical evidence (see Nordhaus 1975, p. 187; Tufte 1978, p. 11), a closer look at these and other studies reveal, at best, a very modest and fragile election-cycle effect on macro economic indicators (cf Paldam 1979; Golden & Poterba 1980; Alt 1986; Hibbs 1987, Chapter 8; Lewin 1991, Chapter 3; Schultz 1995). Nordhaus (1975) himself, for instance, finds absolute no election-cycle manipulation of unemployment levels in four out of nine countries; in two countries a modest effect may exist, whereas an apparently significant effect is found in the remaining three countries. In the opinion policy literature introduced above, this model has been given very little notice, but the results of those where proximity of upcoming elections have been controlled for are negative (Brooks 1985, p. 258; Petry 1999, p. 546; Petry & Mendelsohn 2004, p. 520).

Hence, while the underlying logic behind the political business cycle is quite persuasive, its empirical record is rather tarnished, a fact that has led some scholars to abandon the theory altogether (Golden & Poterba 1980; Lewin 1991, Chapter 3). However, since the theory is based on assumptions rather similar to those underlying the studies reviewed above, we should, I believe, hesitate to accept this conclusion without further theoretical and empirical examination. In particular, the following options deserve further exploration.

First, macro economic factors such as unemployment, inflation, real disposable income, and GNP are all quite difficult for politicians to manipulate in the short term and would likely be quite costly to manipulate in the longer term. This preoccupation with economic policies may account for some of the negative findings (cf Schultz 1995, p. 91). In fact, a comprehensive study of local Danish politics has found a rather strong election cycle effect when policy is measured by indicators under more direct control by the politicians such as annual changes in local spending or local revenues (Mouritzen 1991, chap. 14).

Second, in light of the discussion above, it may be counterintuitive to expect that rational politicians always want to manipulate economic factors such as unemployment and inflation. Writing in the late 1970s, a period characterized by high inflation and high unemployment levels in many Western countries, it may have seemed reasonable to expect such policies to be persistent and prominent concerns for re-election minded governments. However, inflation and unemployment levels are not constantly highly

ranked on the political agenda, and in times where both are relatively low, governmental energy and money in election years may be better spent on other issues ranked higher on the political agenda.

This discussion points to the following two revisions. First, on policies under more direct control of the incumbent government, the opinion policy relationship may be stronger in election years compared to non-election years. Second, the opinion policy link on issues ranked high on the political agenda is particularly strong in election years compared to non-election years. In fact, we would not, from this perspective, expect an election cycle effect on issues ranked low on the political agenda. I return to this question in the following chapter.

### The importance of ideology

The importance of government ideology has received some attention within the opinion policy literature (see Page & Shapiro 1983; Brooks 1985; 1987; 1990; Petry 1999; Petry & Mendelsohn 2004). This variable has its own well-established literature known as the "politics matter" tradition, which basically investigates whether voting for certain governments instead of another has an independent effect on public policy.[6] The central hypothesis is that governments make decisions based on policy preferences instead of bureaucratic interests or public surveys. As a consequence, this model is often considered an alternative to the Downsian model, where policy is only a means to re-election (cf Wittman 1973; 1983). Nevertheless, the concept of ideology has in fact been examined in the opinion policy literature.

The prevalent approach explores whether governments of different ideological stripe respond differently to public majority opinions (Page & Shapiro 1983; Brooks 1985; 1987; 1990; Petry 1999; Petry & Mendelsohn 2004). However, neither the logic nor the outcomes of the empirical studies are straightforward. Why, for instance, would socialist governments in general be more responsive to public opinion than bourgeois governments? In order to make sense, public opinion (changes) or issues at least have to be divided into categories that match the ideological distinctions between succeeding governments. For example, liberal governments may, for various reasons, be more responsive to liberal opinion changes than conservative governments and vice versa. Furthermore, socialist governments might in general be more sensitive to public opinion on redistributive issues than non-socialist governments, since the former emphasize political or economic equality more than non-socialist parties (Brooks 1985, p. 257).

Somewhat surprisingly, given the lack of good theoretical reasons, Page & Shapiro (1983, p. 183) actually find that ideology exerts the strongest unconditional effect on the opinion policy relationship. In fact, this effect comes out

as the only significant estimate in their multivariate statistical analysis. However, the authors hasten to point out, quite convincingly, why this result is most probably unreliable and mainly spurious (ibid., pp. 183-4). Brooks (1987, p. 471), however, seems to find a similar result in his study of the opinion policy nexus in France, but does not report this effect in his study of four other Western countries. Of more interest, he finds socialist governments to be significantly more responsive to public opinion on redistributive issues than bourgeois governments in Britain and France (Brooks 1985; 1987), though he is not able to reproduce this result in his study of German politics (Brooks 1990, p. 521). Again, however, Brooks' findings are contradicted by the otherwise very similar studies of Petry and Mendelsohn. Petry (1999) finds conservative Canadian governments to be more responsive on redistributive issues than liberal governments, just as conservative governments overall, according to Petry & Mendelsohn's (2004, p. 523) analysis, are more responsive to public opinion than liberal governments.

In sum, this research leaves us with a rather vague impression of the importance of government ideology when studying the opinion policy link and the present study does not provide further enlightenment in this respect. As explained at greater length in the next chapter, in the ongoing debate between studies assuming either re-election or policy motives to be ranked first among political leaders, I join the former Downsian perspective at the expense of the assumption underlying the "politics matter" tradition. Nevertheless, in the empirical analyses reported in Chapters 5 and 6, I include a standard control for government color to account for the additive effect that may stem from different party rules.

### Summary

The literature discussed in this section has provided some evidence on dynamic conditions affecting the opinion policy relationship. The idea that citizens' opinions matter more the more salient they are to the citizens has received the most interest in these studies. A majority of studies provides some empirical support for this idea, but nothing near the level of support expected theoretically. One might argue that what we need is simply more empirical studies of this hypothesis. The same could be said about other dynamic conditions, such as the ideology of the incumbent government. The logic underlying the latter expectation, however, is not entirely clear, as argued above.

All in all, the most striking conclusion derived from this review of dynamic conditions is nevertheless that some of the most promising ideas have yet to be explored. In particular, the representational effects of Baumgartner and Jones' (1993) venue model have not yet been tested, but also

some pretty straightforward revisions of the political business cycle hypothesis deserve further theoretical and empirical exploration.

## Summary and conclusion

As mentioned by Weisberg (1976, p. 1), concern for the role of public opinion in public policy is probably as old as government itself. On the other hand, the tradition of making systematic empirical analyses of this phenomenon is fairly young. But it is not too young to be critically evaluated as an identifiable and well-defined empirical discipline. In this concluding section, I summarize the empirical evidence and assess theoretical and methodological problems in the opinion policy literature, as delimited it in this chapter.

Although the question about how much public opinion influences public policy is the most researched within the opinion policy literature, it is still difficult to answer it in more precise terms. Based on a balancing of the empirical evidence provided in this chapter and corroborated by reviews of a larger sample of empirical studies (Burstein 1998, 2003; Glynn et al. 1999; Manza & Cook 2002a, 2002b) it seems safe to conclude that public opinion does influence public policy sometimes, in some instances very much so, and in others hardly at all. However, whether this merely demonstrates a "more than minimal" responsiveness to public opinion (Glynn et al. 1999, p. 332) or "a substantial congruence between opinion and policy" (Burstein 2003, p. 34) is much less clear. Whatever the true estimate is, this evidence clearly suggests that public opinion is not just relevant from a normative democratic point of view, but is also an important determinant in public policy analysis.

Moving to the level of conditional explanations, we can qualify this general conclusion about responsiveness. First, as noticed by Bishin (2006), the idea that issue saliency increases responsiveness has almost achieved the status of a myth for many representation scholars. According to the evidence reviewed in this chapter, it has also gained some empirical leverage, most notably in static modelling of issue saliency in studies of the thermostat model and in the agenda setting inspired dynamic studies of this concept. However, in light of the theoretical expectations, the explanatory power of this determinant has been rather disappointing.

A similar conclusion applies to the idea of a political business cycle effect. Despite a persuasive hypothesis, the empirical evidence has not been adequately supportive. As suggested earlier in this chapter, part of this result might stem from a misapplication of the hypothesis. Combining the idea of a political business cycle with an elaborated model of the policymaking process, I intend to explore this question further in the next chapter.

Finally, this chapter has examined an array of institutional, fiscal, and political conditions that, according to the literature, might influence the

degree of responsiveness. The logic behind some of these conditional hypotheses is more persuasive than that of others and the empirical evidence often seems somewhat contradictory. However, the number of systematic research papers on this topic is very limited and the evidence, consequently, not solid enough to make inferences about the empirical validity of the hypotheses. Therefore, more empirical research on this important question is indeed needed.

With respect to theoretical development, the following improvements seem particularly warranted. As noted in the first part of this chapter, many studies have been very explorative with only a vague reference to very general theoretical positions. If one is only concerned with the degree of responsiveness, this lack of theoretical clarity and specification might be defendable, but if we are to move beyond this simple question and into the realm of causal explanation and contingent relationships, we need the guidance of a clear and explicit theoretical model. In addition, the benefit of starting from a contingency perspective is that the evident variation revealed in this chapter is treated as an outcome to be explained, rather than as a simple deviation from a more important master trend (cf Manza & Cook 2002b, p. 652). An obvious starting point for the development of such a theoretical model, it has been argued in this chapter, is Baumgartner and Jones' (1993) agenda based venue model. What they offer is a model of the policymaking process that, when combined with a more explicit model of the voters and the preferences of central policymakers, points to a set of conditions under which public opinion impacts public policy.

The methodological deficiencies partly reflect the theoretical problems. First, central operational measures often seem to be a consequence of availability rather than theory. Pragmatic decisions are of course unavoidable in empirical research, but without an explicit theoretical framework, it is impossible to evaluate how reasonable these choices are. Second, research has probably been much less cumulative than necessary owing to a lack of standardized and comparable measures of public policy. Whereas public expenditure, as noted by Burstein (2003, p. 38), represents a fairly intuitive and comparable measure of public policy, other measures are often developed on an ad hoc basis and can hardly be compared outside the single-case study. In recent years, however, a growing consensus around the use of public spending measures seems to crystallize within the opinion policy literature, which is beneficial for the accumulation of knowledge. The potential drawback of this trend is the risk that some important aspects of the opinion policy relationship might not be captured by spending decisions. Third, as pointed out in the few comparative opinion policy studies, we need more non-US

studies to improve our conditional evidence of democratic responsiveness, an area of research that seems to be improving lately. Fourth and finally, we need more multivariate statistical tests, either to reduce the risk of spurious relationships or to measure the conditional/partial impact of public opinion on public policy. Recent years have also seen some improvements in this respect, along with the fact that many relevant time series have now reached a point where it makes sense to apply more sophisticated statistical methods.

Summarizing this assessment of the state of the opinion policy literature, in a few words, the impression is a quite viable and steadily maturing research field where the agenda for further improvements seems both clear and feasible. In the next chapters, I focus on the following questions raised in this discussion of earlier research.

Theoretically, inspired by Baumgartner and Jones' agenda based venue model I set forth in Chapter 3 a model, which aims at explaining variation in the impact of public opinion on public policy. In particular, this chapter pinpoints the central representational attributes of the macropolitical venue including an explicit model of the central policymakers and a model of the median voter. It furthermore integrates the idea of political business cycles with the model developed by Baumgartner and Jones. Finally, in the last part of Chapter 3, the basic model is extended with some of the conditional effects pointed out in this chapter, including the constraining effects of local autonomy, fiscal costs, and organized interests.

Methodologically, the model's empirical validity is evaluated in multivariate statistical time series analyses reported in Chapters 5 and 6. The policy variable is a measure of public spending decisions, while public opinion is measured in surveys asking citizens about their spending attitudes. The sample consists of annual observations on seven Danish policy issues from 1980 to 2003. The present study does present some comparative evidence by researching the opinion policy link in a non-US country, but since it is a single-country study, it offers no systematic evaluation of comparative institutional cross-country dynamics.

In Chapter 7, the causal relationship between the variables of core interest is explored in more detail, including a discussion of how the present study relates to the Thermostat Model developed by Christopher Wlezien.

## Notes

1. Reviews of the opinion policy literature are found in Burstein (1998; 2003), Glynn et al. (1999), Manza & Cook (2002a; 2002b), Page (2002) and Weakliem (2003).
2. Introductions to the "new institutionalism" approach are found in Hall & Taylor (1996), Immergut (1998), Peters (1999).
3. Wlezien (2004, p. 21) does acknowledge that public saliency may also increase or decrease over time, although he in a footnote on p. 16 reports that he did not find any effect when controlling for a standard time-variant measure of public saliency. I return to this question about public saliency later.
4. See Boyne (1996, Chapter 4) for an introduction to this literature.
5. The study referred to by Burstein is Jones (1994, Chapter 5). In fact, also Soroka (2003) and Wlezien (2004, 2005) test salience hypotheses within a statistical research design, but still, Burstein's conclusion from 2003 largely applies.
6. See Hibbs 1977; Castles 1982; Sharpe & Newton 1984; Smith 1996; Imbeau et al. 2001.

# Chapter 3

# A model of conditional responsiveness

The aim of this chapter is to develop a theoretical model that can explain variation in the impact of public opinion on public policy. The model is developed in three stages. First, I present a model of the organization of public policymaking that encompasses a model of central decision-makers and one of the so-called median voter. Second, on the basis of this model two main propositions are deduced about the conditional impact of public opinion on public policy. In the last part of the chapter, I discuss how this basic model may be extended further, adding constraints on the behavior of the central policymakers. Theoretically, the chapter draws on ideas put forward in a number of the studies referred to earlier, on insights from rational choice theories about party competition, and on literature on voting behavior.

## The macropolitical venue

The political system as such, including elected politicians, bureaucrats, privileged interest groups, experts, and other policymakers, has a tremendous policymaking capacity. Every year it produces thousands of decisions with wide-ranging consequences for various aspects of people's lives. However, no policymaker or political decisionmaking unit has the capacity to deal simultaneously with all alternatives and all issues that arise at any given time. To prevent overload and continual breakdowns, policymaking is subject to a significant degree of division of labor.

The recognition of such systems of limited participation is not a new discovery and many scholars have commented on the multiplicity of policy making venues characteristic of modern government (e.g. Schattschneider 1960; Redford 1969; Cobb & Elder 1983; Ostrom 1986; Thurber 1991; Baumgartner & Jones 1993). In this chapter, I adopt the simple analytical as well as empirical distinction between subsystem venues and macropolitical venues (see Baumgartner & Jones 1993). The existence, the functioning and the policy effects of subsystem politics are very well documented (the literature is massive, but see e.g. Heclo 1978; Jordan 1981; Williamson 1989; Thurber 1991; Rhodes & Marsh 1992; Börzel 1998; Jordan & Maloney 1997). As pointed out in Chapter 2, we know much less about the systematic effects of non-subsystem politics, i.e. macropolitics. As also noticed in Chapter 2, however, intervention of macropolitical institutions might have some important consequences for the impact of public opinion on public policy.

In this section, I outline the central characteristics of the macropolitical venue and a model of the central decision-makers in this venue. To illustrate

its distinct attributes, the macropolitical venue is described by a comparison with the attributes of subsystem venues. The comparison is narrowed down to those aspects where the two types of venues differ most significantly in respect to the question of the impact of public opionion on public policy. These aspects are (1) central policymakers, (2) primary goals of central policymakers, (3) public visibility of policymaking, and (4) the policy agenda of the venue.

It may be argued that subsystems are many things and that their institutionalization varies significantly, not only across countries but also across policy domains within countries. This is perhaps most clearly illustrated by the literature on subsystem or network typologies (see Rhodes & Marsh 1992; Börzel 1998). On the other hand, as pointed out by Jordan and Maloney (1997, pp. 557-8), whether we call them policy communities, policy subsystems, policy monopolies, subgovernments, etc., these structural arrangements all share certain common features. In the present context, we focus on these similarities in order to isolate the peculiarities of the venue of real interest, the macropolitical venue.

*Central policymakers in the macropolitical venue*

Subsystems are dominated by specialized actors such as policy experts, interest groups, lobbyists, bureaus and other administrative operating agencies, ministerial officials, and in addition perhaps their counterpart congressional or parliamentary committee structure (cf Redford 1969, p. 83; Campbell 1989, p. 5). As mentioned above, the exact structure and range of participants in subsystems may vary considerably across issues, but they are all concerned with a particular area of program specialization (cf Jordan & Maloney 1997).

According to Redford (1969, p. 107), a distinguishing factor between subsystems and the macropolitical system is the breadth of involvement. To illustrate this point, Redford (ibid.) notes that:

> When the policies of the police department break on the front pages of newspapers and into discussions in the city council, when issues of higher education engage the attention of the governor and of those who apportion funds among state functions, and when the affairs of national intelligence agencies erupt into public discussion or the President gives his attention to large foreign and domestic issues, then we have politics in the wider arena defined here as macropolitics.

It follows from this quote that the system of macropolitical and subsystem venues most likely replicates itself at local, state, and national levels of policymaking. In this study, we focus on the national level of policymaking. How-

ever, with a corresponding identification of macropolitical actors within local policymaking systems, the basic model should be applicable to these processes too.

At the national level, the presidency and the floor of Congress, or in parliamentary systems the floor of Parliament, which is often denoted the parliamentary arena, are central macropolitical venues. Consequently, the president, prime minister, party leaders, and other top politicians are the dominant policymakers in macropolitical venues (cf Redford 1969, p. 108; Baumgartner 1989, p. 45; Baumgartner & Jones 1993, p. 22). These macropolitical actors are all affiliated with certain political parties, and hence political parties constitute very important actors in the macropolitical venue. This does not preclude legislators or political party staff members from focusing their individual efforts on certain specialized subsystems and interact with policy experts, but in the macropolitical venue, in contrast to subsystem venues, national political parties are the dominant actors.

Having defined national political parties as the central policymakers in the macropolitical venue, we move on to a discussion of the objectives of these policymakers.

*Goals of central policymakers in the macropolitical venue*

According to the literature on competitive party behavior, political parties can pursue different objectives (cf Strøm 1990). Downs (1957, p. 28) claims that political parties act solely to maximize their votes in order to reap the rewards of holding office. This means that political party leaders are assumed to be indifferent to policies. Or as claimed by Downs (1957, p. 28), "parties formulate policies in order to win elections, rather than win elections in order to formulate policies".

The most prominent alternative to this claim is the policy-seeking party often associated with the work of Wittman (1973), or more broadly, with the "politics matter" tradition mentioned briefly in Chapter 2. In its most orthodox expression (Wittman 1973, p. 490), this model assumes that "political parties are solely interested in policy and that winning the election is just a means to that end".

With reference to Mayhew (1974, p. 16), who argues that the electoral goal is the one that must be achieved over and over if other ends are to be entertained, I assume that re-election is the primary goal of political parties. Contrary to Mayhew (and Downs), however, I do not treat political parties as though they were solely vote seekers, but vote seeking is assumed to be the primary objective of the policymakers who dominate the macropolitical venue. Consequently, these policymakers are assumed to assign low priority to the advance of other goals if such activities threaten their principal goal of vote

seeking. On the other hand, if vote seeking is not at risk, other objectives are pursued, including policy oriented preferences or self-serving policies designed to serve the special interests of a government clientele. We do not need a full specification of these other objectives. The important point is that they will most likely deviate from the policy preferences of the mass public. The latter claim can be defended in various ways, but it suffices to assume that the policy preferences of the median party member or the policy preferences of subsystem policymakers often deviate from those of the median voter.

*Public visibility of the macropolitical venue*

The public visibility of decision-making may vary across subsystems, but as noted by Jordan and Maloney (1997, p. 558), it seems a general characteristic that the public profile of subsystem policymaking is low (see also Baumgartner 1989, p. 45; Baumgartner & Jones 1993, p. 18). Possibly some periodicals, the trade press, or other specialized groups of journalists pay constant attention to a given subsystem venue (cf Redford 1969, p. 83; Baumgartner & Jones 1993, p. 65). However, in times of normal subsystem politics, overall media attention is low and the few articles that are published probably support the existing policies and the prevailing view on the problems of interest (cf Baumgartner & Jones 1993, Chapter 6).

The macropolitical venue is different. In his seminal book from 1922, *Public Opinion*, Walter Lippman in his chapter about "the nature of news" gives the following characterization of news media coverage (1922 [1997], p. 214):

> Newspapers do not try to keep an eye on all mankind. They have watchers stationed at certain places, like Police Headquarters, the Coroner's Office, the County Clerk's Office, City Hall, the White House, the Senate, House of Representatives, and so forth.

According to this quote, news is not a mirror image of social conditions, but depends disproportionately on activities within certain focal institutions. A more systematic treatment of this media attribute is found in Bennett's (1990) article about the "index-hypothesis". According to Bennett's study of press-state relations in the US, media coverage rises and falls as if "indexed" to the tides of congressional debate. If political parties engaged in a debate or discussion about a given issue, then the ratio of media coverage on that given issue increased. If the congressional debate slowed down, so did the media coverage of that particular issue (p. 119).

Apparently, this linkage between political debates in the American Congress and news coverage by leading American media outlets can be general-

ized to parliamentary systems. In his study of education politics in France, Baumgartner (1989, p. 162) concludes that:

> People who would not have been aware of an issue can be made aware of it during the parliamentary debate because of the media coverage that it can generate. The Parliament is therefore a prime vehicle for the expansion of a debate to the general public and is responsible for putting many items on the general political agenda.

This observation appears to be corroborated by a recent study of the national Danish Parliament, where Jensen (2002, Chapter 7) finds that, contrary to meetings in standing committees and working groups, activities on the floor of the parliament are entirely public activities often monitored and reported by media professionals.

Hence, the two types of venues differ markedly with respect to public visibility of decision-making. While normal subsystem politics is carried out in relatively closed circles characterized by a low public profile, activities in the macropolitical arena are generally widely reported in the press. Consequently, the visibility of policymaking in the macropolitical venue is relatively high compared to that of subsystem venues.

### *The agenda of the macropolitical venue*

From the literature on agenda setting, we adopt the term *agenda*. As noted by Dearing & Rogers (1996, p. 1), every social system must have an agenda if it is to prioritize the problems it faces. Consequently, every policymaking venue has an agenda, in this context defined as a ranking of the relative importance of the issues relevant to the given venue (cf Soroka 2002, p. 6). As implied by this agenda concept, the number of issues is always restricted and issues are always ranked in terms of their importance to the policymakers in a given venue at a given point in time (cf Dearing & Rogers 1996, p. 2). However, despite this common feature of all venues, some conspicuous differences also exist between subsystem venues and the macropolitical venue.

The agenda of a given subsystem is always rather specialized and limited. For instance, policymakers in a defense subsystem venue cannot suddenly begin to make decisions about public schools, just as problems relating to national defense are irrelevant to a subsystem venue focusing on educational policy. Degree of specialization may vary across subsystems, but characteristically, they are all somewhat functionally constrained by the subject they were created to deal with originally (cf Baumgartner & Jones 1993).

The macropolitical venue is different. Although the policymaking capacity of this venue is also restricted at any given point in time, the number of issues of potential relevance to this venue is vast compared to subsystem ven-

ues. For instance, very diverse issues such as defense, health, and education politics may all become subject to decision-making within the macropolitical venue. Therefore, while some kind of subsystem probably exists for every major activity of the government, as pointed out by Redford (1969, p. 102), almost every major activity most probably also gets attention in the macropolitical venue at a given point in time. Consequently, and in accord with the agenda concept, it appears reasonable to talk about the degree of importance of an issue to policymakers in the macropolitical venue instead of making a dichotomous distinction between important and unimportant issues.

Please note, furthermore, that the introduction of the agenda concept bridges part of the discussion about cross-sectional versus dynamic representational effects laid out in the previous chapter. As the agenda concept implies, the number of issues is always ranked according to their importance to the policymakers and this ranking may stay constant for some time, perhaps for years. However, as may be recalled from Dearing & Rogers' (1996, p. 8) definition quoted in Chapter 2, agenda setting research is the study of the rise and fall of issue salience over time (see also Soroka 2002, p. 5). Therefore, the composition of the agenda of the macropolitical venue is likewise expected to change significantly across time.

*Summary*

In this section, central attributes of the macropolitical venue were defined in a comparison with central attributes of subsystem venues. The similarities of subsystems were deliberately emphasized, while the many underlying differences between subsystems were ignored. The purpose of this comparison was to highlight a set of peculiar characteristics of what I term the macropolitical venue. Table 3.1 summarizes, in stylized form, the main points of this comparison.

In several ways, as Table 3.1 clearly shows, the macropolitical venue is an almost inverse picture of standard subsystem venues. The dominant vote-seeking objectives, the relatively high public visibility, and the agenda characteristics indeed hint at a link between the macropolitical venue and the opinion policy relationship. Before we can deduce some more systematic propositions about this linkage, however, we need a model of the voters. But first we need to address a few important questions about the relationship between the macropolitical venue and subsystem venues.

Table 3.1. Central characteristics of the macropolitical venue

| | Macropolitical venue | Subsystem venues |
|---|---|---|
| Central actors | *The prime minister, the president, party leaders, political parties* | *Bureaucrats, pressure groups, policy experts, and policymakers with a special interest in the policy domain* |
| Primary goals of central actors | *Vote-seeking objectives* | *Others than vote-seeking* |
| Public visibility of policy making | *High* | *Low* |
| Policy agenda | *Broad and general* | *Narrow and specialized* |

## The connection between macropolitics and subsystem politics

The literature on agenda setting is awash with studies of how and why policy agendas change. No simple explanation has been crystallized, however, and it appears that changes to the agenda are often results of a complex mix of reality and perceptions, level of conflict in existing subsystems, new ideas, new policy images, clever policy entrepreneurs, focusing events, media coverage, national mood, expired legislation, technical innovations, etc. (Schattschneider 1960; Cobb, Ross & Ross 1976; Walker 1977; Cobb & Elder 1983; Baumgartner 1989; Stone 1989; Baumgartner & Jones 1993; 2002; Kingdon 1995, Birkland 1997; Soroka 2002). This complex picture clearly suggests that agenda changes are not just a simple an automatic effect of, for instance, social conditions or the distance between public policies and the preferences of the mass public.

On the other hand, the literature has paid very scant attention to the systematic policy consequences of these agenda changes (cf Baumgartner & Jones 1993, p. 12). Questions about "how" and "why" agendas change are indeed important, but from a policy perspective they seem all the more important when we feel confident that the phenomenon of agenda change actually asserts a systematic effect on public policies and, in the present

context, a systematic effect on the representation of citizens' attitudes. In this study, I confine my focus to the systematic policy consequences of variation in the agenda of the macropolitical venue. From this perspective the important outcome of agenda setting studies is that most policy issues move up and down on the agenda of the macropolitical venue.

Regarding the power relationship between subsystems and the macropolitical venue, I assume a hierarchical relationship with the national macropolitical venue placed atop of the hierarchy (see also Redford 1969, Chapter V; Thurber 1991, pp.320-22; Baumgartner 1989, Chapter 3). As demonstrated convincingly by Baumgartner & Jones (1993), macropolitical actors have powerful tools to restructure, modify, or even disrupt various subsystems. It is not necessarily a zero-sum relationship where increased macropolitical activities around an issue preclude subsystem activities, but the outcome in terms of representation is expected to be determined by the tides of the macropolitical agenda. Later, however, I relax this assumption when discussing how the introduction of constraints may alter the main predictions generated from the basic theoretical model.

## The median voter

It can be argued that in a study of voter impact on public policies, what really counts are the perceptions of voters among politicians rather than a description of real voter behavior (cf Arnold 1990, p. 11). Since politicians with a very inaccurate perception of voters are not likely to survive long in politics, it does, however, seem justified to inform our main assumptions by the empirically based literature on public opinion and voter behavior. I therefore adopt the latter strategy.

In accord with the opinion policy literature introduced in the previous chapter, I am concerned with aggregate aspects of public opinion known as mean, median, majority, plurality opinions, etc. (see Page & Shapiro 1992, p. 17). For consistency I use the term median voter opinion throughout this section when referring to this aggregate public opinion. As a consequence of the focus on aggregates, variation in individual opinions and voting behavior is ignored.

Three aspects of the median voter are important to the theoretical argument: (1) the existence and stability of median opinions, (2) the saliency of median opinions, and (3) how these two characteristics affect the voting decisions of median voters.

### The median voter opinion

First, the median voter does form opinions on public policies. Occasionally, new policy issues will appear where a firm opinion has not yet crystallized,

just as it may be hard to identify a clear median opinion on existing but very remote and distant policy issues. In that case, where only latent opinions exist (cf Key 1961, p. 263) the model developed in this chapter does not have much to offer and one would need more sophisticated explanations, such as Arnold's (1990) modeling of the policy effects of opinions not yet formed. However, according to Page & Shapiro's (1992) comprehensive analysis of more than 10,000 opinion surveys, we are not really short of issues where a measurable and rather firm median opinion exists.

Second, contrary to the fluctuations and sometimes, it seems, random movements in individual voter opinions, aggregated public opinions are normally highly stable (Page & Shapiro 1992; Kinder 1998, p. 797; Monroe 1998, p. 10). If the median opinion was in fact characterized by capricious fluctuations, then it would send a very unreliable signal to the political leaders, and hence it would be unlikely to assert any systematic influence on public policies. Instead, the stability of the median opinion suggests that individual fluctuations are cancelled out by statistical aggregation (cf Page & Shapiro 1992, p. 14), implying that the signal from the aggregate public is clear and stable on most issues. Identifiable and understandable movements in median opinions are not a problem to the theoretical model developed in this chapter. The important thing is that the median voter forms opinions about policies and that these opinions on many issues provide a clear and accessible signal to the policy makers.

### Saliency of median voter opinions

Median voter opinion is assumed to consist of the two central components direction and saliency (see Weisberg 1976, pp. 15-16; Arnold 1990, p. 17; Geer 1996, p. 14). The former of these two components, direction, was introduced above and it refers in this context to the assumption that there is a median voter opinion on many issues. The latter, saliency, is one of those words used so frequently that very few have found it necessary to provide a clear definition. Generally, however, political scientists seem to use the word 'saliency' as a reference to the importance individuals ascribe to certain issues (cf Wlezien 2005, p. 557). Within the opinion policy literature, saliency has mainly referred to the importance voters attach to different issues. Furthermore, importance has often in this context been measured by the visibility of a given issue as reflected in the following operational definition by Glynn et al. (1999, p. 300):

> By 'salient' we mean widely visible to the public, particularly in the mass media, or felt directly by people and their families and friends, as a poor economy might be.

In this perspective, the public is assumed to form opinions on a broad range of issues, but only a few of them may be widely visible, or salient, to the public at a given point in time. As pointed out by Jones (1994, p. 74), saliency and visibility/attention is not the same thing since one can be attentive to an opinion without this opinion be salient in an actual choice situation. However, as also argued by Jones (ibid.), empirically it is highly likely that the two are closely connected and indeed visibility may be the best cue for politicians trying to estimate the public salience of a given issue. Hence, in a more fine-grained psychological literature, these words may mean something different, but in this more bold and aggregated opinion policy context we simply see them as equivalents.

Two characteristics of the median voter are important in this respect. First, the median voter generally devotes very little attention to politics (Jones 1994, p. 9; Kinder 1998, p. 785). When forced to do so in questionnaires, he is able to express an opinion on a broad range of issues, but when it comes to the number of salient policy issues, the agenda of the median voter is narrowed down to a very limited set of issues at a given point in time (cf Dearing & Rogers 1996, Chapter 3). Hence, a similar agenda concept as the one used above can be applied to the median voter's ranking of issues in terms of saliency.

Second, movement in the median voter's attention to politics is assumed to be crucially dependent upon mass media coverage. If there is a common truism in the agenda setting literature the best candidate is Cohen's (1963, p. 113) classic conclusion that, "while the media cannot tell the public what to think, they can have a great impact on what the public think about" (cf McCombs & Shaw 1972; Funkhouser 1973; Miller, Goldenberg & Erbring 1979; Erbring, Goldenberg & Miller 1980; Iyengar & Kinder 1987). The vast majority of government policies goes unnoticed by the median voter and he generally shows a remarkably lack of knowledge about even simple political questions (cf Delli Carpini & Keeter 1996). However, what the median voter does notice and attend to is primarily a result of mass media coverage. This finding is also well documented in the literature on priming (Krosnick & Kinder 1990; Krosnick & Brannon 1993; Iyengar & Simon 1993).

### The median vote

The characteristics of the median voter emphasized above might be irrelevant to the re-election calculus of political leaders if they do not affect the median voter's decision on what to vote. In the following, I point to three findings within the literature on voting behavior that link the attributes described above with the decision to vote, and consequently, with the re-election cal-

culus of political leaders. The three relevant findings are issue voting, retrospective voting, and myopic voting. I discuss each aspect in turn.

First, the issue voting hypothesis basically states that voters are influenced by issues, and that issues are important determinants of election outcomes (cf Borre 2001, p. 12). Election outcomes indeed have multiple causes and, according to the overwhelming literature on this subject, issue voting is only one determinant (for a review, see Kinder 1998, pp. 835-843). Other important determinants are class voting or party loyalty, where the voter is expected to be continuously attracted to a specific party. If the electorate votes out of habit, social inclinations or adherent party loyalty, issue-oriented competition for votes may not be very beneficial to political parties.

Apparently, the phenomenon of class-voting or persistent party loyalty have not disappeared (see Evans 1999; Kinder 1998, p. 837), but several scholars have pointed to a substantial decline in class voting and a decrease in party loyalties over the last decades (Carmines 1991; Dalton et al. 2000; Dalton 2002). At the same time there is a growing consensus that issues play an important role in modern elections (cf Page & Shapiro 1992, p. 8), not least because the decisive median voter often will be among the so-called swing-voters positioned in the middle of the ideological spectrum, where specific party loyalties are weakest.

The literature on issue voting, however, has presented diverse approaches to this phenomenon. The standards for voting to qualify as issue voting set by Campbell et al. (1960, Chapter 8) in their seminal book *The American Voter*, were rather high, and as a consequence they found little empirical evidence of issue voting. Based on a more modest set of requirements, Key (1966) countered this pessimistic conclusion of Campbell and colleagues. According to Key (1966, p. 7), "voters are not fools" and the electorate as such is moved by concerns about central and relevant questions of public policy. It is especially the many "switchers" that move across party lines at each election that lead Key (1966, p. 151) to conclude that voters are not just party stalwarts whose preferences hinge on the leaders of their own parties. Over time, Key's view on issue voting has become increasingly accepted (cf Page & Shapiro 1992, p. 8; Dalton 2002, p. 196).

Inspired by Jones' (1994) model of preference activation introduced in Chapter 2, issue voting in the present context simply means that the median voter evaluates the incumbent government on the relatively few issues he perceives as most salient on election day. The modest requirements in this model correspond to what we know about the widespread political ignorance in the electorate (cf Delli Carpini & Keeter 1996). Therefore, instead of representing the outcome of a very comprehensive act of ranking, comparing,

and selecting across a broad range of issues and policy attitudes, election results are assumed to be the median voter's evaluation of the government on the few issues he perceives as most salient on election day.

The second assumption is that the median voter evaluates political performance. This touches upon a discussion in the election literature about whether voting is retrospective or is mainly based on forward looking expectations (see Downs 1957, pp. 103-109; Popkin et al. 1976; Fiorina 1981; Mattei & Weisberg 1994). The literature on this subject has produced strong evidence in support of the hypothesis of retrospective voting, implying that people base their vote on an assessment of the performance of the incumbent government (for reviews, see Nannestad & Paldam 1994; Kinder 1998). As Downs (1957, pp. 103-109) argued, the two decision rules may be closely linked in that even if policy proposals do enter the voting calculus, it seems reasonable to expect that the credibility of these promises of future action increases significantly if they can be supported by reference to former government initiatives. Combined with the concept of issue voting the assumption is that the median vote is based primarily on the policy performance of the incumbent government on those issues the median voter perceives as most salient on election day.

Finally, the median voter is assumed to have a decaying memory of past events, as emphasized by the literature on political business cycles introduced in Chapter 2. This idea about myopic voting is well established in the literature on VP-functions, where most effects seem to decay within one year (for a review, see Nannestad & Paldam 1994). It has also gained strong, albeit more indirect, support from the literature on priming (Krosnick & Kinder 1990; Krosnick & Brannon 1993; Iyengar & Simon 1993). In short, the priming theory posits that citizens generally focus only on the aspects of their knowledge that happen to be most accessible at the time of judgment (Krosnick & Kinder 1990, p. 499). Therefore, political performance in election years weights higher in the median voter's evaluation than performance in non-election years.

*Summary*

How the median vote is cast is assumed to be affected both by the direction of the median voter's opinion and by his attention to various issues. While the former is expected to be rather stable, the latter may vary significantly over time, corresponding quite closely with variation in mass media coverage. Furthermore, the distribution of visible issues in election years matters more to how he votes than the distribution in non-election years.

The last link in the policymaking model has now been introduced. The second part of this chapter combines the individual components into a single

model, from which a set of propositions about democratic responsiveness can be derived.

## A model of conditional responsiveness to public opinion

The first part of this chapter stressed the following characteristics of the macropolitical venue. First, vote seeking is a primary objective of national macropolitical actors. Second, their activities and actions are very visible to the general public. Third, they have the power to adjust policies if they want. The median voter, on the other hand, is assumed to base his vote on the most visible issues. Furthermore, he is expected to evaluate the deviation between actual policy and his preferred policy on only the most visible issues. Finally, he is expected to weigh recent information higher than information obtained in a more distant past.

A combination of these assumptions leads to the following reasoning. If the median voter casts his vote based on an evaluation of the incumbent government's political performance on the set of most visible issues, then deviation on those issues from the policy attitudes of the median voter will be associated with electoral costs. Consequently, deviation from median voter opinion on these issues now affects the electoral calculus of the macropolitical actors and, since these policymakers are primarily vote seekers, they are expected to accommodate their policies to fit a more popular position. The immediate implication is that the higher an issue is placed on the macropolitical agenda, the more strongly macropolitical actors will seek to narrow the gap between public opinion and public policy on that given issue. This is a fairly straightforward prediction hinted at by earlier research (see Schattschneider 1960; Redford 1969, Chapter V; Baumgartner & Jones 1993; Jones 1994, Chapter 5). However, as pointed out in Chapter 2, it is a prediction that has hardly ever been explicated and never really been tested systematically.

Although straightforward, the proposition implies a set of more specific questions that need to be addressed. First, there may be a static as well as a dynamic effect of macropolitics. Averaged over a span of years, some issues may rank higher on the macropolitical agenda than others. It follows from the assumptions, as pointed out above, that popular policy adjustments on average will be more likely on high-agenda than low-agenda issues. What is less clear, however, is the functional form of this relationship. In particular, should we expect the relationship to be linear or is it characterized by threshold effects? Does the vote seeking calculus of macropolitical actors only involve, for instance, the five most salient issues in the macropolitical venue or do all levels of macropolitical intervention exercise some kind of influence on public policy making?

This question seems even more relevant when we turn to the dynamic effects of changes in the macropolitical agenda. Imagine that an issue's share of the macropolitical agenda moves from 2 to 4 %. Does this have a similar effect on public policies as would a move from 10 to 12 %? One could argue both yes and no. On the one hand, if it already accounts for 10 % of the macropolitical agenda, a 2 % increase may not alter the calculated loss or gain of votes significantly. Furthermore, even when increasing its share from just 2 to 4 %, the issue is still not a major one on the macropolitical agenda, and hence may not constitute a major electoral threat. On the other hand, every change in the composition of the macropolitical agenda might adjust the probabilities of electoral outcomes, and hence the vote seeking calculus of the macropolitical policymakers. Whereas the former line of reasoning would suggest threshold effects, the latter implies a simple linear prediction.

The theoretical model described above is not sufficiently fine-grained to guide us in this respect. Furthermore, as shown in the previous chapter, the existing literature has yet to provide a solid basis from which we can derive such specific hypotheses about the relationship between the macropolitical venue and public policy. Looking at regulatory issues, Baumgartner and Jones (1993) stressed the punctuated nature of macropolitical intervention, but as their later studies clearly reveal, this is just part of a broader picture where policy changes can be both small, moderate and fairly large (see Baumgartner & Jones 2005). Therefore, the question is best decided by comparing the empirical fit of models with different functional forms. In Chapters 5 and 6, I explore this question further. For now, I proceed with the simple linear assumption, which has it that popular policies on a given issue are more likely the higher the issue is ranked on the macropolitical agenda.

Another but closely related question concerns whether the representational effects of macropolitics are symmetrical. If increased macropolitical attention raises the probability of popular policies, does lower macropolitical attention then lead to a widening of the gap between median voter opinion and public policy? In the first part of this chapter, political parties were assumed to pursue objectives other than vote seeking. Furthermore, these objectives and the objectives of specialized subsystem actors were assumed to differ significantly from median voter attitudes. How much they differ may vary across issues and policy domains, but the general assumption is that a gap always exists. Increased saliency in the macropolitical venue is therefore expected to narrow the gap between public policy and public opinion, while decreased saliency in the macropolitical venue is expected to widen it, implying a symmetrical effect.

We can sum up the main implications of this discussion in the following core proposition.

> P1: The larger the share of the agenda of the macropolitical venue devoted to issue m, the more likely are popular policy adjustments on issue m.

We now turn to a discussion of political business cycle effects. Please recall from the literature review in Chapter 2 that the empirical evidence of a political business cycle has not been promising so far. Furthermore, it was suggested that part of this poor empirical record might stem from disaggregation from the concept of political agendas. Based on the model introduced in this chapter, we can now argue more systematically for this suggestion.

The political business cycle hypothesis enters the model via the assumption of myopic voting behavior, which holds that the median voter weights *recent* political performance higher than that of a more distant past. For an incumbent party, this attribute raises the electoral stakes of policy deviations when election draws near. Combined with the assumption of issue voting introduced above, the median voter is expected to evaluate government policies mainly on those issues most visible to him on election day. Furthermore, in combination with the other set of assumptions this suggests an interaction effect, where an issue's share of the macropolitical agenda exerts a greater impact on the opinion policy relationship in election years compared to non-election years.

While this multiplicative effect follows immediately from the theoretical model outlined above, there is no firm reason to expect a direct and unconditional political business cycle effect upon the opinion policy relationship. If an issue is low on the macropolitical agenda, and hence not very visible to the public, governmental energy and resources in election years may be better spent on other and more important issues in terms of electoral gains.

Again, when concerning the more specific aspects of the business cycle effect we can imagine different scenarios where the literature or our theoretical model does not firmly guide us. For instance, will the multiplicative effect complement or substitute the direct effect of macropolitics discussed above? Or stated differently, is the force of variation in the macropolitical agenda strong enough to exert both a direct and a multiplicative effect, or will it show up only when election is very close? Theoretically, our model does expect the macropolitical agenda to exert a strong impact on the opinion policy relationship, but we do not know beforehand just how strong it will be. Discussing various constraints on macropolitical actors, as we do below, may provide a more accurate anticipation of when and where a strong effect

might occur, but at this stage of theory development the question can only be decided by empirical examination. The discussion of political business cycle effects leads to the following proposition.

> *P2*: The closer the next election, the more likely that a larger share of the macropolitical agenda devoted to issue m results in popular policy adjustments on issue m.

This and the proposition derived above constitute the main implications of the basic theoretical model outlined in the first part of this chapter. They are compatible with many ideas explored in the literature discussed in Chapter 2, but they also add to these by explicating the effect exerted by the macropolitical agenda on the relationship between public opinion and public policy, and by integrating the idea of political business cycles in this agenda based model of public policymaking. Contrary to much of the opinion policy research discussed in Chapter 2, which often examine the policy effects of voter opinions without further theorizing about the existence of different policymaking venues, the model developed in this chapter anchors responsiveness in a certain venue, that is, in the macropolitical venue. Furthermore, the model extends the policy prediction of earlier research on subsystems and macropolitical systems (Thurber 1991; Baumgartner & Jones 1993). When macropolitical actors intervene in public policymaking, policy does not just change. It changes in the direction preferred by a majority of the voters.

Before continuing with an empirical test of these main propositions, I conclude this chapter with a reflection on how this theoretical model can be extended and refined in light of some of the constraining conditions emphasized in the opinion policy literature discussed in Chapter 2.

### *Constraints on macropolitical actors*

Constraints on behavior may be difficult to distinguish from behavioral incentives. If voters realize that the incumbent government is not responsible for an unpopular policy, they may refrain from punishing the incumbent government even if the issue is very visible, and hence the government will lack the incentive to adjust the given policy. In light of our model of limited voter awareness and lack of detailed political knowledge, it seems most logical, however, to conceptualize them as behavioral constraints, implying that politicians have the incentives but not the power to reach their preferred goals. As to the opinion policy relationship, however, it leads to very similar empirical predictions whether these conditions are modeled as constraints or as incentives.

Chapter 2 mentioned, among several institutional constraints, the decentralization of decision-making in some policy domains. For various and often complex historical reasons decision-making authority on many issues is delegated from the national policy making level to local or regional political units. Political regimes differ with respect to how institutionalized and irreversible this delegation is, just as there are differences across policy domains. In most cases local autonomy, nevertheless, constrains national political leaders' ability to intervene and adjust policies compared to what they can do in relation to issues that do not involve local decision-making. Hence, even if a given issue moves up the national macropolitical agenda and government leaders have strong incentives to adjust policies in a popular direction, they may be constrained from doing so, compared to a similar situation occurring in a purely central level policy domain.

It should be noted that this discussion is partly a consequence of our focus on the opinion policy relationship at the national level. As mentioned previously, the macro-/subsystem mechanisms likely replicate on a minor scale within local jurisdictions, but the timing of local dynamics may not correspond closely to national dynamics. Depending on local political conditions, the incentives of national macropolitical actors to comply with the mass public may coincide with the incentives of local policymakers, but this need not be, for instance because the timing of local election cycles may be out of sync with national election cycles. Hence, decentralization of political authority often generates constraints from the viewpoint of national political leaders.

As mentioned in Chapter 2, the idea that responsiveness depends on whether the issue is redistributive or not has received some attention in the opinion policy literature (Brooks 1985; 1987; 1990; Petry 1999). The benefits of redistributive policies are widely distributed, whereas the costs are often concentrated in organized elite groups. Organized groups will therefore seek to block redistributive policies, and consequently, responsiveness is expected to be lower on redistributive policies compared to non-redistributive ones (ibid.). Applying this line of reasoning to our theoretical model, it still follows that political leaders will seek to adjust policies to match public opinion in times of increased macropolitical attention. However, given the additional assumption of stronger opposition on redistributive issues, the goal of vote seeking may be pursued more eagerly on non-redistributive issues.

Third, as noted in Chapter 2, political behavior might be constrained by fiscal costs. In cases where adjustment to a certain policy does not invoke fiscal costs, this is of course a negligible constraint. However, combined with the fact that most policies do have a monetary reflection (see Danziger 1978, p. 15), the assumption that fiscal resources are limited imply that politicians

cannot satisfy all possible demands even if they preferred to do so (cf Schick 1988, p. 68). At the disaggregated level of spending programs, fiscal costs from one policy adjustment can often be offset by changes in another program. Still, however, if political leaders have to choose between two otherwise identical popular policies, they are, from this perspective, expected to choose the one that invokes the least fiscal costs. Since vote seeking is assumed to be the primary operational goal of political parties, macropolitical intervention is still expected to improve the responsiveness of median voter opinions. However, responsiveness to issues on the macropolitical agenda might be greater the lower fiscal costs associated with the issue position of the median voter opinion.

One can think of multiple other extensions to the basic theoretical model. Common to these and to the examples mentioned here is the fact that they are expected to temper, not eliminate, the main effects of macropolitical intervention derived above. We can summarize the effect of these constraints in a general proposition:

> P3: The more constraints on macropolitical actors' behavior, the weaker the relationship described in propositions P1 and P2.

Exactly how the constraining effect will crystallize is an empirical question. For instance, when constraints are high will the effect of macropolitics show up only in election years, or will high constraints lead to threshold effects at the expense of simple linear effects of macropolitics? In line with similar questions reflected on earlier in this chapter, such theoretical uncertainty is best informed by empirical exploration.

The empirical study conducted in the following chapters concentrates on the two main propositions derived above, leaving a more thorough examination of the proposition set forth in this section to future studies. As explained further in the next chapter, the analytical design does however offer some suggestive evidence, in particular on the constraining effects produced by local autonomy.

## Conclusion

In this chapter, I have set forth a theoretical model of the conditions under which public policy is most likely to reflect public opinion. According to this model, many policymakers are not particularly concerned with public opinion when they pass certain policies, just as the average voter is unaware of most public policies. However, some policymakers are deeply concerned with voter reactions, just as some policies are very visible to the average voter. The latter is a characteristic of policymakers in the macropolitical venue and of issues

taking up a large share of the agenda of this venue. Consequently, the impact of public opinion depends on a given issue's place on the macropolitical agenda. And this effect is probably even stronger the closer the next election is. In the next chapter, the two main propositions derived from this model are translated into a set of testable hypotheses, which will be examined empirically in Chapters 5, 6, and 7.

# Chapter 4

# Research design, data, and methods

How to test the model developed in the previous chapter is the theme of this chapter. It discusses and decides upon questions about case selection, translation of the theoretical propositions into observable implications, and measurements of key dependent and explanatory variables. The chapter is in three parts. The first part considers the application of the theoretical model outlined in Chapter 3 to Danish politics. In the second part, the conversion of key theoretical terms into observable variables is discussed in more details. The third and last part of the chapter is concerned with a set of methodological questions. In particular, questions about the use of time series regressions and the question about causality will be addressed in this section.

## Applying the model to the Danish political system

In this section, I argue why it is reasonable to apply and test the theoretical model outlined in the previous chapter within a political regime like the Danish one. In particular, it is argued that the central assumptions that political parties behave as unitary actors and that a two-bloc party competition system exists are sufficiently met in the Danish national parliament. In such a two-bloc system, similar to a Downsian two-party system, vote and office seeking strategies will coincide because the best way to achieve government power is to win votes (cf Green-Pedersen 2002, p. 37).

The national Danish parliament, *Folketinget*, is a unicameral assembly and its members are elected for a maximum of four years in a proportional election system. The cabinet/government answers to the parliament, which can vote the government out of office, but the government also has the right to dissolve parliament at almost any time (cf Damgaard 1997, p. 79). Like other Nordic countries, it has often been difficult to fit the Danish system of parliamentary government into the models suggested by the literature (Damgaard 1992, p. 47; 2006, p. 282). This is so for a variety of reasons such as the many minority governments and a high degree of corporatism (cf Damgaard 2006, pp. 282-283), but one important modification concerns the role of political parties. Political parties are not mentioned in the Danish Constitution, nor in any of its predecessors, and according to the constitution, MPs are bound only by their own convictions (Damgaard 1997, p. 79). In practice, however, Danish MPs are party agents to such an extent that representative government in Denmark has developed into party government (cf Jensen 2000, p. 210). The strong party cohesion in Danish parliament is a well established phenomenon (Arter 1999; Jensen 2000, Damgaard 1997;

2006) and according to Skjæveland (1999, p. 35), Denmark is in fact among those liberal democracies that exhibit the highest degree of party cohesion.

Historically, Danish politics has been dominated by a left wing and a right wing bloc of parties, where Social democrats and usually also the Social Liberals constituted the left wing bloc and Conservatives and Liberals the right wing bloc (see Rasmussen 1972, p. 244; Damgaard 1992, p. 24). Hence, in this period before the "landslide" election in 1973, the Danish party system was a rather clear example of a two-bloc system.

The 1973-election, where a number of new political parties gained representation in parliament, did not change the bloc nature of Danish party competition, but it did complicate Danish politics and majority formation. After 1973, to form a working majority both blocs had to include more parties, some of which were extremist (Green-Pedersen 2002, p. 113). One implication was that minority governments without stable support replaced the tradition of either minority governments with stable support or majority governments. Another outcome was that the small centre parties, the Christian Democrats, the Centre Democrats, and in particular the Social Liberals, after 1973 have played a pivotal role in building majorities in Danish parliament (cf Arter 1999, p. 209). However, they have always joined either the two bourgeois parties, Liberals and Conservatives, or the main left wing party, the Social Democrats. Therefore, as noted by Green-Pedersen & Thomsen (2005, p. 154), there have always been two blocs of parties presenting themselves as clear government alternatives.[1] In addition, studies of government formation in Denmark clearly show the importance of controlling the median legislator (see Skjæveland 2003).

To sum up this discussion, party competition in the Danish parliament displays more consensual traits than a pure two party Westminster system. On the other hand, party competition in Denmark clearly exhibits many of the characteristics of a two-bloc system in that achieving government status has been rather closely connected with vote seeking. On balance, therefore, the two-party/-bloc competition logic underlying the model developed in Chapter 3 seems to approximate the national Danish party system sufficiently well. We might nevertheless expect more noise in the results than if the theory had been applied within an ideal-type Westminster two-party competition system. In Chapter 8, I return to this question in a discussion of the relevance of the model and the empirical findings to political systems in countries other than Denmark.

## Measuring public opinion

In Chapter 2, public opinion was defined as the majority opinion that could be measured on selected issues through responses to questions in opinion

polls that ask explicitly about identifiable government policies. One commonly used measure that satisfies this definition of public opinion is a survey question asking respondents whether they prefer more, the same, or less public spending on a set of particular policy issues. Indeed, many alternative measures comply with the above definition, but the spending question has some particularly useful qualities. First, it refers to a policy indicator, public spending, which for reasons given later in this chapter has proven superior to many alternative proxies of public policy. Second, studies of public opinion that rely on this measure indicate that at the aggregate level, the survey question seems to tap variation in public attitudes quite well (Eismeier 1982; Page & Shapiro 1992; Jacoby 1994; Borre 2003; Togeby 2004). Third, the question has been widely applied in earlier opinion policy studies and the use of this measure therefore enhances comparability with these studies (see Page & Shapiro 1983; Monroe 1998; Wlezien 1995; 1996; 2004; Soroka 2003; Soroka & Wlezien 2004; 2005; Jones 1994; Eichenberg & Stoll 2003; Hartley & Russett 1992). Finally, election studies in Denmark have repeated this survey question rather frequently since 1979, thus providing crucial information about variation in public opinion across time.

Having decided to look at public spending attitudes, the next question is how to interpret these data and how to identify the median spending attitude. First, the spending item provides no guidance about the preferred level of spending in the public in any absolute sense. Instead, it taps the relative spending preferences, the preferred direction of spending change away from status quo (cf Wlezien 1995; Stimson et al. 1995; Borre 2003). Presumably, as argued by Soroka & Wlezien (2005, p. 667), this is also how people think about most policies. For instance, instead of a clear idea about how much education policy or the optimal amount of money spent on education, people have an opinion about whether the government should spend more or less money on education compared to the status quo. Such relative opinions are exactly what this survey item is supposed to measure.

Second, when identifying the median opinion in these data, I join what seems to be the dominant approach in the field. The central question in this respect is how to interpret respondents who say that the government is spending “a suitable amount of money” on a given issue. The majority of responses quite often fall within this middle category, which could indicate satisfaction with current spending levels. However, as pointed out by Wlezien (1995, p. 985), the middle category in such items most probably encompasses variation from those who are close to favoring less to those who are close to favoring more. It therefore seems most useful to represent the median preference by ignoring the middle category and subtracting the percentage of

respondents who think the government spends “too little”, from those who think the government spends “too much”. While not perfect, this seems the most appropriate way to identify the median opinion in these surveys and I therefore adopt this approach.[2]

Table 4.1 reports the result of a sample of national Danish surveys asking the spending question in various years since 1979. Based on the clear and rather constant pattern of responses over time, some issues are categorized as popular, whereas others are categorized as unpopular. A positive value in Table 4.1 means that the proportion of people preferring increased spending is larger than the proportion preferring less spending. A negative value indicates the opposite.

We can draw at least three important lessons from Table 4.1. First, in all programs displayed in Table 4.1 adjustments of current spending levels could lead to a more popular slice of the budgetary pie, a fact that Kristensen (1982) has interpreted as an empirical rejection of Downs’ (1957) classic median voter model. The pattern in Table 4.1, however, does not contradict our modified model of electoral competition where the positive and negative deviations merely imply that on any one of the issues in Table 4.1, there is room for popular spending adjustments in times of increasing saliency in the macropolitical venue.

Table 4.1. Public spending attitudes

| | PDI: Percentage preferring more spending – percentage preferring less spending | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Topic | 1979 | 1981 | 1985 | 1987 | 1990 | 1994 | 1998 | 1999 | 2000 | 2001 | 2002 | 2003 |
| Popular issues | | | | | | | | | | | | |
| Health care | 28 | 31 | 61 | 60 | 61 | 73 | 77 | 65 | 68 | 64 | 62 | 62 |
| Education | 22 | 26 | 44 | 41 | 45 | 42 | 39 | . | . | 45 | . | 46 |
| Police | 37 | 41 | 44 | . | . | 52 | 51 | . | . | 56 | . | . |
| Environmental problems | 37 | 46 | 57 | 60 | 49 | 42 | 28 | 33 | 27 | 27 | 27 | 37 |
| Unpopular issues | | | | | | | | | | | | |
| Cultural purposes | –30 | –31 | –12 | . | –19 | –34 | –39 | . | . | –32 | . | . |
| Aid to developing countries | . | . | . | . | –26 | –35 | –40 | –27 | –28 | –29 | –17 | –13 |
| Defense | –44 | –49 | –36 | –42 | –40 | –35 | –35 | –16 | –14 | –14 | –17 | –26 |

Sources: See Appendix 1.

Note: (.) means that the question has not been asked.

Another lesson from Table 4.1 is that at the aggregate level people express rather stable spending attitudes over time, a finding that corroborates earlier US (Page & Shapiro 1992) and Danish public opinion studies (Togeby 2004). The relative spending preferences in Table 4.1 do vary over time, but in none of the policy domains do we see the preferred direction of spending changes shift sign. Therefore, median voters' preferred direction of spending change in each program is always either negative or positive. Observations from each year would of course have been ideal, but as long as we are mainly concerned with the direction of preferred spending changes, the stable pattern in Table 4.1 implies that such attitudes are quite constant over time. Consequently, in the following analyses in Chapters 5 and 6, I ignore the underlying longitudinal variation displayed in Table 4.1. However, I return to this variation in Chapter 7 in a discussion of the empirical results in light of Wlezien's Thermostat Model introduced in Chapter 2.

Selection of the seven issues reported in Table 4.1 is largely guided by availability of data on public spending, which will be discussed at greater length later in this chapter. There are other issues where longitudinal variation in public opinion is much more evident and where the direction of preferred spending changes shifts sign. This is most pronounced on issues about social assistance levels, unemployment benefits, and child allowances levels (see Andersen 1999, p. 187). Such variation is not an inherent problem in our theoretical model, but including these issues would give a design-related problem because we do not have consistent time series, and hence cannot observe exactly when opinion shifts occur. Restricting our analysis to issues characterized by a clear and stable public opinion, the missing observations from certain years is much less problematic. This point will be further elucidated as the methodological discussion in this chapter unfolds. There are other issues where a stable public opinion can be identified such as immigration or transport related issues, but due to a lack of valid corresponding spending data these issues cannot be included in the analysis.

When used as indicators of the context facing political leaders making decisions on an issue on the macropolitical agenda, a caveat might apply to the spending attitudes reported in Table 4.1. This stems from another finding in the literature on public spending, namely that general spending cuts might be more popular among the electorate than specific spending cuts (Jørgensen 1981; Kristensen 1987; Jacoby 1994). For instance, an important difference might exist between opposing public spending on defense in general versus rewarding the government for closing down military barracks in small towns where the loss of jobs represents costs visible to the public.

This potential difference between popularity of spending cuts and popularity of spending increases may point to an asymmetrical effect of macropolitical intervention. Issues categorized as unpopular in Table 4.1 might not benefit from increased macropolitical attention compared to popular issues, but on the other hand, increased macropolitical attention to unpopular programs does not lead to spending decreases. It is, however, also possible that the caveat about specific versus general spending preferences is negligible in the present case. In fact, the survey responses on which Table 4.1 is based are normally assumed to be measuring people's specific spending preferences (see Jacoby 1994), implying that on average, real spending cuts would be quite popular in the policy areas categorized as unpopular in Table 4.1. I find the latter possibility most likely and straightforward, and hence I continue with this model throughout the chapter. However, this is largely an empirical question, and the analyses in Chapters 5, 6, and 7 will provide suggestions for empirical answers.

### Public spending as a measure of policy

In correspondence with the operational measure of public opinion, I choose public spending as the measure of public policy. In addition, this policy measure is not only utilized in many previous opinion policy studies, but is also widely used in other approaches to the study of public policy. Hence, the choice of policy proxy is a rather common and non-controversial policy measure, enhancing comparability with the results of previous research.

While not a perfect indicator of public policy, public spending does have some useful features. First, spending represents a unified and standardized measure of public policy, which improves comparability over time and across programs (Hogwood 1992, p. 33). Second, public spending is a central aspect of many of the activities in the political system (Danziger 1978, p.15). As argued by Hofferbert and Budge (1992, pp. 159-60):

> Money certainly is not all there is to policy. But there is precious little policy without it. One need watch very little of the public debate at party conferences to note that the most frequently invoked indicator of commitment to a particular policy is a promise to spend or reallocate money.

Hence, in many cases authorized public spending remains a valid and central measure of decisional output, because most policies do have a monetary reflection in the public budgets. However, as also implied by the first sentence of the above quote, public spending certainly does not equal political decisions. This is perhaps most true for regulatory policies. Regulations are not important for what they cost, but for what they do, and in terms of regulatory

policies, very different policy alternatives may be almost indistinguishable in terms of costs. In most of the seven issues displayed in Table 4.1, policy is about service provision rather than regulation and therefore this objection is less relevant to these issues. However, a few of them, such as environmental protection and police-related law and order issues, may contain significant elements of regulatory politics. In these cases public spending as a policy indicator may be flawed. On the other hand, as also noted by Hofferbert and Budge (1992, p. 161), variation in spending for a regulatory policy issue over time may still provide a good index for variation in policy. If politicians, for instance, agree on a stricter policy against criminals, this will probably result not only in longer prison terms, but also in increased spending on activities related to police enforcement and prison services.

*Measuring public spending*

Having decided to use public spending as a measure of public policy, we now face the more specific question of what kind of spending data to use. To some extent, the specific choice of spending indicator is often guided more by availability than by theory because reliable and comparable spending data over just a modest range of years is extremely difficult to come by. This study does not avoid such limitations, but it benefits from an extensive dataset on Danish public spending, allowing for some discretion in the specification of spending data.

In order to evaluate the pro and cons of this dataset, however, it may be worthwhile to consider the characteristics of a hypothetical "ideal" spending dataset. First, we need comparable observations over a substantial span of years in order to assess how fluctuation in the macropolitical agenda affects the relationship between public opinion and public spending. Second, changes in spending data should reflect changed political priorities rather than technical changes. Hence, the spending data should be corrected for inflation, technical changes in the accounting system, and changes stemming from reorganizations of, for instance, governmental departments and bureaucratic agencies. Third, the level of aggregation should correspond to the measure of public opinion, implying that a "domestic spending" category would for instance be too general to capture the variation in public opinion reported in Table 4.1. Finally, data on appropriations might be preferable to outlays based on the argument that the latter are too far downstream in the political process to reflect political priorities (cf Jones et al. 1996, p. 3; Wlezien & Soroka 2003).

Equipped with these reflections on the ideal spending dataset, we now turn to an evaluation of the spending data available to this study. The dataset was compiled by Statistics Denmark and covers the period 1971 to 2003. It

consists of 14 main policy categories such as Defense, Health, and Education, and 34 sub-functions such as Primary Education, Higher Education, etc.

The major advantage of this dataset is the adjustment for technical changes and the fact that the time series are comparable, at least back to 1980, at a disaggregated functional level. Besides these crucial corrections for budgetary reforms and institutional restructuring, which are frequent political or administrative activities that often effectively obstruct the systematic study of public spending, the dataset has at least one additional quality. Local expenditures are adjusted to fit the functional categories of the national budget, making it possible to trace the total amount of public spending for a given spending purpose. Although the variable of theoretical interest is policy decided at the national level of policy making, it is not always obvious whether we should distinguish between local and central government spending in a unitary state like Denmark. Central government can interfere in local spending priorities, and much central government expenditure is actually spent in local municipalities. Hence, it is important to be able to trace total public spending instead of just central government spending.

Besides these advantages, the dataset provided by Statistics Denmark also has a potential drawback in relation to the theoretical variable it is supposed to reflect in this study. It consists of outlays instead of appropriation data. The standard argument by US agenda setting and opinion policy scholars (Jones et al. 1996, p. 3; Wlezien & Soroka 2003) is that the latter measure is a superior indicator of government priorities because outlays might represent the outcome of bureaucratic battles rather than political decisions (see also Cogan & Murris 1994, p. 82). However, reliable, technically adjusted disaggregated time series on appropriation data are not available in a Danish context or, to my knowledge, in countries other than the US and the UK, so outlay is the best available empirical spending measure. Hence, on the one hand, we have to make do. On the other, however, the following aspects suggest that using public outlays does not necessarily invalidate our analysis.

First, according to the Danish constitution, no public money can be spent without permission granted via a law approved by a legislative majority in parliament (see Eldrup et al. 1992, p. 158). Therefore, all outlays represent politically approved decisions and if they reflect more bureaucratic power than appropriations, this might tell us something important about governmental decisions rather than representing an inherent problem with outlays as a measure of public policy.

Second, the claim that outlays are too close to bureaucratic decision-making implies that the measurement of the dependent spending variable may be systematically biased against the hypotheses in this study. The conse-

quence is that if we are to uncover any effect from the macropolitical agenda and from public opinion on public outlays, we may safely infer that they probably would have been stronger using appropriation data. In other words, our study might underestimate the "real" effect on public policies.

Third, the vast majority of budget appropriations in the Danish parliament cover one year only (Eldrup et al. 1992, p. 51), which reduces the lag problem in the Danish outlay data. In addition, the lag problem is more critical in capital spending than in current spending. Hence, another way to reduce the problem is to confine the analysis to either current spending or to a combined measure of both current and capital spending. I have chosen to adopt the latter approach. Please note that I do not argue that outlays are superior to appropriations, but for the above reasons, I take public outlays as a valid and reliable proxy for governmental decisions.

As mentioned earlier, Statistics Denmark has data going back to 1971, but according to their own assessment, there is considerable uncertainty in spending data from before 1980.[3] Longer time series are always preferable, but the fact that successive and comparable observations on public spending over time is extremely difficult to obtain suggests that the 23 annual observations from 1980 to 2003 actually represent a remarkably long time series. Furthermore, observations on many of the control variables introduced later are also restricted to around 1980, just as observations on public opinion are not available before 1979 (see Table 4.1). We therefore focus on the period 1980 to 2003 in this study.

All in all, the functional categories in the spending dataset fit quite well with the survey categories displayed in Table 4.1. As noted, it would have been preferable to include a larger set of issues where a clear and stable public opinion has been measured. However, the most obvious issues, opinion on immigration, children's day care, old age pensions, and traffic related issues, could not be isolated in this dataset on public spending.

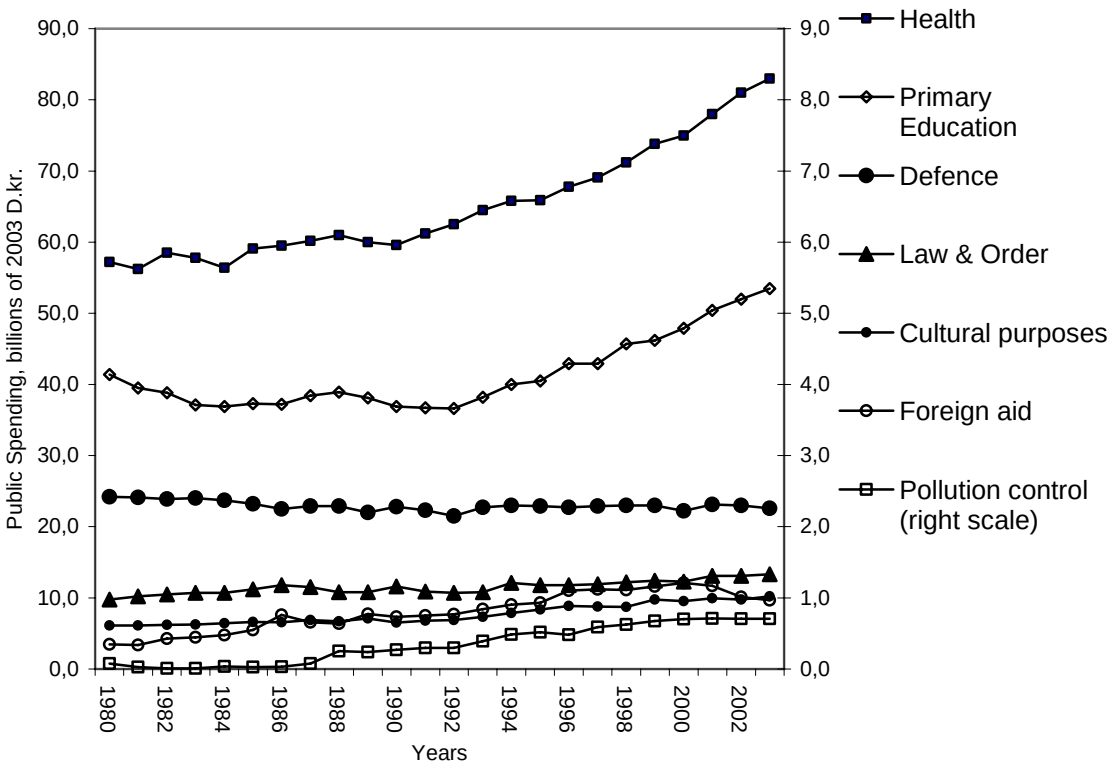
With respect to the police issue, it has been necessary to construct a broader and more robust "law and order" category by including, in addition to police, spending on prisons and courts of law. However, it seems reasonable to expect public opinion on police and law and order issues to correspond very closely. Spending attitudes on education, as touched upon by Borre (2003, p. 175), might depend on whether people think of education in relation to primary schools or academic education. In this case, I take spending on primary and lower secondary education as a proxy for education based on the impression that this part of education spending is indeed very popular in the electorate.[4] Finally, spending on pollution control and foreign aid are represented by relatively small and narrowly defined spending categories,

making them more vulnerable to measurement errors. As it will appear from Table 4.2 below, in particular the pollution control category is marked by some very high annual spending increases, which could be a concern in light of the – often incremental – nature of public spending. First, however, newer research on public spending clearly shows that major changes in spending from year to year are not in fact as unlikely as the classic studies of incrementalism would have it (see Jones et al. 2003; John & Margetts 2003; Jordan 2003; Mortensen 2005). Furthermore, a control dataset from the Ministry of Finance introduced in Appendix 1, covering the period from 1992 to 2003, reveals similar trends in a corresponding pollution control category. That said the pollution control category in the Statistics Denmark dataset certainly does not capture all relevant government spending on environmental protection, and if a larger and more all-encompassing budget category had been available, it would probably have displayed both a higher level of public spending directed at environmental protection and more modest fluctuations over time. However, in this case we take the pollution control category as a sufficient approximation of trends in public spending on environmental protection. Further questions about reliability and setup of the spending data are discussed at length in Appendix 1.

Figure 4.1 shows annual inflation-adjusted measures of public spending from 1980 to 2003 on the seven policy issues. Since the level of spending on pollution control is significantly lower than spending in the other domains, this series is shown on the right scale in Figure 4.1 to allow for an assessment of variability over time. Except for defense, spending in all of the domains shows an upward trend over time, documenting a well-established characteristic of government growth. However, in all domains spending also contracts in some periods, at least in inflation-adjusted prices, and all programs grow faster in some periods than in others.

Table 4.2 provides additional information about this longitudinal variation in public spending. Except defense, all programs have expanded from 1980 to 2003. It furthermore shows the remarkable increases in spending on pollution control when measured in percentage changes. Most relevant in view of the research question guiding this study, Table 4.2 does not indicate a strong direct relationship between public spending attitudes and public spending. First, there is simply not as much variation between spending on popular and unpopular issues as we would expect if public opinion exerted a strong and unconditional effect on public spending. Indeed, defense spending decreases while spending on popular issues increases, but so does spending on foreign aid and cultural purposes, and at an even higher rate when measured as percentage changes.

Figure 4.1. Public spending by function, 1980-2003




Source: Based on data from Statistics Denmark, see Appendix 1.

Table 4.2. Descriptive statistics for public spending, 1980 to 2003

| Topic | Mean (%) | Median (%) | Standard deviation (%) | Number of annual spending increases |
|---|---|---|---|---|
| *Popular issues*: | | | | |
| Law & order | 1.42 | .63 | 4.18 | 15 |
| Pollution fighting | 27.15 | 6.23 | 80.54 | 15 |
| Primary education | 1.17 | 1.18 | 3.16 | 14 |
| Health | 1.65 | 2.00 | 2.06 | 18 |
| *Unpopular issues*: | | | | |
| Defense | –.28 | –.34 | 2.41 | 9 |
| Development aid | 5.20 | 2.71 | 12.21 | 15 |
| Cultural purposes | 2.32 | 2.70 | 4.23 | 16 |

Source: Based on data from Statistics Denmark, see Appendix 1.

Notes: N=23. Public spending is measured as annual percentage changes calculated as $t - t_{-1} / t_{-1} * 100$.

Another way to estimate this relationship is to consider the outcome displayed in Table 4.2 as the result of 23 (years) * 7 (programs) = 161 spending decisions. At each of the 161 decision making points politicians could have decided to adjust public spending in a popular or an unpopular direction. Based on the right-hand column in Table 4.2, total spending has been adjusted in a popular direction in 91 out of the 161 spending decisions, or 57 percent. While admittedly simplistic, this number actually corresponds quite closely with the response levels found in other bivariate opinion policy studies reported in Chapter 2. Furthermore, like in those studies, the causal effect is likely overestimated because of the lack of control for alternative explanations that might account for this variation in Table 4.2.

As emphasized in the previous chapter, this is not a strong test of the theoretical model, nor does it add significantly to what we already know from earlier studies. It nevertheless offers evidence in support of our basic model of public policymaking, which implies that the direct effect of public opinion on public policy is only moderate. Information on the main propositions about when the effect of public opinion is particularly strong or weak, however, cannot be obtained until we have introduced the measures of the macropolitical agenda and political business cycles, and addressed a set of more specific methodological questions.

## Measuring the macropolitical agenda

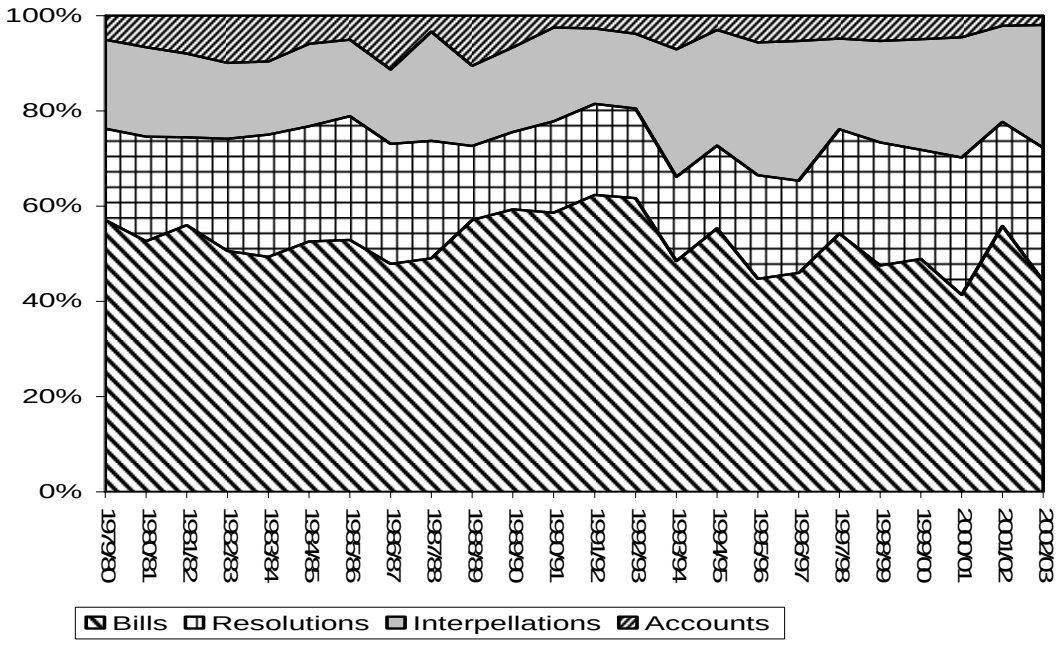
In Chapter 3, the presidency, the floor of congress or, in parliamentary systems the floor of parliament, were mentioned as classic examples of national macropolitical venues. Hence, in a parliamentary system like the Danish one, the floor of the parliament seems a natural place to search for a measure of the macropolitical agenda. In accord with the theoretical expectation, this venue is clearly dominated by national political parties and activities in this venue are rather visible to a broader public audience.

Policy making nearly always generates some debate on the floor of the parliament, but this does not necessarily imply that the issue is ranked high on the agenda of this venue. Hence, it is not enough just to know whether the macropolitical actors address an issue in parliament or not. As implied by the agenda concept, and in line with the theoretical model outlined in the previous chapter, we need an empirical estimate of the hierarchy of issue importance in the macropolitical venue.

Debates in the Danish national parliament centre on proposals of new bills, interpellation debates, parliamentary resolutions, and accounts by the ministers. Figure 4.2 shows the relative occurrence of these four activities in the period 1979 to 2002. Historically, the majority of the activities reported in Figure 4.2 have been initiated by new bills proposed by the government.

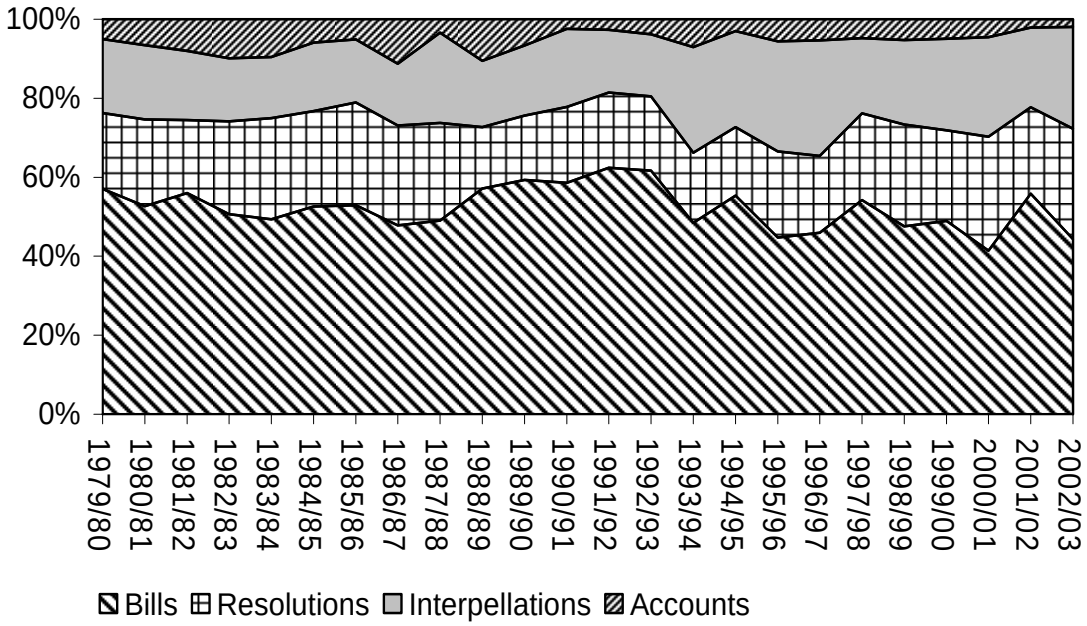
This clearly reflects the formal agenda setting power of the incumbent government. However, the proportion of parliamentary resolutions and interpellation debates – activities mainly initiated by the opposition parties – has grown significantly over time. From a low of 19 percent in 1980/81, 65 percent of the debates in parliament in 2000/01 were over resolutions or interpellation debates. Furthermore, in correspondence with the "index-hypothesis" (see Chapter 3), introducing a bill or scheduling an interpellation debate does not necessarily generate significant parliamentary debate. While some bills are largely ignored in parliament, others are subject to extensive debate, depending on how the opposition parties choose to address the issue. Hence, *length of debate in parliament* seems to be a better proxy for the macropolitical agenda than a mere registration of the number of times various activities took place in parliament. Most importantly, length of debate in parliament reflects the sum of political parties' public involvement in a particular policy issue at a particular point in time. For instance, if legislators choose to spend a large proportion of their limited time and energy on parliamentary debates over environmental issues, this probably indicates that environmental politics is high on the macropolitical agenda.

Figure 4.2. Activities in the Danish parliament, 1979/80-2002/03




Source: See Appendix 1.

Figure 4.3. Parliamentary debate on various activities, 1979/80-2002/03




Source: See Appendix 1.

Figure 4.3 shows how much parliamentary debate the four kinds of parliamentary activities introduced in Figure 4.2 above has generated over time. According to Figure 4.3, resolutions initiated by the opposition and interpellations are discussed almost as much as government proposed bills throughout the period from 1980 to 2003. This balanced pattern supports the use of length of parliamentary debates as a proxy for the macropolitical agenda rather than the government's or the opposition's agenda.

To get a balanced and robust measure of activities on the floor of parliaent, our units of analysis are summary measures of annual length of debates in parliament in relation to not only bills, but also interpellation debates, acounts by the ministers, and parliamentary resolutions. Interpellation debates may be called by members of the government parties, but normally they are raised by opposition parties. Parliamentary resolutions have a semi-law status and can require a minister to put forward a law. Most of these resolutions never gather the support of a majority and 'die' in the standing committee after the first reading. But they always generate some debate in parliament.

The present study utilizes a database on Danish parliamentary activities collected by the project *Party Competition, Agenda setting and Public Policies in Western Europe,* directed by Christoffer Green-Pedersen of the Department of Polical Science, University of Aarhus (see www.ps.au.dk/greenp). The full database covers all Danish parliamentary activities from October 1953 to September 2003, including 'questions to the minister' (N=63.737), 'bills' (N=11.952), 'interpellation debates' (N=1.313), 'accounts by ministers'

(N=581), and 'parliamentary resolutions' (N=4.418).[5] In the database, all of these activities have attached a content code based on a content scheme developed by the American "Policy Agendas Project" (see www.policyagendas.org). The content code specifies whether the given activity was about defense, education, traffic, etc. The Danish version of the content scheme consists of 19 main categories and 236 subcategories, making it possible to track the content of the parliamentary activity down to a very disaggregated level. In addition, the database contains a short description of each parliamentary item, making it rather easy to recode data based on a different content scheme. The original functional categories, however, correspond quite well to the seven issues identified above in the surveys and in the spending dataset. Hence, a recode has only been necessary in order to isolate debate on primary education.[6] The time units in the parliamentary database are sessions or years, which corresponds to the frequency of our spending measures. The measures of parliamentary debates are divided into seven consistent categories corresponding to those in the spending dataset from Statistics Denmark.[7] Furthermore, lengths of debates are measured as the number of columns covering debates in the parliamentary records.

Table 4.3 summarizes central characteristics about the variation in length of parliamentary debate on the seven issues included in the analysis. It reflects variation both over time and across issues. While environmental protection, for instance, in some years accounts for over 9 % of the total debate, in other years it only takes up 1.6 % of the debates on the floor of the parliament. A similar pattern is most pronounced for the law and order issue, but longitudinal variation is evident on all issues.

Looking at mean scores, Table 4.3 shows that the environmental and the law and order issues also receive the most attention together with the health issue. The other four issues receive less than 2 % on average, but keep in mind that the total number of issues that can be debated in parliament is very large, which implies that all these issues probably are located on the upper end of the macropolitical agenda.

The comparison across issues, however, also reveals the limitations of this measure of the macropolitical agenda. While health related debates on average account for a little more than 5 % of total annual debates in parliament and a maximum of 7.8 %, debates on primary education never summed up to more than 1.7 % percent of the total length of debate. Looking at the sum of all education related categories the corresponding numbers would be 4.8 % on average and a maximum of 7.3 %. On the other hand, if we look only at a subset of health related issues such as hospitals between 1980 and 2003, these account on average for 1 % of the total parliamentary debates with a

maximum of 2.6 %. Hence, depending on the structure of public attitudes, some issues are more homogenous and hence "larger" than others in terms of parliamentary debate, but this does not necessarily imply a higher absolute ranking on the macropolitical agenda. Still, however, variation over time in parliamentary debate on a given issue does provide a good index for comparing its own rise and fall in importance to the macropolitical actors. While our theoretical model points to both cross-sectional and longitudinal effects, we therefore confine our empirical analysis in Chapters 5 and 6 to the effect of the dynamic tides of parliamentary debate on public spending on a given issue from 1980 to 2003.

Table 4.3. Descriptive statistics for parliamentary debate, 1979/80 to 2002/03

| Topic | Mean (%) | Median (%) | Standard deviation (%) | Minimum (%) | Maximum (%) |
|---|---|---|---|---|---|
| *Popular issues* | | | | | |
| Law & Order | 4.6 | 4.5 | 1.7 | 1.6 | 8.2 |
| Environmental protection | 5.9 | 5.8 | 2.1 | 2.8 | 9.4 |
| Primary Education | .9 | .9 | .4 | .4 | 1.7 |
| Health | 5.2 | 5.4 | 1.7 | 1.7 | 7.8 |
| *Unpopular issues* | | | | | |
| Defense | 1.5 | 1.3 | 1.4 | .2 | 6.3 |
| Aid to developing countries | 1.1 | .9 | .7 | .1 | 2.4 |
| Cultural purposes | 1.7 | 1.4 | .8 | .4 | 3.3 |

Note: N=23. Parliamentary debate is measured as the percentage of the total debate devoted to a given topic.

Having converted the three central theoretical concepts (i.e. public opinion, public policy, and the macropolitical agenda) into observable variables, proposition P1 from Chapter 3 can now be expressed by the following two operational hypotheses:

> *H1.a. On issues where the median voter prefers more spending, there is a positive relationship between share of parliamentary debate and public spending changes.*

> *H1.b. On issues where the median voter prefers less spending, there is a negative relationship between share of parliamentary debate and public spending changes.*

The discussion from Chapter 3 about linear versus threshold effects of course still applies and will be addressed in Chapters 5 and 6 in a competition between different functional models. Furthermore, it should be noted that all seven issues concern policies where survey researchers have found it worthwhile to measure public opinion on a regular basis over time, which suggests that all issues are cases of relatively broad generality and public salience. This indicates a slightly conservative bias in the research design. If we are to uncover an additional effect of time variation in parliamentary debates on these issues, there is reason to believe that this effect would be even stronger on issues that on average are more invisible.

### Measuring political business cycles

As in many other countries, national elections in Denmark can be called before the end of a term. That the election date is not exogenously fixed should not undermine the political business cycle hypothesis, but it might slightly complicate the prediction. As argued by Schultz (1995, p. 91), the power to choose the election date gives the incumbent government an extra tool for influencing electoral outcomes. Either it can manipulate policy in order to engineer a pre-election boost, or it can call an election when it, for some reason, is doing well in the polls. The government will probably rely on a mixture of both, but it is constrained by the four-year maximum length of the electoral period, just as it may seem a very risky strategy if they do not utilize their policy instrument too. Therefore, I still expect a policy effect even when the election date it not exogenously fixed.

That said, the next question is how to measure election cycle effects in the absence of fixed election dates. Given legislators' uncertainty about the actual time of the next election, Skjæveland (2001) in his study of a Danish party cohesion cycle seems to suggest an election cycle measure where each year represents a probability of election based on the date set by the constitution. Following that logic, in year 1 after an election the probability would be 25 %, in year 2 it would be 50 %, 75 % in year 3, and 100 % in year 4. This measure seems reasonable when studying individual MP behavior, but it probably underestimates the incumbent government's ability to predict the time of the next election, given the fact that the prime minister is empowered to call it. Furthermore, in the 1980s, when premature national Danish elections were very frequent, it would probably be misleading to operate with a probability of only 25 % in the first year after an election, and working more or less arbitrarily with different probabilities in different epochs does not seem warranted. Consequently, I choose the most simple and straightforward business cycle measure and construct a dummy variable that separates elec-

tion years from non-election years. Exactly what fiscal years are coded as election years is described in Appendix 1.[8]

Given this measure of political business cycle effects, the theoretical proposition P2 can now be represented by the following four empirical hypotheses. The first two reflect the assumption that the seven issues analyzed in this study are cases of relatively broad generality and macropolitical saliency.

*H2.1a. On issues where the median voter prefers more spending, public spending is higher in election years than in non-election years.*

*H2.1b. On issues where the median voter prefers less spending, public spending is lower in election years than in non-election years.*

*H2.2a. On issues where the median voter prefers more spending, the positive relationship between share of parliamentary debate and public spending is stronger in election years than in non-election years.*

*H2.2b. On issues where the median voter prefers less spending, the negative relationship between share of parliamentary debate and public spending is stronger in election years than in non-election years.*

Together with H1.a and H1.b introduced above, these represent the main empirical hypotheses to be tested in the next chapters. Selection of relevant control variables is discussed in relation to the empirical analyses in Chapters 5 and 6. Before concluding this chapter with a discussion of methodological questions, we dwell on the potential effects of variation in local autonomy across the seven issues selected for analysis.

## The impact of local autonomy

With seven issues, we do not have sufficient variation on the local autonomy variable to strongly rule out alternative explanations and to fully separate out individual effects. However, we do have some variation that can provide us with at least suggestive evidence on the explanatory power of this constraint on national policymakers. Denmark is a unitary state characterized by a large and rather decentralized public sector, where counties and municipalities in fact act as the implementing agencies of the Danish welfare state (Bogason 1987; 1991; Christensen 1998). With respect to the seven issues, this is most pronounced for primary education and health where most public resources are spent by the local level of policy making. However, this does not preclude central government intervention on these issues. Substantial rule-making takes place in parliament and the activities and priorities of individual local

and regional governments are annually coordinated in bargaining sessions between local authority organizations and the central government (Blom-Hansen 1999). With these limitations, local policy-makers are nevertheless entitled to prioritize resources and to impose local taxes.

On the other hand, the dataset includes issues that are the sole responsibility of central government. These are law & order, foreign aid, and defense issues. Pollution control and cultural purposes are intermediate cases, involving both local and central government spending. Degree of local autonomy has varied over time, but the main variation is across issues, at least in the period under scrutiny in this study. Consequently, we would expect to find the strongest macropolitical and business cycle effects on the three central government issues and the weakest effects on health and primary education spending, the two issues with the most local government involvement.

There are of course many other constraints on national policymakers than local autonomy. For instance, following the logic introduced in Chapter 3, opposition from public officials or organized interests might make it more costly to comply with voter preferences for spending decreases. In contrast, however, constraints from fiscal costs might imply lower responsiveness on issues where the median voter prefers spending increases. Hence, such effects seem to work in opposite directions just as they may interfere with the earlier point about a potential difference between the voter popularity of specific versus general spending cuts. The analyses in Chapters 5, 6, and 7 do allow a comparison between the four popular issues where a majority of the voters prefers increased public spending and the three unpopular issues where a majority prefers decreased spending. However, for the above reasons this comparison will be very tentative and a more definitive examination of these individual effects must therefore await future studies.

## Methodological questions

In addition to the questions about case selection, translation of theoretical hypotheses into observable propositions, and measurement of key dependent and explanatory variables that have now been discussed and decided upon, a series of methodological questions has yet to be answered. In particular, aspects related to the use of time series regressions need to be addressed in this section.

The backbone of the following chapters is a set of time series regressions on the development in public spending in seven different policy domains. A variety of research strategies has been applied in the opinion policy literature, but among the quantitative studies, to which the present study belongs, the use of time series regression seems a particularly well suited method for evaluating the opinion policy propositions deduced in earlier chapters.

First, as I will discuss further below, time series data improves the detection of causality through a separation of observations in time. In addition, by looking at the development in seven policy domains over almost a quarter of a century we get a good impression of the empirical validity of the propositions in both time and space. Furthermore, analyzing national politics at the level of individual policy domains requires observations over a substantial span of years in order to obtain enough information on our variables of theoretical interest.

Second, by subjecting our propositions to a rigid multiple regression design, we raise the requirements of the empirical burden of proof. Contrary to the often-used eyeballing of time series plots (cf Glynn et al. 1999, pp. 306–17) or statistical evaluation of bivariate time series relationships (Monroe 1979; 1998; Page & Shapiro 1983), the use of multiple regression imposes a set of exact and standardized evaluation criteria and not least a simultaneous control for a set of other relevant explanations. Regression techniques, of course, do not exclude discretion and interpretations of data, but they do reduce these to a minimum compared with the methods mentioned above.

The main caveat when conducting time series regression concerns the issue of trends and non-stationarity (cf Gujarati 2003, chapter 21). Many time series trend upward or downward and failure to correct for these trends may lead to 'spurious regression', where two uncorrelated variables seem to be significantly correlated only because both time series, for instance, trend upward (ibid., p. 792). Hence, it is of crucial importance to impose stationarity on the variables before we apply the standard ordinary least squares (OLS) regression techniques. Furthermore, the famous inheritance and upward drift in many public spending programs, as also evident in Figure 4.1 above, makes the issue of stationarity highly relevant to the present study.

The issue of stationarity is handled by analyzing annual spending changes instead of spending levels. As argued by Kittel and Winner (2005, p. 280), it is more reasonable to assume that parties in government can influence the growth of public spending than to assume that they can fully and immediately change the level of public expenditures at their will. Furthermore, based on the belief that reactions by the public and policy makers will most often be to relative changes in the various real-world indicators rather than to their actual values, most of the explanatory variables introduced in the next chapters are also measured in changes (cf Soroka 2002, p. 78).

Given the importance of the issue of stationarity, I have empirically estimated the nature and extent of autocorrelation in the time series, employing the Box-Jenkins methodology supplemented with the Dickey-Fuller and Philips-Peron unit root tests (cf Greene 2003, chapter 20). In most instances,

percentage changes or first differences have sufficed, but on a few occasions it has been necessary to difference the time series twice. Nevertheless, both the unit root tests and the autocorrelation functions and partial autocorrelation functions of the time series indicate that the variables are all stationary in the form in which they appear in the regression models presented in the following chapters.

The specification of the two central variables, public spending and length of parliamentary debate, may prove particularly interesting.[9] As to the former, I begin by taking the logarithm of public spending in order to better fulfil the linearity assumptions of the OLS regression technique. In line with the standard advice in the time series literature (Gujarati 2003, p. 820; Greene 2003, p. 620), I then calculate changes in public spending by differencing the time series once. If multiplied by 100, as we do in the analyses in Chapters 5, 6, and 7, this measure approximates percentages change (cf. Gujarati 2003, p. 179). Therefore, the estimated coefficients reported in the next chapters can be interpreted roughly as the percentage change in public spending for a one unit change in the explanatory variables. This approximation is more accurate for smaller changes, but since most of the estimated coefficients in the following analyses are within a five percentage point change in public spending for a one-unit change in the explanatory variables, interpreting the estimated effects as percentage changes appears to be justified.

Another question is whether these spending changes should be measured as changes in the proportion of total spending allocated to the given program or as changes in the absolute amount of money dedicated to the program. In this study, I avoid the use of spending proportions because the measure of people's spending attitudes does not relate to proportions but to changes in the absolute amount of money spent on a given issue. For instance, if we look at spending proportions a decrease in one program might misleadingly be counted as a popular adjustment in another program as the relative share of this program increases without a real increase of the amount of money spent on the program. In addition, our propositions are derived from a model that emphasizes the politicians' need to justify their spending decisions in times of increasing public visibility. In that case, it may not count much to announce: "we have addressed the problems in the health sector by cutting down spending on defense!"

Regarding the measure of parliamentary debate, the definition of the macropolitical agenda implies that it is the proportion of the total debate devoted to a given issue that matters. Proportions and absolute values are to some extent similar entities if the total size of the macropolitical agenda

remains fixed over time, but this has not been the case in Danish politics (cf Green-Pedersen 2004, pp. 5-6). With respect to imposing stationarity in the time series, it turns out that measuring proportions of annual parliamentary debate instead of total length of annual debate suffices to render the time series stationary.

Having secured stationarity in all variables, we can move on to estimate the coefficients using standard OLS regression techniques. Still, of course, the regressions must live up to the set of standard assumptions underlying the OLS method.[10] In that respect, it may be worthwhile dwelling on the potential consequences of applying OLS regression to a time series consisting of 23 annual observations. If the number of observations is relatively low, the regression results may be vulnerable to a few extreme observations, some of the nice properties stated in the Central Limit Theorem may not apply, it becomes more difficult to assess whether the regression diagnostic is healthy, and it may be difficult to obtain statistically significant coefficients (cf Agresti & Finlay 1997, pp. 140-41).

For reasons stated earlier in this chapter, it is not possible to avoid these small n problems by expanding the period of investigation beyond 1980. At least three points suggest, however, that the length of time series is not invalidating to our analysis. First, the examination of seven different time series reduces the uncertainty of the results by increasing the total number of observations available. Second, in making it more difficult to obtain significant coefficients, the low number of observations imparts a systematic bias against our hypotheses, which may suggest that the true effects are actually stronger than they seem in the tables below. Third, although many textbooks on time series regression advocate a minimum sample of 50 observations or more, the actual number of observations in earlier multiple regression studies within the opinion policy field ranges from 9 to 38, the modal number of observations lying somewhere between 15 and 20 (Hartley & Russett 1992; Jones 1994, chapter 5; Stimson et al. 1995; Wlezien 1995, 1996; Erikson et al. 2002; Soroka & Wlezien 2004, 2005; Hobolt & Klemmensen 2005). Still, the uncertainty stemming from the low number of observations should be reflected in the reporting and interpretations of the regression results, and I intend to do so in the analyses that follow. Furthermore, in Chapters 5, 6, and 7 I explore the effect of various adjustments in the model assumptions in order to test the robustness of the results and reduce the uncertainty about the statistical estimates (cf Leamer 1983).

Before concluding this section, the question about significance tests requires a few words. Use of significance tests is normally based on an ambition to make statistical inference to the population from which the random sample

of observations has been drawn. Our data, however, is not a random sample and may instead be argued to constitute the population of relevant data, and hence the standard logic behind significance testing breaks down. However, all real-world behavior involves a stochastic element just as all observations are encumbered by random measurement errors, which may suggest the use of significance tests even in this case (cf Thomsen 1997). For these reasons and in accordance with the dominant approach within the field of opinion policy studies, statistical significance tests are used in this study as one way to discriminate between what is worth discussing and what is merely an accidental occurrence. Given the relatively small number of observations, I find a .10 cut-off point to be appropriate.

### Time Lags, parliamentary debate and public spending

A time lag always exists between the decision to spend public money and the time where the money is actually spent. Therefore, it is often necessary to lag the explanatory variables in order to account for the lag in spending data. Exactly what time lag to use, however, may vary across countries (see Eichenberg & Stoll 2003) and across different policy domains. The dominant choice of time lag in earlier research of responsiveness, however, seems to be a one-year time lag, which simply reflects that a large part of public spending is decided in the previous fiscal year (see Page & Shapiro 1983; Brooks 1990; Wlezien 1995; 1996; 2004; Soroka & Wlezien 2004; 2005). However, the choice of time lag may also depend on whether one looks at budgets or expenditures since the latter include many supplementary grants decided during the fiscal year.

The central issue is to determine the time lag between our variables of central theoretical interest, extent of parliamentary debate and changes in public spending. Whereas the fiscal year follows the calendar year, the parliamentary year in Denmark goes from October 1 to September 30 with debate in parliament terminating around June 1. The major part of public spending in, for instance, 2003 is decided in the parliamentary session 2002/2003. In general, we would expect spending decisions to respond to current variation in the extent of debate in parliament. However, we do not have any firm theoretical guidance in this respect and it could be that friction and inertia in the budgetary process result in a longer time lag between parliamentary debate and public spending.

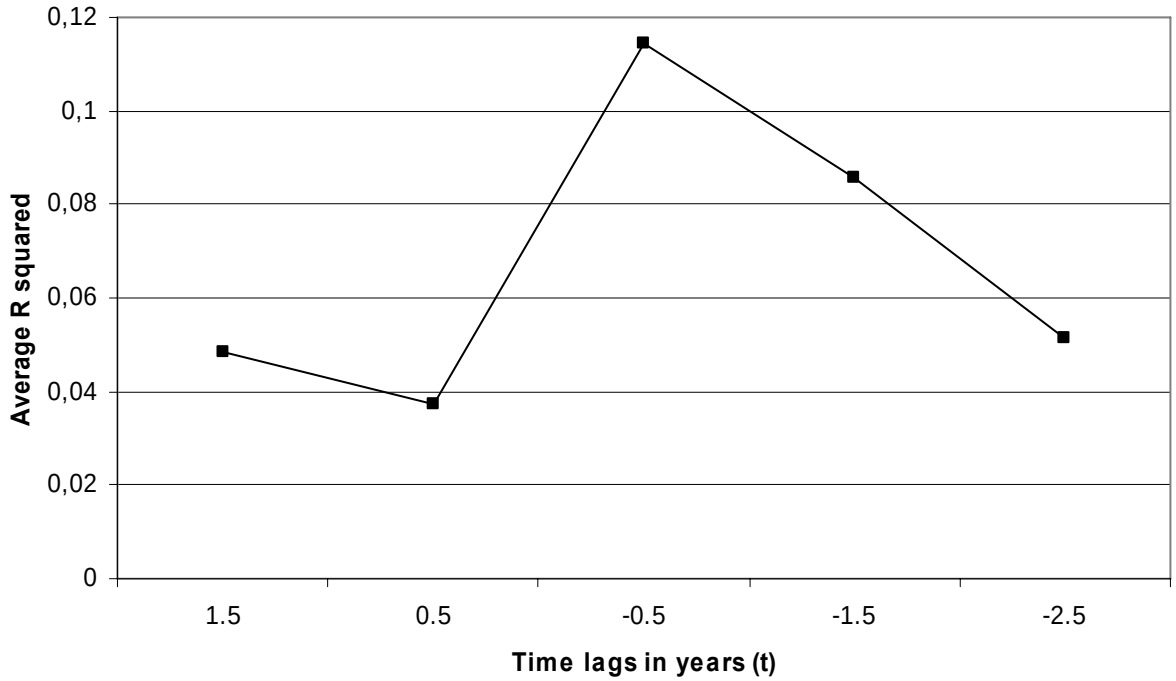
Figure 4.4 gives us a first empirical clue about the time lag between share of parliamentary debate and public spending. For all seven issues analyzed in the next chapters, I have regressed public spending changes on the share of parliamentary debate from 1980 to 2003 using different time lags. Figure 4.4 shows the R square value for each time lag averaged across the seven issues.

A time lag of t–0.5 refers to a correlation between an issue's share of parliamentary debate in, for instance, 2002/03 and public spending in 2003. Consequently, t–1.5 refers to a correlation between parliamentary debate in 2001/02 and public spending in 2003. On average, Figure 4.4 shows that we find the best model fit when we operate with a time lag equal to t–0.5 and the second-best model fit when t equals –1.5.

The results presented in Figure 4.4 cover variation in R square values across issues. However, based on Figure 4.4 and our theoretical expectation of a rather immediate policy effect from variation in the macropolitical agenda, we continue with a time lag of t–0.5 in the statistical models in Chapters 5 and 6. In Chapter 7, we return to this question and explore in much greater detail potential differences in time lags across issues.

Closely related to this discussion about time lags is the question about the direction of causality between an issue's share of parliamentary debate and public spending on that given issue. One might reasonably object that the length of parliamentary debates not only affects public spending but also responds to public spending, thereby giving rise to a potential endogeneity problem that may lead to biased inference (cf King, Keohane & Verba 1994, p. 185).

Figure 4.4. Model fit for different time lags, 1980-2003




Notes: R square values when testing the statistical model: $\Delta Spending_t = a + b*(Parliamentary\ debate)_{(t)} + e$.

According to Figure 4.4 there is no strong correlation when public spending is lagged and correlated with parliamentary debate, which indicates that on average the extent of parliamentary debate does not respond to previous spending decisions. However, this does not entirely rule out the potential endogeneity problem since debate in year t–0.5 and perhaps even year t–1.5 may respond to proposed spending decisions. Such an effect probably exists, but I would argue that the direction of bias caused by this effect mainly works against our hypotheses for the following reason.

First, recall that we expect a positive relationship between the share of annual debate in parliament devoted to a given issue and the popularity of spending decisions on that given issue. For instance, if the popular law and order issue is subject to increased debate in parliament, it is plausible that the government will take a closer look at this issue and – if our theoretical model has empirical validity – increase spending on law and order related issues in a manner so that it is more consistent with public opinion. On the other hand, if there is hardly any debate in parliament over law and order issues, the money is better spent – in terms of electoral gains – in another more visible policy domain. Consequently, in this case we will find a positive relationship between debate in parliament and popularity of policy adjustments.

On the other hand, it seems quite unlikely that very popular spending proposals such as increasing the budget for law and order related activities would generate fierce and extensive debate in parliament. As noted by Scharpf (1997, p. 186), a rational opposition is not likely to oppose policies that are public interest oriented. If the government proposes a very unpopular bill instead, for instance to cut down spending on law and order issues, it seems much more likely that this proposal will generate debate in Parliament. Therefore, the direction of the causal effect that popularity of public spending decisions will exert on extent of parliamentary debate is most likely negative.

This little example helps to ascertain the direction of bias caused by the potential endogeneity problem. Given the annual and rather aggregated meaures, we cannot fully control for the causal effect from spending proposals on extent of parliamentary debate. Based on the above reasoning, however, we may infer that the correct estimate of the effect of parliamentary debate on public spending is probably larger than the one we find in the statistical analyses conducted in Chapters 5 and 6. In other words, some of the effect of debate in parliament on public spending changes may be suppressed by an opposite effect going from popularity of spending proposals to degree of parliamentary debate. In light of the empirical results, I shall return to this important question of causality in Chapter 7.

## Summary

In the first part of this chapter, I stated that the central assumptions about the party competition system in the macropolitical venue are met sufficiently well in the national Danish political system. However, the multiparty Danish bloc-system may be considered a rather conservative test of the model, whereas some of the postulated mechanisms may be more evident in a classic Westminster two-party system with a strong majority government.

In the second part of the chapter, the theoretical model from Chapter 3 was turned into a testable empirical model. Using public spending and surveys on public spending attitudes as measures of public policies and public opinions, respectively, corresponds quite closely with the approach adopted in many earlier opinion policy studies. A more novel item is the empirical measure of the macropolitical agenda based on length of annual debates on the floor of the Danish national parliament.

The backbone of the next chapters consists of a set of time series regressions on the development in public spending on the seven policy domains selected in this chapter. The specific statistical models to be estimated, including a set of relevant control variables, are introduced in the following chapters.

## Notes

1. The only exception from the bloc nature of Danish party competition was the short-lived and not very successful social democratic-liberal government from 1978 to 1979.
2. For studies using a similar approach, see Kristensen 1982; Hartley & Russett 1992; Wlezien 1995; 1996; Eichenberg & Stoll 2003.
3. Based on personal communication with representatives from Statistics Denmark.
4. This category covers education to children of age 6 to 15 years old. In the following, the spending category will just be termed "primary education".
5. For a detailed description, see Green-Pedersen (2004).
6. The categorization is explained further in Appendix 1 and in subsequent chapters.
7. This categorization is explained further in Appendix 1 and in the following chapters.
8. Out of curiosity, the measure based on fixed probabilities has been tried in the analyses in Chapters 5 and 6, and though it does seem to capture some of the same trends as the simple dichotomous measure, the explanatory power of the latter is on the whole much stronger.
9. Please note that the third central variable, public opinion, in the setup of this study enters the analyses in a more indirect way.
10. Where worrisome indications of violations of the OLS assumptions, these are reported and discussed along with the evaluation of the regression results in Chapters 5, 6, and 7.

# Chapter 5

# Parliamentary debate and spending on popular issues

Common to the four issues studied in this chapter is the fact that a majority of the public prefers increased public spending. Therefore, given the model presented in the two previous chapters, we examine the empirical validity of the hypothesis that there is a positive relationship between share of parliamentary debate and public spending on these four issues. We also examine whether this relationship is stronger in election years than in non-election years.

The chapter is structured as follows. The question about control variables is addressed at the outset. Then the analysis is presented in two steps. First, we are interested in the main effects of parliamentary debates and election cycles. We therefore estimate the statistical models in each of the four policy domains without the interaction term between length of parliamentary debate and the election year dummy. Second, in the latter part of the chapter, we estimate the same models including the interaction term in order to evaluate how this affects the statistical models of public spending changes.

## Control variables

The number of control variables is naturally restricted in a statistical time series design such as the one pursued in this study; partly because of the small number of observations and partly because of difficulties in obtaining reliable time series from 1980 to 2003. However, earlier policy studies have pointed to a set of usual suspects, which clearly should be controlled for in the following statistical models. By including these variables, we reduce the risk of omitted variable bias in the statistical estimates and improve the model's forecasting of future values of the dependent variable (cf King, Keohane & Verba 1994, p. 169).

First, from studies of budgetary incrementalism we know that last year's spending changes are likely to affect this year's spending changes (cf Danziger 1978, p. 157; Boyne et al. 2000, p. 59). Furthermore, studies by Soroka and Wlezien (2005, p. 686) have shown that year-to-year variation in lagged public spending attitudes is correlated with a lagged spending variable. Hence, while we cannot in this study measure this underlying variation in spending attitudes directly (cf Chapter 4), including a lagged spending variable clearly reduces the potential bias from our time constant measurement of majority spending attitudes. I shall return to this question in much

greater detail in Chapter 7 in a discussion of Wlezien's Thermostat Model as an alternative explanation.

Second, the economic condition of society is a variable that may affect public spending via changing "objective needs" for certain programs. Improved economic conditions may also imply an enlargement of the total budgetary pie, which by itself may lead to public spending increases (cf Boyne 1996, chapter 4). Consequently, we control for development in national economic variables. The two indices we include in the following analyses are annual changes in unemployment levels and annual changes in levels of inflation. The purpose of including both indicators is to cover different aspects of the same underlying economic dimension (cf Bawn 1999).[1] As mentioned in the previous chapter, we look at changes instead of levels based on the belief that reactions by the public and policy makers will most often be to relative changes in real-world indicators rather than to their actual values.

Third, the ideological color of the government is another variable that may affect the relationship described in the model above. In the "politics matter" tradition, it has been a much contested issue whether this variable has a systematic effect on public spending (see Hibbs 1977; Castles 1982, 2001; Sharpe & Newton 1984; Cusack 1999). This is perhaps most clearly expressed by the fact that the two most extensive literature reviews arrive at diametrically opposite conclusions (Schmidt 1996; Imbeau et al. 2001). However, given the attention to this variable also within the opinion policy literature (cf Chapter 2) it seems relevant to control for this additive effect. The variable is constructed as a dummy variable assigned the value 1 if the government is led by the left-wing bloc of parties in parliament and 0 otherwise.[2]

Finally, in addition to these controls we may also find more domain specific indicators to be important. When the number of children of age 6 to 15 years old increases, then spending on primary education likely increases too, or when the number of annual crime reports increases then public spending on law enforcement may increase too. Such effects may or may not enter via changes in the composition of the macropolitical agenda, but in some policy areas, they probably do exert some influence on the median spending preferences of the electorate, the parliamentary debate, and public spending on the given issue. Therefore, in order to isolate the effect of the macropolitical agenda, we should control for such domain specific policy determinants. In many instances, however, it is not at all obvious how to identify the objective need for a given policy (cf Hilgartner & Bosk 1988; Kingdon 1995). For instance, what is the objective need for cultural policy or defense policy? Nevertheless, where reasonable and possible we attempt to control for such

domain specific "need indicators". The exact content of these variables is described along with the presentation of the issue-specific analyses below.

The question about appropriate time lags also applies to the control variables. Since we do not have any firm theoretical prediction about the appropriate lag structure, I have tried to be rather explorative and have examined the effect of different time lags. As a general decision rule, I have aimed to give the control variables as much leverage as possible in order to raise the requirements of the empirical burden of proof with respect to the hypotheses of main theoretical interest. In some instances, in particular for measures of changes in demographic structures, it seems that the policy makers anticipate these developments when deciding on the budget. In others, it would seem they respond to previous variation in the control variables.

Having laid out the basic components of the statistical models explored in this and the following chapter, we move on to a presentation of the empirical results for each of the four popular issues examined in this chapter. The analyses are presented in two steps. First, we are interested in the main effects of parliamentary debates and election cycles. Therefore, we estimate the models in each issue area without the interaction term between length of parliamentary debate and the election year dummy. Second, in the latter part of the chapter, we estimate the same models including the interaction term in order to evaluate how this affects the statistical models of public spending changes.

## Law and order

In Denmark, the police force and other law and order related institutions such as prisons are solely under the jurisdiction of the central state. Hence, there is no spending by municipalities or regional counties on these activities, including law court institutions. About two thirds of Danish public spending on law and order related issues goes to the national police force, whereas the remaining one third is mainly spent on prisons and on the administration of justice.

In addition to the control variables introduced above, we include in this statistical analysis two measures related specifically to the law and order issue. First, we control for the annual number of police reports per 100 persons 15 years old or more. Second, we control for the reports on violence as a share of total annual police reports. As noted by Lauersen (2001, p. 22), it is far from obvious how these police statistics should be interpreted. Sometimes they do reflect a factual increase or decrease in the number of reports, but often they may just reflect a change in registration practise. Furthermore, the statistics are clearly influenced by legislation about what is legal and what is not, which means that they are most likely also a consequence of political decisions.

Despite this uncertainty about the status of these control variables, they should nevertheless be included in a study of spending effects of annual length of parliamentary debate. If a statistical effect of our proxy of the macropolitical agenda disappears when variation in the level of crime is controlled for, then this would indeed call into question the relevance and empirical validity of the theoretical model outlined in Chapter 3. In that respect, please note that the political system can easily react to an increase in reports on violence, without this increase necessarily engendering an increase in the length of debate on the floor of the national parliament. In other words, the agenda measure applied in this dissertation enables us to isolate spending effects caused by variation in the macropolitical agenda from the spending effects that follow from, for instance, policymakers' problem solving activities that may or may not give rise to variation in the macropolitical agenda.

Table 5.1 displays the main results of the analysis of spending on law and order issues in Denmark from 1980 to 2003. It consists of four statistical models. As argued by Leamer (1983, p. 38), statistical estimates may be more or less sensitive to changes in model assumptions and the inferences we make should be judged by the robustness of the estimates. The number of changes in model assumptions that we can evaluate directly in this study is clearly limited owing to the number of observations in each time series. However, the robustness of the factor of most interest, length of parliamentary debate, can be addressed to some extent based on models that include alternative combinations of this variable and the statistical control variables (cf Scruggs 1999, p. 19). This is the logic behind displaying the four models in Table 5.1.

Please recall from Chapter 4 that changes in the log of a variable denote relative changes which, if multiplied by 100, approximate percentage changes (Gujarati 2003, p. 857). Hence, all coefficients have been multiplied by 100, such that the estimates shown in Table 5.1 roughly represent how many percentage points annual spending on law and order issues change if the explanatory variable changes by one unit. Given the importance of the non-autocorrelation assumption in standard OLS-regression, the lower part of Table 5.1 displays the result of three different tests for serial autocorrelation in the error terms. In addition to the conventional Durbin-Watson statistic, the Durbin's h statistic and the more powerful Breusch-Godfrey test have been calculated. In contrast to the classic Durbin-Watson statistic, the latter two tests also work on models with a lagged dependent variable (Gujarati 2003, p. 471). The null hypothesis in these tests is that there is no serial correlation in the error terms and Table 5.1 reports the p-value from this test.

Table 5.1. Spending on law and order, 1980-2003

| Dependent variable = Δ Log of real spending | | | | |
|---|---|---|---|---|
| | Model I | Model II | Model III | Model IV |
| Constant | –.2.883 | –.119*** | –3.041* | –11.709*** |
| | (2.334) | (.024) | (1.634) | (2.417) |
| Debate in parliament$_{t-.5}$ | .917* | 1.844*** | | 2.059*** |
| | (.478) | (.442) | | (.426) |
| Δ [a]Crime reports$_{t-1}$ | | 7.900*** | 5.000** | 6.830*** |
| | | (1.700) | (2.300) | (1.700) |
| Δ Reports on violence$_{t-1}$ | | 30.155*** | 20.870** | 27.568*** |
| | | (7.001) | (9.606) | (7.021) |
| Δ Log of real spending$_{t-1}$ | | –.260 | –.374 | |
| | | (.160) | (.229) | |
| Election year$_t$ | | 2.639* | 1.821 | 3.224** |
| | | (1.371) | (1.964) | (1.366) |
| Left government$_{t-1}$ | | 2.425 | 4.484** | |
| | | (1.490) | (2.035) | |
| Δ Inflation$_{t-1}$ | | –1.036 | –1.173 | |
| | | (.668) | (.966) | |
| ΔΔ Unemployment$_{t-1}$ | | –2.964*** | –1.126 | –3.088*** |
| | | (.940) | (1.202) | (.894) |
| N (observations) | 23 | 23 | 23 | 23 |
| $R^2$ | .15 | .75 | .43 | .67 |
| $R^2$ (adjusted) | .11 | .60 | .17 | .57 |
| F-test statistic for model | 3.69* | 5.18*** | 1.64 | 6.87*** |
| Durbin-Watson statistic | 2.03 | – | – | 2.17 |
| Durbin's h (p-value) | .923 | .372 | .120 | .621 |
| Breusch-Godfrey (p-value) | .917 | .245 | .065 | .556 |

Notes: Unstandardized betas with standard errors in parentheses. *$\alpha \leq 0.10$; ** $\alpha \leq 0.05$; ***$\alpha \leq 0.01$.

As noted in Chapter 4, both unit root tests and the Box-Jenkins autocorrelation and partial autocorrelation functions have been employed to ensure stationarity in the dependent and the explanatory variables. These tests indicate that the variables are all stationary in the form in which they appear in Table 5.1 and the tables that follow. In most instances, percentage changes or first differences sufficed, but on a few occasions it was necessary to difference the time series twice.

Model I in Table 5.1 shows the bivariate correlation between share of annual length of debate in parliament on law and order related issues and annual changes in public spending on those issues. As expected, the effect of parliamentary debate is positive and statistically significant. According to this model, spending on law and order issues increases by almost one percent,

when the percentage of annual parliamentary debate devoted to law and order issues increases by one.

Model II represents the "full" model, including the control variables discussed above. When these variables are controlled for, the effect of parliamentary debate almost doubles. Upon further investigation, it appears that it is the introduction of the measures of crime reports and reports on violence that account for the increase of this estimate. While these two factors, according to Table 5.1, have a positive and highly statistically significant effect on public spending changes, additional analyses show a weak but negative correlation between these two variables and the parliamentary debate variable. It is not straightforward what accounts for this negative correlation, but it clearly suggests that the positive correlation between debate and public spending is not just a simple reflection of underlying variation in the two control variables.

Furthermore, according to Model II, public spending on law and order issues increases on average 2.6 percentages more in election years than in non-election years. As expected, last year's spending on law and order issues has a negative impact on this year's spending, but this effect is not statistically significant. The effect of party rule and the effect of changes in the levels of inflation are also statistically non-significant. On the other hand, acceleration in unemployment changes seems to exert a negative effect on public spending, suggesting a reasonable trade-off between economic problems and spending on law and order issues. One should note that using this "acceleration" measure instead of a simple first order change measure is a consequence of the stationarity tests. This measure, however, should capture dynamics that are very similar to a first order change measure, and additional analyses have indicated that this is in fact so.

It could be that length of parliamentary debate suppresses the effect of other more underlying factors. In Model III, to address this question, we run an analysis similar to Model II, but this time without the parliamentary debate variable. The signs of the coefficients in Models II and III are identical and the effects of crime reports and reports on violence are still positive and statistically significant. On the other hand, the statistical significance of some of the control factors in the lower part of Table 5.1 does vary between Model II and Model III. In Model III, the party rule government turns significant, while the effects of election years and unemployment become insignificant. The positive coefficient of the government dummy may seem a bit puzzling in light of the traditional ideological cleavages between the two party blocs and it may likely reflect a time effect rather than an ideological one. In Denmark, law and order issues were rather high on the macropolitical agenda during

the left-wing government rule in the 1990s and additional analyses show a positive correlation between the government dummy and the issue's share of parliamentary debate. Hence, what we see may be an agenda effect rather than an ideological effect. Nevertheless, the notable decline in the goodness of fit ($R^2$) from Model II to Model III suggests that extent of debate in parliament is not just a simple proxy for the color of the government or for some of the other control variables. Instead, it appears to be a quite strong determinant of public spending on law and order activities.

An important objective behind the model specifications conducted thus far was to reduce the risk of specification bias stemming from an omission of relevant explanatory variables. However, over-fitting a model by including irrelevant variables also leads to model misspecification. The consequences of such specification errors are inefficient estimates, that is, their variances will generally be larger than those of the true model. Therefore, the probability inferences about the parameters are less precise, and in addition, we lose degrees of freedom by including irrelevant variables, a problem of particular relevance to a small n study like the present one. As Gujarati warns (2003, p. 516), model specification should not be a mechanical process and should involve an evaluation of a broad range of model diagnostics, including:

> …that all the estimated coefficients have the "right" signs, they are statistically significant on the basis of the t and F tests, the $R^2$ value is reasonably high and the Durbin-Watson d has acceptable value (around 2), etc.

Based on these criteria, Model IV in Table 5.1 represents what seems to be the "best" model based on the sum of such diagnostics. According to the summary statistics displayed in the lower part of Table 5.1, the adjusted $R^2$ is a quite impressive .57, the F-test of the model is strongly statistically significant and the Durbin-Watson d statistic is close to the value of 2. Additional tests for heteroscedasticity, multicollinearity, and outliers do not indicate influential violations of the standard assumptions behind OLS-regression. One observation deviates moderately from the model, but according to a leverage versus residual squared plot, this observation is less important because its values on the explanatory variables are close to the mean.

In Model IV, as one would expect, when the number of crime reports and the share of reports on violence increase, so does public spending on law and order issues. The negative impact of unemployment increases on spending on law and order issues is also fully understandable, although we did not have strong theoretical expectations beforehand on the sign of this coefficient. Of more theoretical interest, the effect of parliamentary debate in Model IV is positive, strong, and clearly statistically significant, and the same is true for

the election year dummy. When using comparable standardized betas, the corresponding coefficients are .868 for the parliamentary debate variable, .774 for the number of annual crime reports, .688 for the share of reports on violence, –.684 for the unemployment variable, and .384 for the election cycle dummy. This clearly supports the conclusion that share of parliamentary debate exerts a strong influence on spending on law and order issues.

In Chapters 3 and 4, it was speculated that the functional form of the relationship between the variables of theoretical interest could be characterized by threshold effects. One threshold transformation is to construct a dummy variable taking the value of 1 when the extent of annual parliamentary debate devoted to law and order issues is above a certain threshold and equal to zero otherwise (see Soroka 2003, p. 42). Following Soroka, we employ threshold values of the fiftieth percentile and the seventy-fifth percentile. Having applied the former variable transformation the estimate of parliamentary debate remains positive and statistically significant, but the effect is much smaller. Applying the seventy-fifth percentile transformation, the effect is positive but statistically insignificant.

Another possibility is to replace the measure of parliamentary debate with its square value and retest the models shown above. In that case, years with much debate on law and order issues would also weigh stronger than years with only little debate. However, when retesting the models shown above using this measure of parliamentary debate, the coefficient remains positive and statistically significant, but its explanatory power is considerably lower. Hence, it seems that the relationship between parliamentary debate and public spending on law and order issues is best represented by a linear functional form.

To sum up, the analysis of the law and order issue is consistent with the hypothesis that when a majority of the public prefers increased spending the relationship between parliamentary debate and public spending is strong and positive. The estimated effect of a one percent increase in the share of annual parliamentary debate ranges between one and two percent increases in public spending across the models shown in Table 5.1. The hypothesis that spending on popular issues in election years will be higher than in non-election years is also supported by the analysis of the law and order issue. The election year effect is more fragile and less strong, but generally, it is positive and statistically significant.

## Pollution control

With some variation across years, the responsibility for pollution control in Denmark has been divided between central government institutions and the municipalities in the period covering 1980 to 2003. In the spending data

from Statistics Denmark, about half of the public spending is specified in the central state budget, whereas the other half is spent by the municipalities or in the counties. Earlier research on Danish environmental issues has noted the politicization and increased political debate over various aspects of this issue in the period of interest (Andersen & Hansen 1991; Christiansen 1999). In this section, we investigate the systematic spending consequences of these changes in the environmental issue's status on the macropolitical agenda.

The analysis in this section follows the scheme laid out above. First, we consider what kind of issue specific controls to include. In that respect, we are looking for indicators of the environmental conditions, that is, of the extent of pollution problems. Degree of pollution, however, is a disputed factor and the evaluation of this condition has probably changed somewhat over time along with the development of more and better analytical tools and along with the extension of scientific knowledge. Furthermore, there is no strong expectation of an automatic relationship between extent of pollution and spending on pollution control. And if such a relationship exists, part of the causality probably goes from spending on pollution control to extent of pollution. However, to lower the risk of omitted variable bias we seek to be rather inclusive in this analysis. I have therefore included a "pollution index" ranged from 0 to 10 in the analysis reported in Table 5.2. The index is inspired by Scruggs (1999) and is based on four annually measured pollution indicators. The higher the value of this index, the stronger the indication of pollution problems. We would thus expect a positive relationship between a lagged version of this indicator and public spending on pollution control. The construction of the index is explained in Appendix 1.

Table 5.2 shows the result of the analysis. For the same reasons as laid out above it includes four statistical models. Not surprisingly, given the volatile character of this spending category shown in Chapter 4, the coefficients in Table 5.2 are much larger than those in Table 5.1. It should however be noted that if measured in absolute values, the size of the coefficients would be much smaller (cf Figure 4.1).

In Table 5.2, all models including the measure of share of annual length of debate devoted to environmental issues show a positive, strong, stable and statistically significant effect of this variable. Depending on which controls are included, a one unit increase in the percentage of the total parliamentary debate devoted to environmental questions is associated with a 10-13 percent increase in public spending on pollution control. The effect of election years is even more impressive. On average, the government has spent around 40 % more on pollution control in election years compared to non-election years. Again, when evaluating the magnitude of these coefficients, keep in mind

that the pollution control category is a rather small spending category, which means that in absolute values the spending effects are more moderate.

Table 5.2. Spending on pollution control, 1980-2003

| Dependent variable = Δ Log of real spending | | | | |
|---|---|---|---|---|
| | Model I | Model II | Model III | Model IV |
| Constant | –70.225** | –59.077* | 2.396 | 77.408*** |
| | (29.654) | (29.155) | (17.275) | (23.726) |
| Debate in parliament$_{t-.5}$ | 13.520*** | 10.571** | | 11.241*** |
| | (4.746) | (4.293) | | (3.797) |
| Δ Pollution index$_{t-1}$ | | 2.337 | 16.311 | |
| | | (23.093) | (25.683) | |
| Δ Log of real spending$_{t-1}$ | | –.049 | –.005 | |
| | | (.161) | (.184) | |
| Election year$_t$ | | 47.172** | 57.825** | 40.051** |
| | | (20.101) | (22.551) | (16.099) |
| Left government$_{t-1}$ | | –28.179 | –26.417 | |
| | | (20.189) | (23.149) | |
| Δ Inflation$_{t-1}$ | | –12.560 | –13.062 | –21.480*** |
| | | (10.907) | (12.512) | (6.761) |
| ΔΔ Unemployment$_{t-1}$ | | –5.056 | –11.459 | |
| | | (11.780) | (13.183) | |
| N (observations) | 23 | 23 | 23 | 23 |
| $R^2$ | .28 | .65 | .50 | .59 |
| $R^2$ (adjusted) | .24 | .48 | .32 | .53 |
| F-test | 8.11*** | 3.92** | 2.70* | 9.22*** |
| Durbin-Watson statistic | 1.67 | – | – | 1.95 |
| Durbin's h (p-value) | .639 | .673 | .222 | .935 |
| Breusch-Godfrey (p-value) | .617 | .591 | .149 | .927 |

Notes: Unstandardized betas with standard errors in parentheses. *$\alpha \leq 0.10$; ** $\alpha \leq 0.05$; ***$\alpha \leq 0.01$.

Similar to the law and order issue, the effects of the economic indices are negative, this time with the inflation indicator being statistically significant, as shown in Model IV. The negative effect of party rule may seem a bit surprising in light of the left-wing parties' traditional ideological stance on this issue. Again this might reflect a time effect more than an ideological one, since Danish mobilization on the environmental issue took off during the bourgeois government in the 1980s. Nevertheless, the effects of last year's changes in public spending and the effect of party rule are both clearly statistically insignificant.

As expected, the effect of the pollution index is positive, but it does not come close to being statistically significant in any of the models. I have also explored alternative time lags of this variable, but the coefficients of these are

even more statistically insignificant. The coefficient does increase notably in Model III, indicating that some of the effect from this variable may enter via the variable measuring length of parliamentary debate. Additional analyses show that there is a positive correlation between the pollution index and extent of debate, but it is rather weak and statistically insignificant. A positive correlation between the extent of the problem and the level of parliamentary debate is fully reasonable, but the empirical results clearly suggest that this cannot account for the strong correlation between public spending changes and the extent of parliamentary debate on this issue. In fact, the general stability of coefficients across Models II and III reassures us that the variable of central theoretical interest, the debate variable, is not just a simple function of some of the other variables.

The summary statistic shown in the lower part of Table 5.2 is healthy and does not imply serious auto-correlation problems. Furthermore, in Model IV we end up with a quite impressive explanatory power with an adjusted $R^2$ equal to .53. Additional tests for deviating observations indicate a potential outlier in 1982. As a supplementary control, the model has been reanalyzed with a dummy variable for 1982. The effect of this dummy is negative and statistically significant. Including this dummy does reduce the effect of the inflation variable, but it only invokes a very slight reduction in the coefficients for parliamentary debate and election years. Consequently, the estimates of theoretical interest in Table 5.2 do not seem to be driven by a single influential observation. On the contrary, they appear robust to the various model adjustments explored above. In addition, tests for heteroscedasticity and multicollinearity do not indicate serious problems with respect to these standard OLS assumptions.

Replacing the unstandardized betas in Model IV in Table 5.2 with comparable standardized betas, the corresponding coefficients are .439 for the parliamentary debate variable, .368 for the election year dummy, and –.472 for the inflation variable. Replacing the measure of parliamentary debate with its square value and retesting the models shown above, the coefficient remains positive and statistically significant, but the overall model fit is considerably lower. Using the fiftieth percentile dummy described above, the effect is positive and statistically significant across all models, but again the model fit is somewhat poorer. For the seventy-fifth percentile dummy, the effect is positive but statistically insignificant. Consequently, it seems that the relationship between parliamentary debate and public spending on pollution control is best estimated by a linear functional form.

All in all the analysis in this section is consistent with the hypothesis that on issues where the median voter prefers increased public spending, there is

a positive relationship between extent of parliamentary debate and public spending. In fact, the analysis has shown that this effect is rather strong on the pollution control issue. Furthermore, the analysis has demonstrated that on average, public spending on pollution control increases much more in election years than in non-election years.

### Primary education

Danish primary education covers education for children aged 6 to 15 years old. The public schools are run by the Danish municipalities, and hence most of the money is spent in the municipalities. This does not preclude central government spending, but most is given as unspecified grants. However, politicians at the central national level do have influence on primary education in Denmark. Annual negotiations between the central government and the municipalities are often quite detailed, and substantial rule-making on this issue is carried out by national politicians. However, compared to the law and order issue and the pollution control issue, there is much greater local autonomy on this issue. In fact, according to the data from Statistics Denmark, averaged over the years, almost 75-80 % of public spending on primary education was spent by the municipalities. Consequently, earlier studies of Danish primary education have often focused on variation between municipalities or between schools (Mouritzen 1991, pp. 98-102; Houlberg 2000; Ministry of the Interior 2000; Blom-Hansen 2004). This research has pointed to various issue specific explanations, such as number of single parent households, size of the schools or the municipalities, local election cycles, number of children aged 6 to 15, and sometimes number of immigrant children aged 6 to 15.

In this section, we look at the development on an aggregated national scale. At this level of analysis, some of the variables mentioned above become less relevant, whereas others remain very appropriate. Hence, I have employed measures of single parent households, local election cycles, number of the population aged 6 to 15 years, and number of immigrants aged 6 to 15 years. Only the latter two of these variables turned out to affect the model fits significantly, and hence, the others are not included in the analysis of public spending on primary education in Denmark from 1980 to 2003 presented in Table 5.3.

According to Model I in Table 5.3, the bivariate correlation between the extent of parliamentary debate on primary education and total public spending on primary education is positive, but clearly statistically insignificant, and judged by the summary statistics in the lower part of Table 5.3, Model I is a very weak and unhealthy statistical model. Including a year

counter removes the serial correlation, but does not change the magnitude or statistical significance of the coefficient.

Table 5.3. Spending on primary education, 1980-2003

| Dependent variable = Δ Log of real spending | | | | |
|---|---|---|---|---|
| | Model I | Model II | Model III | Model IV |
| Constant | .002 | –4.074* | –3.128** | –3.604* |
| | (1.865) | (2.178) | (1.377) | (1.739) |
| Debate in parliament$_{t-.5}$ | 1.204 | 1.097 | | 1.167 |
| | (1.881) | (1.926) | | (1.407) |
| Δ Number of immigrants | | 1.36** | 1.43** | 1.360*** |
| Age 6-15$_t$ | | (.558) | (.530) | (.406) |
| ΔΔ Number of children | | 1.790 | 1.950 | 2.120* |
| Age 6-15$_t$ | | (1.510) | (1.450) | (1.190) |
| Δ Log of real spending$_{t-1}$ | | .058 | .050 | |
| | | (.243) | (.237) | |
| Election year$_t$ | | 1.407 | 1.387 | 1.284 |
| | | (1.277) | (1.247) | (1.048) |
| Left government$_{t-1}$ | | .728 | .403 | |
| | | (1.481) | (1.336) | |
| Δ Inflation$_{t-1}$ | | –.511 | –.641 | |
| | | (.718) | (.665) | |
| ΔΔ Unemployment$_{t-1}$ | | .118 | .005 | |
| | | (.837) | (.790) | |
| N (observations) | 23 | 23 | 23 | 23 |
| $R^2$ | .02 | .56 | .55 | .53 |
| $R^2$ (adjusted) | –.03 | .31 | .34 | .43 |
| F-test statistic for model | .41 | 2.24* | 2.63* | 5.13*** |
| Durbin-Watson statistic | 1.06 | – | – | 1.54 |
| Durbin's h (p-value) | .047 | .850 | .954 | .572 |
| Breusch-Godfrey (p-value) | .051 | .802 | .941 | .515 |

Notes: Unstandardized betas with standard errors in parentheses. *$\alpha \leq 0.10$; ** $\alpha \leq 0.05$; ***$\alpha \leq 0.01$.

In Model II, we add the set of other policy determinants. With respect to the two issue-specific variables, I have explored various lag structures, but it turns out that the non-lagged version results in the highest leverage. This indicates that politicians either anticipate developments in these demographic measures when deciding on the budget, or that local politicians are largely able to adjust spending to match current changes in demographic factors.

In Model II, variation in the number of immigrant children is the only factor that exerts a statistically significant effect on public spending. According to this model, when the number of immigrants aged 6 to 15 increases by

1000 people, then public spending on primary education increases by 1.36 percent. The national election year dummy is also positive, but not close to reaching the level of statistical significance. In order to impose stationarity in the time series measuring number of people aged 6 to 15 it was necessary to difference it twice. Hence, the coefficient in Model II in Table 5.3 displays the percentage spending increase if the number of children between the ages of 6 and 15 increases by 10,000 more than the average increase. However, this variable is also statistically insignificant in Model II. The lagged spending variable, the government dummy, and the economic indices are all clearly statistically insignificant.

Despite the lack of the parliamentary debate variable, the coefficients in Model II and Model III are almost identical. Judged by the many statistically insignificant effects in Models I, II, and III, it clearly seems that the models are over-fitted, and thus could be reduced notably. Model IV represents what seems to be the best model specification given the variables examined in this section. Since extent of parliamentary debate is the variable of main theoretical interest, I retain it in the model despite its statistical insignificance. If it is left out, the adjusted $R^2$ would increase from .41 to .43. I also keep the election year dummy, which neither increases nor decreases the explanatory power of the statistical model.

Hence, according to Model IV the two population measures alone explain more than 40 % of the variation in total public spending on primary education in Denmark from 1980 to 2003. Judged by the empirical outcomes of the studies referred to above, this result covers much underlying variation across municipalities and across schools. However, looking at the aggregate national level we get a fairly sound model of primary spending by knowing the variation in these two variables only. Furthermore, the regression diagnostics of this model seem quite healthy.

Theoretically, the important point of the above analysis is the weak explanatory power of the parliamentary debate variable and the election year dummy. As expected, the spending effects of both variables are positive, but none of them is strong enough to come near statistical significance. Additional exploration of threshold effects does not change this conclusion.

According to the theoretical reflections in Chapters 3 and 4, the explanation of these results may be found in the degree of local autonomy involved in spending on primary education in Denmark, but at this stage of the analysis it is too soon to conclude anything in that direction. Hence, we return to this question later in light of the outcome of the analysis of all seven issues. In addition, we still need to evaluate the combined effect of extent of parliamentary debate and the election year dummy. In fact, as shown in the last

part of this chapter, this interaction term somewhat qualifies the conclusion drawn from the analysis in this section.

### Health

Provision of health related services is a large part of the Danish welfare state. Between 1980 and 2000, between 8 and 9 % of GNP was spent on health related issues (Ministry of Interior and Health 2003, p. 38). Similar to primary education, the vast majority was spent in municipalities or counties. In the dataset from Statistics Denmark, health related spending consists of two main sectors: hospitals and individual health services (see Appendix 1). Whereas the counties have been responsible for hospital services, the municipalities have mainly been responsible for individual health related services. This local involvement in the health area clearly constrains the actions of the central government, but it does not preclude central spending and central government legislation in this area (cf Pallesen 1999, p. 129). For instance, during the 1990s the Danish national parliament has to an increasingly greater extent included health related issues in the annual negotiations over the national budget (ibid. p. 144).

In the analyses of the development in total public spending on health related issues, I have tried out a number of issue-specific control variables. First, changes in demographic factors such as the share of elderly people or an increase in the total population may affect public spending on health because of an increased or decreased demand for health related services (cf Ministry of Interior and Health 2003, p. 50). To account for these effects, I have included a measure of annual changes in the number of people aged 67 and above, and a measure of annual changes in the total size of the Danish population.

Second, technological developments may either decrease or increase public spending on health related services, depending on whether they lead to increased demand or increased efficiency in service production (ibid., p. 52). It is not entirely straightforward just what kind of proxy to use in this respect. In the present case, I have utilized a measure of annual changes in US health patents based on the argument that such technological evolution may be rather similar across these two highly developed countries, or at least disseminate very quickly across such countries (cf Green-Pedersen & Wilkerson 2006).[3]

Finally, the massive local involvement in this issue could imply an effect from a local business cycle measure. In Denmark, the election date is the same across all municipalities and all counties. Consequently, a simple local election year dummy equal to 1 in local election years and else 0 was examined.

It turned out, however, that none of these control variables contributes significantly to the statistical models of Danish public spending on health

related issues, a finding that seems to be consistent with the main conclusion of another recent study looking at the development in Danish public spending on health related issues in the 1990s (Ministry of the Interior and Health 2003, pp. 48-52). Consequently, Table 5.4 below only reports the result of the analysis including the demographic factors, which were the variables that came closest to attaining the level of statistical significance.

Table 5.4 shows the main results of the analysis of public spending on health related issues in Denmark from 1980 to 2003. In both Models I and II, the spending effect of extent of parliamentary debate is positive, but weak and statistically insignificant. According to Models II and III, a similar conclusion holds for the estimate of population effects and for that of unemployment changes. These coefficients are all positive, but statistically insignificant. The effect from changes in inflation levels is negative, but indistinguishable from 0. The only statistically significant effects in Table 5.4 come from the two dummy variables measuring election cycles and party rule, respectively. As expected, the election dummy variable is statistically significant, but contrary to our theoretical expectations, the effect is negative, which means that on average, spending changes on public health have been lower in election years than in non-election years. The positive and statistically significant effect of the party rule coefficient is more comprehensible. On average, according to Models II and III in Table 5.4, spending on public health has increased by almost two percentage points more in times of left-party rule than in times of a bourgeois government.

The overall explanatory power of the statistical models shown in Table 5.4 is not impressive. According to the F-test statistic in the lower part of Table 5.4, none of the models reaches the level of statistical significance. In fact, based on the factors displayed in Table 5.4 it was not possible to construct a model, which is statistically significant at the .10 cutoff point. That is also why Table 5.4, in contrast to the tables above, does not contain a Model IV column. If we reduce the model further, every coefficient ends up being statistically insignificant, including the election year dummy and the party government dummy. Adding some of the other potentially relevant policy determinants, such as measures of local election cycles and development in the number of health patents, does not affect this conclusion. Neither does it affect the model fit, nor the explanatory power of the parliamentary debate variable if we apply the threshold variable transformations explored above.

Of main theoretical interest, the results in Table 5.4 clearly suggest that the percentage of total annual debate in parliament devoted to health related issues does not systematically affect public spending on Health. Furthermore, Table 5.4 shows that public spending on public health issues is lower in elec-

tion years than in non-election years, although this effect turns statistically insignificant when some or all of the other explanatory factors are removed from the model.

Table 5.4. Spending on health, 1980-2003

| Dependent variable = Δ Log of real spending | | | |
|---|---|---|---|
| | Model I | Model II | Model III |
| Constant | .784 | –.375 | 1.599* |
| | (1.393) | (1.465) | (.831) |
| Debate in parliament$_{t-.5}$ | .159 | .347 | |
| | (.253) | (.263) | |
| ΔΔ People$_t$ | | .165 | .158 |
| | | (.105) | (.108) |
| ΔΔ People aged > 67$_t$ | | .300 | .348 |
| | | (.201) | (.202) |
| Δ Log of real spending$_{t-1}$ | | –.135 | –.098 |
| | | (.214) | (.217) |
| Election year$_t$ | | –2.488** | –1.953* |
| | | (1.040) | (.982) |
| Left Government$_{t-1}$ | | 1.861* | 1.783* |
| | | (.939) | (.960) |
| Δ Inflation$_{t-1}$ | | –.375 | –.445 |
| | | (.450) | (.458) |
| ΔΔ Unemployment$_{t-1}$ | | 1.032 | 1.039 |
| | | (.798) | (.817) |
| N (observations) | 23 | 23 | 23 |
| $R^2$ | .02 | .47 | .40 |
| $R^2$ (adjusted) | –.03 | .16 | .12 |
| F-test statistic for model | .40 | 1.52 | 1.42 |
| Durbin-Watson statistic | 2.04 | – | – |
| Durbin's h (p-value) | .759 | .101 | .277 |
| Breusch-Godfrey (p-value) | .742 | .047 | .181 |

Notes: Unstandardized betas with standard errors in parentheses. *$\alpha \leq 0.10$; ** $\alpha \leq 0.05$; ***$\alpha \leq 0.01$.

Our confidence in these unexpected findings, however, is somewhat shaken by the general lack of a satisfying and statistically significant model of the development in health related public spending. One reason for the poor model fits could be the high degree of local involvement in Danish public health spending. The noise from underlying variation at the local and regional level of policymaking might make it very difficult to identify a robust explanation in analyses conducted on national data. In theory, one could pursue this alternative explanation further, separating out central government spending from local government spending on health. In practice, however, it is not

feasible to make this distinction in the spending data because the vast majority of central government spending on health related issues is registered as general grants, and is hence unspecified in the national budget.

Alternatively, the poor fit of the statistical models of national health spending could stem from the aggregation across what could potentially be rather diverse health subcategories. In that respect, our data allows us to divide health spending into two separate, and perhaps more internally homogenous categories. The one subcategory covers spending on hospitals, while the other covers spending on individual health services such as medical treatment, medication, dental treatment, public health, etc. (see Appendix 1). Consequently, to examine the robustness of our results we have re-run the analyses in Table 5.4 on each of these two health related subcategories, including a division of health related debate in parliament into two corresponding categories (see Appendix 1).

These analyses, however, do not change any of the main conclusions based on Table 5.4. With one exception, the directions of all coefficients are similar to those displayed in Table 5.4. The statistical models for public spending on hospitals are very similar to those for the health area as such, although it is possible to identify a statistically significant model consisting of the positive spending effect of left-wing government rule only (the result of this analysis is shown in Table A2.1 in Appendix 2). The effect of parliamentary debate is positive, but statistically insignificant. The effect of the election year dummy is negative, but barely statistically significant. All other effects are statistically insignificant.

As is shown in Table 5.5 below, it is a little more puzzling to interpret the results of the analysis of spending on individual health services. On the face of it, the parliamentary debate estimate and the measures of population changes are positive and statistically significant in the reduced Model IV. However, a closer look at data clearly reveals that this is a very fragile and non-robust result driven by one very deviant observation. As shown in Model V in Table 5.5, when controlling for that particular observation, all coefficients except the one measuring changes in the number of elderly people turn statistically insignificant. Exploring the usual transformations of the parliamentary debate variable does not change this result, nor does the inclusion of a local election cycle dummy or measures of health patents. Therefore, the effect of parliamentary debate on spending on individual health service is too dependent on a single deviant observation to change the impression from the other health related analyses. Consequently, based on the empirical evidence provided in Table 5.4, supplemented with information from the additional analyses of spending on hospitals and on individual health services, we con-

clude that there is no solid and statistically significant effect of the share of annual debate in parliament on annual changes in health related public spending. The election year effect seems equally fragile, but if there is a statistically significant effect, it is most likely a negative one, implying higher spending in non-election years than in election years.

Table 5.5. Spending on individual health services, 1980-2003

| Dependent variable = Δ Log of real spending | | | | | |
|---|---|---|---|---|---|
| | Model I | Model II | Model III | Model IV | Model V |
| Constant | .233 | 1.989 | 3.351** | 1.204 | 1.792 |
| | (1.455) | (1.751) | (1.255) | (1.263) | (.887) |
| Debate in parliament$_{t-.5}$ | 1.709 | 1.620 | | 2.456** | .688 |
| | (1.352) | (1.464) | | (1.160) | (.900) |
| ΔΔ People$_t$ | | .217 | .227 | .203* | .119 |
| | | (.167) | (.168) | (.104) | (.075) |
| ΔΔ People Aged > 67$_t$ | | .834** | .851** | .841*** | .826*** |
| | | (.303) | (.305) | (.284) | (.197) |
| Δ Log of real spending$_{t-1}$ | | –.336 | –.281 | –.336* | –.131 |
| | | (.208) | (.204) | (.284) | (.132) |
| Election year$_t$ | | –2.189 | –2.374 | –1.939 | –1.253 |
| | | (1.511) | (1.513) | (1.239) | (.874) |
| Left government$_{t-1}$ | | –.410 | –.518 | | |
| | | (1.520) | (1.528) | | |
| Δ Inflation$_{t-1}$ | | –.670 | –1.070 | | |
| | | (.786) | (.704) | | |
| ΔΔ Unemployment$_{t-1}$ | | .696 | 1.252 | | |
| | | (1.417) | (1.335) | | |
| Dummy (year 1991 = 1, else 0) | | | | | 10.102*** |
| | | | | | (2.301) |
| N (observations) | 23 | 23 | 23 | 23 | 23 |
| $R^2$ | .07 | .57 | .53 | .53 | .79 |
| $R^2$ (adjusted) | .03 | .32 | .31 | .39 | .71 |
| F-test statistic for model | 1.60 | 2.30* | 2.42* | 3.86** | 9.89*** |
| Durbin-Watson statistic | 2.42 | – | – | – | – |
| Durbin's h (p-value) | .238 | .690 | .982 | .843 | .129 |
| Breusch-Godfrey (p-value) | .221 | .597 | .977 | .813 | .080 |

Notes: Unstandardized betas with standard errors in parentheses. *$\alpha \leq 0.10$; ** $\alpha \leq 0.05$; ***$\alpha \leq 0.01$.

The analyses conducted thus far have provided some supportive evidence for the hypotheses tested in this section. They have also pointed to substantial variation across issues, however. This variation seems to correspond quite closely to variation in degree of local autonomy. But before we can conclude anything in that direction, we need to include the three unpopular spending

issues analyzed in the next chapter. Before doing so we examine the potential interactive effect of share of parliamentary debate and the election cycle dummy.

## The combined effect of election years and parliamentary debates

In this last part of Chapter 5, we examine whether there is a combined spending effect of election years and the extent of parliamentary debate. As expressed by hypothesis H2.2a in Chapter 4, we expect the positive relationship between share of parliamentary debate and public spending to be stronger in election years than in non-election years.

The analysis in this section proceeds as follows. First, using an F test we examine the incremental contribution of the interaction term to the models explored above. Because the interaction term is based in part on the parliamentary debate and election variables, they will probably exhibit multicollinearity. In fact, analyses of the variance inflation factor indicate rather strong collinearity between the election cycle dummy and the interaction term between election years and parliamentary debate. While multicollinearity causes each individual term to be estimated less precisely, it does not affect the statistical significance of the group as a whole. Hence, despite collinearity we can assess the statistical significance of the interaction term using the restricted F test (cf Gujarati 2003, p. 271). Second, having identified those issues where the interaction term contributes significantly to the statistical models, we continue with a closer look at these time series.

Table 5.6 shows the result of a restricted F test for each of the four popular issues analyzed in this chapter. The null hypothesis is that adding the

Table 5.6. Assessing the incremental contribution of interacting election year and extent of debate in parliament

| | Model II[b] | | Model IV[c] | |
|---|---|---|---|---|
| Policy category | F ratio[a] | Numerator df / Denumerator df | F ratio[a] | Numerator df / Denumerator df |
| Law & order | .81 | 1/13 | .93 | 1/16 |
| Pollution control | 3.21* | 1/14 | 4.95** | 1/18 |
| Primary education | 4.16* | 1/13 | 6.11** | 1/17 |
| Health | .01 | 1/13 | .[d] | . |

Notes: *$\alpha \leq 0.10$; **$\alpha \leq 0.05$; ***$\alpha \leq 0.01$. a: Displays the incremental contribution to $R^2$, when adding the interaction term "Debate in Parliament$_{t-5}$ * Election Year$_t$" to the models developed earlier in this chapter. b: Refers to columns "Model II" in the respective tables presented in the first part of this chapter. c: Refers to columns "Model IV" in the respective tables presented in the first part of this chapter. d: Cf Table 5.4 above, this one could not be reduced to a statistically significant model.

interaction term to the models scrutinized above does not contribute significantly to the explanatory power of these models, measured by the $R^2$. Models II and IV refer to the corresponding models II and IV in the tables presented above.

According to Table 5.6, the interaction term between percentage of parliamentary debate and election year is statistically significant on two of the four issues. The two issues are pollution control and primary education. For law and order and health issues the effect is not statistically significant. In the following sections, we explore the two significant interaction terms further, including an assessment of the direction of their effect.

*Pollution Control*

Table 5.7 shows the coefficients for spending on pollution control. Models IIa and IVa are identical to Models II and IV in Table 5.2. That means they represent the "full" model and what seems to be the model with the best fit, respectively. In Models IIb and IVb the interaction term is added in order to evaluate its contribution in more detail.

Table 5.7 clearly shows that the interaction term exerts a positive effect on public spending on pollution control. Furthermore, according to the summary statistics, it is a quite strong effect resulting in a .08 increase in adjusted $R^2$. When adding the interaction term the effect of changes in inflation levels is lowered somewhat, but generally the other coefficients in the models do not change notably from this addition. The only exception is the election year dummy, but the coefficient of this variable cannot be adequately assessed in isolation from the interaction term. With respect to the parliamentary debate coefficient, it actually seems that it exerts both a direct effect and a combined effect with the election year dummy. In other words, there is a positive and statistically significant relationship between extent of parliamentary debate and public spending in all years, but this effect is particularly strong in election years. The size of these effects can more readily be appreciated in Figure 5.1 below.

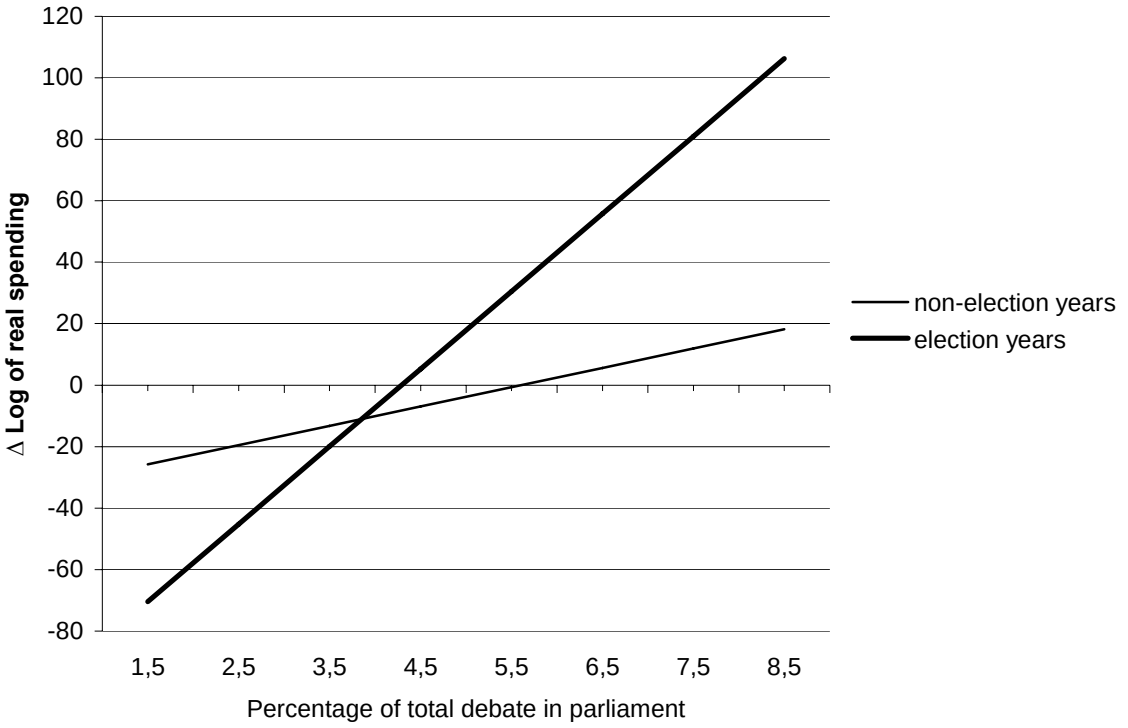
Figure 5.1 clearly shows the additional spending effect of the interaction term. From 1980 to 2003, the percentage of total debate in parliament devoted to environmental issues varied between 1.5 and 8.5. Within this range, the interaction term exerts a clear effect on public spending on pollution control. The relationship between parliamentary debate and public spending is positive in non-election years, but the effect is much stronger in election years.

Table 5.7. Spending on pollution control, 1980-2003

| Dependent variable = Δ Log of real spending | | | | |
|---|---|---|---|---|
| | Model IIa | Model IIb | Model IVa | Model IVb |
| Constant | –59.077* | –38.523 | 77.408*** | –50.285* |
| | (29.155) | (29.531) | (23.726) | (24.788) |
| Debate in parliament$_{t-.5}$ | 10.571** | 6.280 | 11.241*** | 7.018* |
| | (4.293) | (4.668) | (3.797) | (3.941) |
| Election year$_t$ | 47.172** | –73.060 | 40.051** | –82.425 |
| | (20.101) | (69.792) | (16.099) | (56.944) |
| Debate in parliament$_{t-1}$* | | 18.958* | | 19.922** |
| Election year$_t$ | | (10.575) | | (8.951) |
| Δ Pollution index$_{t-1}$ | 2.337 | 5.334 | | |
| | (23.093) | (21.621) | | |
| Δ Log of real spending$_{t-1}$ | –.049 | .056 | | |
| | (.161) | (.161) | | |
| Left government$_{t-1}$ | –28.179 | –16.019 | | |
| | (20.189) | (20.029) | | |
| Δ Inflation$_{t-1}$ | –12.560 | –11.848 | -21.480*** | –14.807** |
| | (10.907) | (10.190) | (6.761) | (6.843) |
| ΔΔ Unemployment$_{t-1}$ | –5.056 | –4.248 | | |
| | (11.780) | (11.006) | | |
| N (observations) | 23 | 23 | 23 | 23 |
| $R^2$ | .65 | .71 | .59 | .68 |
| $R^2$ (adjusted) | .48 | .55 | .53 | .61 |
| F-statistic for model | 3.92** | 4.34*** | 9.22*** | 9.59*** |
| Durbin-Watson statistic | – | – | 1.95 | 2.23 |
| Durbin's h (p-value) | .673 | .856 | .935 | .583 |
| Breusch-Godfrey (p-value) | .591 | .813 | .927 | .527 |

Notes: Unstandardized betas with standard errors in parentheses. *$\alpha \leq 0.10$; ** $\alpha \leq 0.05$; ***$\alpha \leq 0.01$.

Figure 5.1. Illustration of interaction effect, pollution control




Note: The figure is based on Model IIb in Table 5.7 above. All other variables are represented with their mean value. The government dummy is set to 0.

The fact that the two lines in Figure 5.1 intersect might suggest that public spending on pollution control would be even lower in election years than in non-election years when the share of parliamentary debate is very small. Substantively, this could imply that the competition for public spending among subsystems is more intense in election years than in non-election years. From the viewpoint of macropolitical actors, if the public visibility of a given issue is low, money in election years is better spent – in terms of electoral gains – in other and more visible policy domains. However, upon closer inspection it seems that the steeper positive slope in election years is mainly driven by a higher share of debate in parliament, corresponding to high increases in spending on pollution control and not by very little debate, corresponding to very small spending changes. Consequently, the spending effects depicted in the lower left corner of Figure 5.1, where the share of parliamentary debate is low, appears to be merely a consequence of the restrictions that we impose on our model rather than a substantial result.

Primary education

Table 5.8 shows the coefficients for spending on primary education. In earlier analyses in this chapter, we found a positive, but statistically insignificant effect from the parliamentary debate variable and from the election year

dummy. When combined, however, the effect of these two variables appears to be quite strong. According to the summary statistics in the lower part of Table 5.8, adjusted $R^2$ increases in Models IVa and IVb from .43 to .56, when the interaction term is added. As expected, the coefficient is positive.

Table 5.8. Spending on primary education, 1980-2003

| Dependent variable = Δ Log of real spending | | | | |
|---|---|---|---|---|
| | Model IIa | Model IIb | Model IVa | Model IVb |
| Constant | –4.074* | –.235 | –3.604* | –1.062 |
| | (2.178) | (2.722) | (1.739) | (1.847) |
| Debate in parliament$_{t-.5}$ | 1.097 | –1.815 | 1.167 | –1.347 |
| | (1.926) | (2.250) | (1.407) | (1.605) |
| Election year$_t$ | 1.407 | -5.598 | 1.284 | -4.568* |
| | (1.277) | (3.621) | (1.048) | (2.542) |
| Debate in parliament$_{t-.5}$* Election year$_t$ | | 7.327* | | 6.313** |
| | | (3.590) | | (2.554) |
| Δ Number of immigrants aged 6-15$_t$ | 1.360** | 1.170** | 1.360*** | 1.300*** |
| | (.558) | (.512) | (.406) | (.359) |
| ΔΔ Number of children aged 6-15 | 1.790 | 2.26 | 2.120* | 1.860* |
| | (1.510) | (1.38) | (1.190) | (1.060) |
| Δ Log of real spending$_{t-1}$ | .058 | .119 | | |
| | (.243) | (.221) | | |
| Left government$_{t-1}$ | .728 | –.616 | | |
| | (1.481) | 1.491) | | |
| Δ Inflation$_{t-1}$ | –.511 | .0271 | | |
| | (.718) | (.769) | | |
| ΔΔ Unemployment$_{t-1}$ | .118 | .173 | | |
| | (.837) | (.769) | | |
| N (observations) | 23 | 23 | 23 | 23 |
| $R^2$ | .56 | .67 | .53 | .66 |
| $R^2$ (adjusted) | .31 | .44 | .43 | .56 |
| F-statistic for model | 2.24* | 2.90** | 5.13*** | 6.49*** |
| Durbin-Watson statistic | – | – | 1.54 | 1.68 |
| Durbin's h (p-value) | .850 | .780 | .572 | .660 |
| Breusch-Godfrey (p-value) | .802 | .700 | .515 | .600 |

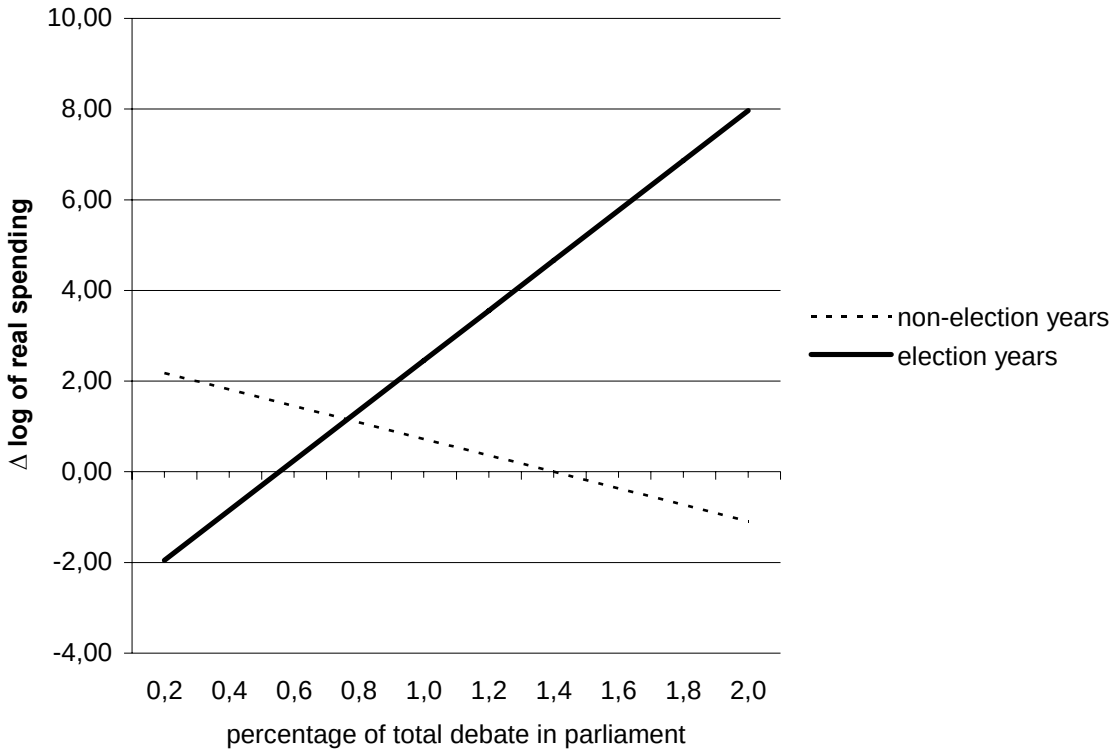
Notes: Unstandardized betas with standard errors in parentheses. *$\alpha \leq 0.10$; ** $\alpha \leq 0.05$; ***$\alpha \leq 0.01$.

When we add the interaction term, the individual coefficient of the parliamentary debate variable turns negative, but it is still highly statistically insignificant. The individual coefficient of the election year dummy also turns negative and a t-test shows the effect to be statistically significant. However,

this variable exhibits strong collinearity with the interaction term, and consequently, the effect of the election dummy cannot be estimated in isolation from the effect of the interaction term. The direction of some of the coefficients shown in the lower part of Table 5.8 also changes when the interaction term is added, but since they are all strongly statistically insignificant, the fragile character of these estimates do not give cause for serious concern in this case.

Figure 5.2 provides a graphical representation of the combined spending effect of extent of parliamentary debate and election year. Compared to the pollution control category, when measured in changes in log values, variation in spending on primary education is smaller and so is the range of debate in parliament devoted to this issue. Within this range of parliamentary debate, however, we find the interaction term to exert what seems to be a quite strong and positive effect on public spending on primary education. In non-election years, the effect of parliamentary debate is negative, but the slope is statistically indistinguishable from 0. In election years, on the other hand, the effect is positive and strongly statistically significant.

Figure 5.2. Illustration of interaction effect, primary spending




Note: The figure is based on Model IIb in Table 5.8 above. All other variables are represented with their mean value. The government dummy is set to 0.

Similar to the discussion above, the fact that the two lines in Figure 5.2 intersect might suggest that public spending on primary education would be even lower in election years than in non-election years when the share of parliamentary debate on primary education is very small. In this case, a closer inspection of data seems to suggest that this finding is primarily a consequence of some rather high increases in spending on primary education in non-election years, where the share of parliamentary debate is low. Hence, as expected there is a positive relationship between extent of parliamentary debate and public spending on primary education in election years. In non-election years, however, the relationship between parliamentary debate and public spending on primary education clearly seems to be driven by a different logic.

## Summary and conclusion

In the first part of this chapter, the empirical validity of hypotheses H1.a and H2.1a presented in Chapter 4 were examined. First, because a majority of the electorate prefers increased public spending on the four issues analyzed in this chapter, we expected a positive relationship between percentage of annual length of parliamentary debate and public spending. Supportive empirical evidence on this hypothesis was found on the law and order issue and on the issue of pollution control. For primary education and health, the relationship between parliamentary debate and public spending was positive, but not statistically significant.

Second, owing to the assumption that the four issues examined in this chapter are cases of relatively broad generality and macropolitical saliency, we expected public spending to be higher in election years than in non-election years. Again, variation in public spending on law and order related issues and on pollution control clearly supported this expectation. The analysis of spending on primary education suggested a positive, but statistically insignificant effect from being in an election year compared to a non-election year. The difference between spending on health related issues in election years compared to non-election years is barely significant, but to the extent that there is an effect, public spending actually seems to be higher in non-election years than in election years.

The analyses conducted in the last part of this chapter provided some empirical support to hypothesis H2.2a, which predicted that the positive relationship on popular issues between share of parliamentary debate and public spending is stronger in election years than in non-election years. It should be noted that estimating two regression lines on a sample of twenty-three observations necessarily reduces the robustness of the results. With this caveat in mind, the analyses in this chapter nevertheless showed that in addition to the positive relationship between share of parliamentary debate

and spending on pollution control in non-election years, there is an even stronger positive effect in election years. With respect to spending on primary education, the relationship in non-election years is negative but statistically insignificant, whereas the spending effect of parliamentary debate in election years is positive and statistically significant. In other words, in this policy domain election years seem to be a necessary condition for parliamentary debate to affect public spending. With respect to the law and order and health issues, the interaction term was statistically insignificant, suggesting no difference in the effect of parliamentary debate across election and non-election years.

All in all, this chapter has demonstrated that there is a positive relationship between macropolitical attention and spending on at least two of the four issues, and a positive effect on primary education spending in election years. Hence, it seems that the more macropolitical actors talk about a given issue, the more they spend. I have argued that the theoretical explanation is to be found in the fact that a consistent majority of the electorate prefers more spending on these issues. However, it might also be because a bigger share of parliamentary attention always tends to result in increased public spending. Analyzing the spending effects of parliamentary debate on three issues, where a majority of the public prefers decreased spending, Chapter 6 provides an answer to this question.

## Notes

1. In addition, I have controlled for annual growth in GNP, but since this variable did not contribute significantly to any of the statistical analyses, it was excluded in the subsequent analyses to reduce the number of restrictions on the sample.
2. See Appendix 1 for a further description of the construction of this variable.
3. The measure is made available by Christoffer Green-Pedersen & John Wilkerson.

# Chapter 6

# Parliamentary debate and spending on unpopular issues

Common to the three issues studied in this chapter is the fact that a majority of the public prefers decreased public spending. Therefore, given the model presented in Chapters 3 and 4, we examine the empirical validity of the hypothesis that there is a negative relationship between annual length of parliamentary debate and public spending on the three issues of defense, aid to developing countries, and cultural affairs. We then go on to examine whether this relationship is stronger in election years than in non-election years.

The statistical analyses and the presentation of empirical results follow the scheme from Chapter 5. First, we focus on the main spending effects of variation in the share of parliamentary debate and of election cycles, estimating the statistical models for each of the three issues without the interaction term between the parliamentary debate variable and the election year dummy. We then re-estimate these models including the interaction term in order to evaluate its contribution to the statistical models introduced in the first part of the chapter. The set of standard controls is similar to the one explored in Chapter 5, but also in this chapter it is accompanied by considerations of more issue specific policy determinants.

## Defense

The national defense apparatus is one of the biggest employers in Denmark (cf Heurlin 2004). As shown in Figure 4.1 in Chapter 4, each year from 1980 to 2003 the Danish government spent around DKK 20-25 billion (2003 prices) on national defense. As in many other countries, defense is a central government responsibility, and hence there is no local government spending involved in this policy domain.[1] Historically, there has been substantial and sometimes overt disagreement in the Danish parliament on the question of more or less defense. According to observers of Danish defense politics, the issue is not constantly high on the political agenda, but on the other hand, it is an issue with a substantial degree of politicization potential (Petersen 1981; Heurlin 2004). As speculated by Heurlin (2004, p. 41), and in line with the spending attitudes shown in Table 4.1 in Chapter 4, there are probably not many votes in Danish defense politics, but when there are votes to be gained, these are won by proposals of spending cutbacks. In this section, we test whether defense spending is in fact adjusted towards the median voter

position in times of increased macropolitical attention, measured as the percentage of total debate in parliament devoted to the defense issue.

Compared to the development in national spending on defense in the US (cf Hartley & Russett 1992; True 2002), the development in Danish defense spending has been much more stable and incremental (cf Figure 4.1). A major reason for this is probably the lack of Danish involvement in military conflicts in the post war era. For instance True (2002) in his time series study of US defense spending, utilizes a long list of war dummies, which seem to account for many of the observed spending changes. In Denmark, it is less obvious what kind of special conditions to control for in a study of Danish national defense policy. However, two defense related developments seem rather obvious to control for. First, the end of the cold war represents a major change in the international security context of Danish defense politics. Second, from some perspectives the Danish participation in the US-led international coalition in Iraq constitutes a radical break with previous Danish defense politics. The effect of the cold war is measured using a dummy variable with the value of one from 1980 to 1989 and else zero. The effect of the Iraq involvement is measured as a dummy that equals one in fiscal year 2003, else zero.[2]

Following Heurlin (2004, p. 31), I distinguish in this study between security politics and defense politics. In this view, the former concept refers to diplomatic aspects and questions about participation in international security organizations, including questions about NATO. Defense politics, on the other hand, is more directly related to questions about the size, organization, and use of the Danish defense system. In this study, I analyze only on defense policy and hence exclude from the dataset on parliamentary debates those categories that refer to security politics (see Appendix 1).

Table 6.1 shows the results of the analysis of public spending on Danish national defense from 1980 to 2003. As expected, given the median voter's preferred direction of spending changes on this issue, the correlation in Model I between share of parliamentary debate and public spending increases is negative and statistically significant. According to Model I, when the percentage of debate in parliament devoted to defense related questions increases by one, spending on national defense is on average reduced by slightly more than half a percent.

Turning to the multivariate analyses in Models II and III in Table 6.1, the empirical pattern becomes much more complex to interpret. First, however, we look at the more readily understandable results in Models II and III. According to Model II, spending increases are often followed by spending

Table 6.1. Spending on defense, 1980-2003

| Dependent variable = Δ Log of real spending | | | | |
|---|---|---|---|---|
| | Model I | Model II | Model III | Model IV |
| Constant | .735 | 1.712 | –.414 | .654 |
| | (.711) | (1.134) | (1.080) | (.681) |
| Debate in parliament$_{t-.5}$ | –.686* | –2.376*** | | -1.393*** |
| | (.350) | (.803) | | (.405) |
| Cold war dummy$_{t-1}$ | | .892 | –1.381 | |
| | | (1.197) | (1.131) | |
| Iraq dummy | | 10.883** | -1.517 | 5.869** |
| | | (4.655) | (2.495) | (2.674) |
| Δ Log of real spending$_{t-1}$ | | –.246** | –.206 | –.273** |
| | | (.113) | (.139) | (.101) |
| Election year$_t$ | | 2.791*** | 3.246** | 2.892*** |
| | | (.919) | (1.115) | (.767) |
| Left government$_{t-1}$ | | –.778 | –.268 | |
| | | (1.027) | (1.247) | |
| Δ Inflation$_{t-1}$ | | -.586 | .435 | |
| | | (.539) | (.510) | |
| ΔΔ Unemployment$_{t-1}$ | | .189 | –.418 | |
| | | (.579) | (.666) | |
| N (observations) | 23 | 23 | 23 | 23 |
| $R^2$ | .16 | .66 | .44 | .60 |
| $R^2$ (adjusted) | .11 | .46 | .19 | .51 |
| F-test statistic for model | 3.85* | 3.37* | 1.72 | 6.64*** |
| Durbin-Watson statistic | 2.63 | – | – | – |
| Durbin's h (p-value) | .116 | .685 | .139 | .322 |
| Breusch-Godfrey (p-value) | .111 | .592 | .078 | .263 |

Notes: Unstandardized betas with standard errors in parentheses. *$\alpha \leq 0.10$; ** $\alpha \leq 0.05$; ***$\alpha \leq 0.01$.

decreases, just as decreases tend to be followed by increases. This negative feedback process, evident from the negative coefficient of the lagged spending measure, is compatible with classic incremental views of public spending (Danziger 1978, p. 157; Boyne et al. 2000, p. 59). Furthermore, Model II shows a negative effect of the government dummy, suggesting lower defense spending in times of left-wing governments. Based on the historical positions of the two party blocs in the Danish parliament on this issue, the direction of this effect seems reasonable, but please note that it does not reach the level of statistical significance. In fact, according to Table 6.1, changes in inflation and unemployment levels do not exert a systematic influence on defense spending.

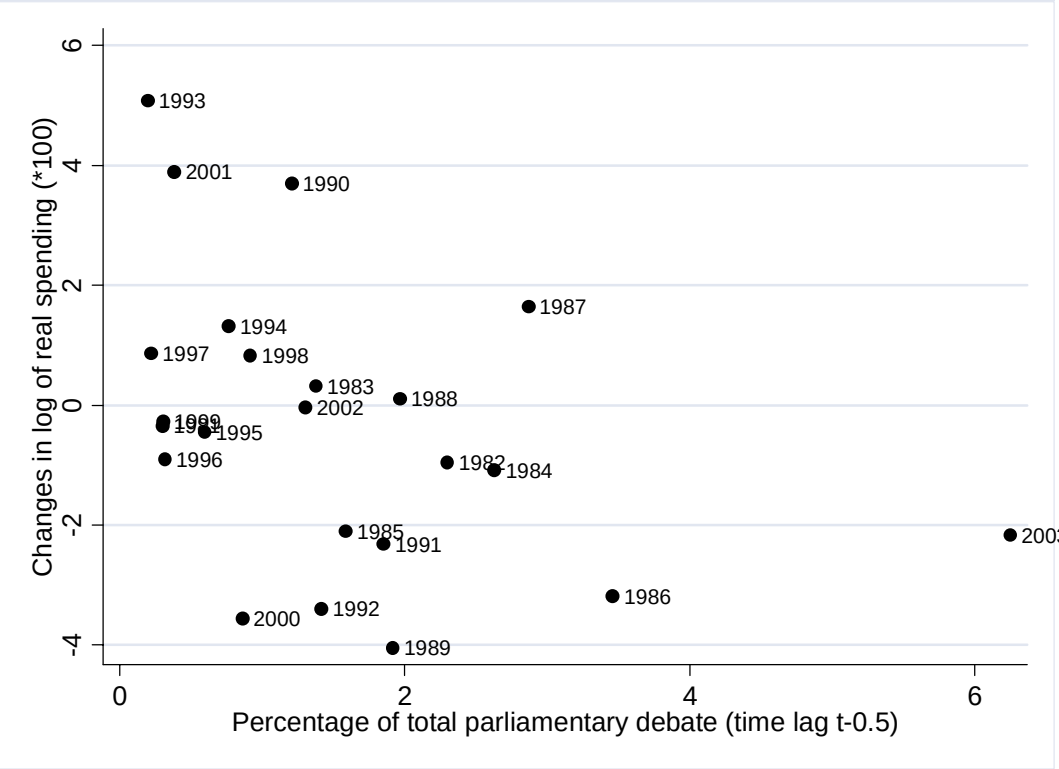
The apparently puzzling results in Models II and III concern our variables of main theoretical interest. First, in Model II the negative parliamentary

debate coefficient increases markedly to a value of around –2.5, which clearly suggests collinearity with some of the added variables. Second, in Model II in Table 6.1, the coefficients of the two measures of the international security context are both positive, which fits with our intuitive expectations. In fact, according to Model II the effect of the Iraq dummy is very strong. However, when removing the parliamentary debate variable from the statistical model, both of the international security coefficients turn negative and statistically insignificant. Third, contrary to our theoretical expectations, the effect of being in an election year is positive and even highly statistically significant and stable across the models shown in Table 6.1. I return to the question about the election year dummy below, focusing first on the relationship between the parliamentary debate variable, the cold war dummy, and the Iraq dummy.

Figure 6.1 shows a plot of spending changes as a function of percentage of parliamentary debate. The markers are labeled with the variable measuring fiscal years. This simple scatterplot appears to provide some order and reason to the results shown in Models II and III in Table 6.1. First, Figure 6.1 clearly shows the negative relationship found in Model I in Table 6.1 between share of parliamentary debate and direction of annual spending changes on national defense. Second, however, it also shows a pattern of more defense related debate in parliament in the 1980s than in the 1990s. Third, it shows an exceptionally large share of the debate in parliament devoted to defense related questions in 2003.

Together these three observations evident from Figure 6.1 point to an interesting explanation of the pattern found in Models II and III in Table 6.1. The 1980s coincide with the cold war years, giving rise to a rather strong positive correlation between the cold war dummy and the extent of debate about defense in the Danish parliament. In times of increased international tension, defense related issues are debated more. However, while the cold war dummy explains part of the variation in the percentage of parliamentary debate on defense issues, it does not explain the policy outcome of this increased debate. As we see in Figure 6.1, the higher level of parliamentary debate in the 1980s was associated with lower spending on defense, which clearly suggests that extent of debate in parliament is not just a simple mediating variable between international tension and public spending on national defense. If the latter were the case, we should find a positive relationship between parliamentary debate and spending on national defense. Instead the observed pattern supports the expectation that extent of parliamentary debate in itself, when other relevant policy determinants are

Figure 6.1. Scatterplot, parliamentary debate and public spending on defense




controlled for, exerts a negative effect on public spending on national defense. Including the Iraq dummy variable merely supports this conclusion. The extensive debate in the parliamentary session 2002/03 was generated by the question of Danish participation in the US-led Iraq coalition. In fact, the debate over this parliamentary resolution alone covers more than 209 columns in the parliamentary records for that session, that is, a little more than six percent of the total number of columns covering parliamentary debate for that session. When controlling for the effect of length of debate in parliament, the spending effect of the Iraq dummy is positive, as one would intuitively expect. However, the Iraq debate also reveals that sometimes much parliamentary debate does not result in decreased defense spending. Nevertheless, if we do not control for the special circumstances in 2003, we still find a negative and statistically significant correlation between parliamentary debate and Danish national defense spending in the period from 1980 to 2003.

Model IV in Table 6.1 shows what seems to be the best model fit given the imposed restrictions and available explanatory factors. The coefficient for the parliamentary debate variable suggests that on average, a one percent increase in the share of parliamentary debate devoted to defense related issues results in a 1.3 percent decrease in public spending changes on national defense. Hence, based on Table 6.1 and the additional analysis, it clearly seems that extent of parliamentary debate has a negative effect on changes in spending on national defense.

We now turn to the spending effect of the election year dummy. The effect is strong and positive across the models shown in Table 6.1, indicating higher defense spending in election years than in non-election years. Additional inspections of this relationship clearly suggest that this is not just the result of a few deviant and influential observations. One explanation of this unexpected finding could be found in the potential difference between general and specific opinions discussed in Chapter 4. Preferring decreased spending in general is one thing and does not necessarily imply that one approves budgetary cuts with visible and specific consequences. However, this supplementary explanation is significantly weakened by the fact that defense spending increases even more in election years than in non-election years and by the above finding about the effect of increased parliamentary debate. Whatever the explanation behind this election year effect, we note that this finding clearly works against our theoretical expectations.

Additional transformations of the parliamentary debate variable do not change the pattern observed in this section. The autocorrelation diagnostics in the lower part of Table 6.1 do not indicate serious autocorrelation problems in the final statistical model and in general, the regression diagnostics seem acceptable. Actually, however, according to the initial non-stationarity tests, the time series measuring share of defense debate in parliament might be on the edge of stationarity. However, using a clearly stationary measure of *changes* in the annual share of debate in parliament does not alter the conclusions drawn in this section.

### Aid to developing countries

Measured as share of GNP, Denmark for many years has been one of the most generous contributors of aid to developing countries. In recent years, the share of GNP dedicated to these activities has declined, but Denmark is still among the countries that spends the largest share of their GNP on aid to developing countries. However, judged by the spending attitudes of the Danish population reported in Table 4.1, this generosity does not have solid support from a majority of the electorate. We only have measures from 1990 and onwards, but in this period a majority of voters always prefers decreased instead of increased public spending on this issue. Based on this pattern, we infer that this is true for the whole period from 1980 to 2003. Hence, a gap between voter preferences and public spending is evident with respect to aid to developing countries. What we examine in this section is whether this gap narrows in years of increasing share of parliamentary debate, and whether it widens in years of decreasing share of parliamentary debate. In other words, given the spending attitudes of the median voter, we expect a negative relationship between extent of parliamentary debate and public spending on aid to

developing countries. It should be noted that public spending on aid to developing countries does not involve local or regional authorities in Denmark.

Changing conditions in developing countries such as catastrophes and humanitarian disasters may act as "real-world" factors that account for variation over time in spending on this issue. However, it is almost impossible to control systematically for such factors. In a global context, almost every year has its humanitarian catastrophe that could justify additional aid and resources. Furthermore, none of these can reasonably be expected to exert some kind of automatic influence on the aggregate level of Danish spending on aid to developing countries. Consequently, the analysis in this section does not include such issue-specific control variables. This ignorance might inflict an omitted variable bias in our statistical estimates. However, the direction of this potential bias probably works against our theoretical hypothesis for the following reason. Intuitively, to the extent that a concrete and visible humanitarian catastrophe would affect our variables of theoretical interest, it would probably raise the public demand for spending in that particular geographical area, it would likely also lead to an increase in parliamentary debate, and probably also increase spending on aid to developing countries, at least to the disaster area. If this is so, the relationship between debate and public spending would be positive, which means that being able to control for this effect would lead us to expect to find an even stronger negative spending effect of length of parliamentary debate.

Table 6.2 shows the result of the analysis of spending on aid to developing countries. According to Model I in Table 6.2, the correlation between extent of debate in parliament and spending on aid to developing countries is negative and statistically significant. If the share of debate increases by one, then annual changes in spending on aid to developing countries are cut by almost seven percent.

In Model II, the effect is still negative and about equal to the coefficient in Model I, but it does not quite reach the level of statistical significance. The effect of election years is also negative, but statistically insignificant. The positive sign of the left government coefficient is as one might expect, just as the negative coefficient of last year's spending changes corresponds to common incremental theories. A negative effect of unemployment and inflation changes could suggest some kind of tradeoff between economic problems in Denmark and Danish public spending on problems in developing countries. However, neither of these coefficients in Model II reaches the .10 level of statistical significance.

Table 6.2. Spending on aid to developing countries, 1980-2003

| Dependent variable = Δ Log of real spending | | | | |
|---|---|---|---|---|
| | Model I | Model II | Model III | Model IV |
| Constant | 12.209*** | 11.068* | 4.616 | 13.645*** |
| | (4.281) | (5.680) | (3.874) | (4.151) |
| Debate in parliament$_{t-.5}$ | –6.891** | –5.563 | | –5.713* |
| | (3.281) | (3.688) | | (3.189) |
| Δ Log of real spending$_{t-1}$ | | –.128 | –.060 | |
| | | (.228) | (.232) | |
| Election year$_t$ | | –6.829 | –9.894* | –7.936* |
| | | (5.737) | (5.562) | (4.432) |
| Left government$_{t-1}$ | | 4.044 | 5.587 | |
| | | (5.178) | (5.263) | |
| Δ Inflation$_{t-1}$ | | –2.449 | –3.227 | |
| | | (2.579) | (2.620) | |
| ΔΔ Unemployment$_{t-1}$ | | –1.267 | .720 | |
| | | (3.846) | (3.746) | |
| N (observations) | 23 | 23 | 23 | 23 |
| $R^2$ | .17 | .35 | .25 | .29 |
| $R^2$ (adjusted) | .13 | .10 | .04 | .22 |
| F-test statistic for model | 4.41** | 1.42 | 1.16 | 4.04** |
| Durbin-Watson statistic | 2.34 | | | 2.29 |
| Durbin's h (p-value) | .293 | .716 | .509 | .347 |
| Breusch-Godfrey (p-value) | .272 | .653 | .435 | .312 |

Notes: Unstandardized betas with standard errors in parentheses. *$\alpha \leq 0.10$; ** $\alpha \leq 0.05$; ***$\alpha \leq 0.01$.

Removing the parliamentary debate variable does not change the effect of the other variables substantially. The negative election year effect turns statistically significant, but all other coefficients are largely unaffected. The change in the election year coefficient suggests some collinearity between extent of parliamentary debate about foreign aid and election years. An additional test for collinearity indeed shows a positive relationship between the two variables, but the effect is rather weak and clearly statistically insignificant.

Model IV in Table 6.2 shows what seems to be the best model fit based on the included policy determinants. According to this model, there is a negative and statistically significant correlation between extent of parliamentary debate and public spending on aid to developing countries. On average, when the percentage of parliamentary debate increases by one, spending changes are reduced by a little more than five percent. Furthermore, on average, spending changes in election years are almost eight percent lower than in non-election years. Expressed in standardized beta coefficients, the effect of the two variables in Model IV is almost identical in that each has a beta

coefficient of around –.35. Transforming the debate variable according to the procedures followed in the analyses of the other issues does not result in a better statistical model.

The introduction to this analysis of Danish spending on aid to developing countries was a reflection on the gap between voter spending preferences and public spending. The above study has provided an empirical answer to the question about when this gap narrows and widens, respectively. Two central conditions for responsiveness to voter preferences, it seems, are extent of parliamentary debate in parliament and proximity of the next election. Exactly where public spending changes cross the value of zero and turn negative is impossible to say based on these models, because it depends on the values we assign to the other variables in our "all else being equal" consideration. What we can say is that share of parliamentary debate and proximity of next election by themselves exert a negative effect on changes in public spending on aid to developing countries.

### Cultural purposes

In this study, cultural purposes are defined as activities related to cultural affairs such as theatres, museums, libraries, support of artists, etc. That means that parliamentary debate about and public spending on recreational activities, sports facilities, etc. are excluded from the analysis. Hence, the interpretation of the spending attitudes shown in Table 4.1 in Chapter 4 is that it is in relation to the types of cultural affairs mentioned earlier that a majority of the Danish voters in the period from 1979 to 2003 prefers cuts in public spending.

In Denmark, public financing of cultural affairs is shared between the central government and the municipalities. The balance between local and central government spending has changed over time, but on average, according to the spending data from Statistics Denmark, about 40 % of public spending is specified in the central government budget. Similar to spending on aid to developing countries, but in contrast to the majority of the citizens' negative view of this spending issue, public spending on cultural affairs has increased over the period from 1980 to 2003. In particular the 1990s saw some large spending increases (see Figure 4.1). Hence, a gap between voter attitudes and public spending also seems to exist with respect to cultural affairs. In this section, I analyze whether this gap narrows in times of increased parliamentary debate and whether it widens in times of decreased parliamentary debate. I also test whether public spending on cultural affairs decreases more in election years than in non-election years.

While increased demand measured by demographic variables for education or health related services is expected to involve increased public spending on

these issues, the mechanisms are probably different for cultural purposes. A multiplicity of different schemes and financial subsidies exists in this policy domain (cf Bakke 1988, chapter 7), but a common characteristic of many of these arrangements seems to be that they are intended to compensate cultural institutions in times of declining private financing (cf Hjort-Andersen 1991). In the statistical models below, I control for annual changes in number of attendants at theatrical performances and for annual changes in the number of cinema tickets sold per inhabitant. Danish cinemas are mainly private enterprises, although this sub-sector also receives some public subsidies. In the present context, I therefore tend to interpret this variable as a general proxy for year to year changes in the Danish population's demand for cultural services. Annual changes in the number of attendants at theatrical performances should cover aspects of the same dimension. In fact, this variable may be more directly linked to public spending because public theatres constitute quite a large part of public spending on cultural affairs, not least on central government spending. In addition, I have controlled for annual changes in book lending by Danish public libraries, which today is mainly a local government responsibility. However, the results of this test are not shown in Table 6.3 because the variable clearly did not correlate with variation in aggregate spending on cultural affairs.

Table 6.3 shows the main results of the analysis of public spending on cultural affairs. One can rather quickly ascertain that none of the statistical models shown in Table 6.3 works very well. In Model I the effect of parliamentary debate is positive, but highly statistically insignificant. In Model II the debate coefficient turns negative, but it is virtually equal to zero. The only statistically significant coefficient in Models II and III is the negative effect of last year's spending changes. However, this effect turns statistically insignificant if we try to reduce the model to one which is statistically significant according to the F test statistic shown in the lower part of Table 6.3. That is also why Table 6.3 does not include a Model IV. The sign of the election dummy coefficient is negative as expected, but statistically insignificant. The coefficient of one of the two "demand" variables changes quite markedly across Models II and III, which suggests some collinearity with the debate variable. However, the debate variable certainly does not suppress any major effects since both of these demand related coefficients are highly statistically insignificant in both models. The signs of the other coefficients in Models II and III seem reasonable, but none of them reaches the level of statistical significance.

Table 6.3. Spending on cultural purposes, 1980-2003

| Dependent variable = Δ Log of real spending | | | |
|---|---|---|---|
| | Model I | Model II | Model III |
| Constant | 2.102 | 2.256 | 2.220 |
| | (2.124) | (2.816) | (1.766) |
| Debate in parliament$_{t-.5}$ | .660 | -.036 | |
| | (1.155) | (1.389) | |
| Δ Cinema tickets sold per inhabitants$_{t-1}$ | | -.998 | -.919 |
| | | (7.044) | (6.125) |
| Δ Number of attendances to theatrical performances$_{t-1}$ | | -.260 | .094 |
| | | (27.708) | (23.295) |
| Δ Log of real spending$_{t-1}$ | | -.482* | -.482* |
| | | (.264) | (.254) |
| Election year$_t$ | | -1.559 | -1.557 |
| | | (2.139) | (2.065) |
| Left government$_{t-1}$ | | 3.572 | 3.573 |
| | | (2.138) | (2.065) |
| Δ Inflation$_{t-1}$ | | -.170 | -.161 |
| | | (1.234) | (1.143) |
| ΔΔ Unemployment$_{t-1}$ | | -1.040 | -1.047 |
| | | (1.477) | (1.404) |
| N (observations) | 23 | 23 | 23 |
| $R^2$ | .00 | .36 | .36 |
| $R^2$ (adjusted) | –.05 | -.01 | .06 |
| F-test statistic for model | .00 | .97 | 1.19 |
| Durbin-Watson statistic | 2.64 | - | - |
| Durbin's h (p-value) | .109 | .403 | .374 |
| Breusch-Godfrey (p-value) | .105 | .279 | .268 |

Notes: Unstandardized betas with standard errors in parentheses. *$\alpha \leq 0.10$; ** $\alpha \leq 0.05$; ***$\alpha \leq 0.01$.

On the whole, the explanatory power of the statistical models shown in Table 6.3 is extremely low. Exploring alternative transformations of the debate variable does not affect this result. As was the case with the models of health related spending in Chapter 5, the disturbingly poor fit of the models in Table 6.3 invites some further exploration of this policy domain. As suggested in Chapter 5, the lack of a significant statistical model could be a consequence of noise from underlying variation across functional subcategories or from variation at the local level of policymaking. Contrary to the study of Danish health spending, it is not possible to divide the cultural affairs spending category into sub-functions. However, since a relatively large part of public spending on cultural affairs is specified in the national budget, it is feasible to

separate out central government spending on cultural affairs. Consequently, to examine the robustness of the results I have re-estimated the analyses in Table 6.3 on central government spending only. The results of this exercise are shown in Table 6.4.

Table 6.4. Central spending on cultural purposes, 1980-2003

| Dependent variable = Δ Log of real spending | | | | |
|---|---|---|---|---|
| | Model I | Model II | Model III | Model IV |
| Constant | 1.325 | 5.021 | 2.911 | 5.094 |
| | (6.049) | (5.167) | (3.077) | (4.816) |
| Debate in parliament$_{t-.5}$ | –.162 | –1.317 | | –1.439 |
| | (3.288) | (2.555) | | (2.324) |
| Δ Cinema tickets sold per inhabitants$_{t-1}$ | | –.231* | –203* | –.251** |
| | | (.121) | (.106) | (.101) |
| Δ Number of attendances to theatrical performances$_{t-1}$ | | –.953* | –.818* | –.945* |
| | | (.533) | (.453) | (.491) |
| Δ Log of real spending$_{t-1}$ | | –.695*** | –.688*** | –.680*** |
| | | (.159) | (.155) | (.145) |
| Election year$_t$ | | –12.909*** | –12.812*** | –13.549*** |
| | | (3.953) | (3.851) | (3.533) |
| Left government$_{t-1}$ | | 7.880* | 7.932* | 8.155** |
| | | (3.937) | (3.839) | (3.241) |
| Δ Inflation$_{t-1}$ | | .380 | .663 | |
| | | (2.064) | (1.940) | |
| ΔΔ Unemployment$_{t-1}$ | | –1.251 | –1.442 | |
| | | (2.542) | (2.452) | |
| N (observations) | 23 | 23 | 23 | 23 |
| $R^2$ | .00 | .73 | .73 | .73 |
| $R^2$ (adjusted) | –.05 | .58 | .60 | .63 |
| F-test statistic for model | .00 | 4.80*** | 5.72*** | 7.13*** |
| Durbin-Watson statistic | 3.01 | – | – | – |
| Durbin's h (p-value) | .007 | .230 | .414 | .561 |
| Breusch-Godfrey (p-value) | .013 | .130 | .306 | .476 |

Notes: Unstandardized betas with standard errors in parentheses. *$\alpha \leq 0.10$; ** $\alpha \leq 0.05$; ***$\alpha \leq 0.01$.

A look at the parliamentary debate coefficient in Table 6.4 reveals that the effect is negative, but statistically insignificant across all models. The election year dummy, on the other hand, is negative and highly statistically significant in Models II, III, and IV. On average, annual changes in central government spending on cultural affairs are between 12 and 13 % lower in election years than in non-election years. The effect of the government dummy variable is positive and statistically significant, which accords with the two party blocs'

classic ideological stances on this issue. The negative effect of last year's spending changes is statistically significant and quite strong for central government spending on this issue. In contrast, the effect of the economic indices is rather small and statistically insignificant.

The effects of the "demand" variables are statistically significant and negative, which is quite understandable given the subsidy nature of public spending in the area of cultural affairs. In fact, one could say that this negative "demand" effect corroborates the impression that public spending on cultural affairs is not primarily driven by public demand. However, sometimes citizens' spending preferences seem to matter and according to Table 6.4 this is so mainly in election years.

Compared to the models in Table 6.3, the models in Table 6.4 generally seem quite healthy and with an adjusted R square on .63 Model IV represents a fairly good explanation of the variation in central government spending on cultural affairs in the period from 1980 to 2003. Besides Model I, the summary statistics in the lower part of Table 6.4 does not indicate disturbing auto-correlation problems and the regression diagnostics do not suggest serious violations of the central assumptions behind OLS-regression. Neither the percentile dummy variables for the parliamentary debate variable, nor the squared transformation of this variable are statistically significant.

Theoretically, one important point of the analyses reported in this section is the relatively weak explanatory power of the parliamentary debate variable. The effect of this coefficient is negative, as expected, but in all models the coefficient is statistically insignificant. On the other hand, when looking at central government spending on cultural purposes only we found that annual spending changes were significantly lower in election years than in non-election years, which is a finding consistent with our theoretical expectation. When looking at total government spending, the effect of this coefficient was negative but statistically insignificant. The general difficulties encountered in explaining total government spending on culturally related issues seem to support the impression from Chapter 5, that our explanatory model works best on central government spending issues. However, we return to a more systematic evaluation of this suggestion in Chapters 7 and 8.

All in all the analyses conducted thus far have provided some empirical support for hypothesis H1.b, which predicted a negative relationship between share of parliamentary debate and public spending changes. Such an effect does indeed seem to exist for defense spending as well as spending on aid to developing countries. When it comes to spending on cultural purposes, however, the effect is barely statistically significant when we look at central government spending only, and clealy insignificant when we look at total

government spending on cultural purposes. Hypothesis H2.1b held that spending would be lower in election years than in non-election years. This claim was supported with respect to aid to developing countries and central government spending on cultural affairs. Defense spending, on the other hand, turned out to be significantly higher in election years than in non-election years. In the last part of this chapter, we examine whether the share of debate in parliament exerts a stronger effect in election years than in non-election years.

## The combined effect of election years and parliamentary debates

This last part of Chapter 6 examines whether there is a combined spending effect from election years and extent of debate in parliament. As expressed by hypothesis H2.2b in Chapter 4, we expect that the negative relationship between share of parliamentary debate and public spending is stronger in election years than in non-election years.

First, we examine the incremental contribution of the interaction term to the statistical models explored above. Due to potential collinearity between the interaction term and the debate and election year dummy, we assess the statistical significance of the interaction term using the restricted F test (see Chapter 5). Second, having identified those issues, where the interaction term contributes significantly to the statistical models, we continue with a closer examination of these models.

Table 6.5 shows the results of a restricted F test for each of the three issues analyzed in this chapter. The null hypothesis is that adding the interaction

Table 6.5. Assessing the incremental contribution of interacting election year and debate in parliament

| | Model II[b] | | Model IV[c] | |
|---|---|---|---|---|
| Policy Category | F ratio[a] | Numerator df / Denumerator df | F ratio[a] | Numerator df / Denumerator df |
| Defense | .39 | 1/14 | .62 | 1/18 |
| Aid to developing countries | 2.25 | 1/15 | .61 | 1/19 |
| Cultural purposes[d] | 3.11* | 1/13 | 3.52* | 1/15 |

Notes: *$\alpha \leq 0.10$; **$\alpha \leq 0.05$; ***$\alpha \leq 0.01$. a: Displays the incremental contribution to $R^2$, when adding the interaction term "Debate in Parliament$_{t-.5*}$ Election Year$_t$" to the models developed earlier in this chapter. b: Refers to columns "Model II" in the respective tables presented in the first part of this chapter. c: Refers to columns "Model IV" in the respective tables presented in the first part of this chapter. d: Only includes specified central government spending.

term to the statistical models does not contribute significantly to their explanatory power measured by the R square value. Models II and IV refer to the corresponding models in the tables presented above.

Not surprisingly, given the above finding of a positive election year effect, the interaction term in the defense area is clearly statistically insignificant. With respect to spending on aid to developing countries, Table 6.5 shows that there is no additional effect of parliamentary debate in election years compared to non-election years. There is no statistically significant effect on total spending on cultural affairs either, but if we focus on central government spending only, Table 6.5 suggests an effect from interacting parliamentary debate and the election year dummy. The effect is statistically significant in both Models II and IV. Table 6.6 explores this interaction effect further.

Table 6.6 shows the coefficients for central government spending on cultural purposes. Models IIa and IVa are identical to Models II and IV in Table 6.4. They represent the "full" model and what seems to be the model with the best fit, respectively. In Models IIb and IVb the interaction term is added in order to evaluate its contribution in more detail.

In Table 6.4 we found a negative but statistically insignificant effect of the parliamentary debate variable on central government spending for cultural purposes. When parliamentary debate about cultural purposes occurs in an election year, however, the negative effect is statistically significant as shown in Table 6.6. The effect is not particularly strong, but according to the summary statistics in the lower part of Table 6.6, adjusted R square does increase by around .05 when the interaction term is added. Besides the government coefficient, the other coefficients in Table 6.6 remain stable when the interaction term is added. Additional analyses show a modest negative relationship between socialist government and debate about cultural affairs in election years. This may suggest that in this instance, some of the party rule effect enters via the extent of parliamentary debate about cultural affairs in election years. In Table 6.6 the individual coefficient of the parliamentary debate variable turns positive, but it is still highly statistically insignificant. The same goes for the individual coefficient of the election year dummy.

Figure 6.1 is a graphical representation of the combined spending effect of extent of parliamentary debate and election years. In non-election years, there is hardly any systematic relationship between the percentage of debate in parliament devoted to cultural questions and central government spending for cultural purposes. If there is a relationship, it is positive, just as the average value of spending changes is positive in non-election years. On the other hand, being in an election year, parliamentary debate about cultural affairs

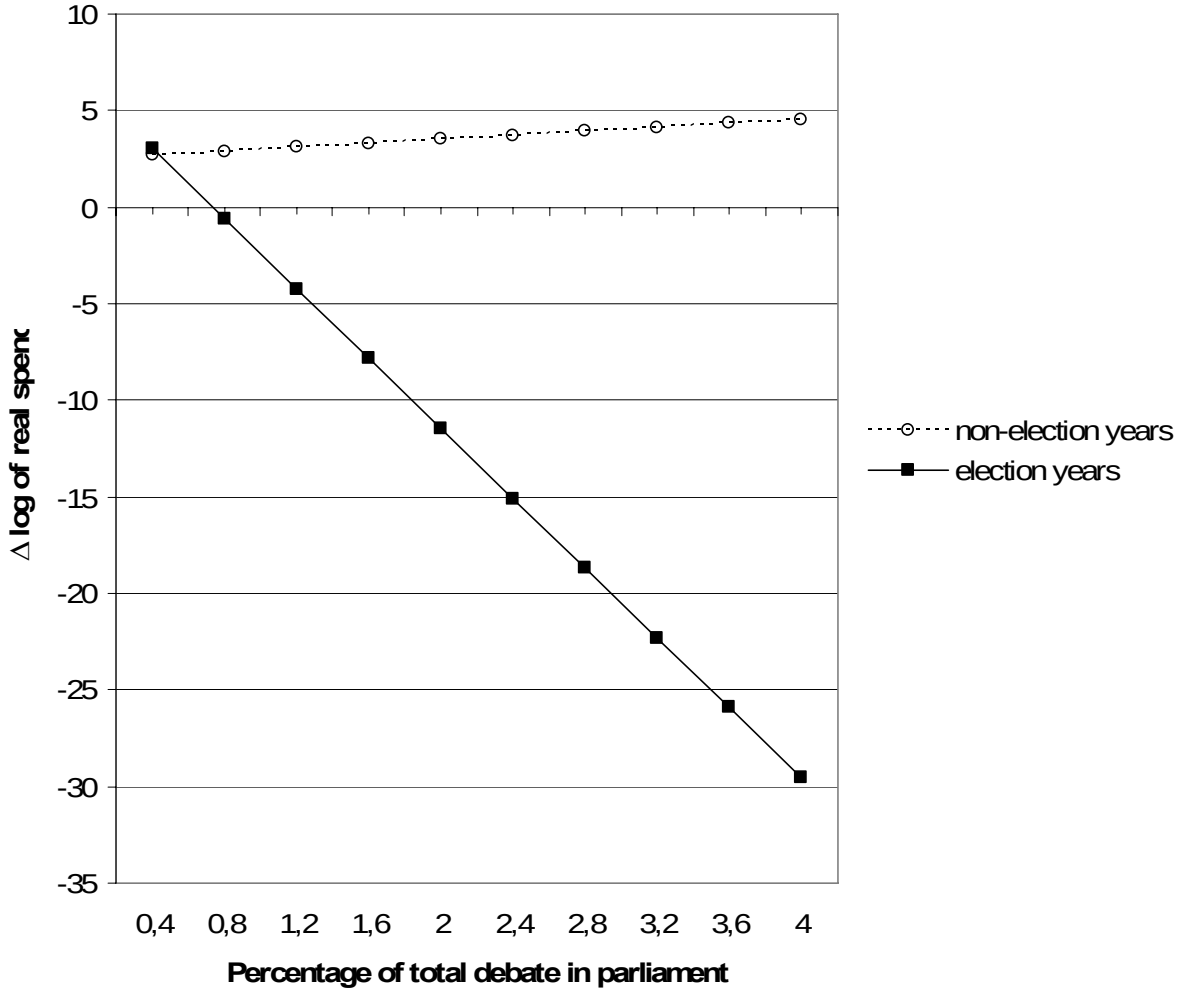
Table 6.6. Central spending on cultural purposes, 1980-2003

| Dependent variable = Δ Log of real spending | | | | |
|---|---|---|---|---|
| | Model IIa | Model IIb | Model IVa | Model IVb |
| Constant | 5.021 | 3.078 | 5.094 | 3.143 |
| | (5.167) | (4.942) | (4.816) | (4.620) |
| Debate in parliament$_{t-.5}$ | –1.317 | .516 | –1.439 | .473 |
| | (2.555) | (2.599) | (2.324) | (2.389) |
| Election year | -12.909*** | 4.148 | -13.549*** | 3.515 |
| | (3.953) | (10.357) | (3.533) | (9.675) |
| Debate in parliament$_{t-.5}$* | | –9.555* | | –9.541* |
| Election year$_t$ | | (5.421) | | (5.088) |
| Δ Cinema tickets sold per | –.231* | –.249** | –.251** | –.263** |
| inhabitants$_{t-1}$ | (.121) | (.113) | (.101) | (.094) |
| Δ Number of attendances to | –.953* | –1.002* | –.945* | –.981** |
| theatrical performances$_{t-1}$ | (.533) | (.498) | (.491) | (.457) |
| Δ Log of real spending$_{t-1}$ | –.695*** | –.624*** | –.680*** | –.613*** |
| | (.159) | (.154) | (.145) | (.140) |
| Left government$_{t-1}$ | 7.880* | 4.683 | 8.155** | 4.731 |
| | (3.937) | (4.095) | (3.241) | (3.523) |
| Δ Inflation$_{t-1}$ | .380 | .135 | | |
| | (2.064) | (1.929) | | |
| ΔΔ Unemployment$_{t-1}$ | –1.251 | –1.132 | | |
| | (2.542) | (2.371) | | |
| N (observations) | 23 | 23 | 23 | 23 |
| $R^2$ | .73 | .78 | .73 | .78 |
| $R^2$ (adjusted) | .58 | .63 | .63 | .68 |
| F-test statistic for model | 4.80*** | 5.25*** | 7.13*** | 7.58*** |
| Durbin-Watson statistic | – | – | – | – |
| Durbin's h (p-value) | .230 | .743 | .561 | .949 |
| Breusch-Godfrey (p-value) | .130 | .652 | .476 | .934 |

Notes: Unstandardized betas with standard errors in parentheses. *$\alpha \leq 0.10$; ** $\alpha \leq 0.05$; ***$\alpha \leq 0.01$.

exerts a rather strong negative effect on central government spending on cultural purposes. The range of debate displayed on the x-axis covers the empirical range observed in data. In Table 4.3 in Chapter 4, we saw that the mean percentage of parliamentary debate about cultural affairs is 1.7. According to Figure 6.1, at this mean level of parliamentary debate, central government spending for cultural affairs would be around 10 percent lower in election years than in non-election year.

Figure 6.2: Illustration of interaction effect, cultural purposes




Note: The figure is based on Model IIb in Table 6.6 above. All other variables are represented with their mean value. The government dummy is set to 0.

While the combined effect of parliamentary debate and the election cycle dummy was positive with respect to the popular issues of pollution control and primary education, the spending effect on the unpopular cultural affairs issue is negative. This finding supports hypothesis H2.2b, which claimed that when a majority of the voters prefers decreased spending, the negative spending effect of parliamentary debate in election years will be stronger than the effect in non-election years. In fact, with respect to central government spending on cultural purposes the effect of parliamentary debate is only statistically significant in election years. On the other two issues analyzed in this chapter, we found a direct effect of variation in parliamentary debate, but discovered no additional effect when interacting election years and parliamentary debate.

## Summary and conclusion

On all three issues analyzed in this chapter, we found a negative relationship between share of parliamentary debate and public spending. With respect to defense and aid to developing countries, the effect was direct and independent of variation in the election year dummy. With respect to the effect of parliamentary debate on spending for cultural affairs, being in an election year turned out to be a necessary condition. Furthermore, we only found an effect when examining central government spending alone. As expected, we also found that spending on aid to developing countries and central government spending on cultural affairs was lower in election years than in non-election years. With respect to defense spending, however, we found the opposite effect, namely that spending was higher in election years than in non-election years.

All in all the analyses conducted in this chapter have shown that extent of parliamentary debate does affect public spending changes. And even more interestingly, in light of the findings in Chapter 5, the chapter has demonstrated that extent of parliamentary debate can exert a negative influence on public spending. Hence, it does not appear that the more politicians talk about an issue, the more they spend. Rather, it supports the theoretical expectation that the more they talk about a given issue in the macropolitical venue, the more likely it is that they adjust spending in a manner more consistent with the median voter's spending preferences. In other words, the theoretical model seems to provide a rather valid prediction about what happens when an issue moves up and down the macropolitical agenda.

It is still too soon to draw conclusions about the systematic variation in our theoretical model's explanatory power across all seven issues. In addition to a discussion of potential alternative explanations, the next chapter presents a closer examination of the relationship between our variables of central theoretical interest.

## Notes

1 The Civil Defense organization does in fact involve local spending, but this organization has been excluded from this analysis since it undertakes many non-defense related civilian tasks.
2 Another candidate could be Denmark's participation in the military intervention in Kosovo in 1999, but adding a dummy that equals one in 1999, else zero, does not show any relation to the dependent spending variable or to the other explanatory variables.

# Chapter 7

# Delayed effects, causality, and alternative explanations

The purpose of this chapter is to challenge and qualify the conclusions drawn in Chapters 5 and 6 by making supplementary analyses of the relationships of main theoretical concern. In particular the following questions will be addressed. First I take a closer look at the relationship between extent of parliamentary debate and public spending. Figure 4.4 in Chapter 4 indicated that, on average, the best model fit between parliamentary debate and public spending is attained when we use a time lag of t–.5, which corresponds to correlating debate in parliament from October 1, 2002 to around June 1, 2003 with public spending in 2003. Additional analyses reported in this chapter reveal that this result covers significant and understandable variation across issues. Issues characterized by a substantial degree of local autonomy are especially prone to displaying delayed spending effects.

Second, a set of supplementary causality analyses generally supports the claim that parliamentary debate affects public spending changes more than public spending changes affect parliamentary debate.

Third, I take up the discussion from Chapter 2 about the role played by public saliency in democratic responsiveness. While many earlier opinion policy studies have focused on a measure of public saliency, the preceding chapters have shown that degree of debate in the macropolitical venue plays a vital role in understanding the governmental responsiveness to the median voter opinion. In this chapter, I adopt a measure of public saliency to see how it relates to the two central variables, extent of parliamentary debate and public spending.

Fourth, in view of the attention accorded the Thermostat Model in the opinion policy literature reviewed in Chapter 2, I discuss the empirical results produced in Chapters 5 and 6 in light of this explanation. There is not necessarily a contradiction between the Thermostat Model and the one tested in this dissertation. The important issue is to ascertain whether the Thermostat Model offers an adequate alternative account for the pattern observed in Chapters 5 and 6.

Fifth, the chapter examines in more detail the dynamics between election years and parliamentary debate. Are the observed spending effects driven mainly by election cycle dynamics in the sense that parliamentary debate about certain issues is systematically more intense in election years than in non-election years?

Sixth and finally, the chapter concludes with a panel data analysis in order to arrive at an overall estimate of the effects of parliamentary debate on public spending changes. This analysis does not supersede the time series regressions in Chapters 5 and 6, but on balance, as a complementary tool, it does provide some concluding information of the main effects and various interaction effects.

## Delayed spending effects

As noted in Chapter 4, there is always a time lag between a decision to spend public money and the point in time when the money is actually spent. This time lag may for various reasons differ across countries (see Eichenberg & Stoll 2003) and across policy domains. Based on Figure 4.4 in Chapter 4, which shows the result of an empirical exploration of model fits averaged across all seven issues, we used a time lag equal to t–0.5 in Chapters 5 and 6. Howver, as shown in Table 7.1 below, by using the same time lag across issues we miss important information about the relationship between an issue's share of parliamentary debate and public spending on that given issue.

For all seven issues analyzed in Chapters 5 and 6, I have made a regression of the share of parliamentary debate on changes in public spending from 1980 to 2003 using different time lags. Table 7.1 reports the result for each issue and for each time lag. To be clear, a time lag of t–0.5 refers to a correlation between an issue's share of parliamentary debate in, say, October 1, 2002 to June 1, 2003 and public spending in 2003. Consequently, t–1.5 refers to a correlation between parliamentary debate in 2001/02 and public spending in 2003. On the other hand, a time lag of t+0.5 refers to a correlation of public spending in year 2002 with the issue's share of parliamentary debate in 2002/03, indicating that in this case spending changes precede share of parliamentary debate.

Table 7.1. Model fit for different time lags, 1980-2003

| Time lag (t) | Law & order | Pollution control | Primary education | Health | Defense | Aid to developing countries | Cultural purposes |
|---|---|---|---|---|---|---|---|
| 1.5 | .00 | .18 | .03 | .07 | .02 | .02 | .02 |
| .5 | .07 | .09 | .05 | .00 | .00 | .01 | .04 |
| –.5 | *.15* | *.28* | .02 | .02 | *.16* | *.17* | .00 |
| –1.5 | .02 | .17 | .02 | *.14* | .00 | .03 | *.22* |
| –2.5 | .04 | .11 | .00 | *.06* | .03 | .09 | *.03* |

Notes: R square values when testing the statistical model: $\Delta Spending_t = a + b*(Parliamentary\ debate)_{(t)} + e$

The cells in Table 7.1 containing the highest R square values are shaded, indicating where the best model fit occurs. In the column for primary education, no cells are highlighted since none of the models comes close to the level of statistical significance. According to Table 7.1, in four of the seven issues we get the best model fit with a time lag of t–.5, clearly suggesting that for these issues the time lags used in Chapters 5 and 6 are the most appropriate. These are the three issues that do not involve local government spending decisions (law and order, defense, and aid to developing countries) and the issue of pollution control. Probably reflecting positive feedback mechanisms, Table 7.1 further shows that with respect to pollution control other time lags are also quite well correlated with spending on pollution control.

The most intriguing result in Table 7.1 concerns the issues of health and cultural purposes. In Chapter 5, we found no statistically significant effect from share of parliamentary debate on public spending on health services and in Chapter 6, we merely discovered an effect on cultural purposes when we looked at specified central government spending only. What the result in Table 7.1 suggests is that on these two issues, spending effects are delayed by a time lag of t–1.5. In view of the substantial degree of local involvement in these two spending domains, this result actually seems very reasonable. In Denmark, the central government and organizations representing the counties and municipalities agree on aggregate local tax levels, local service priorities and local spending levels for the upcoming year in meetings held during the preceding summer, which again follows the end of a parliamentary session (Blom-Hansen 1999, pp. 244-5). Hence, in a normal course of central-local government relations, debate in parliament in year 2001/02 can affect negotiations in the summer of year 2002, which again, at the aggregate level, determines the main priorities of local public spending in year 2003. In light of this policy making cycle, a time lag of t–1.5 seems quite reasonable. Consequently, while the preceding chapters, inspired by former literature on agenda setting effects, anticipated institutional friction and constraints from local autonomy to be reflected in threshold effects and non-linear relationships, the results in Table 7.1 clearly suggest that these constraints are first and foremost reflected in longer time delays. To explore this result further, we re-estimate the models of public spending on health and cultural purposes using a time lag of t–1.5 between share of debate and public spending.

Table 7.2 displays the results of the analysis on health related issues in Denmark from 1980 to 2003. Besides the different lag of the parliamentary debate variable, the choice of control variables is a reflection of the corresponding analysis reported in Table 5.4 in Chapter 5. Given the delayed effects of the parliamentary debate variable, it could be that some of the other

coefficients would be increased as well if alternative time lags were used. However, an explorative investigation of this question shows no improved explanatory power of these alternative factors when lagged by another year.

Table 7.2. Spending on health, 1980-2003

| Dependent variable = Δ Log of real spending | | | |
|---|---|---|---|
| | Model I | Model II | Model III |
| Constant | –.648 | –.545 | 1.599* |
| | (1.295) | (1.928) | (.831) |
| Debate in parliament$_{t-1.5}$ | .445* | .360 | |
| | (.241) | (.293) | |
| ΔΔ People$_t$ | | .124 | .158 |
| | | (.110) | (.108) |
| ΔΔ People aged > 67$_t$ | | .254 | .348 |
| | | (.213) | (.202) |
| Δ Log of real spending$_{t-1}$ | | –.107 | –.098 |
| | | (.214) | (.217) |
| Election year$_t$ | | –1.613 | –1.953)* |
| | | (1.004) | (.982) |
| Left government$_{t-1}$ | | 2.026* | 1.783* |
| | | (.965) | (.960) |
| Δ Inflation$_{t-1}$ | | –.648 | –.445 |
| | | (.480) | (.458) |
| ΔΔ Unemployment$_{t-1}$ | | 1.029 | 1.039 |
| | | (.804) | (.817) |
| N (observations) | 23 | 23 | 23 |
| $R^2$ | .14 | .46 | .40 |
| $R^2$ (adjusted) | .10 | .15 | .12 |
| F-test statistic for model | 3.39* | 1.47 | 1.42 |
| Durbin-Watson statistic | 2.14 | – | – |
| Durbin's h (p-value) | .577 | .051 | .277 |
| Breusch-Godfrey (p-value) | .553 | .023 | .181 |

Notes: Unstandardized betas with standard errors in parentheses. *$\alpha \leq 0.10$; ** $\alpha \leq 0.05$; ***$\alpha \leq 0.01$.

Model I in Table 7.2 confirms the increased effect of parliamentary debate on public spending when using a time lag equal to t–1.5. Furthermore, the debate coefficient carries the appropriate sign, thus supporting the theoretical expectation of a positive spending effect on this issue where a large majority of the population prefers increased public spending. According to this model, total public spending on health related issues in year t increases by half a percentage when the share of parliamentary debate in year t–1.5 increases by one percentage. In Model II, which includes the range of other possible determinants, the effect of parliamentary debate becomes insignificant, suggesting

that the effect is rather weak. The explanatory power of the added control variables in both Models II and III is also very modest, and their coefficients are very similar to the ones shown in Model II in Table 5.4. In Models II and III, we find a slightly significant positive effect of left-wing government, and in Model III a barely significant negative effect of election years. However, if we try to reduce the models in Table 7.2 to a statistically significant model we end up with Model I, where only the positive effect of parliamentary debate is statistically significant. Not surprisingly, given the negatively signed election cycle effect the interaction term between parliamentary debate and the election year dummy adds no explanatory power to the model. On the whole, we therefore still need a good model of the development in public spending on health services, but most importantly in light of the theoretical model developed in Chapter 3, the additional analysis reported in Table 7.2 does show a positive and statistically significant effect of share of parliamentary debate on public spending.

Table 7.3 shows the results of the re-analysis of total public spending on cultural affairs using a time lag for the debate coefficient of t–1.5. In Model I, as expected, the parliamentary debate coefficient carries a negative sign and is clearly statistically significant. On average, if the share of parliamentary debate dedicated to culturally related issues in year t–1.5 increases by one percent, public spending on cultural purposes in year t decreases by more than two percentage points. According to the statistics shown in the lower part of Table 7.3, Model I suffers from auto-correlated residuals, which probably indicates that the model is mis-specified. However, adding the control variables in Model II removes the auto-correlation. Furthermore, adding the set of potentially relevant controls does not change the effect of parliamentary debate. As was the case with the corresponding statistical model reported in Table 6.3 in Chapter 6, none of these control variables improves the model's explanatory power significantly. Nor does removing the parliamentary debate variable in Model III alter this conclusion substantively. The negative effect of last year's spending changes achieves the level of statistical significance, while none of the other coefficients changes notably.

Model IV in Table 7.3 shows what seems to be the best model fit, given the imposed restrictions and available explanatory factors. The coefficient for the parliamentary debate variable remains stable across the models and is statistically significant in Model IV. Additionally, we find a negative effect of last year's spending changes, suggesting that on average, large spending increases are followed by more modest changes. It should further be noted that the inclusion of the lagged spending variable in Model IV removes the autocorrelation that plagues Model I in Table 7.3.

Similar to the model of public health spending, the explanatory power of the models in Table 7.3 is not impressive judged by an adjusted R square value of around 0.26. Nevertheless, with respect to the coefficient of main theoretical interest, the effect is statistically significant and in the direction expected theoretically. An interaction of the parliamentary debate variable with the election cycle dummy does not increase the explanatory power of the statistical models shown in Table 7.3.

Table 7.3. Spending on cultural purposes, 1980-2003

| Dependent variable = Δ Log of real spending | | | | |
|---|---|---|---|---|
| | Model I | Model II | Model III | Model IV |
| Constant | 5.997*** | 5.236** | 2.220 | 6.654*** |
| | (1.756) | (2.224) | (1.766) | (1.709) |
| Debate in parliament$_{t-1.5}$ | –2.330** | –2.120* | | –2.299** |
| | (.966) | (1.067) | | (.919) |
| Δ Cinema tickets sold per inhabitants$_{t-1}$ | | –3.947 | –.919 | |
| | | (5.804) | (6.125) | |
| Δ Theatre tickets sold per inhabitants$_{t-1}$ | | –3.761 | .0943 | |
| | | (21.386) | (23.295) | |
| Δ Log of real spending$_{t-1}$ | | –.360 | –.482* | –.332* |
| | | (.241) | (.254) | (.185) |
| Election year$_t$ | | –1.071 | –1.557 | |
| | | (1.904) | (2.065) | |
| Left government$_{t-1}$ | | 2.832 | 3.573 | |
| | | (1.925) | (2.065) | |
| Δ Inflation$_{t-1}$ | | –.695 | –.161 | |
| | | (1.079) | (1.143) | |
| ΔΔ Unemployment$_{t-1}$ | | –.671 | –1.047 | |
| | | (1.298) | (1.404) | |
| N (observations) | 23 | 23 | 23 | 23 |
| $R^2$ | .22 | .50 | .36 | .33 |
| $R^2$ (adjusted) | .18 | .21 | .06 | .26 |
| F-test statistic for model | 5.81** | 1.73 | 1.19 | 4.84** |
| Durbin-Watson statistic | 2.74 | – | – | – |
| Durbin's h (p-value) | .038 | .918 | .374 | .705 |
| Breusch-Godfrey (p-value) | .044 | .891 | .268 | .678 |

Notes: Unstandardized betas with standard errors in parentheses. *$\alpha \leq 0.10$; ** $\alpha \leq 0.05$; ***$\alpha \leq 0.01$.

Taken together, Tables 7.2 and 7.3 suggest an important correction to the conclusions drawn in Chapters 5 and 6. Annual variation in the share of parliamentary debate devoted to cultural and health related issues does affect total public spending changes on these issues in comprehensible ways. The

plausibility of this result is further corroborated by the fact that more immediate effects occur for all three centralized pure state level issues, while the delayed effects occur on issues that involve more local autonomy, and hence, more institutional friction from the perspective of national policy makers. Still, however, the lack of impact of parliamentary debate on public spending on primary education does not support the theoretical model. Using a time lag equal to t–1.5, the coefficient is still positive, but it fails to attain the level of statistical significance.

We have so far focused only on the effect of parliamentary debate on public spending. In the next section, we take a further look at the causal relationship between parliamentary debate and public spending.

## Assessing the direction of causality

Returning to the pattern in Table 7.1 above, it is noteworthy that the best model fits generally occur for time lags $t<0$ than for $t>0$, the latter indicating that public spending changes affect variation in parliamentary debate. Again, however, the pattern varies across issues and in some instances public spending does seem to be correlated with subsequent variation in parliamentary debate. In order to tease out the relationship and direction between these two key variables, we conduct a vector autoregression (VAR) analysis. No exogeneity restrictions are made in VAR, where each series is modeled as functions of lagged values of the series and lagged values of the other series. The use of VAR is closely related to the notion of Granger causality. A variable x is said to Granger cause another variable y if y can be better predicted from the past of x and y together than from the past of y alone (cf Greene 2003, pp. 592-3). Hence, in the language of Granger causality, we hypothesize that change in public spending does not Granger-cause the share of parliamentary debate devoted to the given issue. As discussed further below, the use of annual data and the limited number of observations do not allow a definitive test of this hypothesis, but supportive evidence clearly would add to our confidence in the hypothesized causal mechanism.

Table 7.4 reports the result of Wald tests computed on the difference between a restricted model (excluding the lagged values of the other time series) and an unrestricted model (including these lagged values). A statistically significant Wald test indicates that the lagged term Granger causes the dependent variable, which is thus endogenous to the lagged term (cf Greene 2003, p. 593). Given that the basic time unit is years and given the limited number of observations in each time series, only two lags are included in the VAR model presented in Table 7.4. Since the results may depend crucially on the number of lags, I have also explored a three lag Wald test, which, however, leads to almost identical results as those reported in Table 7.4 (cf

Table A3.1 in Appendix 3). The limited degrees of freedom are also why we in this context only evaluate the direct and bivariate relationship between public spending and parliamentary debate time series. The hypothesis to be evaluated using the Wald statistics reported in the two right hand columns of Table 7.4 is that all the coefficients on the lags of the explanatory variable are jointly zero in the equation for the dependent variable. Hence, in the upper part of Table 7.4 we test whether lagged values of public spending affect parliamentary debate, while in the lower part we test whether lagged values of parliamentary debate affect public spending. In addition, I have summed across the coefficients of the lags in order to assess whether the cumulated effect is negative or positive.

The results in Table 7.4 are very clear. First, when looking at the models with parliamentary debate as the dependent variable shown in the upper part of the table, we cannot for any of the seven issues reject the hypothesis that the lagged values of public spending are jointly zero. This finding clearly suggests that changes in public spending do not Granger-cause share of parliamentary debate. In other words, share of parliamentary debate on a given issue seems to be exogenous to spending changes on that issue. Furthermore, we find no systematic direction of spending effects across issues. For law and order related issues and for primary education the direction is negatively signed, suggesting that spending increases may reduce the issue's share of parliamentary debate, while spending cuts would engender increased debate. Combined with the opposite effects of the two unpopular issues, defense and aid to developing countries, these results might support the argument proposed in the last section of Chapter 4, where it was argued that to the extent that public spending affects parliamentary debate, the effect would likely be the opposite of what we expected, going from parliamentary debate to public spending changes. In other words, unpopular spending decisions may lead to increased parliamentary debate, given the interests of the opposition. However, according to the analysis reported in Table 7.4, this explanation does not fit the issues of pollution control, health and cultural politics. Hence, we can only conclude that public spending does not exert a consistent and systematic effect on variation in parliamentary debate, and in none of the seven series is the effect even close to attaining the level of statistical significance.

Turning briefly to the lower part of Table 7.4, the Wald test merely corroborates the previous findings. With respect to the issues of law and order and pollution control, parliamentary debate seems to Granger cause public spending changes and the sign of the cumulated effect is positive as expected. For the three unpopular issues, defense, aid to developing countries, and

Table 7.4. VAR model of parliamentary debate and public spending

| Issue | Dependent | Independent | Summed lags | Chi2 | Prob>chi2 |
|---|---|---|---|---|---|
| Law and order | Parliamentary $debate_t$ | ΔLog of real $spending_{t-.5,\ 1.5}$ | –.145 | 1.297 | .523 |
| Pollution control | Parliamentary $debate_t$ | ΔLog of real $spending_{t-.5,\ 1.5}$ | .005 | .235 | .889 |
| Primary education | Parliamentary $debate_t$ | ΔLog of real $spending_{t-.5,\ 1.5}$ | –.037 | 1.760 | .415 |
| Health | Parliamentary $debate_t$ | ΔLog of real $spending_{t-.5,\ 1.5}$ | .171 | 2.452 | .293 |
| Defense | Parliamentary $debate_t$ | ΔLog of real $spending_{t-.5,\ 1.5}$ | .258 | 3.067 | .216 |
| Aid to developing countries | Parliamentary $debate_t$ | ΔLog of real $spending_{t-.5,\ 1.5}$ | –.090 | 2.301 | .317 |
| Culture | Parliamentary $debate_t$ | ΔLog of real $spending_{t-.5,\ 1.5}$ | .022 | 1.193 | .551 |
| Law and order | ΔLog of real $spending_t$ | Parliamentary $debate_{t-.5,\ 1.5}$ | **.410** | **5.002** | **.082** |
| Pollution control | ΔLog of real $spending_t$ | Parliamentary $debate_{t-.5,\ 1.5}$ | **17.241** | **22.772** | **.000** |
| Primary education | ΔLog of real $spending_t$ | Parliamentary $debate_{t-.5,\ 1.5}$ | 4.757 | 3.697 | .157 |
| Health | ΔLog of real $spending_t$ | Parliamentary $debate_{t-.5,\ 1.5}$ | .504 | 3.533 | .171 |
| Defense | ΔLog of real $spending_t$ | Parliamentary $debate_{t-.5,\ 1.5}$ | **–1.036** | **7.510** | **.023** |
| Aid to developing countries | ΔLog of real $spending_t$ | Parliamentary $debate_{t-.5,\ 1.5}$ | **–1.699** | **6.811** | **.033** |
| Culture | ΔLog of real $spending_t$ | Parliamentary $debate_{t-.5,\ 1.5}$ | **–11.493** | **9.773** | **.008** |
| Period | | 1980 to 2003 | | | |
| Lags | | 2 | | | |

Notes: Cells contain chi-square results and p values from a Wald test of Granger causality using annual observations from 1980 to 2003. Text in bold is significant at $p < .10$.

cultural purposes, we also clearly reject the hypothesis that the lagged values of parliamentary debate are jointly zero. Furthermore, the cumulated effects of these time series are negative, as we would expect knowing the position of the median voter and in light of the previous empirical findings in Chapters 5 and 6. Finally, the effect on health and education related issues seems to be

positive, as expected, but in this case we cannot reject the null hypothesis, which reflects the weak effects found in the analyses conducted above.

One advantage in the use of time series is that it enables a separation of observations in time. However, three characteristics of our time series may reduce the power of the Granger style analysis above. First, a lot can happen within the span of a year that cannot be observed given that our basic time units are annual observations. Second, there is some overlap between our parliamentary debate and public spending observations as the former follows the parliamentary sessions and the latter the calendar year. Third, even without this overlap there is always a time lag between the decision to spend public money and the time when the money is actually spent, and we can only use actual public spending as a proxy for the underlying spending decisions. Despite this uncertainty, the analyses conducted in this section have nevertheless provided additional evidence in support of our basic theoretical model. Hence, while the Wald tests reported in Table 7.4 do not uncover a smoking gun, they clearly corroborate the conclusions drawn in Chapters 5 and 6.

### Public saliency, parliamentary debate, and public spending

In this section, I take a closer look at the relationship between public saliency, parliamentary debate and public spending. As mentioned in Chapter 2, the concept of public saliency has been a central component in many studies of the opinion policy relationship (see Page & Shapiro 1983; Jones 1994, Chapter 5; Soroka 2003; Baumgartner & Jones 2004; Wlezien 2005). According to the dominant view expressed in these studies, voters hold opinions on a multitude of issues, but these opinions are not of equal salience to the voters. Most relevant in the present context is the proposition made by opinion policy scholars that the more salient an issue is to the public, the more likely that public opinion on that issue will influence public policy. The argument of this study is instead that an important condition for facilitating policy responsiveness to public opinions is how salient a given issue is to the policymakers that dominate the macropolitical venue. As mentioned in Chapter 2, there is not necessarily a close link between the macropolitical agenda and public saliency. It might be, for instance, that an issue is salient to the general public but is not subject to macropolitical intervention, just as macropolitical intervention may anticipate, reduce, or perhaps even prevent increased public saliency for that issue. Given the focus on public saliency in earlier opinion policy studies, it is nevertheless intriguing to see how this concept relates to the two variables of central concern in the present study, namely public policy measured as public spending, and degree of macropolitical attention measured as share of parliamentary debate. It is also a relevant question from

a democratic point of view whether the importance people assign to certain issue opinions matter for the policy representation of these opinions.

As noted by Dearing and Rogers (1996, p. 47), the so-called MIP survey question has become the most widely used index of variation in public saliency at the national level. This survey question usually takes the form: "What do you think is the most important problem facing this country today?" As a measure of public saliency, a clear advantage of this item is its open-ended nature in that issue responses are not proposed to the respondent (cf Dearing & Rogers 1996, p. 47). When estimating the public agenda the main problem of closed-ended questions is that they convey a high degree of suggestibility to the respondent, and consequently, there is a risk that some issues on the public agenda may be overlooked. A further advantage of the MIP question as a measure of the public agenda is that it provides a relative ranking of issues in terms of their saliency, which corresponds closely to the agenda concept.

On the other hand, as a measure of the salience of voters' policy opinions on given issues the MIP question also has some significant drawbacks. First, asking someone to name the most important problem "facing this country" might not tap into what the respondent himself thinks is important, but what he thinks the media or perhaps politicians may consider important (ibid; Funkhouser 1973, p. 69). Second, the MIP question is best suited to study longitudinal variation on major issues, while variation in minor issues is more difficult to capture with this rather crude saliency measure (cf Dearing & Rogers 1996, p. 48). Finally, as argued by Wlezien (2005), the MIP question may confuse the importance of issues and problems, perhaps in part reflecting that the underlying concept of public saliency has not been very clearly defined in the literature.

In Denmark, The Danish National Election Studies have asked an equivalent to the US MIP question since 1971, but mainly around election time, and hence, rather infrequently over time. Instead, to measure public saliency, this section utilizes a survey item asked consistently four times a year since 1984 with respect to three of the seven policy issues analyzed in Chapters 5 and 6. The question is: "I now mention a set of societal problems and ask you kindly to tell me whether it is something that concerns you very much, somewhat, very little or not at all?"[1] The three issues are the law and order, pollution control and the defense. In some years respondents were also asked about their concern for issues such as health and education, but given the infrequency of these questions, I choose to concentrate on the three issues where a consistent and unbroken time series is available covering 1984 to 2003.

Unlike the MIP question, the concern item does suggest issue responses to the respondent, hence lacking the open-ended nature of the MIP question. This is a clear shortcoming when the ambition is to estimate the total public agenda. However, that is not the ambition in this section, where we focus only on the potential effects of longitudinal variation in the saliency of three specific issues. Hence, when used as a measure of longitudinal variation in the saliency of a given issue, this closed-ended nature of the question is much less of a problem. In fact, the closed-ended nature probably improves comparability over time on these three issues, none of which has been a persistently major issue in Denmark in the period of investigation. As mentioned above, one shortcoming of the MIP question is that it is best suited to measure variation in major public issues. An additional advantage of the concern question is that it asks explicitly about the respondent's own concern, not that of the nation, government, or country.

Despite these advantages, and similar to the MIP question, the concern item is still a rather indirect measure of the salience of the spending opinions expressed by the voters. For instance, a more direct measure would be to ask the respondents to rank their expressed spending opinions in terms of importance. However, for lack of a better alternative we use the concern item as an acceptable proxy for longitudinal variation in the public salience of the three issues where the question has been asked consistently since 1984. More specifically, concern for "violence and crime" is used as a proxy for the saliency of the public median preference for increased spending on law and order issues measured in Chapter 4. Concern for "pollution problems" is used as a proxy for the saliency of the aggregate preferences for increased spending on pollution control. The salience indicator for the third and final issue, defense, is perhaps more ambiguous. Here the survey question asks about concern for "outbreak of war". Expressing increased concern for war does not seem a particularly good proxy for the salience of the majority's preference for cutting down defense spending. In fact, it might be that the less concern for war, the more salient the majority preference for spending cuts in this issue domain.

In the following analysis, we let the data inform us about the direction and magnitude of these potential saliency effects. First, we will ascertain whether and how variation on the macropolitical agenda measured by the parliamentary debate variable  and public saliency measured by the concern survey item correspond. We then move on to examine the relationship between public saliency and public spending.

Figure 7.1 is a graphic depiction of the longitudinal variation in public salience and parliamentary debate for the three issues selected for further

analysis in this section. The line displaying public salience represents the percentage of respondents who are "very much" or "somewhat concerned" about the given issue. Furthermore, each observation represents the mean value of the four quarterly surveys conducted from September in year t to June in year t+1, matching the cycle of parliamentary sessions in the Danish Folketing.

Over time, crime and violence have been issues that have been of the most concern to the respondents. As shown in Figure 7.1a, from 1984 to the mid 1990s between 80 and 90 percent of the respondents express concern about these issues, followed by a decline to a level of 65 percent in 2002/03. At the same time, when we look at the share of parliamentary debate devoted to crime related issues, this share seems to increase during the 1990s. According to additional studies conducted by IFKA (Thulstrup et al. 2005) it is difficult to identify the reason for the decline in the level of concern about crime and violence. Of most importance to the present study, the graphic depiction in Figure 7.1a clearly suggests that share of parliamentary debate is not a simple proxy for variation in the public saliency of this issue. In Table 7.5 below, where we apply a Granger style causality analysis, we explore the relationship between these two variables more systematically, but first we take a brief look at the time series for the pollution control and defense issues in Figures 7.1b and 7.1c.

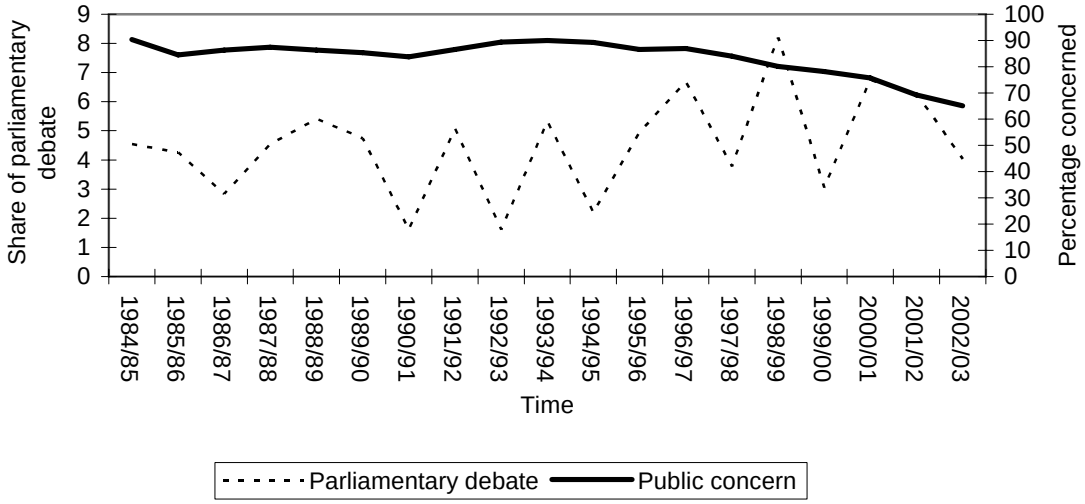
As shown in Figure 7.1b, the level of concern for pollution has declined rather steadily since the mid 1980s, interrupted only by a moderate and rather short-lived increase in the late 1990s. The variation in the share of debate devoted to environmental issues seems to correspond quite closely to the level of expressed concern in the latter half of the 1980s, where after the two time series seem to follow quite different paths. Consequently, some correspondence between the two time series may exist, but based on Figure 7.1b, there is no indication that the extent of parliamentary debate about environmental protection is a simple reflection of variation in the general public's concern about pollution.

Finally, concerning defense, Figure 7.1c does in fact suggest some covariation in the period from 1984 to 2003. Both time series appear to decline during the last years of the Cold War, and after that, they both fluctuate around a lower value of concern as well as share of parliamentary debate. A visual inspection of Figure 7.1c, however, does not suffice to determine whether fluctuations in one of the time series precede or follow fluctuations in the other.

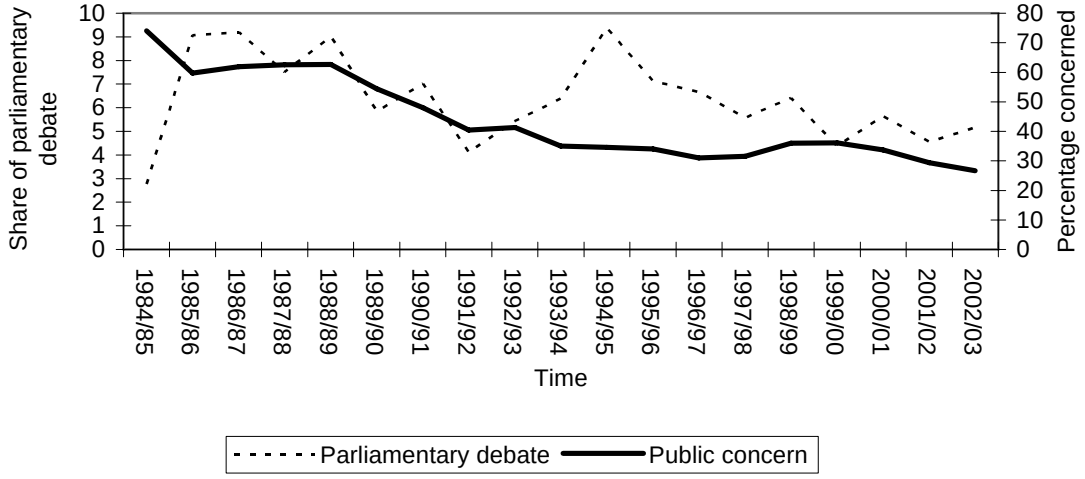
To answer this question, I have conducted a Granger style causality analysis very similar to the Wald tests applied above. Table 7.5 reports the main result of this analysis. Again, the primary purpose is to examine whether

Figure 7.1. Parliamentary debate and public concern

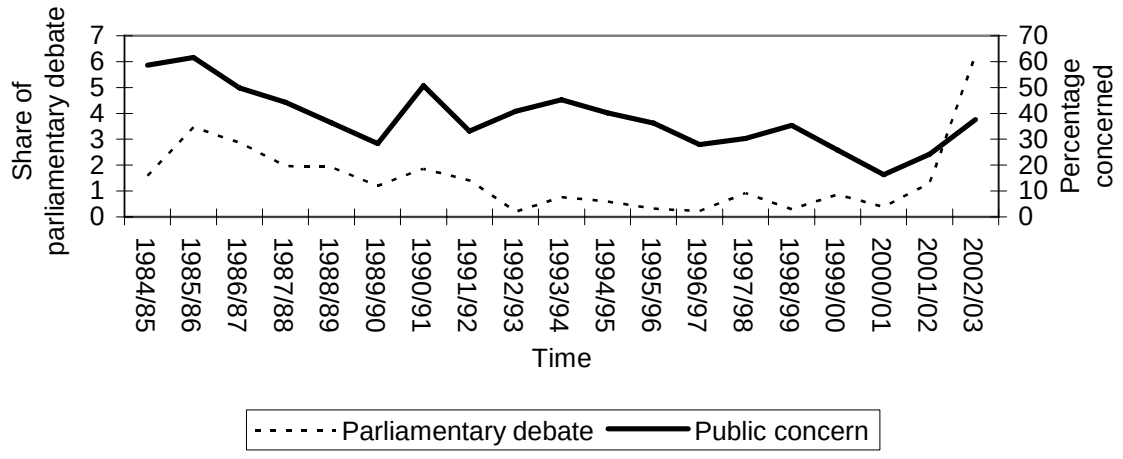
a. Law and order




b. Pollution control




c. Defense



variation in one of the time series can be better predicted from the past of both time series together than from the past of just one time series. However, given the possibility of instant correlation between the two time series, we also test for the relationship between share of debate in year t and public concern in year t, replacing the Wald test used above with a standard F-test based causality analysis. The public concern time series depicted in Figure 7.1 above have a clear trend which, as discussed in Chapter 4, may inflate confidence intervals in standard regression analyses. However, one of the advantages of the Direct Granger method applied in this chapter is that estimates remain unbiased despite auto correlated time series data, which means that we do not have to difference the time series of public concern in order to impose stationarity (cf Soroka 2002, p. 125). In this case, the lagged terms take care of the auto correlation. Theoretically, however, it is not straightforward whether it should be level of public concern or annual changes in these levels that might respond to or affect short-term variation in parliamentary debate. Consequently, the analysis was also run using a stationary time series measuring annual changes in level of public concern, showing largely the same results as those reported in Table 7.5 based on levels of public concern (see Table A3.2 in Appendix 3). With the caveat that the findings are based on only 19 annual observations in the period from 1984 to 2003, Table 7.5 shows the following main results.

Looking first at the law and order issue, the non-lagged term shows a negative relationship between share of parliamentary debate and level of public concern. Our time series data are not sufficiently fine grained to show the causal direction of this effect, but in this case it would seem most reasonable to expect increased parliamentary debate to reduce the level of public concern rather than to expect increased public concern about crime and violence to lead to less parliamentary debate over these issues. This interpretation is supported by the negative and statistically significant effect, when lagged values of parliamentary debate are correlated with the public concern time series. Furthermore, the effect on parliamentary debate when lagging the public concern measure is positive, but statistically clearly non-significant. Hence, with respect to the law and order issue, public saliency and parliamentary debate may be correlated, but they do not seem to cover the same underlying dimension. Furthermore, to the extent that the relationship between the two variables is causal, the direction seems to be from share of parliamentary debate to level of public concern.

The issue of pollution control shows a different pattern where the only statistically significant correlation between parliamentary debate and public concern occurs when using non-lagged time series. In this case, however, the

Table 7.5. VAR model of parliamentary debate and public concern

| Issue | Dependent | Independent | Summed Lags | F-test |
|---|---|---|---|---|
| Law and order | Parliamentary debate$_t$ | Public concern$_t$ | –.514 | 5.02** |
| | | Public concern$_{t–1, 2}$ | .489 | 1.56 |
| | | Parliamentary debate$_{t–1, 2}$ | –1.020 | 3.24* |
| | Public concern$_t$ | Parliamentary debate$_t$ | –.610 | 5.02** |
| | | Parliamentary debate$_{t–1, 2}$ | –1.686 | 4.60** |
| | | Public concern$_{t–1, 2}$ | 1.045 | 35.97*** |
| Pollution control | Parliamentary debate$_t$ | Public concern$_t$ | .193 | 3.60* |
| | | Public concern$_{t–1, 2}$ | –.154 | 1.75 |
| | | Parliamentary debate$_{t–1, 2}$ | .325 | .46 |
| | Public concern$_t$ | Parliamentary debate$_t$ | 1.28 | 3.60* |
| | | Parliamentary debate$_{t–1, 2}$ | .068 | .01 |
| | | Public concern$_{t–1, 2}$ | .868 | 40.60*** |
| Defense | Parliamentary debate$_t$ | Public concern$_t$ | .048 | 1.87 |
| | | Public concern$_{t–1, 2}$ | –.124 | 5.66** |
| | | Parliamentary debate$_{t–1, 2}$ | 1.380 | 5.79** |
| | Public concern$_t$ | Parliamentary debate$_t$ | 3.008 | 1.87 |
| | | Parliamentary debate$_{t–1, 2}$ | –1.052 | .43 |
| | | Public concern$_{t–1, 2}$ | .586 | 1.40 |

Notes: Cells contain results from OLS regressions, using annual data from 1984/85 to 2002/03.

relationship is positive, which indicates either that increased parliamentary debate over environmental issues leads to increased public concern, or that increased public concern leads to increased parliamentary debate. Both effects are plausible just as it might also be the case that both time series merely respond to the same set of exogenous factors, the latter interpretation being supported by the clearly insignificant lagged effects. Hence, in this case public saliency and the macropolitical agenda may in fact reflect some of the same underlying factors, but as evident from Figure 7.1.b, they at times clearly also deviate substantively.

A third type of pattern occurs for defense related issues. According to the lower part of Table 7.5, there is no statistically significant relationship between public concern and parliamentary debate when the non-lagged values of these two defense time series are correlated. Instead, lagged values of public concern seem to exert a negative effect on current values of parliamentary debate. This result suggests that higher levels of public concern about out-

break of war tend to be followed by less parliamentary debate about defense related issues. The explanation of this effect is not obvious, and the fact that a closer inspection shows that the effect is mainly driven by public concern in year t–2 may indicate that something spurious is at play here. Looking at public concern about outbreak of war, we find that lagged variation in parliamentary debate does not exert a systematic influence. Finally, when estimating a three lag model, most of the effects found in Table 7.5 do not attain the level of statistical significance (see Table A.3.3 in Appendix 3).

On the whole, we have not found a clear and consistent pattern in Table 7.5. Some relationship between the two time series appears to exist, but it varies markedly across the three issues. That different dynamics dominate different policy domains have also been suggested by earlier agenda setting studies and is hence not a new discovery (cf Soroka 2002). Concerning the research question guiding the present study, the most important finding in Table 7.5 and Figure 7.1 above is the lack of a strong systematic relationship between public concern and parliamentary debate. Despite the uncertainty stemming from the low number of issues, the limited length of the time series and the use of a slightly different measure of public salience, this finding clearly suggest that the macropolitical agenda is not just a simple proxy for public salience. This conclusion is further corroborated in Table 7.6, where public spending changes are correlated with the measure of public concern. Following the logic of the causality tests conducted above, Table 7.6 shows the result of a Granger style Wald test for a VAR model of public spending and public concern.

Looking at the upper part of Table 7.6, the cumulated coefficients of the lagged public concern time series carry positive signs for the law and order issue and that of pollution control, which was to be expected given the direction of public spending attitudes. Furthermore, the coefficient on the defense issue carries a negative sign, which also corresponds to the persistent majority preference for decreased public spending on defense related issues. However, only the effect on pollution spending reaches the level of statistical significance. In fact, to the extent that a relationship does exist between the two time series, the effect mainly seems to go from public spending changes to public concern, as indicated in the lower part of Table 7.6. These results indicate that increased spending on pollution control and defense leads to increased public concern for these issues. Conducting a similar test using *annual changes* in the level of public concern does not alter any substantial conclusions, other than that the effect found with respect to the defense issue turns statistically insignificant (see Table A3.4 in Appendix 3). Using a three-

lag model merely replicates the results shown in Table 7.6 (see Tables A3.5 and A3.6 in Appendix 3).

Table 7.6. VAR model of public spending and public concern

| Issue | Dependent | Independent | Cumulated lags | Chi2 | Prob. > chi2 |
|---|---|---|---|---|---|
| Law and order | ΔLog of real spending$_t$ | Public concern$_{t-.5, 1.5}$ | .438 | 3.348 | .187 |
| Pollution control | ΔLog of real spending$_t$ | Public concern $_{t-.5, 1.5}$ | **2.472** | **28.253** | **.000** |
| Defense | ΔLog of real spending$_t$ | Public concern $_{t-.5, 1.5}$ | –.013 | 3.969 | .137 |
| Law and order | Public concern$_t$ | ΔLog of real spending $_{t-.5, 1.5}$ | –.211 | 2.323 | .313 |
| Pollution control | Public concern$_t$ | ΔLog of real spending $_{t-.5, 1.5}$ | **.048** | **6.024** | **.049** |
| Defense | Public concern$_t$ | ΔLog of real spending $_{t-.5, 1.5}$ | **1.599** | **8.381** | **.015** |
| Period | | 1985 to 2003 | | | |
| Lags | | 2 | | | |

Notes: Cells contain chi-square results and p values from a Wald test of Granger causality using annual observations from 1985 to 2003. Text in bold is significant at $p < .10$.

It may be that the weaker effects of the public concern variable compared to the parliamentary debate variable stem from the smaller number of observations. Consequently, to examine whether the relationship found between parliamentary debate and public spending in Table 7.4 disappears if we look at the shorter period from 1984 to 2003, I have applied the Wald test to this reduced sample size. As shown in Table A3.7 in Appendix 3, this reduced sample test only displays very marginal changes compared to the full sample results reported in Table 7.4. This is actually a very strong finding. It not only supports the robustness of the claimed relationship between parliamentary debate and public spending changes, but also corroborates the impression that our measure of public saliency and the macropolitical agenda measure something different, the latter being the strongest predictor of responsiveness to public spending preferences.

To sum up, the aggregate nature of this study and the use of years as the basic time unit probably hide a lot of short-term or disaggregated dynamics between public spending changes, parliamentary debate, and the measure of public concern. It should further be noted that this part of the study has been applied only to a subset of the seven policy issues. With this in mind, however, the supplementary analysis conducted in this section indicates that

public saliency as such is not the driving force behind the policy responsiveness observed in Chapters 5 and 6. One cannot infer from these findings, however, that public saliency is an irrelevant condition when explaining policy responsiveness to public opinion. Earlier opinion policy studies cited in Chapter 2 seem to suggest otherwise. What I have demonstrated in this section is merely that public saliency is not an adequate substitute for the macro-political agenda effects observed in this dissertation, and it has been an important objective of this chapter to clarify this question.

## Attitudes, parliamentary debates and public spending changes

Based on Table 4.1 it was argued that the median voter's spending attitudes on the seven issues chosen for this study stayed rather constant in the period of investigation. In every year for which we have observations, a majority of the respondents has preferred either increased or decreased public spending on a given issue. From Table 4.1 in Chapter 4 it also appears, however, that this constant pattern covers some underlying variation. In some years, more than 80 percent of the respondents prefer increased public spending on health related issues, while in other years the corresponding majority sums to only slightly more than 60 percent. Not least in light of Wlezien's influential work on the Thermostat Model (cf Chapter 2), it seems relevant to consider the potential impact of this underlying variation in public spending attitudes. For instance, does variation in the share of parliamentary debate reflect just this marginal variation in public spending attitudes and, consequently, would the observed spending effects of the parliamentary debate simply disappear if we were able to control for annual variation in public spending attitudes?

Theoretically, the Thermostat Model and the model developed in this dissertation could be complementary, and hence there is not necessarily a contradiction between the two. In this section, based on Wlezien's (and Soroka's) own studies, I shall argue why the Thermostat Model does not seem to provide a sufficient explanation of the central findings in Chapters 5 and 6.

According to Wlezien's model, public spending changes ($\Delta P$) will be associated with levels of the public's relative spending preferences (R), which can be expressed by the following equation (see Soroka & Wlezien 2005, p. 668):

$$\Delta P_t = \alpha_0 + \beta R_{t-1} + \gamma Z_{t-1} + e_t \,, \qquad (1)$$

where $a_0$ and $e_t$ represent the intercept and the error term, respectively, and Z represents the set of other spending determinants. Based on a similar reasoning as the one applied in this study, expenditure in year t is modeled as a function of lagged explanatory variables, reflecting that public spending decisions often lag behind actual public spending.

The other central term in the Thermostat Model, "net spending preferences" ($R_t$), which is identical to our measure of the spending attitudes of the median voter shown in Table 4.1, is modeled as the difference between the public's preferred level of public spending (P*) and the actual level of public spending (P) (see Wlezien 1995, p. 986). This relationship can be written as:

$$R_t = P^*_t - P_t \qquad (2)$$

Thus, if the preferred level of public spending or actual spending changes, the relative spending preferences of the public change accordingly. Furthermore, the public is expected to respond immediately to actual spending changes when these are put into effect (cf Soroka & Wlezien 2005, p. 667).

With respect to the spending preference term $R_{t-1}$ in equation (1) above, it follows from equation (2) that:

$$R_{t-1} = P^*_{t-1} - P_{t-1} \qquad (3)$$

Hence, the lagged level of relative spending preferences in the above model (1) of spending changes can be replaced by the lagged difference between the public's preferred spending level and the actual spending level. Given that our main concern in this section is the possibility that unobserved annual changes in public spending preferences may affect our results from Chapters 5 and 6, it might be useful to further transform equation (3) into a model of changes in net spending preferences:

$$R_{t-1} = P^*_{t-1} - P_{t-1} \quad =>$$

$$R_{t-1} = R_{t-2} + \Delta P^*_{t-1} - \Delta P_{t-1} \quad =>$$

$$R_{t-1} - R_{t-2} = \Delta P^*_{t-1} - \Delta P_{t-1} \quad =>$$

$$\Delta R_{t-1} = \Delta P^*_{t-1} - \Delta P_{t-1} \qquad (4)$$

The benefit of this little exercise is that instead of a spending preference term that we cannot in this study measure consistently over time, we now end up with a model based on factors that very much look like something that we already control for in our statistical models in Chapters 5 and 6. Whereas last year's spending changes ($\Delta P_{t-1}$) are incorporated in the statistical models, changes in the public's preferred level of public spending ($\Delta P^*_{t-1}$) is more difficult to observe directly. The solution suggested by Wlezien and Soroka

(see Wlezien 1995; 1996; 2004; Soroka & Wlezien 2004; 2005) is to use a proxy for the variation in the public's preferred level of public spending. For instance, in his study of US defense spending, Wlezien (1995) utilizes a measure of the public's fear for the Soviet Union as a proxy of the variation in the preferred level of defense spending. With respect to domestic areas, more broad proxies such as changes in the unemployment and inflation rates or measures of economic security have been used (cf Soroka & Wlezien 2004, p. 541).

In general, these proxies appear to reflect that changes in the public's preferred level of public spending follow changes in relevant real-world indicators. In the statistical analyses conducted in Chapters 5 and 6, we control for a wide range of such real-world indicators, at least in those policy domains where such indicators are available and potentially known or felt by the public. From this point of view, for instance, variation in the annual number of crime reports can be seen as a proxy for the variation in the public's preferred level of public spending on law and order issues. The Cold War dummy and the Iraq War dummy might be reasonable proxies for variation in the public's preferred level of defense spending, just as the measured demographic changes, at least in theory, might correspond to changes in the public's preferred level of health and educational spending.

Hence, while we cannot – due to data limitations – directly control for annual variation in the median voter's relative spending preferences, we actually do control for many of the factors that, according to Wlezien and Soroka's operationalization of the Thermostat Model, account for variation in those spending preferences. In fact, the only time Soroka and Wlezien (2005, p. 685) try to estimate a statistical model of spending changes using all three explanatory variables, the effect of variation in lagged net spending preferences turns insignificant, while the lagged spending variable and the proxy for the preferred level of spending remain statistically significant. This result might reflect, as Soroka and Wlezien (ibid., p. 686) tend to conclude, that their model of spending preferences is very powerful, although scholars with a deeper inclination towards incremental theory, for instance, could come up with other interpretations of the negative effect of last year's spending changes (see e.g. Danziger 1978).

Whatever the true explanation of these effects, the important outcome of the discussion in this section is that the Thermostat Model does not seem to provide a strong alternative explanation of the main findings in Chapters 5 and 6. Obviously, this does not preclude thermostat spending effects, just as the Thermostat Model does not preclude saliency or attention effects (cf Soroka & Wlezien 2005, p. 668). It is nevertheless an important result, cor-

roborating the significant contribution of the estimated effects of the macro-political agenda discovered above.

## Election cycles and parliamentary debates

In this section, we briefly examine the relationship between election year cycles and parliamentary debate, two of the central factors in the analyses in Chapters 5 and 6. The question we address is whether the election cycle in fact drives part of the explanatory power of the parliamentary debate term. In other words, does parliament systematically pay more attention to certain issues in election years than in non-election years, implying that the election cycle is the real underlying explanation of what we observed in Chapters 5 and 6? The collinearity checks carried out in relation to the statistical models in Chapters 5 and 6 did not suggest a strong relationship between these two explanatory variables, but given their importance to the theoretical model, we scrutinize the relationship between the two in Table 7.7 below.

Table 7.7 shows the average share of parliamentary debate over each of the seven issues in election years compared to non-election years. Since the relationship between debate and elections may be lagged, we explore both lagged and non-lagged variation in parliamentary debate.

Table 7.7. Parliamentary debate and election cycles

| | Share of Parliamentary debates$_{t-1}$ (%) | | Share of Parliamentary debates$_{t}$ (%) | |
|---|---|---|---|---|
| | Election years | Non-election years | Election years | Non-election years |
| Law & order | 4.651 | 4.557 | 4.970 | 4.307 |
| Pollution control | 6.138 | 5.790 | 6.183 | 5.967 |
| Primary education | .910 | .935 | .937 | .961 |
| Health | 6.174** | 4.734 | 5.509 | 5.073 |
| Defense | 1.379 | 1.597 | 1.478 | 1.633 |
| Aid to developing countries | 1.312 | 1.025 | 1.211 | 1.089 |
| Cultural purposes | 1.777 | 1.613 | 1.915 | 1.517 |

Notes: *$\alpha \leq 0.10$; ** $\alpha \leq 0.05$; ***$\alpha \leq 0.01$ (comparison of means, two sample t-test).

Table 7.7 does not reveal systematic differences between the issues' share of parliamentary debate in election years and non-election years. For four of the seven issues, the share is a little higher in the year immediately preceding an election, but the difference is statistically significant only with respect to

health. Furthermore, there is no clear pattern suggesting that the four popular issues, for instance, are debated more right before an election than after.

Election campaigns may be said to constitute a special political process characterized by other agenda dynamic than those we observe in Table 7.7 and above. On average, for instance, some issues very likely get more media attention than others during election campaigns. However, when it comes to the measure of parliamentary debate, Table 7.7 shows no sign of systematic differences between election years and non-election years. Hence, the evidence presented in Chapters 5 and 6 is not driven by a trivial relationship between variation in parliamentary debate and the election year cycle.

### Panel data analysis

The empirical study concludes with a panel data analysis, a statistical method that is increasingly used in several research fields within political science (cf Stimson 1985; Beck & Katz 1995; Kittel 1999; Kittel & Winner 2005; Tsebelis & Chang 2004; Plümper, Troeger & Manow 2005). One reason for the popularity of panel data analysis is the attractiveness of pooling observations across both time and space when the length and number of time series are rather limited, as they are in the present study. In this case, panel data, as noted by Gujarati (2003, p. 637), might: "give more informative data, more variability, less collinearity among variables, more degrees of freedom and more efficiency".

The use of panel data analysis, however, has been subject to some controversy too. In addition to more specific questions about the most appropriate way to estimate a given statistical panel data analysis (see Stimson 1985; Beck & Katz 1995; Kittel & Winner 2005; Plümper, Troeger & Manow 2005), the very ambition of estimating one common statistical model across time and space may often be rather heroic. Furthermore, when using cross-section as well as time dimensions, one must also address problems that plague cross-sectional data, that is, heteroskedasticity, and time series data, that is, autocorrelation (cf Gujarati 2003, p. 652).

Based on the statistical analyses conducted thus far, we already know a great deal about the structure and dynamics of the seven time series that in this section are pooled into a panel data set consisting of 7 panels that each contains 23 time observations. We know for instance, that estimated coefficients are much larger with respect to the pollution control issue than to the issues of primary education or health. We also know that the election cycle effect sometimes works in the expected direction, and sometimes in the opposite direction, and that it sometimes shows no impact at all. Consequently, for each of the seven issues individually, using a panel regression design does

not add to our knowledge about the relationship between central variables. On the contrary, it probably leads to inferior predictions.

The aim behind the present use of panel data regression is instead to merely get an overall and, necessarily, rough indicator of the spending effects estimated across the total number of observations. The question to be addressed is whether the hypothesized effects are strong enough to be appreciable across all seven issues and 23 yearly observations? To answer the question I therefore take a very inclusive approach, pooling all seven issues, despite our a priori knowledge about rather different effects across issues.

The panel data analysis is structured as follows. First, inspired by the scheme laid out in Chapters 5 and 6, a regression model of public spending changes is estimated, but this time based on 161 pooled observations (7 issues * 23 annual observations = 161). In addition, the larger number of observations allows a more formal investigation of the conditional hypotheses about: 1) stronger effects of parliamentary debate in election years; 2) constraining effects of local government involvement; and 3) the potentially stronger spending effects on popular issues compared to unpopular issues. In the panel data analysis, these questions are explored via a set of interaction terms.

In the second part of the panel data analysis, I go on to take a closer look at the robustness of the panel data results. Although the main motivation in this section is to get an overall coefficient estimate, it is still reassuring to know whether the results are driven by just one very deviating issue. To examine this question, I perform a so called jackknife analysis, where the basic panel data model developed in the first part of the analysis is re-estimated by excluding one issue at a time to see if and how the values of the point estimates change accordingly.

### *Specification and findings from the panel data analysis*

Panel data models can be specified in a number of ways, depending on the assumptions made about the intercept, slope coefficients, and error terms (see Gujarati 2003, chapter 16). The one I apply in this section is known as a "fixed effect" model that imposes common slopes but allows for varying intercepts across issues and/or across time. Introducing a dummy variable for each of the seven issues, the fixed effect model produces an issue-specific intercept that captures the unobserved issue-specific variation. This removes the average issue effect, which means that the estimated coefficients represent a cross-issue average of the longitudinal effect (cf Kittel & Winner 2005, p. 272). Similarly, time dummies would capture developments over time common to all issues, eliminating any common trends or external shocks. However, since preliminary analyses did not reveal systematic time-effects,

the analyses reported in Table 7.8 only includes cross-sectional dummies to preserve valuable degrees of freedom.

Applying the same variable transformations as used in Chapters 5 and 6, we are confident that the panel data are not plagued by serious auto-correlation. On the other hand, as shown in the lower part of Table 7.8, the test for groupwise heteroscedasticity is highly significant, suggesting that the variances of the error process still differ from issue to issue (cf Beck & Katz 1995, p. 636). Consequently, I follow Beck & Katz' recommendation to apply panel corrected standard errors to adjust for panel heteroscedasticity.

Table 7.8 shows the main results of the panel data analysis. Besides the central variables, share of parliamentary debate and the election year dummy, the models contain a lagged dependent spending variable, the government dummy, and the two economic indices, inflation and unemployment development. To facilitate the presentation, estimated coefficients for the "fixed effect" issue dummies are not shown, but the lower part of Table 7.8 reports the result of a test of their joint statistical significance. To get a single estimate across the popular and unpopular spending issues, the signs of the debate variable and the election year dummy have been reversed for the three unpopular issues (defense, aid to developing countries, and cultural purposes). This transformation means that in Table 7.8 a positive signed coefficient between these two variables and the dependent spending variable supports our theoretical expectation. Based on the studies of lag structures in the first part of this chapter, we use a lag of t–1.5 for the parliamentary debate variable for the issue of health and cultural purposes. For all other issues we use t–.5. Similar to the models in Chapters 5 and 6, all coefficients have been multiplied by 100, such that the estimates represent approximately how many percentage points annual public spending changes on average if the explanatory variable changes by one unit.

Model 1 in Table 7.8 represents the basic panel data model without interaction terms. As expected, the effect of parliamentary debate is positive and statistically significant. In substantive terms, this coefficient implies that the more parliamentary debate over a given issue, the more likely are popular spending changes. Furthermore, when we look at the size of the coefficient, the effect is stronger than the one found in most of the individual time series regressions, but significantly weaker than the one found in the analysis of the pollution control issue alone. The same is true for the election year dummy, where the estimated coefficient in Model 1 is also positive and clearly statistically significant. Hence, on average, popular spending changes seem to be more likely in election years than in non-election years. In addition, Model 1 shows a negative and slightly statistically significant effect of changes in

Table 7.8. Panel data analysis, 1980-2003 (fixed effects)

| Dependent variable = Δ Log of real spending | | | | | |
|---|---|---|---|---|---|
| | 1 | 2 | 3 | 4 | 5 |
| Constant | –28.986*** | –30.127*** | –15.008 | –32.931*** | –23.852** |
| | (8.002) | (7.576) | (12.090) | (10.121) | (11.560) |
| Debate in parliament$_{(t)}$[ab] | 5.584*** | 4.913*** | 2.669 | 6.292*** | 3.516 |
| | (1.500) | (1.314) | (2.455) | (1.881) | (2.215) |
| Election year$_t$[b] | 7.943** | –8.721*** | 6.931** | 8.092*** | –7.603*** |
| | (2.603) | (2.950) | (2.630) | (2.681) | (2.655) |
| Δ Log of real spending$_{t–1}$ | –.074 | –.080 | –.082 | –.074 | –.086 |
| | (.130) | (.117) | (1.247) | (.129) | (.113) |
| Left government$_{t–1}$ | –3.407 | –4.716** | –4.597* | –2.699 | –4.501* |
| | (2.705) | (2.380) | (2.754) | (2.722) | (2.353) |
| Δ Inflation$_{t–1}$ | –3.109* | –2.183** | –2.499** | –3.058** | –1.655 |
| | (1.244) | (1.102) | (1.202) | (1.240) | (1.073) |
| ΔΔ Unemployment$_{t–1}$ | –.047 | –1.985 | –.619 | –.152 | –2.457* |
| | (1.474) | (1.294) | (1.460) | (1.475) | (1.287) |
| Debate in parliament$_{(t)}$[a] * Election year$_t$ | | 5.282*** | | | 4.717*** |
| | | (1.247) | | | (1.171) |
| Debate in parliament$_{(t)}$[a] * Local spending involvement$_t$ | | | –5.022 | | –4.489 |
| | | | (3.210) | | (3.070) |
| Debate in parliament$_{(t)}$[a] * Unpopular issues | | | | –3.252 | –5.228** |
| | | | | (2.102) | (2.188) |
| N (observations) | 161 | 161 | 161 | 161 | 161 |
| $R^2$ | .23 | .32 | .27 | .24 | .36 |
| $R^2$ (adjusted) | .17 | .26 | .20 | .17 | .29 |
| F (model) | 3.70*** | 5.40*** | 4.12*** | 3.54*** | 5.40*** |
| F (issue dummies) | 24.34*** | 24.55*** | 27.25*** | 31.08*** | 27.48*** |
| Groupwise heteroskedasticity | 1502.74*** | 1431.61*** | 1755.58*** | 2263.23*** | 2345.68*** |

Notes: Estimated coefficients for issue dummies are not shown to facilitate the presentation. Unstandardized betas with panel-corrected standard errors in parentheses. *$\alpha \leq 0.10$; ** $\alpha \leq 0.05$; ***$\alpha \leq 0.01$. a: t–1.5 for health and cultural purposes. t–.5 for all other issues. b: signs reversed for defense, cultural purposes, and aid to developing countries.

inflation rates, suggesting that on average, public spending measured in fixed price levels decreases more in times of increasing inflation. None of the other coefficients in Model 1 attains the level of statistical significance.

Because of the increased sample size, one of the advantages of a panel data design is that it reduces problems of multicollinearity, which renders the examination of multiple interaction terms more amenable. In Models 2 to 5 in Table 7.8 we take advantage of this option and estimate the effect of the main interaction terms hypothesized in Chapter 3. Still, because all three interaction terms are in part based on the same parliamentary debate indicator, some multicollinearity is unavoidable. To evaluate the sensitivity of the coefficients of the interaction terms we therefore add them one after one (Models 2, 3, and 4) and then all together (Model 5).

In Model 2, we test the proposition that the more parliamentary debate and the closer the next election, the more likely are popular spending changes on that given issue. Again, to estimate the effect across all seven issues, signs have been reversed for the three unpopular issues with respect to the election year dummy and the parliamentary debate variable. Consequently, we expect a positive coefficient for the interaction term in Model 2.

In line with expectations, the interaction term in Model 2 in Table 7.8 clearly exerts a positive effect on popularity of public spending changes. When adding the interaction term, the negative effect of left-wing government attains the level of statistical significance, but generally, the other coefficients in the model do not change notably. The only exception is the election year dummy, but because of multicollinearity, the coefficient of this variable cannot be adequately assessed in isolation from the interaction term. When the two coefficients are combined, Model 2 shows that an election year clearly increases the possibility of popular spending changes even for relatively low shares of parliamentary debate. Furthermore, Model 2 suggests that in all years, more parliamentary debate leads to more popular spending changes, but this effect is particularly strong in election years. Hence, while the time series regressions in Chapters 5 and 6 pointed to a statistically significant interaction effect between parliamentary debate and the election cycle on three out of the seven issues, the panel data analysis suggests that on balance, such an effect exists.

Model 3 in Table 7.8 examines the modifying effect of local government involvement in public spending. Local government involvement, or degree of local autonomy, is not easy to define and capture by a single measure (cf Bogason 1997, chapter 4). In Table 7.8, the share of public money spent by municipalities or counties is used as a proxy. This measure is not meaningful in all policy domains, but for the seven issues included in this study, the measure corresponds quite well to the standard ranking in terms of local autonomy. According to the proxy, defense, law and order, and aid to developing countries are strictly central government issues, and primary education

and health are those with the highest degrees of local government responsibility. The issues of pollution control and cultural affairs are intermediate in that they involve a significant degree of both central and local government spending. For each issue, the proportion of public money spent at the local level varies slightly from 1980 to 2003, but the main variation is clearly across issues.

Following from this operationalization of local government constraints, we expect that the larger the share spent at the local level of government, the weaker the relationship between share of parliamentary debate and popular public spending changes. In other words, we expect the coefficient of the interaction term in Model 3 in Table 7.8 to be negative, which indeed it is. On average, share of parliamentary debate is still positively associated with popular spending decisions, but less so the greater amount of money spent in the municipalities or counties. One should note that the latter effect would be even stronger if we use a lag of t–.5 for all issues, instead of using a lag of t–1.5 for the health and cultural issues, which in fact already accounts for some of the local government imposed friction. None of the two parliamentary debate coefficients in Model 3 is statistically significant individually, but according to an additional F-test, they are jointly clearly significant.[2] The sensitivity of these two coefficients indicates that they are positively related, which means that in this sample, central government issues on average tend to take up more of the debate in parliament than local government issues. Compared to Model 1, the other coefficients in Model 3 are largely unaffected by the introduction of this interaction term.

In Chapter 4, it was speculated that the magnitude of the spending effects of macropolitical attention might depend on whether the median voter prefers increased or decreased public spending. All else being equal, opposition from labor or special interests with intensive preferences against spending cuts in the policy domains they are concerned with might make it more costly for politicians to comply with voter preferences, compared to policy domains, where voters and labor groups support increased public spending. Consequently, it could be that popular spending effects caused by increased parliamentary debate are less likely for the three unpopular issues, where the median voter prefers decreased spending, than for the four popular issues, where the median voter prefers spending increases.

To examine this possibility, I have created a dummy variable separating the four popular issues from the three unpopular issues in Model 4 in Table 7.8. Using popular issues as the reference category, we expect a negative effect from this interaction term, which is what we find. On average, a higher share of parliamentary debate in parliament leads to more popular spending

decisions, but as expected, the effect is smaller for unpopular issues than for popular ones. The other coefficients in Model 4 do not differ markedly from the estimated coefficients in Model 1.

Finally, in Model 5 in Table 7.8 we estimate a panel data model that includes all three interaction terms simultaneously. Based on this model we can make the following observations. First, the estimated coefficients of the interaction terms are robust to the simultaneous estimation of the other interaction terms. Second, the direction of all three interaction coefficients is as we expected theoretically. Because they are partially dependent on the election year dummy and the parliamentary debate variable, it is difficult to assess the statistical significance of their individual contributions. Judged by the variation in the model's overall explanatory power (adjusted $R^2$), the interaction term between election year and parliamentary debate nevertheless seems to exert the strongest modifying effect. Besides the coefficients of theoretical interest, the direction of the other coefficients in Table 7.8 remains stable across all five models, while the magnitude of the effects changes somewhat.

As mentioned above, the rationale behind the panel data analysis presented in Table 7.8 is to get an overall estimate of the coefficients of main theoretical interest. The finding in Chapters 5 and 6, for instance, that on four of seven issues an expected effect is statistically significant makes it difficult to assess whether the hypothesis receives support on balance. It is from this perspective that the supplementary panel data analysis seems justified. However, in such a panel data analysis, the contribution of each issue to the overall estimate depends on the magnitude of the coefficients found in the longitudinal studies in Chapters 5 and 6. For instance, in the panel data analysis, very strong support on one issue counts more than moderate support or slight rejection on another.

Despite the aggregated approach taken in this part of the analysis, it would however be less convincing if the overall estimates are driven solely by a single issue. Therefore, I perform a jackknife analysis on Model 1 in Table 7.8 to discover the robustness of the basic panel data results (see Kittel & Winner 2005, p. 285). More precisely, I re-estimate the model by excluding the issues one by one. The resulting minimum and maximum estimates are reported in Table 7.9 along with the estimates from the model including all seven issues. The excluded issue and the minimum values of the point estimates are shown in the first double column, while the maximum ones are shown in the last double column.

Table 7.9. Jackknife analysis

| Dependent variable = Δ Log of real spending | | | | | |
|---|---|---|---|---|---|
| | Minimum estimate | Excluded issue | Original estimate | Maximum estimate | Excluded issue |
| Debate in parliament$_{(t)}^{ab}$ | 1.151*** (.283) | Pollution control | 5.584*** (1.103) | 6.903*** (1.906) | Health |
| Election year$_t^b$ | 1.945** (.764) | Pollution control | 7.943*** (2.603) | 9.936*** (2.953) | Defense |
| Δ Log of real spending$_{t-1}$ | –.076 (.127) | Law & order | –.074 (.130) | –.043 (.127) | Pollution control |
| Left government$_{t-1}$ | –5.623* (3.353) | Health | –3.407 (2.705) | 1.959** (.977) | Pollution control |
| Δ Inflation$_{t-1}$ | –3.818*** (1.483) | Primary education | –3.109** (1.244) | –.777* (.467) | Pollution control |
| ΔΔ Unemployment$_{t-1}$ | –.969 (1.626) | Defense | –.047 (1.474) | 1.176 (1.801) | Law & order |

Note: Entries are from Model 1 in Table 7.8, together with minimum and maximum coefficient estimates from re-estimates of the model while excluding each issue one at a time. The table reveals the responsiveness of the estimates to the inclusion of particular issues. All entries are unstandardized betas with panel-corrected standard errors in parentheses. *$\alpha \leq 0.10$; ** $\alpha \leq 0.05$; ***$\alpha \leq 0.01$. a: t–1.5 for health and cultural purposes. t–.5 for all other issues. b: signs reversed for defense, cultural purposes, and aid to developing countries.

Table 7.9 clearly shows the disproportionate impact of the pollution control issue. By excluding this issue, the estimated coefficient of parliamentary debate drops from around 5.5 to a little more than 1.1. On the other hand, when the health issue is excluded, the estimated coefficient increases to around 6.9, reflecting the weak effects for the issue of health found in the time series regressions reported in Table 7.2 above. Of most importance, however, the estimated parliamentary debate coefficient remains clearly statistically significant despite the exclusion of the pollution control issue. A similar pattern is found for the election year dummy. The election year effect drops from a high of almost 10 when the defense issue is excluded to a low of 2 percentage points when the issue of pollution control is excluded, but remains statistically significant over the entire range.

Several of the control variable estimates in Table 7.9 are also influenced by the exclusion of the pollution control issue. The effect is most dramatic on the government dummy, which turns positive and appears to be more in line

with the standard perception of increased public spending under left-wing governments when the pollution control issue is excluded.

Finally, as to the interaction terms explored above, additional analyses indicate that the statistical significance of these effects disappears when the pollution control issue is excluded. The weaker and more unstable effects from these interaction terms correspond to the impressions from the individual time series regressions conducted above. Such interaction effects might be systematic, but their overall explanatory power is relatively weak for the seven issues included in this study.

To sum up, the panel data analysis has provided an estimate of the explanatory power of the coefficients of main theoretical interest averaged across the seven issues. As cautioned above, the additional information in this panel data analysis is rather limited and cannot replace the information obtained from the individual time series regressions reported in Chapters 5 and 6 and in the first part of this chapter. In the present context, the panel data analysis is justified as an attempt to summarize the findings from each of the seven time series regressions in order to get a balanced evaluation of the evidence, evidence that, all in all, supports the core expectation in this dissertation, namely that the more parliamentary debate about a given issue, the more likely popular spending changes will be.

## Summary and conclusion

The aim of this chapter was to qualify and challenge the conclusions drawn in Chapters 5 and 6. This objective was pursued from several angles. First, a closer examination of lag structures revealed that with respect to the issues of health and cultural purposes, the spending effects of changes in parliamentary debate do exist, but with a longer time lag than the corresponding effects found for the other issues. Given that these two issues are characterized by a substantial degree of local government spending, this result indicates that the hypothesized friction from local autonomy manifests itself as time delays rather than the expected threshold effects.

Second, these additional studies of time lags generally suggested that on issues where debate and spending changes are correlated, variation in the parliamentary debate time series likely precede variation in public spending. This finding supports the causality claim in the theoretical model examined in Chapters 5 and 6.

Third, the chapter explored the relationship between a measure of public saliency, the measure of parliamentary debate, and that of public spending changes. Some relationship seems to exist between these three variables, but no systematic pattern across the three issues appeared. Of most importance, the study did not indicate that public saliency is the driving force behind the

results found in Chapters 5 and 6. This finding not only challenges the predominant focus on the concept of public saliency in many earlier opinion policy studies, but also points to the relevance and contribution of the explanatory variable dominating the present study.

Fourth, considering the attention accorded Wlezien's Thermostat Model in the recent opinion policy literature cited in Chapter 2, this chapter discussed whether this model could provide an adequate alternative account for the pattern observed in Chapters 5 and 6. Inspired by Wlezien's own studies, I argued in this chapter that many of the control variables included in Chapters 5 and 6 should account for the potential effects of the Thermostat Model, which reassures us that unobserved marginal changes in public spending attitudes are not sufficient to explain the empirical findings in this dissertation.

Fifth, in a brief intermezzo the chapter demonstrated no systematic variation between an issues' share of parliamentary debate in election versus non-election years. This finding clearly indicates that the combined effect of parliamentary debate and election cycles found for some issues is a real interaction effect in the sense that it is not driven by a trivial relationship between variation in parliamentary debate and the election year cycle.

Sixth and finally, at the overall level of analysis the concluding panel data regression provided empirical support for the main propositions derived from the theoretical model developed in Chapter 3. As emphasized above, this result does not reject the significant variation across issues found in Chapters 5 and 6. What it shows is simply that on balance, evidence does seem to support the theoretical model outlined in Chapter 3.

Altogether, the analyses reported in this chapter have extended and qualified many of the conclusions drawn in the two preceding chapters. Together with the findings in Chapters 5 and 6, the many pieces of additional evidence produced in this chapter consolidate the empirical validity of the main hypotheses deduced from the model of conditional responsiveness to public opinion developed in Chapters 3 and 4.

## Notes

1. Data is collected by IFKA and made available by Christoffer Green-Pedersen, University of Aarhus.
2. F-test (”Debate in Parliament” and ”Debate in Parliament*Local spending proportion”) = 18.58***.

# Chapter 8

# Conclusions, generalizations, and implications

The main purposes of this concluding chapter are to summarize the empirical findings of this study, to discuss the prospects of generalizing the insights gained beyond the seven Danish issues analyzed here, and to ascertain how these findings may either fit or challenge various aspects of alternative public policy theories. The chapter closes with a reflection on a set of normative questions about the impact of public opinion on public policy in light of the outcome of the empirical study.

## Summary and discussion of empirical results

In the previous chapters, we largely focused on the seven issues one by one. Based on the sum of analyses presented in Chapters 5, 6, and 7 we are now able to give a more general assessment of the empirical evidence. Table 8.1, which summarizes the main results of the time series and panel data analysis, is helpful in this endeavor. In Table 8.1, √ means that the analysis has provided statistically significant support for the theoretical claim expressed in the left column of the table. A **0** means no statistical relationship between the variables of interest, while a ÷ represents a statistically significant relationship in the direction opposite the one expected theoretically. The brackets ( ) mean that the finding seems vulnerable to minor variations in model assumptions. In all analyses, a .10 level of statistical significance has been used (cf the discussion in Chapter 4).

Table 8.1 can be read both horizontally and vertically. A horizontal comparison across issues makes it possible to evaluate the support of each proposition and the apparent effect of different degrees of local government involvement, while a vertical reading provides a comparison of the empirical evidence across the propositions. In addition, the column to the right summarizes the result of the panel data analysis, but for the present, we ignore that column and focus on the outcome of the time series regressions.

Looking first at the horizontal patterns, Table 8.1 points to the following conclusions. As evident from the first row, the analysis strongly supports the core hypothesis about a positive relationship between an issue's share of parliamentary debate and popular spending adjustments. On five out of the seven issues the relevant coefficient was statistically significant and in the expected direction. The effect was clearly statistically insignificant only with respect to the issue of primary education, while a delayed but relatively weak effect seems to exist on health related issues. In general, it hence seems that

the more an issue is debated on the floor of the national parliament, the more likely that the collective public opinion influences public policy on that issue. This is a strong result that clearly suggests that the basic theoretical model proposed in this dissertation has empirical validity. Furthermore, as expected the weakest effects are found on the two issues with the highest degree of local government involvement, although the two intermediate issues on this dimension (pollution control and cultural purposes) displayed stronger effects than was expected from variation in local autonomy alone. Below, I shall return to the modifying effects of local autonomy in more detail.

The second row in Table 8.1 summarizes the evidence on the election cycle hypothesis. Recall from Chapter 2 that the reading of the more classic election cycle studies revealed a rather poor empirical record. One reason, it was argued, might stem from having disregarded the importance of issue importance, so to speak. If an issue is low on the macropolitical agenda, governmental energy and money in election years may be better spent on other higher ranked issues. Evaluating the direct effect of election cycles in row 2 in Table 8.1 suggests, however, that we might be guilty of a similar misapplication of the theoretical model. Our justification for looking at this direct effect, as spelled out in Chapter 4, is based on the belief that the seven issues studied in this dissertation are all cases of relatively broad generality and macropolitical saliency compared to the many non-institutionalized issues floating around without being subjected to public surveying.

With this caveat in mind, row 2 in Table 8.1 provides some support for the election cycle hypothesis, but also some significant deviations. On three of the seven issues, we found a statistically significant effect carrying the signs we expected theoretically. We also found an effect for cultural purposes, but it attains the level of statistical significance only when we look at central government spending. An effect contrary to the expected might exist with respect to spending on health related issues, suggesting lower public spending growth in election years than in non-election years. As shown in Chapters 5 and 7, however, the statistical estimate turned out to be very sensitive to even minor variations in model assumptions, which places it somewhere between a null finding and a negative finding. On the other hand, a clearly negative and robust finding cropped up on the defense issue. In Denmark, contrary to what we expected given the spending attitudes of the median voter, public spending on national defense decreased more in non-election years than in election years in the period from 1980 to 2003. The effect for primary education is positive, but clearly not statistically significant. Please note that for the two local government issues we also explored the effect of

Table 8.1: Summary of empirical findings

| | No local autonomy | | | Medium local autonomy | | High local autonomy | | |
|---|---|---|---|---|---|---|---|---|
| | Law & order | Aid to developing countries | Defense | Pollution control | Cultural purposes | Health | Primary education | All issues (panel data analysis) |
| *The larger the share of parliamentary debate, the more likely are popular spending adjustments (H1.a. & H1.b.)* | √ | √ | √ | √ | √ | **(√)** | **0** | √ |
| *Popular spending adjustments are more likely in election years than in non-election years (H2.1a. & H2.1b.)* | √ | √ | ÷ | √ | **(√)** | **(÷)** | **0** | √ |
| *The effect of the share of parliamentary debate on public spending adjustments is stronger in election years than in non-election years (H2.2a. & H2.2b.)* | **0** | **0** | **0** | √ | **(√)** | **0** | √ | √ |
| *The lower the degree of local government involvement, the stronger the effect of the share of parliamentary debate on public spending adjustments* | | | | | | | | **(√)** |
| *The effect of the share of parliamentary debate on public spending adjustment is stronger on issues where a majority of the voters prefers increased spending than on issues, where a majority prefers decreased spending* | | | | | | | | √ |

Notes: √ = statistically significant support for the theoretical claim; **0** = no statistical relationship between the variables of interest; ÷ = a statistically significant relationship in a direction opposite the one expected theoretically; ( ) = the finding is vulnerable to even minor variations in model assumptions. In all analyses a .10 level of statistical significance has been used as a cut off point.

local election cycles, but that did not affect the conclusions reported in Table 8.1.

All in all the empirical evidence presented in row 2 in Table 8.1 suggests that election cycles do matter for public spending decisions. But the effect varies significantly across issues and in particular the negative effect found on one or two of the seven issues clearly indicates that our theoretical model does not capture all the relevant dynamics involved in this process. One reason for the weaker election cycle effects compared to the macropolitical effects reported in the first row in Table 8.1 may have to do with a potential flaw in the belief that the seven issues are cases of constant importance to voters and macropolitical actors alike. Based on the explicit testing of an interaction effect between variation in the macropolitical agenda and the election cycle dummy, row 3 in Table 8.1 provides an indication of the leverage of this suggestion.

First, on four out of the seven issues we found no additional statistical effect when we interacted the measure of the macropolitical agenda with that of election cycles. Second, on two of the seven issues - pollution control and primary education – being in an election year clearly amplifies the effect of parliamentary debate in the direction we expected theoretically. With respect to pollution control we found both an additive and an interaction effect of parliamentary debate, while being in an election year seems to be a necessary condition for parliamentary debate to show an effect on primary educational spending. Finally, looking at central government spending on cultural purposes only, we find the expected combined effect, but if we look at total public spending for cultural purposes, the effect disappears.

On balance, the findings reported in row 3 in Table 8.1 are mixed. On the one hand, the empirical support is sufficiently strong to warrant further exploration of this relationship in future studies. On the other hand, the findings do not explain the deviating observations reported in row 2 in Table 8.1, which suggests that the potential misrepresentation of the macropolitical agenda in the analyses reported in the second row in Table 8.1 is not an adequate explanation for these deviations. I shall return to a few more general implications that may be derived from these findings, but first we will take a closer look at the variation across the local autonomy dimension.

In Table 8.1, the seven issues are divided into three categories of local government involvement. Ignoring the middle category, a different pattern seems to exist for the three central government issues in the upper left corner of the table and the two local government issues in the upper right corner. While we hardly find any statistically significant effects with respect to health and primary education, the core hypothesis is clearly corroborated on all

three central government issues and the election cycle hypothesis for two of them. Furthermore, the finding of delayed effects with respect to the health and cultural issues could hint at more constraints on the actions of national policymakers when local government involvement is relatively high. This finding, however, could not be replicated with respect to primary education. On the other hand, the statistically significant interaction effect found for the latter issue suggests that the two central motivational factors need to operate in concert in order to show through on primary education spending. Finally, the two intermediate issues in the middle columns of the table do not fully fit into the expected pattern. In particular the very strong effects found in the study of the pollution control issue stands out.

With only seven issues, we do not have sufficient variation on the local autonomy variable to completely rule out alternative explanations and fully separate out individual effects. Based on the evidence at hand, however, it seems that degree of local government involvement modifies the spending impacts of election cycles and variations in the macropolitical agenda. In some instances, it seems that local government constraints are only overcome by national policymakers when the motivation from increased macropolitical attention and election cycles are combined, while in other instances the modifying effect shows up as delayed responses. To examine these different dynamics in more details would, however, require additional studies.

Looking at the vertical variation in Table 8.1 to compare across propositions, we note a declining level of empirical support. While the main macropolitical agenda proposition is clearly corroborated by the empirical evidence, the evidence on the propositions involving election cycle effects is more mixed. Hence, based on the sample analyzed in this dissertation, election cycles matter sometime and for some issues, but generally, the macropolitical agenda is the most important determinant. Based on the solid support for the idea of myopic voters in the literature on voting behavior (cf Chapter 3), the relatively weak support for the election cycle hypothesis may imply that national policymakers use a wrong model of the average voter. It might also imply that national policymakers are too risk averse to rely on the decaying memory of voters, and thus they react very quickly to changes in the macropolitical agenda whether the next election is near or not. Data utilized in this dissertation does not inform us about these underlying reasons, hence inviting further research on the election cycle questions. Finally, we take a brief look at the findings of the panel data analysis shown in the right column in Table 8.1. This column reveals that the panel data regression in Chapter 7 provided empirical support for the propositions examined in the upper three rows of Table 8.1. In addition, the increased sample size used in the panel

data analysis also allowed for a more formal examination of the interaction effects introduced in the lower part of Table 8.1. First, by interacting the parliamentary debate variable with a variable measuring degree of public money spent at the local governmental level, the panel data analysis provided some support for the idea that local government involvement dampens the spending effect of the macropolitical agenda. The brackets ( ) in Table 8.1 indicate that the precise effect was difficult to estimate due to collinearity between the parliamentary debate variable and the variable measuring degree of local government involvement. Second, it has been speculated that opposition from labor or special interests with intensive preferences against spending cuts in the policy domains they cover might make it more costly for politicians to comply with voter preferences compared to those policy domains where both voters and special interests support increased public spending. Consequently, it could be that popular spending effects from increased parliamentary debate are less strong for the three unpopular issues where the median voter prefers decreased spending, than for the four popular issues where the median voter prefers spending increases. In line with this expectation, the panel data analysis showed that on average, a higher share of parliamentary debate leads to more popular spending decisions, but the effect is lower for unpopular issues than for popular ones. However, owing to the aforementioned questions about specific versus general policy preferences and the potential effects of fiscal constraints, identifying the exact cause of this difference between popular and unpopular spending programs would require further studies.

The results of the panel data analysis do not replace or reject the horizontal and vertical variation emphasized above. What the panel data analysis shows is merely that on balance, when averaging effects across the seven issues instead of counting the number of statistically significant coefficients from each time series regression, the sum of empirical observations does seem to support the theoretical model outlined in Chapters 3 and 4.

To complete this discussion of the empirical findings, Chapter 7 – in addition to the delayed effects and the panel data analysis already mentioned – consolidated and qualified many of the conclusions shown in Table 8.1. Though the use of annual data precludes a detailed examination of the causal relationship, the causality analyses in Chapter 7 nevertheless seem to support the claim that the public spending changes observed in this study are primarily an effect of changes in the macropolitical agenda, not a cause. Furthermore, the additional analyses in Chapter 7 did not indicate that the concept of public saliency – central to many earlier opinion policy studies – is the driving force behind the results generated in Chapters 5 and 6. This is a

finding that points to the relevance and contribution of the explanatory variable that dominates this study, but also a finding that invites further research into the relationship between these variables. Finally, inspired by Wlezien's studies of the Thermostat Model it was argued in Chapter 7 that unobserved marginal changes in public spending attitudes do not seem to be an adequate alternative explanation of the findings reported in Chapters 5 and 6.

In sum the study of the development in public spending on seven Danish policy issues across almost a quarter of a century clearly suggests that the more national elected politicians talk about a given issue on the floor of the national parliament, the more likely that public spending changes respond to the spending preferences expressed by a majority of the people. Hence, this dissertation has identified the macropolitical venue as an important venue of responsiveness to public opinion. Consistent with the findings in other opinion policy studies, public policy may often deviate from the preferences of a majority of the electorate, but mobilization and debate in the macropolitical venue generally narrow this gap between the public and the government policies. This is a strong result and it holds across popular issues, defined as issues where a majority of the public prefers increased public spending, and unpopular issues, defined as issues where a majority of the public prefers decreased public spending.

### To what extent can the findings be generalized?

The question about generalization of the empirical findings beyond the present study can be approached from various angles. In this section, the subject is divided into three separate questions. First, I reflect on the relevance to policy issues other than the seven scrutinized above. Second, I consider the relevance to other countries and, third, it is discussed whether the findings may be special to the period from 1980 to 2003 studied in this dissertation.

With respect to the relevance to other policy issues, please recall from Chapter 4 that the seven issues studied in this dissertation were not selected as particularly likely or least likely test cases. Together they represent a fairly broad sample of different policy areas, which increases the likelihood that the results can be generalized to other policy domains. Generalization to other issues may, however, be constrained by the fact that some basic assumptions underlying the model must be sufficiently met. First, a median voter opinion must be identifiable to the researcher and to the macropolitical actors. New policy issues occasionally emerge where a firm public opinion has yet to crystallize, just as it may be difficult to identify a clear median opinion on existing, but very remote and distant policy issues. If that is the case, the model developed in this dissertation does not have much to offer. On the other hand, and as noted in Chapter 3, according to newer public opinion

studies we are not really short of issues where a measurable and rather firm median opinion exists. The latter suggests that the model is relevant to a broader set of issues than the seven studied here.

Second, the logic of the model appears to require that a gap exists between actual policy and the median voter policy preferences. If this is not so variation in the macropolitical agenda may not matter in a systematic way and a different logic might apply than the one claimed in the model developed in this dissertation. Several international and Danish empirical studies propose the existence of such a gap (Kristensen 1982; Togeby 2004; Wlezien 2004; Soroka & Wlezien 2004; 2005). However, to evaluate the impact and potential restrictions of this assumption requires further empirical studies.

Third, the basic theoretical model implies that macropolitical actors have the power to adjust public policies if they wish. As already uncovered, this assumption can easily be endogenized by building friction and constraints into the model. But the model still requires that the macropolitical actors have at least a minimum of formal policy making authority. Many issues easily meet this minimum requirement, but programs involving, for instance, mandatory spending on welfare schemes or a significant degree of international or supranational decision-making might not comply with this assumption, at least not in the short term, thus reducing the relevance of the model in such cases.

In addition to these potential limits to generalization, it might of course be necessary to apply alternative measures in order to capture the dynamics on issues other than the ones analyzed here. Authorized public spending, for instance, is in many cases a valid proxy for decisional output, but regulation of human behavior and more symbolic policies like human rights declarations are two basic forms of policies that are often largely unaffected by public spending. There is no reason to believe that the model does not apply to such issues in general, but it may be necessary to use other proxies to uncover the hypothesized relationships. Hence, unlike the potential obstacles mentioned above, the latter is primarily a matter of measurement validity.

Turning to the model's relevance to political systems other than the Danish political regime, the assumption about a two-party/-bloc system seems particularly important to address. The theoretical model developed in this dissertation is based on the premise of a political system where office and vote seeking strategies coincide in the sense that the best way to gain office is to win as many votes as possible. A close approximation of such a system is found in the classic two-party Westminster system. As argued in Chapter 4, although party competition in the Danish national parliament has more consensual traits than a pure two-party Westminster system, achieving government status in Denmark has become rather closely associated with vote

seeking because of the two-bloc logic characterizing Danish party competition. Consequently, the model should be applicable to Danish national politics, but perhaps even more so to a range of other Scandinavian and Anglo-Saxon countries whose attributes more closely resemble a two-party competition system.

The more fundamental assumption about a macropolitical venue and an almost infinite range of subsystems is, on the one hand, a crude simplification of a complex reality. On the other hand, however, such a division of labor is such a characteristic phenomenon in all modern political regimes that it likely applies almost universally. The macropolitical venue may materialize differently in different political systems, such as parliamentary systems versus presidential systems, but then we get back to the question about variation in empirical measurements, which is substantively different from that about theoretical generalization. In a similar vein, and as noted in Chapter 3, the system of macropolitical and subsystem venues most likely replicates itself at local, state, and national levels of policymaking. The present study has focused on the national level of policymaking, but based on a corresponding identification of macropolitical actors within local policymaking systems, the basic model should be applicable to many of these processes too. And perhaps, in light of the empirical outcome of the present study, such applications would improve the model's explanatory power with respect to issues characterized by a substantial degree of local government involvement.

The choice of time period for the analyses was solely dictated by the availability of reliable and consistent time series data, and hence it was not possible to go back and test the model before 1980. Still, though, it may be worth a brief reflection over the extent to which the results could be broadened to cover the period before 1980 and, perhaps of greater interest, to what extent they are relevant in the years to come. A few broader structural developments hint at an answer to these questions. In particular, some fundamental trends in the electorate, in the functioning of political parties, and in the media structure seem relevant.

First, as touched upon in Chapter 3, research has demonstrated that class-voting has declined over the last decades, and hence that an increasing share of the electorate no longer vote out of habit or based on social inclinations (Borre 2001; Dalton 2002; Andersen & Borre 2003). The phenomenon of class voting has not disappeared (see Evans 1999), but in several countries the number of swing voters has increased substantially, which means that the mobility in aggregate and individual electoral behavior has increased (Carmines 1991, p. 65).

Second, the structure and function of the mass media have changed considerably. Already in the 1960s, Epstein (1967) hypothesized that technological changes in mass communication would affect the characteristics of political parties. More particularly, he noted that European parties as a consequence would become more like American political parties characterized by: "…lesser class-consciousness, the absence of strong opposition to the existing social structure, and [a] more purely electoral (as opposed to membership) organization" (Epstein 1967, p. 356). To what degree this transformation has taken place is still debatable, but recent studies give credence to the hypothesized trend (Bille 1997; Wattenberg & Dalton 2000). Mass media today fulfills many of the functions formerly conducted by political parties (Dalton 2002), and in Denmark, the old party-dominated newspaper system has been replaced by one of independent newspapers (Lund 2002, p. 15-17).

Together these structural trends point to an increased importance of issue-based electoral competition for votes among the political parties. Furthermore, many new issues such as environment, law and order, and immigration have come to political prominence in many countries over the last decades, and hence the total number of issues to compete over is larger.

What these trends seem to suggest is that the model of the voters and that of electoral competition in the macropolitical venue proposed in this dissertation approach reality more closely over time. Thus, while the empirical findings of this dissertation may not easily be generalized to earlier periods, their relevance and applicability seem to be on the increase. Since many of the structural trends have occurred more or less in parallel across the Western countries, there is little reason why this prediction should not apply across these political regimes.

The model of responsiveness outlined in this dissertation is rather parsimonious. Such a model certainly misses some of the more fine-grained and contextual variation in responsiveness, variation that may be better accounted for by applying more complex theories and more detailed in-depth analyses. The benefit of parsimony and quantitative studies, on the other hand, is the detection of general and systematic patterns of apparent relevance across a wide range of issues and political regimes. The fewer assumptions, the more likely that the model applies to a given issue or a given political system, and as argued in this section, the relevance of the theoretical model of responsiveness proposed above seems to extend in both time and space.

### Some implications for the policy literature

The role of public opinion in public policymaking is a core question in political science and nearly all policy studies therefore have something to say

about it, whether directly or indirectly, by demonstrating the power of alternative policy determinants. This research question, and not least the way it was approached in this dissertation, is more relevant to some traditions than to others, however. In the following, I focus on the opinion policy literature, the agenda setting literature, and literature advocating a supply-based approach to public policymaking. It is not possible to give a comprehensive and fully balanced discussion of all aspects of these approaches within a single chapter. The primary aim is instead to highlight a few central implications and some avenues for future research that spring from the present study.

*Implications for the opinion policy literature*

Earlier research within the so-called opinion policy literature was already introduced and discussed in Chapter 2, which also pointed to some potential contributions of the model developed and tested in the subsequent chapters. Knowing the outcome of the empirical analysis, we can now sum up the main implications for this strand of research.

First, contrary to the dominant approach in earlier opinion policy studies, the present study has started from a contingency perspective and pointed to a set of conditions that affect the impact of public opinion on public policy. The empirical results of this endeavor clearly show the value of such a conditional approach. In that respect, the present study lends credence to what seems to be a growing awareness within the opinion policy literature of the conditional nature of this relationship (cf Manza & Cook 2002b; Burstein 2003).

Second, and more specifically, this dissertation has focused attention on a set of conditions borrowed from adjacent literature on agenda setting and political business cycles. The concept of political agendas as such probably does not contribute very much to our understanding of the opinion policy relationship. However, when we combine it with a model of the policymaking process, such as the venue model developed by Baumgartner and Jones (1993) and a more explicit model of the voters and the preferences of the policymakers, the agenda concept actually appears to add some important explanatory leverage. It can also as a concept be further developed and made more sophisticated in future studies of the impact of public opinion on public policy. One potential improvement, as touched upon in the discussion of the agenda setting literature below, might be to use a more comprehensive and fine-grained coding of the macropolitical agenda, including, for instance, a coding of how a given issue was defined and discussed in parliament and not just how much it was debated.

The election cycle condition was not shown to hold similar explanatory power, but it did engender a sufficiently strong effect to warrant increased awareness of this variable in future opinion policy studies. In particular, we

still need a better understanding of how and when it interacts with the macropolitical agenda.

Third, at a more general level, the explanatory power of the variables mentioned above shows the potential of a more explicit model of the impact of public opinion on public policy. One need not discard the parsimonious flavor that characterized earlier opinion policy studies, but in order to improve the explanatory power, focus may have to be shifted from the preoccupation with voter attributes to an increased focus on the policymaking process, the institutional organization of this process, and the preferences of central policymakers. For instance, just as all discrepancies between public opinions and public policies are not punished by the voters, all policymakers are most likely neither equally nor constantly concerned about potential voter reactions. Identifying the conditions that facilitate either high or low degrees of responsiveness seems to require models of responsiveness that are somewhat more elaborate than those used in many earlier opinion policy studies. The model adapted and elaborated in this dissertation could provide a framework for such further model development.

*Implications for the agenda setting literature*

Based on a belief that mobilization and changes in the political agenda are essential democratic tools that counterbalance the power of a pressure system dominated by special interests, Schattschneider in his seminal essay from 1960 asked the question: "what makes things happen?" Motivated by a similar belief, the later literature on agenda setting is awash with studies of how and why policy agendas change. As noted in Chapter 3, this research question is indeed important, but from a policy perspective, it acquires even more importance if it can be shown that the phenomenon of agenda change actually has a systematic impact on public policy. Or more to the point, if it can be established that Schattschneider's underlying belief about increased responsiveness in times of increased mobilization and attention is correct.

To the best of my knowledge, this basic assumption made by Schattschneider and subsequent agenda setting scholars has never been subjected to systematic empirical scrutiny. Hence, while the literature has pointed to a wide range of reasons why political agendas may change, the original reason for studying this phenomenon has largely been ignored. It is from this perspective that this dissertation contributes to the literature on agenda setting. By demonstrating empirically that changes in the macropolitical agenda result not only in policy changes, but in policy changes that move in the direction preferred by a majority of the citizens, the study has lent credence and validity to the basic motivation behind studying changes in political agendas. The variation across issues and the potential limits to generalization discussed

above obviously still apply, but even with that in mind, the present study has established a clear empirical link between agenda dynamics and responsiveness to the preferences of the general public. It has also pointed to various conditions under which this link appears to be particularly strong. Consequently, this study has shown that agenda dynamics matter for democratic responsiveness, thereby revitalizing the original question about what makes things happen and what causes agenda changes.

In addition, the present study also points to a few secondary implications of relevance to the agenda setting literature. First, while much empirical agenda setting research appeared to have been attracted to rather spectacular cases of public policymaking where even minor disturbances generate major changes in the political agenda,[1] the present study has sought to integrate the agenda concept in a more linear and predicable model of public policymaking. The demonstration of abrupt and major changes in political agendas has certainly provided an important correction to the incremental picture painted by many scholars of public policy making. However, in order to improve its general relevance to our understanding of public policymaking, the agenda concept needs to be combined with a more elaborate model of the institutional context of public policymaking and the preferences of the relevant decision-makers. Building on previous studies (Baumgartner & Jones 1993), the present study represents another step in that direction. While the causes of agenda change may be complex and the timing and magnitude of change more or less unpredictable, what happens ones the agenda changes actually seems rather predictable when we know the institutional context and the preferences of the relevant policymakers. Based on the present study, for instance, we can fairly confidently predict that public policy measured as public spending is adjusted in the direction preferred by a majority of the voters when the given issue's share of the macropolitical agenda increases. This result corroborates the potential of the agenda-concept in studies of more "normal politics", as opposed to the spectacular policy processes analyzed in so many earlier applications.

Last, but not least, the agenda setting literature also points to some fruitful ways to improve the kind of analysis conducted in this dissertation. In particular, the concept of issue definition, which has been a central concern in many studies of agenda setting,[2] may extend and qualify the results of this dissertation. Issue definition implies that agenda setting is not just about changes in the saliency of a given issue, but also about how that issue is discussed and defined in the political discourse. For instance, as demonstrated in Baumgartner & Jones' (1993, Chapter 4) longitudinal analysis of regulation of nuclear power production in the US, it mattered whether nuclear

power was discussed as an issue of cheap energy or as one of potential reactor accidents or waste disposal. Hence, by counting only how much the seven issues were debated in parliament within a given year, we may have ignored important variation in how the issues were debated. It could be, for instance, that defense at times has been defined and discussed in terms that would lead to a popular demand for more spending in the specific case despite the general public preference for decreased defense spending.

One can draw different conclusions from this question of issue definition. From one perspective, the relatively strong effects we uncovered when using the parsimonious measure of the macropolitical agenda may be the most striking finding, indicating that a more comprehensive measure of the macropolitical agenda would have added but little explanatory power in this case. Hence, based on this reading of the results, the definition of the seven issues has remained rather constant over time. From another perspective, however, a more comprehensive agenda measure sensitive to the tone of the debate in parliament might provide us with a more precise estimate of the agenda effects. And perhaps more importantly, it might improve the model's applicability to other issues where aggregated public opinion and tone of the debate have fluctuated more over time. Hence, from the latter perspective, if it is possible to apply a more comprehensive and fine-grained coding of the macropolitical agenda without violating the basic ambition of parsimonious explanations, this seems to represent an important avenue for future model developments.

*Implications for supply based public spending theories*

An important theoretical and empirical ambition behind the present study was to develop and test a conditional explanation of responsiveness that acknowledges electoral competition models as well as models of subsystem politics. While the former type of models emphasizes the demand side of the political equation, the latter tends to emphasize the power and preferences of supply side actors such as public officials, bureaucrats, or others involved in the supply and provision of public services.

As noted in Chapter 3, the literature on subsystem politics is vast and quite heterogeneous. In this section, I confine the discussion to a couple of intellectually related supply side theories that represent perspectives also found within the broader subsystem literature. Common to the two theories is an explicit focus on public spending effects, which means that they may readily be discussed in light of the spending outcomes found in the empirical analysis above. The first one is the theory by Niskanen (1971; 1994), and the second one is a theory inspired by the ideas of Olson (1965) and developed by Kristensen (1980; 1987).

Niskanen's initial assumption is that public officials are not motivated by a desire to serve the public or the politicians. Instead, officials are mainly concerned about their own private ends, and the best way to serve these ends is to maximize the budget of the official's bureau. Combined with the monopoly status of the bureau, a central assumption about a very passive and friendly political sponsor, and a set of more particular assumptions, Niskanen deduces a series of propositions about public service provision and public expenditures. In the present context, the most relevant hypothesis is that public expenditures are determined by the preferences of public officials, which differ significantly from those of the median voter (Niskanen 1994, p. 279).

Over the years, various aspects of Niskanen's theory have been subject to scholarly critique (e.g. Migué & Bélanger 1974; Miller & Moe 1983; Dunleavy 1991). Most relevant with respect to this dissertation is the line of criticism in which the role of the politicians was questioned. In Niskanen's model, it is the bureaucrat who has the upper hand in the interaction with the politician, but for a number of reasons, most clearly formulated by Miller and Moe (1983), this assumption seems to be deficient. In Niskanen's model, policy decisions are made in committees whose demand for bureau services is assumed to be significantly higher than the demand of the legislature as a whole. According to Niskanen, the crucial advantage of the monopoly suppliers, the public officials, is that they know the true cost of production, and over time, they get to know the demand curve of the politicians in the sponsor committee. However, as argued by Miller and Moe (1983, p. 309), this model misrepresents the strategic power and incentives of the politicians and ignores that

> …committee demand is prone to change over time with such factors as changes in membership, constituency demands, and issue salience; to the extent this is true, past decisions are less useful as indicators of present committee demand…

Hence, committee members can be motivated to comply with other claimants than public officials, and the context provided by the larger legislative arena may at times intervene more or less directly in public expenditure decisions. As suggested by Miller and Moe, the mediating variable might be the given issue's political salience in the broader parliamentary arena.

What the present study offers to this perspective is first and foremost empirical leverage to the criticism of Niskanen's theory. According to the empirical analysis, public spending on a given issue depends on the level of debate/saliency in the macropolitical venue identified as the floor of the parliament. Apparently, public expenditures to a given policy area are not always determined by electoral strategies in the broader legislature, but neither are

they always determined by bureaucratic preferences. In areas where a majority of the voters prefers increased spending, public expenditures actually increase at a higher level, when the salience in parliament is high. And perhaps even more devastating to Niskanen's theory, in areas where a majority of the voters prefers decreased public spending, bureaucrats and public officials seem to suffer in times of increased debate on the floor of the parliament.

Agreeing with Miller and Moe's criticism, Kristensen (1987, p. 43) has developed an expenditure theory, which he claims provides a better integration of the politicians. Similar to Niskanen, he expects that public officials, regardless of the policy content, always ask for more funding in relation to the programs they are working with, and likewise resist cutbacks in these programs (Kristensen 1980, p. 255). Furthermore, also in accordance with Niskanen, the core proposition of this theory is sustained and universal growth in public expenditures, a growth that is expected to exceed the demand of the median voter.

The logic underlying this theory differs from Niskanen's, however, and Kristensen tries to provide a more elaborate answer as to why politicians comply with demands for higher public expenditures. Rather than because of its function as a monopoly supplier, the impact of the bureaucracy is caused by the general institutional characteristics of the public sector and the policy making system (Kristensen 1987, p. 44). According to Kristensen (1980, p. 256), public spending decisions are asymmetrical by nature in the sense that proponents of public spending always have an institutionally based advantage over opponents of public spending. Owing to the atomized decision making structure and the almost invisible tax structure, benefits of public expenditure cuts are widely distributed over a large group of citizens, whereas the benefits of public expenditure increases are concentrated on a narrow group of actors. Hence, a professional bureaucratic complex is expected to develop around most public service programs, including public officials, experts, and clients who have a common interest in higher program spending.

While Kristensen's (1987) own empirical studies of the 1970s seemed to support his theoretical model, readings of later applications of Kristensen's theory generally call the scope of the model into question. In several ways, Danish expenditure policy from the 1980s and onwards has developed in directions opposite to what the theory predicted (cf Damgaard 1987; Christiansen 1990; Green-Pedersen 1999). One major problem seems to be that the winners in the public expenditure game are often found outside the professional-bureaucratic complex.

Compared to Niskanen's theory, the theory developed by Kristensen is less 'economic' and more 'political', but in light of the model developed and tested in this dissertation, it is not sufficiently political to escape the aforementioned problem with Niskanen's theory. Many of Kristensen' s (1987) ideas more or less implicitly rest on arguments about the distribution of visibility or saliency in the policy making system and in the general public, for example the argument related to the atomized decision making structure and the invisibility of the cost side of the equation. However, I would argue that a central problem is that he did not integrate these ideas in a more explicit model of the politicians, their motives and strategies, and the shifting contexts they face. Sometimes, for various reasons, the benefits of privileged groups may become visible and subject to debate in a broader public arena, and when that happens, politicians outside the expenditure coalition may sense a potential change in electoral gains or costs and become active and very attentive to that issue. More generally, politics is not always determined in the corporate or subsystem arena, where the bureaucrats have a privileged position, but sometimes also in the larger parliamentary arena, where politicians apparently pay more attention to claimants outside the bureaucratic complex. And in contrast to the modeling in Kristensen's theory, the above analysis showed that these processes are dynamic and not constant, and hence not determined solely by the institutional characteristics of the policymaking system.

Therefore, while the models of electoral competition – which are integral to many of the opinion policy studies discussed above – underemphasize the role of bureaucrats and other special interests, the supply side models tend to underemphasize the role of elected politicians. By focusing on the bureaucrats instead of the politicians, scholars fail to see the bureaucrats as just one aspect of the politicians' environment. Public officials not only compete for funding with other public officials or special interests, but sometimes also with voters and the electorate as such.

The next and final part of this chapter is devoted to a reflection over a set of normative questions about the impact of public opinion on public policy in light of the outcome of the empirical study reported in this dissertation.

## Democratic responsiveness?

Democracy can be evaluated based on a long list of different criteria (see e.g. Berelson 1952; Dahl 1971; Lijphart 1984), many of which the present study does not shed much light on. Nevertheless, the connection between the policy preferences expressed by citizens, on the one hand, and what their government does, on the other, is a central concern in much political theory of democracy. Therefore, I believe this dissertation contributes to our understanding of the functioning of our democracy. I also believe that it directs our

attention to a set of important questions that should be addressed in order to further improve the democratic qualities of our political system. To appraise the democratic implications of the empirical findings, it may be constructive to highlight a few pros and cons about responsiveness to citizens' policy preferences.

The main objection against responsive rule comes from scholars who question citizens' ability to know their own interest or the public good. According to Pennock (1952, p. 971), for instance, there is no such thing as a majority will on many questions because of widespread ignorance and indifference. If popular demand represents nothing more than a passing whim, the argument goes, then the public would be better off with a political regime where elites compete for voters' consent without necessarily responding to their policy preferences (cf Lippman 1922 [1997]; Schumpeter 1943 [1994]). With support from Converse's (1964) studies of unstable individual survey responses and weak ideological structures, this skeptical view of majoritarian democracy has become quite influential (cf Page & Shapiro 1992, p. 387).

However, more recent empirical studies have challenged this picture of irrational and fluctuating public policy preferences (see Page & Shapiro 1992; Wlezien 1996; 2004; Stimson et al. 1995; Erikson et al. 2002). In particular, Page & Shapiro's (1992) longitudinal studies of more than 10,000 opinion surveys call into question the widespread mistrust in the public's policy preferences. In opposition to criticisms of the idea of majoritarian democracy, they conclude (ibid. p. 384) that collective responses make sense, that the aggregated public draws fine distinctions among different policies, and that they form meaningful patterns consistent with a set of underlying beliefs and values. A similar, although perhaps slightly more nuanced, conclusion is found in Togeby's (2004) study of aggregated public opinions in Denmark.

What we have found in this study is responsiveness to collective and rather firm preferences about how to prioritize government resources at a quite aggregated level of public spending. Since these expressed majority preferences seem to represent clear and settled popular demands, it could be argued that responsiveness at this level of analysis is both feasible and desirable. Concerning more specific and disaggregated resource allocation across and within public spending programs, the desirable aspects of responsive rule may be somewhat more questionable. Because of the complexity involved in the provision of many public goods and the limited decision-making time available to ordinary citizens, few would probably object to Warren's (1996, p. 49) claim that "we want safe airplanes and food, not the chance to participate in meat inspection and airline safety". Applied to the topics studied in this dissertation, while it may seem both desirable and

feasible that the public has influence on important resource priorities among broader spending programs, it should probably not decide exactly how the money is spent on education or health, for instance. The latter may be better decided by expert deliberation within more or less open policy communities.

The responsiveness found in this dissertation also actualizes a set of classic concerns about the quality of collective citizen opinions, however. For instance, how do we ensure not only policy responsiveness to voter opinions, but also voter responsiveness to changed policies? The design of the present study has utilized the stability in voter preferences on the seven issues, but from a normative perspective, the public may seem too unresponsive to policy changes to fulfill their democratic obligations. As proposed by Wlezien's Thermostat Model, public spending preferences do react to spending changes, in particular with respect to pollution control and aid to developing countries it appears (cf Table 4.1 and Figure 4.1). However, when it comes to health or education, for instance, the demand for public spending is stable or slightly increasing despite a clear upward trend in actual public spending on these issues during the 1990s (ibid.).

One reason for this lack of responsiveness to changed policies may stem from a lack of public information about public policies. In addition, there may be an element of so-called fiscal illusion in what we observe here. Fiscal illusion denotes a situation where people want something for nothing, which often means that they demand increased public spending but are unwilling to pay for it via increased taxes. In the empirical literature, it has been disputed to what extent fiscal illusion is a general and important phenomenon (May 1982; Sørensen 1992; Winter & Mouritzen 2001; Blom-Hansen 2005). Particularly relevant in this context is the study by Winter & Mouritzen (2001) that clearly demonstrates how adequate information about costs reduces the problem of fiscal illusion. Consequently, a chief focus for improvement, as also concluded by Page & Shapiro (1992, p. 398), should be the political information system. If we are able to ensure that collective public preferences are formed based on more balanced and useful information about public policies, then we may, in light of the present study, also be able to improve public policy itself.

This study has demonstrated that the impact of public spending attitudes on public policies is contingent upon the variation in the macropolitical agenda. But how do we then ensure that important issues are put on the macropolitical agenda on a regular basis? To what extent are political leaders, entrepreneurs, or special interest groups able to control this agenda? Judged by the agenda setting literature, some pluralistic openness and fairness should be expected, but as shown in Larsen & Andersen's (2004) study of several

major and unpopular political reforms in Denmark, a small group of political leaders is sometimes capable of initiating and implementing unpopular reforms without any public debate in parliament or other public forums. That unpopular reforms are decided in the dark is consistent with the findings in this dissertation, but it clearly invites further studies of how to ensure increased publicity and public debates over such political decisions.

Over the years, the general level of parliamentary debate has increased markedly (see Green-Pedersen 2005a). In light of the present study, we therefore might expect that responsive rule is stronger today than it was earlier. Part of this trend is fueled by changes in the way political parties compete, but the trend is also a consequence of formal developments where ordinary MPs have been allocated increasing institutional resources to politicize and question government policies in parliament (see Damgaard 2003, p. 128), resources that seem warranted in view of the findings in this dissertation.

Furthermore, the outcome of this study invites more research on the relationship between saliency of voter opinions and parliamentary debate. While citizens may hold opinions on a staggering number of issues, these opinions are probably not of equal salience to the citizens and, all else being equal, responsiveness to salient majority opinions may be preferable to responsiveness to non-salient majority opinions (cf Pennock 1952, p. 791). In that regard, the study in Chapter 7 represents only a very preliminary investigation of this question.

Unlike the first generation opinion policy studies introduced in Chapter 2, this dissertation does not conclude with an overall estimate of, say, 40 or 60 percent correspondence between public opinion and public policy. Such estimates are not only often biased in either an upward or downward direction (cf Chapter 2); they are also of little prospective value if the aim is to improve state of things. From that perspective, the important outcome of this dissertation is the identification of a central condition of responsiveness to public opinions, and this condition is the agenda of the macropolitical venue. Hence, whether we seek to improve our knowledge of the impact of public opinion on public policies or to improve the democratic qualities of such responsiveness, one rich place to focus our efforts seems to be the agenda of the macropolitical venue.

## Notes

1. Characteristic examples are found in Schattschneider (1960); Walker (1977); Cobb & Elder (1983). Also earlier Danish agenda setting studies report case-studies of rather spectacular policymaking processes (see Riiskjær 1988; Andersen & Hansen 1991.
2. See Riker (1986); Stone (1989); Schneider & Ingram (1993); Baumgartner & Jones (1993); Rochefort & Cobb (1994); Birkland (1997).

# Appendix 1

# Construction of variables

## Public spending

In this section, I give a detailed description of the setup of the public spending dataset from Statistics Denmark as well as an assessment of data reliability.

Every entry in the dataset from Statistics Denmark has a code reporting whether the money is spent by the central government, the municipalities, the regional counties, or by social funds. Hence, the first step is to select the relevant spending categories among these four entities. When analyzing on *general government spending*, I pool these four sector categories in order to arrive at an estimate of total public spending in a given year. When analyzing *central government spending* only, I exclude spending registered under municipalities, counties, and social funds.

In addition, every entry in the dataset from Statistics Denmark is accompanied by a national account code intended to reflect how societal activities are affected by the given spending decision. For instance, money may be spent on wages, buildings, production facilities, etc. There are almost 900 such different national account codes in the raw data material grouped under a few main categories, some of them reported in Table A1.1 below. Using these main categories, Table A1.1 reports how the spending measures used in this dissertation were constructed. In this second stage of the identification of general and central government spending, the main difference between the two measures is the correction for intergovernmental payments in relation to general government spending. Failure to correct for these transactions would imply that a relatively large share of public spending would be counted twice (cf Ministry of Finance 1994, p. 327).

Table A1.1. Categorization of general and central government spending

| General government spending | Central government spending |
|---|---|
| + Public consumption | + Public consumption |
| – Sale of goods and services | – Sale of goods and services |
| + Public transfers | + Public transfers |
| – Intergovernmental payments | |
| = Current general spending | = Current central spending |
| + Capital accumulation | + Capital accumulation |
| + Capital transfers | + Capital transfers |
| – Intergovernmental payments | |
| = General government spending | = Central government spending |

The next stage is to identify the programmatic spending categories, that is, how much money is spent in a given year on, for instance, education, health, etc. The original dataset from Statistics Denmark consists of 14 such main programmatic categories constructed from 33 sub-categories, which are again constructed using 186 basic functional categories. Statistics Denmark has published data from 1971 and onwards down to the level of the 33 sub-categories.

The time series on public spending utilized in this dissertation were constructed from these functional categories as described in Table A1.2 below. Some of them are exact replications of the main categories from Statistics Denmark, while others are modified based on information from the underlying sub-categories. The latter is also the reason why it has been necessary to construct my own dataset from raw data material provided by Statistics Denmark rather than just relying on their published tables. For example, contrary to these publications, my dataset does not include civil defense in the defense category because this organization performs many civilian tasks not closely related to national defense. Another example is "pollution control" or "aid to developing countries," where the two main categories published by Statistics Denmark, "sanitary services" and "foreign affairs", are too broad to capture the programmatic content that the spending is supposed to reflect in this study.

Table A1.2 describes the programmatic content of the seven time series used in this dissertation. In the column to the right, I report the corresponding categories in the "control" dataset from the Ministry of Finance, which is used for the reliability analyses in Table A1.5 further below.

Table A1.2. Functional coding of public spending

| Policy domain | Name and description of the subcategories in the Statistics Denmark dataset (codes in dataset[a]) | Corresponding categories in the Ministry of Finance dataset[b] |
|---|---|---|
| 1. Law & order | Administration, police, prisons, justice (1310-1340; 1390) | "Ministry of Justice" (§11) |
| 2. Environmental protection | Pollution control (6360) | "Environmental protection" (§23.2) |
| 3. Primary education | Teaching, educational support, administration, etc.(3200-3299; 3610) | "Primary education" (§20.2) |
| 4. Health | Administration, hospitals, primary health care, etc. (3740; 4100-4300) | "Ministry of the Interior and Health" (§16)<br>"Hospitals" (§16.5) |
| 4.1 Hospitals | Hospitals (4200) | "Primary health service" (§16.4) |
| 4.2 Primary health | Primary health (4300) | |
| 5. Defense | Defense (3720; 2100) | "Ministry of Defense" (§12) |
| 6. Cultural purposes | Administration, cultural services, etc. (3772; 7200-7299) | "Ministry of Culture" (§21) |
| 7. Aid to developing countries | Aid to development of third world countries and Eastern European countries (1210-1211) | "Aid to developing countries" (§06.3) |

Note: a: These codes refer to the lowest level of programmatic codes in the dataset from Statistics Denmark. b: These codes refer to the paragraphs in the 2004 version of the national Budget published by the Ministry of Finances.

Sources: Based on information provided by Statistics Denmark and the Danish Ministry of Finance.

The next and final stage in the setup of the Statistics Denmark spending data is to adjust for inflation in order to increase comparability over time. First, I have divided the spending data into current and capital spending, respectively, and then transformed both spending measures to 2003-prices based on two deflators provided by Statistics Denmark. The annual values of the two deflators are reported in Table A1.3.

Table A.1.3. Deflators from Statistics Denmark

| Fiscal year | Public consumption | Public investments |
|---|---|---|
| 1979 | 2.755 | 2.066 |
| 1980 | 2.488 | 1.898 |
| 1981 | 2.232 | 1.667 |
| 1982 | 1.988 | 1.513 |
| 1983 | 1.848 | 1.404 |
| 1984 | 1.770 | 1.326 |
| 1985 | 1.706 | 1.282 |
| 1986 | 1.667 | 1.250 |
| 1987 | 1.553 | 1.203 |
| 1988 | 1.468 | 1.157 |
| 1989 | 1.399 | 1.109 |
| 1990 | 1.355 | 1.071 |
| 1991 | 1.304 | 1.067 |
| 1992 | 1.264 | 1.080 |
| 1993 | 1.253 | 1.059 |
| 1994 | 1.241 | 1.029 |
| 1995 | 1.220 | 1.042 |
| 1996 | 1.195 | 1.015 |
| 1997 | 1.163 | 1.015 |
| 1998 | 1.135 | 1.019 |
| 1999 | 1.114 | 1.034 |
| 2000 | 1.085 | 1.027 |
| 2001 | 1.049 | 1.001 |
| 2002 | 1.027 | 0.988 |
| 2003 | 1.000 | 1.000 |

Source: Provided by Peter Rørbæk Jensen, Statistics Denmark.

*Assessment of reliability*

In chapter 4, I discussed the validity of public spending as a policy indicator in general, and the potential strength and weaknesses involved in using Statistics Denmark's spending data in particular. In this section, I take a closer look at potential measurement errors in this choice of indicator. Measurement errors are unavoidable in most empirical research and this dictum is also true with respect to public budgets, including the data collected by Statistics Denmark. Although the international account system on which the spending dataset is based is deliberately constructed to enhance precision and comparability over time and across sectors, adjustments to the system have been made since it was introduced several decades ago and the specific coding procedure of course involves an element of discretion.

It is not possible to get a full and precise estimation of measurement errors in this case, partly because there is neither a non-controversial definition of,

for example, public sector activities in general (see Kristensen 1987) nor functional program definitions such as education-related activities in particular (see Hogwood 2003). However, it is possible to say more than this and in this section I describe the steps taken to reduce the uncertainty about data quality.

First, by eliminating observations from 1971 to 1980 we increase comparability between observations over time. Based on personal communication with representatives from Statistics Denmark, the uncertainty of the exact functional coding procedure was significantly higher before 1980.

Second, the Danish national account system follows the systematic procedure formulated by the United Nations. In the period from 1980 to 2003, a major international revision of this procedure was made in 1997 to comply with the guidelines in the new European System of National and Regional Accounts (ESA95)[1]. A closer look at the spending data series used in this dissertation does not indicate any breaks around 1997 and based on communication with representatives from Statistics Denmark, these spending measures may be largely unaffected by the revision. Furthermore, a major revision of the "classifications of the functions of the government" (COFOG) procedure has just been implemented,[2] but the time series used in this dissertation end in 2003 and are all based on the previous classification system, which has been used rather consistently since at least 1980.

Third, errors may be generated in various stages of the data compilation process. The raw data from which the time series are generated were delivered in 32 SAS-files with between 2,000 and 60,000 entries and up to 105 variables in each. These original data files have been broken down and pooled into only 7 STATA-files with 23 observations and one spending variable in each. Somewhere in this specification process, data errors may crop up. One way to evaluate this is to correlate the time series generated from this process with comparable time series published by Statistics Denmark. This is done in Table A1.4 below, where I correlate percentage changes instead of levels to avoid spurious correlation stemming from trending time series. Additional visual plots have shown that, measured in absolute levels, the fit between the corresponding time series is also very close to perfect, probably deviating only because of rounding errors or a slightly different deflation procedure.

As noted above, the programmatic content of some of the time series used in this dissertation is not directly comparable with the time series published by Statistics Denmark and these are hence not included in Table A1.4. However, since the time series are constructed using the same general data syntax as the comparable time series in Table A1.4, the close to perfect fit of the

latter reduces the overall concern about the potential error production at this stage of the data generation process.

Table A.1.4. Correlation between time series published by Statistics Denmark and the time series used in this dissertation (1980-2003)

| Policy domains | Correlation (Pearson's r) |
|---|---|
| 1. Law & order | .972 |
| 2. Pollution control[a] | – |
| 3. Primary education | .978 |
| 4. Health | .996 |
| 4.1 Hospitals | .995 |
| 4.2 Primary health | 1.000 |
| 5. Defense[b] | .9722 |
| 6. Cultural purposes | .961 |
| 7. Aid to developing countries[a] | – |

Note a: These two time series are generated from sub-categories not published by Statistics Denmark and hence there is no comparable time series to correlate them with. Note b: The defense time series from Statistics Denmark includes the national Civil Defense organization. For comparison between the two datasets, I have included civil defense in my defense category only in the analyses reported in this table.

The main impression from Table A1.4 is an almost perfect fit between the time series, indicating only very minor measurement errors in this phase of the data generation process.

Comparing the raw data from Statistics Denmark with the published data from Statistics Denmark does not tell us anything about the quality of data. As noted above, we have reason to expect a relatively high degree of precision in these standardized and double-checked data collection procedures. One way to obtain a more direct estimate of this reliability, however, is to compare with seemingly similar data from another dataset. As discussed in Chapter 4, comparable spending data over time is rare, but the availability of the Ministry of Finance dataset may be useful in this respect. It has the same basic ambition of tracking public spending on various programs over time. Nevertheless, it must be underscored that the definition of public sector activities and the principles regarding the setup of this data are different from the ones guiding the construction of Statistics Denmark data. Furthermore, data from the Ministry of Finance database is net spending, while that from Statistics Denmark is gross spending. Hence, we should not expect fits as good as those in Table A1.4 above. Furthermore, the dataset from the Ministry of Finance covers 1992 to 2003 only, and therefore this comparison does

not inform us about the quality of data between 1980 and 1992. On the other hand, if the two datasets reveal a rather similar record of the development in public spending on those programs that we are concerned with, then this should lead to increased confidence in the spending data from Statistics Denmark.

Table A1.5 reports the result of this exercise. Again I rely on change data instead of absolute levels in order to avoid spurious correlation caused by time trends.

Table A1.5. Correlation between time series from different spending datasets (1992-2003)

| Policy domain[a] | Correlation (Pearson's r) |
|---|---|
| 1. Law & order | .735 |
| 2. Pollution control | .700 |
| 3. Primary education | .305 |
| 4. Health | .813 |
| 4.1. Hospitals | .733 |
| 4.2. Primary health | .656 |
| 5. Defense | .724 |
| 6. Aid to developing countries | .945 |
| 7. Cultural purposes | .728 |

Note a: For a specification of the policy domains, see Table A1.3 above.

The correlations in Table A1.5 are not perfect, but they are strong enough to indicate that the pairs of time series across the two datasets do seem to capture similar spending dynamics. Other than for the education category, they all display a strong positive fit. Especially reassuring is the extraordinarily good fit in the "aid to developing countries" category, which was a category of particular concern owing to the fact that it has been generated from a rather narrow, and hence more fragile, sub-category in the dataset from Statistics Denmark. A similar concern existed with regard to the narrow pollution control category. The fit in this category in Table A1.5 is not as strong as the one for the developing aid category, but it is still strong and positive, signaling that we are on the right track of environment related spending here.

The only major deviant category is spending on primary education. It has not been possible to track down the exact cause of this weak correlation. Further inspections have shown that the level and trends in spending on this category are almost identical across the two datasets, but the time series from Statistics Denmark is more punctuated, which apparently accounts for the misfit reported in Table A1.5 above. One option has been to exclude the

category because of this misfit. However, the fact that the explanatory variables that we know should correlate with spending on primary education, such as the number of children aged 6 to 15 and the number of bilingual children, in fact really do explain the variation in our time series from Statistics Denmark gives us reason to keep this series in the analyses. Also keep in mind that the Ministry of Finance dataset is in all likelihood beset with similar uncertainties as that from Statistics Denmark. It does not serve as some absolute error-free standard. We include it because we do not know the true spending values and in such cases observations involving two cases most often provide significantly more information than observations on only one case. However, the misfit does reduce the degree of certainty in the results of the analysis of the primary spending category.

*Annual length of parliamentary debates*

Data on length of debate in the national Danish parliament, Folketinget, is obtained from a database collected by the project "Party Competition, Agenda setting, and Public Policies in Western Europe", directed by Christoffer Green-Pedersen, University of Aarhus. The full database contains all bills (11,952), parliamentary resolutions (4,418), interpellation debates (1,313), questions to the minister (63,737), questions to a minister during the weekly questioning hour established in 1997 (526), and accounts by ministers (581) in the Danish parliament from October 1953 (beginning of the parliamentary session) to September 2003 (end of parliamentary session). In the database, all of these entries are equipped with a content code based on a scheme originally developed by the American "Policy Agenda Project" (see www.policyagendas.org).

In this dissertation, I use data on bills, parliamentary resolutions, interpellation debates, and accounts by ministers from the session beginning in 1979 to the session ending in 2003. These data were transformed into the seven time series used in the analyses in Chapters 5, 6 and 7 using the following procedure. First, based on the content codebook (see www.ps.au.dk/greenp/Research/Agenda.htm) debates on the seven issues were identified. This categorization is showed in Table A1.6 below.

Second, in the dataset, length of debate was coded by counting the number of columns covered by the debate in the parliamentary records.[3] In 1994, the readings went from columns to pages and hence it has been necessary to multiply by two after 1994 in order to get comparable measures of extent of debate over time.

Table A1.6. Functional coding of parliamentary debates

| Policy domain | Sub-categories[a] | Description |
|---|---|---|
| 1. Law & order | 1200-1207; 1209; 1210-1211; 1299 | Issues related to authorities working with crime, police, prisons, organized crime, abuse of children, narcotics crime, and crime prevention |
| 2. Environmental protection | 407; 700-799; 1902 | All issues related to environmental protection |
| 3. Primary education | 602 | Primary and secondary education[b] |
| 4. Health | 300-399; 1301 | All health related issues including questions about health reforms, health facilities, psychiatry, misuse of tobacco, alcohol, and narcotics, research in health and nutrition politics. |
| 4.1 Hospitals | 322; 327; 333 | Questions about health facilities, hospital waiting lists, psychiatric illness |
| 4.2 Primary health | 302; 321; 323; 326; 335; 336 | Questions about health insurance, regulation of the health area, financial aid to medication and to dental care. |
| 5. Defense | 1600; 1603-1614; 1616-1699 | All issues related to military defense. Issues explicitly related to security politics (1602) are not included |
| 6. Cultural purposes | 607; 609 | Issues related to public libraries and cultural politics. Issues about sports (1526) are not included |
| 7. Aid to developing countries | 1901; 1905 | Questions about third world countries and Danish support to foreign countries in the third world and in Eastern Europe |

Note a: The numbers refer to the codes in the content codebook (see www.ps.au.dk/greenp/Research/Agenda.htm). Note b: This category was recoded by the author because it covers both primary and secondary education.

Third, the database does not include parliamentary debates on the annual finance bill. This is no major problem with respect to the analysis conducted in this dissertation. In fact, it just underscores that the statistical relationship between public spending and parliamentary debates, as revealed in Chapters 5, 6, and 7 is anything but trivial. In addition, debates on the opening address and end-of-session debates were removed from the dataset used in this dissertation. The extent of these debates varies significantly from session to session, and hence may have a disproportionate effect on the estimates of annual parliamentary debates on a given issue. Furthermore, these debates often have a rather wide-ranging content theme, making it difficult to file them under a given content code. Finally, entries where coders have expressed doubt about the content of a given debate were excluded from the dataset used in this study.

Fourth and finally, an issue's share of the total parliamentary debate was calculated by counting the number of columns in the parliamentary records dedicated to that issue, divided by the total number of columns generated by parliamentary debate in that given year.

For further information about the database, see:

Green-Pedersen, Christoffer (2004). *Coding of Parliamentary Activities in Denmark, 1952-2003. Data Rapport*. University of Aarhus (unpublished paper).

Green-Pedersen, Christoffer (2005b). The Political Agenda in Denmark: Measurement and trends since 1953. Research note. University of Aarhus (unpublished paper).

www.ps.au.dk/greenp/Research/Agenda.htm

*Public spending attitudes*

Data on public spending attitudes is collected from various sources, who all use a similar expression of the survey question, which of course enhances comparability over time. With very minor variations, the phrase of the survey item is: "I want to ask you about your view of public expenditures for various purposes. I shall read some public tasks to you, and for each task I want you to say whether you think the public uses too much money, a suitable amount, or too little money." The annual PDIs shown in Table 4.1 in Chapter 4 have been calculated as the percentage of respondents who think the government spends "too little" subtracted from those who think the government spends "too much" money.

Sources:

Data for years 1979, 1985, and 1987 is found in Andersen, Jørgen Goul (1988). "Vælgernes holdninger til den offentlige udgiftspolitik" in Bentzon,

Karl-Henrik (ed.), *Fra vækst til omstilling – moderniseringen af den offentlige sektor*. Frederiksberg: Nyt fra Samfundsvidenskaberne, pp. 150-151

Data for years 1990, 1994, and 1998 is from the Danish National Election Studies.

Data for year 1981 is found in Kristensen, Ole P. (1982). "Voter Attitudes and Public Spending: Is Thee a Relationship?", *European Journal of Political Research*, 10, p.42 [Table 1]

Data for years 1999-2003 was collected and made available by Lise Togeby, University of Aarhus. Annual PDIs have been calculated as the average PDIs in years, where more than one survey was conducted. For a further description of the data, see Togeby, Lise (2004), *Man har et standpunkt… Om stabilitet og forandring i befolkningens holdninger*. Gylling: Aarhus Universitetsforlag.

*Ideological color of the government*

The dummy variable measuring the ideological color of a government is constructed on the basis of the information provided in Table A.1.7 below. The left column shows the dates where a shift from one government bloc to another took place. The second, third, and fourth columns report changes both between and within the governments and government blocs from 1979 to 2003.

The main challenge when constructing this variable is to handle the three years where government power changed from one bloc of parties to the other. A large part of public spending in year t is approved in year t–1, so the general categorization rule is to assign responsibility to the old government bloc in years when government responsibility moves between the two opposing blocs of parties. The resulting categorization is reported in the right column, where every fiscal year in the period 1979 to 2003 was assigned a value of either 0 or 1, depending on whether a bourgeois or social democratic government was in charge of the Budget.

Table A1.7. Fiscal year and color of the government bloc, 1979-2003.

| Date of government change | Government period | Government | Government bloc | Fiscal year[a] (government) |
|---|---|---|---|---|
| | 1979-81 | Social Democrats | Communists, Left Socialists, Socialist People's Party, Social Liberals | 1979 (1)<br>1980 (1)<br>1981 (1) |
| | 1981-82 | Social Democrats | Left Socialists, Socialist People's Party, Social Liberals | 1982 (1) |
| 9 Oct., 1982 | 1982-84 | Liberals, Conservatives, Centre Democrats and Christian People's Party | Social Liberals, Progress Party | 1983 (0)<br>1984 (0) |
| | 1984-87 | Liberals, Conservatives, Centre Democrats and Christian People's Party | Social Liberals, Progress Party | 1985 (0)<br>1986 (0)<br>1987 (0) |
| | 1987-88 | Liberals, Conservatives, Centre Democrats and Christian People's Party | Social Liberals, Progress Party | 1988 (0) |
| | 1988-90 | Liberals, Conservatives, Social Liberals | Centre Democrats, Christian People's Party, Progress Party | 1989 (0)<br>1990 (0) |
| | 1990-93 | Liberals, Conservatives | Social Liberals, Centre Democrats, Christian People's Party, Progress Party | 1991 (0)<br>1992 (0)<br>1993 (0) |
| 25 Jan., 1993 | 1993-94 | Social Democrats, Social Liberals, Centre Democrats and Christian People's Party | Socialist People's Party | 1994 (1) |
| | 1994-96 | Social Democrats, Social Liberals, Centre Democrats | Unity List, Socialist People's Party | 1995 (1)<br>1996 (1) |
| | 1996-98 | Social Democrats, Social Liberals | Unity List, Socialist People's Party, Centre Democrats | 1997 (1)<br>1998 (1) |
| | 1998-01 | Social Democrats, Social Liberals | Unity List, Socialist People's Party | 1999 (1)<br>2000 (1)<br>2001 (1) |
| 27 Nov., 2001 | 2001-05 | Liberals, Conservatives | Christian People's Party, Danish People's Party | 2002 (0)<br>2003 (0) |

Note a: The number in the parentheses shows the values of the dummy variable used in the analyses in Chapters 5, 6, and 7. A value of 1 indicates that public spending in that fiscal year is mainly decided when a Social-democratic led bloc of parties is in government. Sources: Green-Pedersen & Hoffman Thomsen (2005, p. 158 [Table 1]) supplemented with information provided by Flemming J. Christiansen, University of Aarhus.

*Economic indicators*

Table A1.8 summarizes the economic indicators used in the analyses. Information on each of these variables was obtained from tables in the OECD Economic Outlook for various years. The calculation of unemployment rates changed in 2000. After this point it was calculated as a percentage of the civilian labor force instead of a percentage of the population. Data based on the new information is only available from 1984 and onwards, however. Hence, there is an unavoidable technical break in this time series.

Table A.1.8. Unemployment, inflation, and growth of real GDP

| Year | Unemployment (percent) | Inflation (percentage change in consumer prices) | Growth of real GDP (percent) |
|---|---|---|---|
| 1979 | 6.2 | 9.6 | 3.5 |
| 1980 | 7.0 | 12.3 | –0.4 |
| 1981 | 9.2 | 11.7 | –0.9 |
| 1982 | 9.8 | 10.1 | 3.0 |
| 1983 | 10.3 | 6.9 | 2.5 |
| 1984 | 9.9 | 6.3 | 4.4 |
| 1985 | 8.9 | 4.7 | 4.3 |
| 1986 | 7.7 | 3.7 | 3.6 |
| 1987 | 7.7 | 4.0 | 0.3 |
| 1988 | 8.4 | 4.5 | 1.2 |
| 1989 | 9.2 | 4.8 | 0.2 |
| 1990 | 9.4 | 2.6 | 1.0 |
| 1991 | 10.3 | 2.4 | 1.1 |
| 1992 | 11.0 | 2.1 | 0.6 |
| 1993 | 12.1 | 1.3 | 0.0 |
| 1994 | 12.0 | 2.0 | 5.5 |
| 1995 | 10.2 | 2.1 | 2.8 |
| 1996 | 8.7 | 2.1 | 2.5 |
| 1997 | 7.7 | 2.2 | 3.0 |
| 1998 | 6.4 | 1.8 | 2.5 |
| 1999 | 5.2 | 2.5 | 2.6 |
| 2000 | 4.7 | 2.9 | 2.9 |
| 2001 | 4.3 | 2.3 | 1.4 |
| 2002 | 4.6 | 2.4 | 2.1 |
| 2003 | 5.6 | 2.0 | 0.5 |

Sources: OECD Economic Outlook, various years.

*Election cycles*

Table A1.9 displays the values of the dummy-variable differentiating between election- and non-election years. “1” indicates election year and “0” non-election year. The column to the left shows the date of the election. Only fiscal years in which an election takes place are coded as election years.

Table A1.9. Construction of the election dummy

| Election date[a] | Fiscal year | Election years |
|---|---|---|
| 23 Oct., 1979 | 1979 | 1 |
| | 1980 | 0 |
| 8 Dec., 1981 | 1981 | 1 |
| | 1982 | 0 |
| | 1983 | 0 |
| 10 Jan., 1984 | 1984 | 1 |
| | 1985 | 0 |
| | 1986 | 0 |
| 8 Sept., 1987 | 1987 | 1 |
| 10 May, 1988 | 1988 | 1 |
| | 1989 | 0 |
| 12 Dec., 1990 | 1990 | 1 |
| | 1991 | 0 |
| | 1992 | 0 |
| | 1993 | 0 |
| 21 Sept., 1994 | 1994 | 1 |
| | 1995 | 0 |
| | 1996 | 0 |
| | 1997 | 0 |
| 11 March, 1998 | 1998 | 1 |
| | 1999 | 0 |
| | 2000 | 0 |
| 20 Nov., 2001 | 2001 | 1 |
| | 2002 | 0 |
| | 2003 | 0 |

Note a: Source http://www.ft.dk/valg/Statistik/Folketingsvalg%201849-2001.asp.

*Policy domain specific control variables*

*Law & order*

Δ Crime reports:

This variable measures annual percentage changes in the number of police reports per 100 persons over 15 years of age.

*Source*: Statistics Denmark (2003). *Kriminalstatistik* (Table 1.01).

Δ Reports on violence:

This variable measures annual percentage changes in reports on violence measured as per thousands of total annual police reports.

*Source*: Statistics Denmark (2003). *Kriminalstatistik* (Table 1.01).

*Pollution control*

Δ Pollution index:

This variable measures annual percentage changes in an index ranging from 0-10 based on four pollution indicators. The measure is inspired by Scruggs (1999). The four annually measured pollution indicators are: 1) total emissions of $NO_x$; 2) total emissions of $SO_x$; 3) apparent consumption of nitrogenous fertilizers; and 4) apparent consumption of phosphate fertilizers. All are measured in 1,000 tons. To construct a single pollution index I divided each value for a series by its maximum value and then, for each year, averaged across the four series. Finally, the index was rescaled so that it ranges from a value of 0 to a value of 10. The higher the value of this index, the stronger the indication of pollution problems.

Source: OECD's Environmental Data Compendium, various years.

*Primary education*

Δ Number of immigrant children aged 6-15:

This variable measures annual growth in the number of children between 6 and 15 years of age from a foreign country of origin. Annual growth is measured in 1,000s of children.

Source: Table BEF3 from www.statistikbanken.dk

ΔΔ Number of children aged 6-15:

This variable measures annual changes in growth of the total number of Danish children between 6 and 15 years of age. The number of children is measured in 10,000s.

Source: Table BEF1 from www.statistikbanken.dk

*Health*

ΔΔ People:

This variable measures annual growth of the total Danish population measured in 1,000s inhabitants.

Source: Table BEF1 from www.statistikbanken.dk

ΔΔ People age > 67:

This variable measures annual growth of the total Danish population more than 67 years of age measured in 1,000s inhabitants.

Source: Table BEF1 from www.statistikbanken.dk

*Cultural purposes*

Δ Cinema tickets sold per inhabitant:

This variable measures annual changes in the number of sold tickets to movie theatres in Denmark measured per 100 inhabitants.

Source: Statistics Denmark's *Statistisk tiårsoversigt* various years.

Δ Number of attendants at theatrical performances:

This variable measures annual changes in the number of attendants to subsidized theatrical performances in Denmark measured per 100 inhabitants.

Source: Statistics Denmark's *Statistisk tiårsoversigt* various years.

# Appendix 2. Table related to Chapter 5

Table A2.1. Spending on hospitals, 1980-2003

| Dependent variable = Δ Log of real spending | | | | |
|---|---|---|---|---|
| | Model I | Model II | Model III | Model IV |
| Constant | .847 | –.613 | 1.156 | .456 |
| | (.1.517) | (1.707) | (.934) | (1.453) |
| Debate in parliament$_{t-.5}$ | .444 | 1.467 | | .179 |
| | (1.014) | (1.193) | | (.971) |
| ΔΔ People$_t$ | | .155 | .111 | |
| | | (.139) | (.137) | |
| ΔΔ People aged > 67$_t$ | | .170 | .214 | |
| | | (.245) | (.246) | |
| Δ Log of real spending$_{t-1}$ | | –.117 | –.089 | |
| | | (.247) | (.250) | |
| Election year$_t$ | | –2.783* | –1.900 | |
| | | (1.356) | (1.169) | |
| Left government$_{t-1}$ | | 2.362* | 2.411* | 1.763* |
| | | (1.158) | (1.177) | (.956) |
| Δ Inflation$_{t-1}$ | | –.261 | –.240 | |
| | | (.547) | (.556) | |
| ΔΔ Unemployment$_{t-1}$ | | 1.242 | .873 | |
| | | (1.084) | (1.059) | |
| N (observations) | 23 | 23 | 23 | 23 |
| $R^2$ | .01 | .39 | .32 | .15 |
| $R^2$ (adjusted) | –.04 | .04 | .01 | .07 |
| F–test statistic for model | .19 | 1.12 | 1.03 | 1.81 |
| Durbin-Watson statistic | 1.88 | – | – | 2.12 |
| Durbin's h (p-value) | .924 | .392 | .185 | .494 |
| Breusch–Godfrey (p-value) | .919 | .268 | .109 | .458 |

Notes: Unstandardised betas with standard-errors in parentheses. *$\alpha \leq 0.10$; ** $\alpha \leq 0.05$; ***$\alpha \leq 0.01$.

# Appendix 3. Tables related to Chapter 7

Table A3.1 VAR model of parliamentary debate and public spending (three time lags)

| Issue | Dependent | Independent | Summed lags | Chi2 | Prob > chi2 |
|---|---|---|---|---|---|
| Law and order | Parliamentary debate$_t$ | ΔLog of real spending$_{t–.5, 2.5}$ | .037 | 3.678 | .298 |
| Pollution control | Parliamentary debate$_t$ | ΔLog of real spending $_{t–.5, 2.5}$ | .013 | .485 | .922 |
| Primary education | Parliamentary debate$_t$ | ΔLog of real spending $_{t–.5, 2.5}$ | **–.068** | **8.245** | **.041** |
| Health | Parliamentary debate$_t$ | ΔLog of real spending $_{t–.5, 2.5}$ | .219 | 2.120 | .548 |
| Defense | Parliamentary debate$_t$ | ΔLog of real spending $_{t–.5, 2.5}$ | .133 | 4.653 | .199 |
| Culture | Parliamentary debate$_t$ | ΔLog of real spending $_{t–.5, 2.5}$ | **–.103** | **7.242** | **.065** |
| Aid to devel-oping countries | Parliamentary debate$_t$ | ΔLog of real spending $_{t–.5, 2.5}$ | .057 | 3.502 | .321 |
| Law and order | ΔLog of real spending$_t$ | Parliamentary debate $_{t–.5, 2.5}$ | **1.041** | **7.167** | **.067** |
| Pollution control | ΔLog of real spending$_t$ | Parliamentary debate $_{t–.5, 2.5}$ | **20.200** | **33.158** | **.000** |
| Primary education | ΔLog of real spending$_t$ | Parliamentary debate $_{t–.5, 2.5}$ | 3.836 | 2.224 | .527 |
| Health | ΔLog of real spending$_t$ | Parliamentary debate $_{t–.5, 2.5}$ | .669 | 5.369 | .147 |
| Defense | ΔLog of real spending$_t$ | Parliamentary debate $_{t–.5, 2.5}$ | **–.251** | **6.676** | **.083** |
| Culture | ΔLog of real spending$_t$ | Parliamentary debate $_{t–.5, 2.5}$ | **–3.127** | **16.87** | **.001** |
| Aid to devel-oping countries | ΔLog of real spending$_t$ | Parliamentary debate $_{t–.5, 2.5}$ | **–12.640** | **7.291** | **.063** |
| Period | | 1980 to 2003 | | | |
| Lags | | 3 | | | |

Notes: Cells contain chi-square results and p values from a Wald test of Granger causality using annual observations from 1980 to 2003. Text in bold is significant at $p < .10$.

Table A3.2. VAR model of parliamentary debate and public concern (Δ)

| Issue | Dependent | Independent | Summed Lags | F-test |
|---|---|---|---|---|
| Law and order | Parliamentary debate$_t$ | ΔPublic concern$_t$ | –.458 | 3.06 |
| | | ΔPublic concern$_{t-1, 2}$ | –.348 | .74 |
| | | Parliamentary debate$_{t-1, 2}$ | –1.130 | 3.31* |
| | ΔPublic concern$_t$ | Parliamentary debate$_t$ | –.512 | 3.06 |
| | | Parliamentary debate$_{t-1, 2}$ | –1.799 | 4.68** |
| | | ΔPublic concern$_{t-1, 2}$ | –.268 | .59 |
| Pollution Control | Parliamentary debate$_t$ | ΔPublic concern$_t$ | .155 | 1.69 |
| | | ΔPublic concern$_{t-1, 2}$ | –.039 | .50 |
| | | Parliamentary debate$_{t-1, 2}$ | .586 | 1.98 |
| | ΔPublic concern$_t$ | Parliamentary debate$_t$ | .932 | 1.69 |
| | | Parliamentary debate$_{t-1, 2}$ | –.739 | 1.10 |
| | | ΔPublic concern$_{t-1, 2}$ | .252 | 1.27 |
| Defense | Parliamentary debate$_t$ | ΔPublic concern$_t$ | .072 | 4.52* |
| | | ΔPublic concern$_{t-1, 2}$ | –.004 | 1.84 |
| | | Parliamentary debate$_{t-1, 2}$ | .861 | 2.87 |
| | ΔPublic concern$_t$ | Parliamentary debate$_t$ | 4.331 | 4.52 |
| | | Parliamentary debate$_{t-1, 2}$ | –5.616 | 1.37 |
| | | ΔPublic concern$_{t-1, 2}$ | –.587 | 1.47 |

Notes: Cells contain results from OLS regressions, using annual data from 1985 to 2003.

Table A3.3. VAR model of parliamentary debate and public concern (3 time lags)

| Issue | Dependent | Independent | Summed Lags | F-test |
|---|---|---|---|---|
| Law and order | Parliamentary debate$_t$ | Public concern$_t$ | –.431 | 2.05 |
| | | Public concern$_{t–1, 3}$ | .490 | .87 |
| | | Parliamentary debate$_{t–1, 3}$ | –.636 | 1.91 |
| | Public concern$_t$ | Parliamentary debate$_t$ | –.472 | 2.05 |
| | | Parliamentary debate$_{t–1, 3}$ | –2.045 | 2.38 |
| | | Public concern$_{t–1, 3}$ | 1.081 | 16.20*** |
| Pollution Control | Parliamentary debate$_t$ | Public concern$_t$ | .195 | 2.02 |
| | | Public concern$_{t–1, 3}$ | –.134 | 1.07 |
| | | Parliamentary debate$_{t–1, 3}$ | .058 | 1.03 |
| | Public concern$_t$ | Parliamentary debate$_t$ | 1.033 | 2.02 |
| | | Parliamentary debate$_{t–1, 3}$ | .315 | .86 |
| | | Public concern$_{t–1, 3}$ | .801 | 8.04*** |
| Defense | Parliamentary debate$_t$ | Public concern$_t$ | .044 | 1.08 |
| | | Public concern$_{t–1, 3}$ | –.121 | 2.70 |
| | | Parliamentary debate$_{t–1, 3}$ | 1.294 | 2.72 |
| | Public concern$_t$ | Parliamentary debate$_t$ | 2.687 | 1.08 |
| | | Parliamentary debate$_{t–1, 3}$ | –.357 | .34 |
| | | Public concern$_{t–1, 3}$ | .388 | .64 |

Notes: Cells contain results from OLS regressions, using annual data from 1984 to 2002.

Table A3.4. VAR model of public spending and public concern (Δ)

| Issue | Dependent | Independent | Cumulated lags | Chi2 | Prob > chi2 |
|---|---|---|---|---|---|
| Law and order | ΔLog of real spending$_t$ | ΔPublic concern$_{t-.5,1.5}$ | –.429 | 2.524 | .283 |
| Pollution control | ΔLog of real spending$_t$ | ΔPublic concern $_{t-.5,1.5}$ | **–.456** | **11.530** | **.003** |
| Defense | ΔLog of real spending$_t$ | ΔPublic concern $_{t-.5,1.5}$ | -.040 | 1.711 | .425 |
| Law and order | ΔPublic concern$_t$ | ΔLog of real spending $_{t-.5,1.5}$ | –.202 | 1.267 | .531 |
| Pollution control | ΔPublic concern$_t$ | ΔLog of real spending $_{t-.5,1.5}$ | **–.002** | **7.248** | **.027** |
| Defense | ΔPublic concern$_t$ | ΔLog of real spending $_{t-.5,1.5}$ | 2.015 | 1.826 | .401 |
| Period | | 1985 to 2003 | | | |
| Lags | | 2 | | | |

Notes: Cells contain chi-square results and p values from a Wald test of Granger causality using annual observations from 1985 to 2003. Text in bold is significant at $p < .10$.

Table A3.5. VAR model of public spending and public concern (3 time lags)

| Issue | Dependent | Excluded | Summed lags | Chi2 | Prob > chi2 |
|---|---|---|---|---|---|
| Law and order | ΔLog of real spending$_t$ | Public concern$_{t-.5\ ,2.5}$ | .044 | 1.493 | .684 |
| Pollution control | ΔLog of real spending$_t$ | Public concern$_{t-.5\ ,2.5}$ | **.105** | **38.805** | **.000** |
| Defense | ΔLog of real spending$_t$ | Public concern$_{t-.5\ ,2.5}$ | –.021 | 1.525 | .677 |
| Law and order | Public concern$_t$ | ΔLog of real spending$_{t-.5\ ,2.5}$ | –.131 | 3.463 | .326 |
| Pollution control | Public concern$_t$ | ΔLog of real spending$_{t-.5\ ,2.5}$ | **4.07** | **9.980** | **.019** |
| Defense | Public concern$_t$ | ΔLog of real spending$_{t-.5\ ,2.5}$ | **1.599** | **9.662** | **.022** |
| Period | 1985 to 2003 | | | | |
| Lags | 3 | | | | |

Notes: Cells contain chi-square results and p values from a Wald test of Granger causality using annual observations from 1985 to 2003. Text in bold is significant at $p < .10$.

Table A3.6. VAR model of public spending and public concern (Δ, 3 time lags)

| Issue | Dependent | Independent | Cumulated lags | Chi2 | Prob > chi2 |
|---|---|---|---|---|---|
| Law and order | ΔLog of real spending$_t$ | ΔPublic concern$_{t-.5,\ 2.5}$ | –.083 | .184 | .980 |
| Pollution control | ΔLog of real spending$_t$ | ΔPublic concern $_{t-.5,\ 2.5}$ | **–1.823** | **27.002** | **.000** |
| Defense | ΔLog of real spending$_t$ | ΔPublic concern $_{t-.5,\ 2.5}$ | .046 | 3.452 | .327 |
| Law and order | ΔPublic concern$_t$ | ΔLog of real spending $_{t-.5,\ 2.5}$ | –.274 | 6.194 | .103 |
| Pollution control | ΔPublic concern$_t$ | ΔLog of real spending $_{t-.5,\ 2.5}$ | **–.066** | **14.729** | **.002** |
| Defense | ΔPublic concern$_t$ | ΔLog of real spending $_{t-.5,\ 2.5}$ | 1.431 | 4.697 | .195 |
| Period | | 1985 to 2003 | | | |
| Lags | | 3 | | | |

Notes: Cells contain chi-square results and p values from a Wald test of Granger causality using annual observations from 1985 to 2003. Text in bold is significant at $p < .10$.

Table A3.7. VAR model of parliamentary debate and public spending, 1985-2003

| Issue | Dependent | Excluded | Summed lags | Chi2 | Prob > chi2 |
|---|---|---|---|---|---|
| Law & order | Parliamentary debate$_t$ | ΔLog of real spending$_{t–.5, 1.5}$ | –.178 | 1.989 | .370 |
| Pollution control | Parliamentary debate$_t$ | ΔLog of real spending $_{t–.5, 1.5}$ | .030 | 4.350 | .114 |
| Primary education | Parliamentary debate$_t$ | ΔLog of real spending $_{t–.5, 1.5}$ | **–.709** | **7.994** | **.018** |
| Health | Parliamentary debate$_t$ | ΔLog of real spending $_{t–.5, 1.5}$ | .072 | .079 | .961 |
| Defense | Parliamentary debate$_t$ | ΔLog of real spending $_{t–.5, 1.5}$ | .319 | 2.241 | .236 |
| Culture | Parliamentary debate$_t$ | ΔLog of real spending $_{t–.5, 1.5}$ | –.078 | 1.414 | .493 |
| Aid to developing countries | Parliamentary debate$_t$ | ΔLog of real spending $_{t–.5, 1.5}$ | –.053 | .495 | .781 |
| Law & order | ΔLog of real spending$_t$ | Parliamentary debate $_{t–.5, 1.5}$ | **.113** | **4.714** | **.095** |
| Pollution control | ΔLog of real spending$_t$ | Parliamentary debate $_{t–.5, 1.5}$ | **15.043** | **10.806** | **.005** |
| Primary education | ΔLog of real spending$_t$ | Parliamentary debate $_{t–.5, 1.5}$ | .194 | .006 | .997 |
| Health | ΔLog of real spending$_t$ | Parliamentary debate $_{t–.5, 1.5}$ | .931 | .360 | .835 |
| Defense | ΔLog of real spending$_t$ | Parliamentary debate $_{t–.5, 1.5}$ | **–1.623** | **20.278** | **.000** |
| Culture | ΔLog of real spending$_t$ | Parliamentary debate $_{t–.5, 1.5}$ | **–1.995** | **10.966** | **.004** |
| Aid to developing countries | ΔLog of real spending$_t$ | Parliamentary debate $_{t–.5, 1.5}$ | **–11.213** | **5.761** | **.056** |
| Period | | 1985 to 2003 | | | |
| Lags | | 2 | | | |

Notes: Cells contain chi-square results and p values from a Wald test of Granger causality using annual observations from 1985 to 2003. Text in bold is significant at $p < .10$.

## Notes

1. For a brief introduction to the history and major revisions of the Danish National Account System, see Statistics Denmark (2002, pp.427-35).
2. See www.dst.dk/HomeUK/Guide/documentation/NatAcc/Datarevision%202005.aspx for an overview of the latest revisions and United Nations (2000) for an introduction to the COFOG-system.
3. Where two or more laws are debated together in parliament, the number of columns was divided by the number of laws.

# Danish Summary / dansk sammenfatning

*Kapitel 1: Indledning*

Sammenhængen mellem den førte politik og befolkningens holdninger udgør en central problemstilling inden for statskundskaben. Hvor mange tidligere studier af dette spørgsmål har fokuseret på *om* befolkningens holdninger har en effekt, fokuserer denne afhandling på spørgsmålet om *hvornår* den førte politik bliver tilpasset til befolkningens holdninger. Udgangspunktet er, at befolkningens holdninger kun er én blandt flere politik-determinanter, hvorfor det bliver relevant ikke blot at undersøge om denne determinant har en effekt, men navnlig under hvilke omstændigheder, den påvirker de politiske beslutninger.

Med afsæt i tidligere studier af relevans for denne problemstilling, formuleres i afhandlingen en model, der forklarer under hvilke omstændigheder de politiske beslutninger tilpasses til befolkningens holdninger. Den centrale implikation af modellen er, at jo mere de nationale politikere taler om et givet emne i et offentligt forum – i afhandlingen målt ved debat i Folketingssalen – desto større er sandsynligheden for, at de politiske beslutninger vedrørende det pågældende emne afspejler et flertal af befolkningens præferencer. Ydermere er forventningen, at disse effekter forstærkes i år med folketingsvalg samt svækkes på områder, hvor de nationale politikeres beslutninger begrænses af det kommunale selvstyre. Argumentet bag disse påstande formuleres i kapitel 3 og involverer blandt andet en model for den politiske beslutningsproces, den institutionelle organisering af denne samt en karakteristik af de centrale beslutningstagere og vælgernes evaluering af disse.

*Kapitel 2: Befolkningens holdninger og den førte politik. En diskussion af tidligere forskning*

I de seneste par årtier har der været en betydelig vækst i antallet af empiriske studier, der systematisk undersøger i hvilken grad politiske beslutninger afspejler befolkningens præferencer. Det billede, der tegner sig, er ikke helt entydigt, men samlet set kan det på grundlag af denne litteratur konkluderes, at befolkningens holdninger påvirker de politiske beslutninger. I nogle tilfælde synes påvirkningen at være markant og betydelig, mens den i andre forekommer svag og nærmest negligerbar.

Et centralt, og i litteraturen stadig temmelig underbelyst, spørgsmål er hvilke faktorer, der betinger denne sammenhæng mellem befolkningens holdninger og den førte politik. I kapitlet diskuteres en række potentielt relevante omstændigheder, såsom emnets vigtighed i vælgernes bevidsthed og en

række institutionelle, fiskale, og politiske faktorer. Særlig argumenteres der for, at ideer udviklet inden for litteratur om dagsordens-fastsættelse er interessante at forfølge nærmere.

*Kapitel 3: Den makropolitiske arena og effekten af befolkningens holdninger*
Inspireret af især Baumgartner og Jones (1993) skelnen mellem subsystem- og makropolitik argumenteres der i dette kapitel for, at en række karakteristika ved den såkaldt makropolitiske arena tilsammen peger på, at denne arena er særlig vigtig i en model for hvornår befolkningens holdninger påvirker den førte politik. For det første antages det, at stemmemaksimering er et dominerende mål for de centrale beslutningstagere i den makropolitiske arena. For det andet er aktiviteterne i denne arena særligt synlige for en bredere offentlighed sammenlignet med de aktiviteter, der foregår i de politiske subsystemer. For det tredje befinder den makropolitiske arena sig i toppen af et temmelig komplekst hierarki af politiske beslutningssystemer hvorfor (et flertal af) aktørerne i denne arena kan justere den førte politik, hvis de ønsker det. For det fjerde er den makropolitiske arena nøjagtig som alle andre beslutningsarenaer på ethvert givet tidspunkt kendetegnet ved en bestemt dagsorden, hvilket i denne sammenhæng betyder en de facto rangordning af de emner, der behandles i arenaen på grundlag af hvor meget opmærksomhed aktørerne tildeler dem. Med henvisning til litteraturen om dagsordensfastsættelse antages det endvidere, at denne dagsorden på et givet tidspunkt altid er begrænset, men at dens sammensætning ændres over tid.

Med afsæt i nyere vælgeradfærdsstudier antages det, at den gennemsnitlige vælger – medianvælgeren om man vil – baserer sin stemmeafgivelse på en evaluering af afstanden mellem sin foretrukne og den førte politik på de emneområder, der er mest synlige for ham på valgdagen. Derudover antages det, at hans stemmeadfærd er myopisk, altså at han vægter nylige begivenheder tungere end ældre begivenheder, når han afgiver sin stemme.

Kombineres disse antagelser, kan følgende centrale påstand udledes fra den model, der opstilles i dette kapitel: jo større en andel et givet emne optager af den makropolitiske dagsorden, desto stærkere effekt har befolkningens præferencer på de politiske beslutninger, der vedrører det pågældende emne. Som følge af antagelsen om vælgernes kortsigtede hukommelse og beslutningstagernes ønske om genvalg forventes det ydermere, at denne effekt slår stærkere igennem jo tættere man er på næste valg. Omvendt er de nationale politikeres adfærd ikke kun styret af incitamenter, men også af begrænsninger og derfor forventes det, at effekten af ændringer i den makropolitiske dagsorden slår svagere igennem på områder med stor lokal autonomi.

*Kapitel 4: Data, design og metode*

I dette kapitel omsættes den teoretiske model til testbare hypoteser. For det første argumenteres der for det hensigtsmæssige i at anvende modellen på danske forhold. Centralt i den forbindelse er argumentet om partierne som enhedsaktører samt argumentet om, at partikonkurrence i Folketinget kan anskues som en konkurrence mellem to blokke af partier.

For det andet introduceres de empiriske mål for de tre centrale teoretiske begreber: befolkningens holdninger, politiske beslutninger og den makropolitiske dagsorden. Befolkningens holdninger måles ved surveys, der med jævne mellemrum siden 1979 har adspurgt et repræsentativt udsnit af befolkningen om de mener der bruges "for mange penge", "passende" eller "for få penge" på et givet velfærdsområde. På grundlag af besvarelserne af disse surveys identificeres fire områder, hvor et stabilt flertal i alle de surveys, hvor man har stillet spørgsmålet siden 1979, angiver at der bruges for få penge. Dette vedrører områderne kriminalitetsbekæmpelse, miljøbeskyttelse, sundhed samt uddannelse. Derudover identificeres på samme vis tre områder, hvor et stabilt flertal tilkendegiver, at der bliver brugt for mange penge. Disse områder er forsvar, kulturpolitik og ulandshjælp. I overensstemmelse med dette mål på befolkningens holdninger vælges årlige ændringer i de offentlige udgifter som indikator for de politiske beslutninger på disse syv områder i perioden 1980 til 2003.

Den nye variabel i forhold til tidligere studier er omfanget af debat i Folketingssalen, der bruges som indikator for den makropolitiske dagsorden. Argumentet er, at debatterne i dette forum er offentlige og ofte genstand for mediedækning, og hvis de centrale makropolitiske aktører – de politiske partier – taler om et emne i dette forum, gør de det formodentlig også i andre offentlige fora, hvorfor debat i Folketingssalen er en fornuftig indikator for den makropolitiske dagsorden. Afhandlingen drager i den forbindelse nytte af en database, der er indsamlet som led i et større forskningsprojekt under ledelse af Christoffer Green-Pedersen, Århus Universitet (se http://www.ps.au.dk/greenp/Research/Agenda.htm). Alle debatter i Folketinget er gengivet i publikationerne Folketingets Årbog. På grundlag af disse publikationer indeholder databasen en årlig opgørelse af, hvor mange spalter debatten om forskellige politiske emner dækker, herunder de syv emner, der indgår i denne afhandling. Det endelige mål baserer sig på en sammentælling af debat i Folketingssalen om lovforslag, beslutningsforslag, redegørelser og forespørgselsdebatter.

Operationaliseret på denne måde kan de teoretiske forventninger udledt i kapitel 3 formuleres i følgende empirisk testbare påstande. På de fire områder, hvor et stabilt flertal af vælgerne ønsker højere offentlige udgifter, er

der en positiv sammenhæng mellem omfanget af debat i Folketinget og ændringerne i de offentlige udgifter i perioden 1980 til 2003. Omvendt på de tre områder, hvor et stabilt flertal af befolkningen ønsker færre udgifter, er der en negativ sammenhæng mellem omfanget af debat i Folketinget om det givne emne og udviklingen i de offentlige udgifter. Ydermere forventes det, at disse sammenhænge er stærkere i år med folketingsvalg end i år uden folketingsvalg. Omvendt er forventningen, at sammenhængene er svagere på områder, hvor de nationale politikeres beslutningskompetence er begrænset som følge af eksempelvis stor kommunal autonomi.

I den sidste del af kapitlet redegøres for fordele og ulemper ved brug af multivariat tidsserieregression, den metode, der i kapitel 5 og 6 benyttes til at teste ovenstående påstande. Desuden argumenteres der for, at den påståede kausalretning mellem debat i Folketingssalen og udgiftsændringer er plausibel – en påstand, der undersøges nærmere i kapitel 7.

*Kapitel 5: Parlamentarisk debat og udgifter til populære udgiftsområder*

Kapitel 5 tester modellen på de fire udgiftsområder, hvor et klart og stabilt flertal i befolkningen tilkendegiver, at der efter deres mening bruges for få penge. Med kontrol for en række alternative forklaringer på udgiftsudviklingen, såsom regeringsblokkens ideologiske orientering, udviklingen i en række makroøkonomiske faktorer, sidste års udgiftsændringer samt udviklingen i mere områdespecifikke (ofte demografiske) faktorer, viser analyserne følgende: Med hensyn til kriminalitetsbekæmpelse viser afhandlingen, at jo større andel af den samlede debat i Folketingssalen, der vedrører kriminalitetsrelaterede emner, desto mere stiger udgifterne til institutioner, der bekæmper kriminalitet såsom politi, fængsler og domstole. Samme positive effekt finder vi på miljøområdet, hvor øget debatandel også fører til øgede udgiftsstigninger. På sundheds- og folkeskoleområdet er effekten som forventet positiv, men ikke statistisk signifikant.

Desuden viser kapitel 5, at udgifterne til forureningsbekæmpelse og lov- og ordensspørgsmål stiger mere i valgår end i ikke-valgår. På sundhedsområdet og folkeskoleområdet er der ingen statistisk signifikante forskelle mellem udgifterne i valgår og ikke-valgår.

Endelig giver analyserne i kapitel 5 en vis støtte til forventningen om, at debatomfanget i Folketingssalen slår stærkere igennem i valgår end i ikke-valgår. Således er interaktionsleddet mellem omfanget af debat og valgårsvariablen statistisk signifikant og positiv på udgifterne til folkeskoleområdet og til forureningsbekæmpelse. På de to andre populære områder er der ingen statistisk signifikant effekt af dette interaktionsled.

En relativt simpel forklaring på resultaterne i kapitel 5 kunne være, at øget debat og makropolitisk opmærksomhed altid fører til højere udgifter.

Ved i kapitel 6 at teste sammenhængen på tre områder, hvor et klart og stabilt flertal i befolkningen ønsker færre udgifter, giver afhandlingen et svar på, om denne simple forklaring er tilstrækkelig, eller om sammenhængen på disse områder er negativ som forventet på grundlag af modellen udledt og operationaliseret i kapitel 3 og 4.

*Kapitel 6: Parlamentarisk debat og udgifter til upopulære udgiftsområder*

I kapitel 6 analyseres udviklingen på de tre områder, hvor et klart og stabilt flertal ønsker lavere udgifter, hvorfor forventningen er, at øget debat i Folketinget på disse områder har en negativ effekt på udgifternes udvikling.

Analyserne i dette kapitel giver overvejende støtte til denne påstand. På alle tre områder har omfanget af debat i Folketingssalen en negativ effekt på udgifternes udvikling på det pågældende område. For kulturområdet er effekten dog ikke statistisk signifikant.

Derimod er der svagere støtte til forventningen om valgcykluseffekter. Med hensyn til ulandshjælpen er udgifterne signifikant lavere i valgår end i ikke-valgår, hvilket var forventet, men på forsvarsområdet er udgifterne signifikant højere i valgår sammenlignet med ikke-valgår, hvilket ikke var forventet i lyset af befolkningsflertallets holdninger til dette udgiftsområde. På kulturområdet er der ingen effekt på de samlede kommunale og statslige udgifter, men til gengæld, som forventet, en negativ og statistisk signifikant interaktionseffekt der indikerer, at de statslige udgifter til kulturområdet rammes særlig hårdt af debat i Folketinget i valgår sammenlignet med ikke-valgår. På ingen af de to andre områder er denne interaktionseffekt statistisk signifikant.

Samlet konkluderes det i kapitel 6, at analyserne på alle tre områder giver en vis støtte til den teoretiske model. Særligt interessant i lyset af resultaterne i kapitel 5 viser analyserne i dette kapitel, at omfanget af debat i Folketingssalen kan have en negativ effekt på udviklingen i de offentlige udgifter på områder, hvor et flertal i befolkningen giver udtryk for, at de ønsker lavere udgifter.

*Kapitel 7: Forsinkede effekter, kausalitet og alternative forklaringer*

Formålet med kapitel 7 er at kvalificere og udbygge resultaterne i kapitel 5 og 6. For det første undersøges det om en større tidsforskydning giver anledning til at ændre konklusionerne i kapitel 5 og 6. I kapitel 5 og 6 benyttes i alle analyserne en relativt kort tidsforskydning, hvilket eksempelvis betyder, at omfanget af debat i Folketingssalen fra 1. oktober 2002 til omkring 1. juni 2003 korreleres med udgifterne i finansåret 2003. På to af områderne, sundhed og kultur, viser analyserne i dette kapitel rent faktisk en statistisk signifikant effekt i den forventede retning, når eksempelvis debat i Folketingssalen

fra 1. oktober 2001 til omkring 1. juni 2002 korreleres med udgifterne i finansåret 2003. Dette tyder på, at debatomfanget også kan påvirke udgifterne på områder, hvor en stor del af udgifterne afholdes på det kommunale niveau, men at denne påvirkning blot sker med en større forsinkelse på disse områder. På de fem andre områder giver analyserne i dette afsnit ikke anledning til at ændre konklusionerne fra kapitel 5 og 6.

For det andet styrker analyserne i kapitel 7 påstanden om, at det først og fremmest er omfanget af debat, der påvirker udgiftsændringerne og ikke udgiftsændringerne, der påvirker omfanget af debat i folketingssalen. Dette argumenteres med udgangspunkt i tidsserie-analyser der viser, at der ingen statistisk signifikant sammenhæng er mellem debat og udgifter, når udviklingen i udgifterne går forud for variationen i debat i Folketingssalen om det pågældende område.

For det tredje inddrages i dette kapitel et mål for, hvor vigtigt vælgerne mener et givet område er. Som omtalt i kapitel 2, har vælgernes vurdering af vigtigheden (saliensen) af et givet område i tidligere studier af sammenhængen mellem vælgerholdninger og den førte politik været fremhævet som en vigtig betingende faktor. På de tre områder, hvor et sådant mål er tilgængeligt, peger analyserne i dette kapitel på, at den makropolitiske dagsorden ikke blot er en simpel afspejling af dette saliensmål, og at modellen udviklet i kapitel 3 dermed leverer et selvstændigt bidrag til denne litteratur.

For det fjerde diskuteres det i dette kapitel om år-til-år variationer i befolkningens udgiftsholdninger kan være en bagvedliggende forklaring på de sammenhænge, der blev fundet i kapitel 5 og 6. Som nævnt ovenfor ønsker et klart flertal i befolkningen enten flere eller færre udgifter på de syv områder, der er undersøgt, men flertallets størrelse varierer i den undersøgte periode fra 1980 til 2003. Med udgangspunkt i Wlezien og Sorokas studier af den såkaldte termostatmodel argumenteres der imidlertid i kapitel 7 for, at de sammenhænge, der findes i kapitlerne 5 og 6, ikke blot kan forklares med år-til-år variationer i befolkningens udgiftspolitiske holdninger.

Kapitel 7 afsluttes med en paneldataanalyse, der giver et samlet estimat af de postulerede sammenhænges styrke. Når alle observationer analyseres i samme paneldataanalyse genfindes den forventede effekt, både med hensyn til variation i omfanget af debat i Folketingssalen og med hensyn til de postulerede forskelle mellem valgår og ikke-valgår. Desuden indikerer paneldataanalysen, at sammenhængene ganske som forventet er svagere jo større del af udgifterne, der bruges i kommunerne, ligesom den indikerer, at effekterne generelt er stærkere på områder, hvor et flertal af befolkningen mener, der bruges for få penge.

*Kapitel 8: Konklusioner, generaliseringer og implikationer*

Kapitel 8 indeholder en sammenfatning og vurdering af de empiriske resultater på tværs af de syv områder og på tværs af de centrale hypoteser. Den samlede vurdering er, at analyserne i afhandlingen har givet støtte til den centrale påstand udledt af modellen opstillet i kapitel 3. Jo mere de nationale politikere taler om et givet emne i den makropolitiske arena – i afhandlingen målt ved omfanget af debat i Folketingssalen – jo større er sandsynligheden for, at de politiske beslutninger vedrørende det pågældende emne afspejler et flertal i befolkningens præferencer.

Den empiriske støtte til forventningen om, at denne effekt er stærkere i valgår end i ikke-valgår, har generelt været svagere, men dog tilstrækkelig stærk til at den ikke kan afvises, samt tilstrækkelig stærk til at indbyde til yderligere studier af dette fænomen. Endelig har forventningen om svagere effekter på områder, hvor de nationale politikeres magt er begrænset af eksempelvis kommunale aktører fået en vis støtte, omend der kræves studier af flere politikområder, hvis vi med nogen større sikkerhed skal kunne vurdere effekten af disse sekundære modelimplikationer.

I kapitel 8 diskuteres det endvidere i hvilket omfang disse resultater kan generaliseres til (1) andre end de syv politikområder undersøgt i denne afhandling, (2) til andre politiske systemer end det danske samt (3) til andre perioder end 1980 til 2003. Vurderingen er, at der er tale om en relativt enkel teoretisk model, hvor forholdsvis få betingelser skal være opfyldt for, at modellen kan finde anvendelse. Der skulle derfor være gode muligheder for, at resultaternes gyldighed rækker længere end de observationer, som modellen er blevet testet på i denne afhandling.

Den anden halvdel af kapitel 8 indeholder en diskussion af, hvilke implikationer resultaterne i afhandlingen har for andre studier og teorier om offentlig politik. Her peges der navnlig på en række implikationer for (1) tidligere studier af befolkningens holdningers betydning for den førte politik, (2) litteratur om dagsordensfastsættelse samt (3) udbudsbaserede udgiftsstudier, der ofte har negligeret betydningen af vælgernes udgiftspolitiske præferencer. Denne diskussion peger omvendt også på nogle punkter, hvor den model, der er udviklet og testet i denne afhandling, med fordel kunne videreudvikles i lyset af disse andre studier og teorier.

Kapitlet afsluttes med en refleksion over hvilke normative spørgsmål om forholdet mellem befolkningens holdninger og den førte politik, afhandlingens analyser kunne give anledning til. Ved at demonstrere at vælgernes holdninger under visse betingelser påvirker de politiske beslutninger, aktualiserer afhandlingen det klassiske spørgsmål om vælgerholdningernes rationalitet og manipulerbarhed. Et andet vigtigt spørgsmål, der udspringer af afhandlingens

resultater, er spørgsmålet om, hvordan man sikrer, at den makropolitiske dagsorden ikke kontrolleres af særinteresser nu hvor det er påvist, at variationen i denne dagsorden har betydning for responsiviteten i de politiske beslutninger.

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