4 Government Fragmentation and Fiscal Policy Outcomes: Evidence from OECD Countries

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4.1 Introduction

This paper investigates on a yearly panel of 20 OECD countries the role of fragmentation in decision making as a determinant of fiscal outcomes.

In very broad terms, fragmentation arises when several agents or groups participate in the fiscal decision-making process, each with its own interests and constituency to satisfy, and each with some weight in the final decision. To participate in the majority, each group demands a share in the budget; as all groups do this, the end result is a high level of expenditure or a large deficit.[1]

At this level of generality, the view that fragmentation is responsible for the high levels of expenditure and deficits observed in industrialized countries in the last 25 years is widely held in both academic and policy circles. Yet, when it comes to empirically testing this notion, one quickly realizes that conceptually fragmentation can take many forms, and each concept can be measured in many ways. Some theoretical guidance is needed.

As we argue in Kontopoulos and Perotti 1997, fragmentation of the fiscal policy decision-making process is closely related to the notion of internalization of the costs of fiscal policy. High expenditure and possibly high deficits result when *individual* policymakers do not fully internalize the costs of *aggre-*

1. In its basic form, this intuition has been first formalized in the static "common pool" models of Weingast, Shepsle, and Johnsen (1981) and Shepsle and Weingast (1981), and subsequently extended in numerous papers. See Alesina and Perotti 1995 and Velasco 1995 for a survey of this literature.

gate expenditure and the associated taxation. This occurs because the expenditure proposed by each agent in the majority can be closely targeted to the group he or she represents, while revenues with their distortionary costs can be spread over a large number of groups.

Two key factors determine how much each agent in the majority internalizes the costs of the fiscal outcome: the number of decision makers participating in the process and the rules that govern the aggregation of preferences, that is, the set of procedures whereby a final decision is arrived at. As the number of decision makers gets larger, each will pay a smaller share of the revenue costs of each dollar of expenditure he or she proposes; the marginal cost of expenditure to each policymaker falls, and in equilibrium each will propose a higher expenditure. As a result, aggregate expenditure will also increase. The budget process is the second important determinant because it determines the game played by decision makers. For example, if the finance minister sets the total *size* of expenditure first, theoretically he or she is in a position to better internalize the costs of aggregate expenditure, and individual policymakers can only bargain over the *distribution* of this expenditure. At the other extreme, if the budget is just the sum of bids made by individual ministers, without any coordination from the top, the degree of internalization of the costs of expenditure will be at a minimum.

In this paper, we focus on the first determinant, the number of decision makers. As we show in Kontopoulos and Perotti 1997, conceptually there are two very different interpretations of this notion, depending on the basic decision-making unit one assumes. In one interpretation, the emphasis is on the legislative side of the fiscal decision-making process. In this case, the elementary unit could be the individual legislator. An empirically more relevant rendition of this interpretation would view the party in the ruling coalition as the basic decision-making unit, on the ground that—for the purposes of fiscal decision making—a party is a more or less cohesive entity representing the interests of specific groups. Thus, the first measure of fragmentation we use is simply the number of parties in the coalition, which we also call *coalition size*.

The second interpretation emphasizes the executive side of the fiscal decision-making process. Hence, the elementary unit is a spending minister. The rationale is that each spending minister participates in the formulation of, and makes demands on, the overall budget. Thus, the second measure of fragmentation we use is the number of spending ministers, which we also call *cabinet size*.

Conceptually, we see no a priori reason to privilege the first, "legislative" interpretation over the second, "executive" interpretation. In fact, all the models that apply to the former—in particular, the seminal "common pool" models of Weingast, Shepsle, and Johnsen (1981) and Shepsle and Weingast (1981) and their numerous extensions—can be applied, with a simple relabeling of variables, to the latter.

Yet without exception, all the existing empirical literature has focused on

the first, "legislative" interpretation of fragmentation, and almost invariably on a very specific variant of this notion. In a seminal contribution, Roubini and Sachs (1989a,b) focused on the effects of a variable that can be termed "government weakness," assigning progressively higher scores to single-party majority, small coalition, large coalition, and minority governments. Based on a sample of 20 OECD countries over the period 1960–85, they concluded that deficits do tend to be positively associated with this variable. In subsequent research, Edin and Ohlsson (1991) found that practically all this effect was due to minority governments. Still later, de Haan and Sturm (1994) concluded that, over the period 1982–95, even this effect does not seem to be robust. Much of the difference in these findings seems to be due to considerable differences in the coding of several governments in several countries.

We see three main shortcomings in this exclusive emphasis on the notion of government weakness. First, this measure has a weak theoretical underpinning, and it can be unrelated to the notion of fragmentation. For instance, it is not clear that a minority government with many parties should be regarded as more fragmented than a single-party majority government. Second, it can be highly subjective: witness the large discrepancy in the classification between, say, Roubini and Sachs (1989b) and de Haan and Sturm (1994). Third, it neglects entirely any notion of "executive" fragmentation.

Thus, throughout this paper, we try to use measures of fragmentation with a well-defined theoretical counterpart, and as objectively measurable and quantifiable as possible. Armed with these measures, we explicitly test the two interpretations of the notion of fragmentation—"legislative" and "executive"—against each other.

As we first showed in Kontopoulos and Perotti 1997, executive fragmentation appears to be extremely important—both economically and statistically—as a determinant of fiscal outcomes. By contrast, legislative fragmentation, in its various forms—government weakness and coalition size—seems to be much less important and robust. These conclusions are of potential policy relevance because the size of a cabinet is typically not a constitutional issue, and in fact it can vary considerably over time within each country. By contrast, the size of a coalition is typically the result of historical traditions and of the whole structure of the political process—such as the electoral system—which are often deeply grounded in the constitutional framework and in the historical and cultural background of a country, and therefore are much less likely to be amenable to change.

Because of their potential policy relevance, it is important to check the robustness of our basic results. This is particularly so because the conclusions of the literature in the tradition of Roubini and Sachs have proved to be extremely sensitive to the sample of countries, the sample period, the specification, the form of the government weakness variable (whether as separate dummy variables or a single variable), the form of the fiscal policy variables (whether as shares of GDP or logarithms of real variables, whether in levels or differences,

etc.), to the point that hardly any definite conclusion can be drawn with any reasonable degree of confidence.

Thus, in this paper we conduct an extensive sensitivity and robustness analysis of our basic results on "legislative" and "executive" fragmentation. As a consequence, we must necessarily narrow the scope of our empirical investigation. As mentioned above, we exclude from the present investigation the role of *procedural* fragmentation, that is, of the set of rules that constitute the budget process. The effects of procedural variables have been studied on panels of OECD countries by de Haan and Sturm (1994), Hallerberg and von Hagen (chap. 9 in this volume), and Kontopoulos and Perotti (1997), with mixed conclusions. One reason for these inconclusive results is that the budget process varies very little over time, so that it is very difficult to estimate its effects given the small cross-sectional dimension of the panel of OECD countries.[2] We also exclude from our investigation the role of fragmentation over time, that is, the effects of government instability and of the frequency of government turnovers. Grilli, Masciandaro, and Tabellini (1991) show that this variable seems to be positively associated with budget deficits, although here also there are considerable problems of definition, measurement, and robustness.[3]

Our main finding is that the relationship between executive fragmentation (i.e., cabinet size) and fiscal outcome is indeed very robust during the seventies and early eighties. In the following period, it is legislative fragmentation (i.e., coalition size) that is more strongly related to fiscal outcomes, although this relationship is somewhat less robust. We argue that these differences across the two periods make sense, given the different types of shocks policymakers faced.

The organization of the chapter is as follows. In the next section we briefly describe our data set. Section 4.3 introduces the basic estimation framework and discusses some key aspects of it. Section 4.4 presents the basic regressions. In section 4.5, we perform a number of robustness and sensitivity tests on our benchmark regressions. Section 4.6 concludes.

4.2 The Data

Our database includes 20 countries and covers the period 1960–95.[4] In this section, we describe the fiscal and political variables.

All the budget data we use refer to the general government and come, with a few exceptions mentioned later, from the OECD Economic Outlook and National Income Account data sets. The available data sets contain several gaps,

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2. The empirical literature on the effects of procedural variables in U.S. states is much larger, and growing. For a survey, see Poterba 1994 and Inman 1996.

3. Estimating the effects of fragmentation over time poses problems similar to those of procedural fragmentation: the frequency of government turnover must be measured over time, and therefore one essentially ends up with cross-sectional estimates.

4. The countries are Australia, Austria, Belgium, Canada, Denmark, Finland, France, Germany, Greece, Ireland, Italy, Japan, Netherlands, Norway, Portugal, Spain, Switzerland, Sweden, United Kingdom, and United States.

mainly due to the shift from the old to the new system of national accounts in many OECD countries during the seventies. We combine the two sets of data to fill most of the gaps without sacrificing comparability of the data. In a few country-years, the two data sets mentioned above were supplemented with data from two other sources: the Revenue Statistics of OECD Member Countries, and EUROSTAT's National Income Accounts. Both these data sets also follow the guidelines of the new system of national accounts and are usually identical to the other two sources when they overlap. Perotti 1998 contains more details on the construction of this data set. As a result of these amendments, the data set contains consistent series on the primary deficit and all its major components covering all the years 1960–95 for all the 20 countries in the sample, with the exception of 23 country-years.

To preserve the comparability of the definitions both over time and across countries, our definition of the primary deficit is slightly different from the conventional one. We define the primary deficit as the difference between expenditure and taxes, net of all net property income. On the expenditure side, property income comprises mainly interests, but on the revenues side it also comprises items like rents and the "operating surplus" of the government that are normally included in the definition of the primary deficit. These items seem to be less consistently defined across countries, and the breakdown between interest and other property income is not available before 1970. We also exclude capital transfers paid by the government from the expenditure side, and current and capital transfers received by the government on the revenue side. These three items have a rather spotty coverage in the databases that we use, particularly for the 1960s and for some countries, so that their inclusion would have introduced substantial breaks in the series and in the consistency of the definitions; in any case, these items are typically very small and therefore are unlikely to affects our results substantially.[5] In addition, there is no reason to expect that their exclusion biases the year-to-year movements in fiscal outcomes—on which we focus in our paper—in any particular direction.

We used three principles in constructing our political data. First, because we have a precise definition of fragmentation, we concentrate on measuring its two interpretations—"legislative" and "executive"—as directly as possible; that is, we try to construct the variable that most immediately captures each component of the definition of fragmentation. Second, while some element of subjectivity is unavoidable when political variables are involved, we focus on quantifiable measures as much as possible. In classifying these governments, we rely exclusively on external sources (from the political science literature mostly) that were also cross-checked several times. Third, we exert a specific effort to match the political variables with fiscal data by tracking the investiture date and thus the duration of the governments, so that we can establish an accurate connection between the institutional framework and the fiscal policy outcome.

5. Recall that our fiscal variables refer to the general government, and therefore all intergovernmental transfers—which can be substantial—are consolidated in our figures.

To classify governments, we must first define them. With annual data, it is not always obvious how to associate the fiscal outcome of a given calendar year with a specific type of government. We follow two criteria. First, we exclude all governments with a duration of less than 60 days, under the assumption that they are too short-lived to have any influence on fiscal outcomes. Second, we pay particular attention to the month of investiture of a government. Because of the long decision and implementation lags, a new government that is formed toward the end of a year is most likely irrelevant for the fiscal results of that year. Hence, we assume that only governments that were formed before the beginning of August of any given year have any significant impact on the fiscal outcome of that year. Obviously, this cutoff point is somewhat arbitrary; hence, we also run our regressions with three different cutoff dates (June, July, September). Since the results are robust to these variations, we present only one set of results in order to economize on space. When there is more than one government before August, all lasting longer than two months, we average the characteristics of the governments of that year, with weights equal to the duration of each government.[6]

In the following two subsections, we briefly describe the construction of the two political variables.

4.2.1   Number of Parties in Coalition (NPC)

As we discuss above, one key criterion in our investigation is to use a direct and unambiguous measure of the two notions of fragmentation that we are investigating. The measure of coalition size that most closely matches its definition is just the total number of parties in the coalition, which we denote "NPC."

The primary source for this variable is Woldendorp, Keman, and Budge 1993, a special issue of the *European Journal of Political Research* (EJPR) with a wealth of information on each government in parliamentary democracies in the postwar period. The data set has been updated annually. The United States, Greece, Portugal, and Spain are not covered in the EJPR database;[7] the first, because it is a presidential system; the latter three, because in the initial parts of the period they were not run by democratic regimes. For these countries, we used the *Europa Yearbook* (EY), an annual publication with information on each country in the world and its government.

4.2.2   Number of Spending Ministers (NSM)

The most immediate measure of the notion of cabinet size that descends from the discussion in the introductory section is simply the number of spending ministers. We construct this variable as the sum of the following ministers: (i) industry, trade, or ministers with related or subdivided competencies like

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6. In another robustness exercise we also ran the bulk of our regressions with a data set that included all governments irrespective of their duration and averaged (weighted) their characteristics in each year. This somewhat agnostic procedure did not significantly alter any of our results.

7. The annual updates cover these countries.

foreign trade, commerce, and state industries (if not attributed to public works—see next); (ii) public works, infrastructure, or ministers with related or subdivided competencies like (public) transportation, energy, post, telecommunications, merchant marine, civil aviation, national resources, construction (if not specifically attributed to housing—see below), urban development, and so forth; (iii) defense; (iv) justice; (v) labor; (vi) education; (vii) health; (viii) housing; (ix) agriculture. We also add all ministers with economics portfolios: (x) finance or ministers with related or subdivided competencies like First Lord of the Treasury, budget, taxation, and so on; (xi) economic affairs or ministers with related or subdivided competencies like (regional) economic planning or development, or small business.

The primary source for this variable is Woldendorp, Keman, and Budge 1993. However, because this source also reports, under each portfolio, all the ministers that held the same portfolio sequentially due to government reshuffles, to avoid overcounting portfolios we cross-checked each entry with the annual volumes of the *Europa Yearbook.*

4.3   Setting up the Basic Framework

Before we can start estimating the effects of fragmentation, we need to discuss the basic approach to estimating the model. Some of this discussion follows Kontopoulos and Perotti 1997; therefore we will keep it at a minimum here.

Our basic specification is of the following form:

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\begin{aligned}
X_t - X_{t-1} &= \alpha_0 + \alpha_1 \text{NPC}_t + \alpha_2 \text{NSM}_t + \alpha_3 \text{NPC}_t{}^*\text{DY}_t \\
&\quad + \alpha_4 \text{NSM}_t{}^*\text{DY}_t + \alpha_5 \text{DY}_t + \alpha_6 \text{DU}_t + \alpha_7 \text{INFL}_t \\
&\quad + \varepsilon_t,
\end{aligned}
$$

(1)

where X_t is a fiscal policy variable, which can be the deficit, total expenditure, or total revenues; DY_t is the rate of growth of GDP; DU_t is the change in the unemployment rate; INFL_t is the rate of inflation of the consumer price index; NPC_t and NSM_t are the two indices of fragmentation, and are defined as the number of parties in the coalition and the number of spending ministers, respectively; $\text{NPC}_t{}^*\text{DY}_t$ and $\text{NSM}_t{}^*\text{DY}_t$ represent the interaction of these two indices with the rate of growth of GDP.

Since the early contributions, virtually all empirical investigations have focused on the effects of political and institutional variables on the *deficit.* Yet, the theoretical case for an effect of fragmentation on expenditure is much stronger than for the effects on the deficit. For instance, in the static common pool models à la Weingast, Shepsle, and Johnsen (1981) the budget is obviously balanced. Hence, any effect of fragmentation must be on both expenditures and revenues, with no effect on the deficit.

One might think that the basic intuition of the effects of fragmentation

should be easily generalizable to the dynamic case. Indeed, Velasco (1995) develops a dynamic extension of a "common pool" model where, as the number of decision makers increases, the deficit increases. But this is the result of the specific functional forms assumed in the model, and there is no general intuition for the overall sign of the relationship between the number of policymakers and the deficit in the models.[8] In fact, Lane and Tornell (1996) present a slightly different dynamic extension of a "common pool" model, where the relationship between the number of decision makers and the deficit is non-linear. Thus, in dynamic "common pool" models the presumption that fragmentation leads to higher deficits does not seem to lend itself to an easy and general formalization. Spolaore (1993) is just about the only contribution we are aware of where there is a direct relationship between the number of decision makers and the deficit, although only in response to a negative shock. The context, however, is slightly different—a combination of the Alesina and Drazen (1991) "war of attrition" model and of the Tabellini and Alesina (1990) "strategic motive" for deficit.

This discussion suggests estimating the effects of fragmentation on expenditure and revenues separately: even in theory, fragmentation might not manifest itself in the deficit, but only in expenditure, and limiting the investigation to the deficit might lead to very misleading conclusions. In addition, disaggregating the deficit into revenues and expenditure sheds considerable light on the channels by which fragmentation affects fiscal outcomes.

This brief theoretical discussion implies that, under the null hypothesis we test, $\alpha_1 > 0$ and $\alpha_3 > 0$ when the dependent variable is expenditure: virtually all theories would imply this hypothesis. When the dependent variable is revenues, however, the existing theories offer much less guidance. Indeed, both a positive and a negative value for α_1 and α_3 can be rationalized in this case. As a consequence, when the dependent variable is the deficit, we expect α_1 and α_3 to be positive or 0.

In equation (1), the two political variables, NPC and NSM, appear also interacted with the rate of growth of GDP, DY. The interactive terms capture the plausible, but rarely tested, notion that political and institutional factors might be particularly important, as determinants of policies, in "difficult" times. This notion, which is part of the conventional wisdom of policymaking, has been formalized in Drazen and Grilli 1993 and also plays a role in Spolaore 1993 and Velasco 1995; more discursive treatments and short historical discussion are in, among others, Bruno and Fischer 1993 and Tommasi and Velasco 1996. The hypothesis we test is that fragmentation is particularly "bad" for fiscal policy in "difficult" times: hence, we expect $\alpha_2 < 0$ and $\alpha_4 < 0$ when the dependent variable is expenditure or the deficit.

Our basic specification also includes a number of macroeconomic variables—DY_t, DU_t and $INFL_t$. There are two reasons for this: first, to capture

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8. Personal communication to the authors.

the effects of the macroeconomic environment on expenditure, via automatic mechanisms like unemployment subsidies, and on revenues, because, for instance, of the progressivity of income taxes. Second, these economic variables capture the reaction function of the policymaker. When the dependent variable is aggregate expenditure, we expect $\alpha_5 < 0$, $\alpha_6 > 0$, and $\alpha_7 < 0$; when the dependent variable is revenues, we expect $\alpha_5 > 0$, $\alpha_6 < 0$, and $\alpha_7 > 0$. These are all intuitive signs for these coefficients, and we comment on them more extensively in Kontopoulos and Perotti 1997.

All regressions in the benchmark specification also include a full set of country and year dummies, whose role we study extensively in section 4.5.

Finally, all the fiscal policy variables we use in this paper are cyclically unadjusted and are expressed as changes in GDP shares. In Kontopoulos and Perotti 1997, we use cyclically adjusted fiscal variables. There are advantages and disadvantages in both strategies. In testing the effects of political and institutional factors, presumably one is interested only in the noncyclical changes in fiscal policy. This calls for using cyclically adjusted figures only if (i) political and institutional variables are correlated with cyclical conditions, and (ii) controlling for GR, DU, and INFL is insufficient, for instance because the elasticity of fiscal outcomes to growth differs across countries. On the other hand, cyclically adjusted figures are provided by international organizations only from 1973 at the earliest, and there is no commonly accepted methodology to cyclically adjust fiscal outcomes, so that all figures involve a large degree of subjectivity and may well involve some additional noise. In addition, using unadjusted GDP shares allows us a more direct comparison with existing results, since this is the definition that has been used in most of the literature.

4.4   Basic Results

Table 4.1 illustrates the basic results. Throughout the table, the dependent variables are the first differences in the ratio of deficit and expenditure to GDP. Table 4.1 delivers three main messages. First, disaggregating the deficit into expenditure and revenues is crucial. Looking at the first two columns, displaying regressions on the whole sample, it is clear that NSM is an important and very significant determinant of the deficit, and that its effects occur almost exclusively via aggregate expenditures.[9] By contrast, by looking only at the deficit one would fail to detect any significant effect of NPC; this is because NPC affects both expenditures and revenues in the same direction, although it is significant only in the expenditure regression. Note that this finding might help explain many negative results in recent papers, like those of de Haan and Sturm (1994), who mostly focused on the eighties and early nineties and failed to detect any significant effect of the "government weakness" variable.

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9. Obviously, the coefficient of all variables in the revenue regression is exactly equal to the coefficient in the expenditure regression less the coefficient in the deficit regression.

Table 4.1   Fragmentation and the Determination of Fiscal Outcome

	ΔDEF 1960–95 (1)	ΔEXP 1960–95 (2)	ΔDEF 1960–73 (3)	ΔEXP 1960–73 (4)	ΔDEF 1974–83 (5)	ΔEXP 1974–83 (6)	ΔDEF 1984–95 (7)	ΔEXP 1984–95 (8)
NPC	.06	.12	.07	.16	−.03	−.02	.50	.29
	(0.80)	(2.15)	(0.39)	(1.26)	(−0.21)	(−0.20)	(2.92)	(2.27)
NSM	.15	.18	−.04	.01	.23	.24	.00	.06
	(3.19)	(4.88)	(−0.47)	(0.08)	(2.22)	(3.25)	(0.63)	(0.63)
NPC*DY	−.01	−.02	−.01	−.02	.05	.00	−.04	−.04
	(−0.45)	(−2.22)	(−0.22)	(−0.85)	(1.76)	(0.14)	(−1.65)	(−2.00)
NSM*DY	−.02	−.01	−.02	−.02	−.03	−.02	−.01	.01
	(−2.64)	(−2.12)	(−1.45)	(−1.82)	(−2.21)	(−1.58)	(−0.29)	(0.66)
DY	.10	−.12	.14	−.02	.06	−.21	−.08	−.38
	(1.07)	(−1.67)	(0.86)	(−0.18)	(0.36)	(−1.61)	(−0.34)	(−2.27)
DU	.44	.09	.49	.13	.28	−.00	.38	.00
	(5.60)	(1.52)	(2.35)	(0.85)	(1.73)	(−0.02)	(3.14)	(0.03)
INFL	−.07	−.02	−.12	−.03	−.13	−.12	−.04	−.05
	(−3.24)	(−1.42)	(−2.40)	(−0.96)	(−2.57)	(−3.19)	(−0.64)	(−1.11)
R^2	.31	.57	.08	.46	.30	.59	.39	.59
N	641	641	207	207	195	195	239	239

Note: NPC: number of parties in coalition; NSM: number of spending ministers; DEF: primary deficit (see text for precise definition); EXP: primary expenditure; REV: primary revenue; DY: rate of growth of GDP; DU: change in unemployment; INFL: rate of change of CPI. All fiscal variables are first differences of their shares in GDP. All regressions include year and country dummies.

Second, there is indeed some evidence that both NPC and NSM are more important in bad than in good times. At zero GDP growth, an extra party in the coalition adds on average 0.12 percent of GDP per annum to aggregate expenditure; but this effect increases by 0.02 percent of GDP for any percentage point of negative GDP growth, so that at 5.0 percent negative growth an extra party in the coalition is associated with an increase in aggregate expenditure by 0.22 percent of GDP. Similarly, at zero GDP growth an extra spending minister adds 0.18 percent of GDP per annum to aggregate expenditure, and 0.23 percent at 5.0 percent negative growth. These are substantial numbers, if one considers that in the sample the number of parties in the coalition ranges from one to five, and the number of spending ministers from 5 (in Switzerland) to 18 (in Italy).

Third, disaggregating the whole 1960–94 period into its three main decades is also of crucial importance. As one would expect, political and institutional variables have very little effect on fiscal policy in the 1960s, a decade characterized by more or less stable growth and little fiscal action in most countries of the sample. The two subsequent decades show much more action, but in very different ways. In the seventies, the coefficient of NSM is positive and highly significant in both the deficit and expenditure regressions, while the coefficient of NPC is virtually 0. In the 1980s, exactly the opposite is true (in addition, the coefficient of NPC is negative in the revenue regression, so that the coefficient in the deficit regression is virtually double that in the expenditure regression).

This difference between the last two decades is striking, and it is so significant that it is unlikely to be due to chance alone. We believe the interpretation lies in the nature of the fiscal shocks in the two decades. In the seventies, the problem common to all countries was how to best contain the growth of expenditure in response to an external negative shock. In the eighties, the main shock to fiscal policy was internal, and the dividing line was between those countries that engaged in large discretionary consolidations and those that did not. The decision to engage in a fiscal consolidation is largely political and requires a cohesive government agreeing on such a fundamental decision. Consequently, one would expect coalition size to play a particularly important role in the last decade. By contrast, in the seventies the goal—containing the growth of expenditure—was common in all countries: how well a country could attain it depended, among other things, on how the executive decision-making process was organized. Hence, one would expect cabinet size to matter particularly in this decade.

These are the three basic results of our approach. In the next section, we start exploring their robustness. Because, as we have shown, virtually nothing is significant in the regressions for the sixties, from now on we will concentrate mostly on the last two decades.

4.5   Robustness and Sensitivity

4.5.1   The Role of Country and Time-Fixed Effects

In table 4.2, we explore the role of country and year dummies. This is a particularly important issue given our sample and the nature of the estimation problem. The macroeconomic shocks that influence fiscal outcomes are likely to be highly correlated across countries. Year dummies can then parcel out the effects of these shocks if the latter are only partially captured by the macroeconomic variables we control for—GR, DU, and INFL. Perhaps even more important is the role of country-fixed effects. On the right-hand side of our regressions we have several political and institutional variables, which are arguably highly correlated with unobservable and time-invariant cultural and historical country-specific characteristics. If the latter also affect the rate of growth of expenditure and revenues, it is extremely important to control for country-fixed effects in order to eliminate this source of endogeneity bias.

The first four columns of table 4.2 display deficit and expenditure regressions with time-fixed effects only. This has rather drastic effects on the estimated coefficients. In the seventies, the coefficient of NSM remains highly significant in both regressions, but its point estimate is cut to about a half. In the eighties, the coefficient of NPC falls much more drastically in both size and significance, and it is now very far from being statistically significant.

In the next four columns of table 4.2, only country-fixed effects are present. Now even the coefficient of NSM in the seventies becomes insignificant, while the coefficient of NPC in the eighties remains significant, although only in the deficit regression.

These results clearly highlight the importance of controlling for time- and country-fixed effects in our regressions. From now on, all our regressions will include both.

4.5.2   Outliers

Before making further inference from the results we have obtained, it is important to make sure that they are not unduly influenced by the inclusion of individual countries, always a real possibility in panels with a small cross-sectional component like the present one. To address this issue, we started by reestimating all our regressions excluding one country at a time. The results of this procedure, reported in table 4.3, reveal a considerable difference between the robustness of the effects of cabinet size and coalition size.

The first panel reports the maximum p-values of the coefficients of NPC, DY*NPC, NSM, DY*NSM in the main regressions, the corresponding point estimate, and the country whose exclusion leads to the maximum p-value of that coefficient. From our previous analysis, we are particularly interested in the coefficients involving cabinet size in the seventies, and the coefficients involving coalition size in the eighties. As one can see, no single country has an overwhelming influence on the coefficients and their significance. The exclu-

Table 4.2   **The Role of Time- and Country-Fixed Effects**

	ΔDEF 1974–83 (1)	ΔEXP 1974–83 (2)	ΔDEF 1984–95 (3)	ΔEXP 1984–95 (4)	ΔDEF 1974–83 (5)	ΔEXP 1974–83 (6)	ΔDEF 1984–95 (7)	ΔEXP 1984–95 (8)
NPC	−.06	.05	.04	.06	.07	.14	.37	.16
	(−0.70)	(0.76)	(0.46)	(0.91)	(0.43)	(1.03)	(2.11)	(1.16)
NSM	.12	.13	−.03	−.03	.15	.14	.16	.16
	(2.73)	(3.64)	(−0.50)	(−0.75)	(1.34)	(1.68)	(1.25)	(1.62)
NPC*DY	.04	−.02	−.04	−.04	.01	−.02	−.04	−.04
	(1.40)	(−0.72)	(−1.69)	(−2.11)	(0.47)	(−0.78)	(−1.50)	(−1.88)
NSM*DY	−.03	−.02	−.01	.01	−.02	.00	−.02	.00
	(−2.74)	(−2.09)	(−0.35)	(0.78)	(−1.29)	(0.00)	(−0.94)	(0.14)
DY	.17	−.06	−.06	−.37	−.04	−.34	.00	−.35
	(1.07)	(−0.46)	(−0.27)	(−2.26)	(−0.20)	(−2.27)	(0.00)	(−1.99)
DU	.27	.06	.38	.07	.22	−.03	.35	.05
	(1.91)	(0.58)	(3.34)	(0.82)	(1.34)	(−0.23)	(3.10)	(0.61)
INFL	−.06	.01	−.03	−.02	−.04	.02	−.01	.01
	(−2.45)	(0.30)	(−1.20)	(−0.83)	(−0.80)	(0.50)	(−0.16)	(0.26)
R^2	.33	.55	.39	.57	.10	.41	.33	.53
N	195	195	239	239	195	195	239	239

Note: Columns 1 to 4: only year dummies; columns 5 to 8: only country dummies. See also note to table 4.1.

Table 4.3 **Sensitivity Analysis of the Country Sample**

Dependent variable	ΔDEF		ΔEXP		ΔDEF		ΔEXP	
Period	1974–83		1974–83		1984–95		1984–95	
Coefficient	NSM (1)	NSM*DY (2)	NSM (3)	NSM*DY (4)	NPC (5)	NPC*DY (6)	NPC (7)	NPC*DY (8)
Maximum p-value, Individual Significance								
Coefficient estimate	.21	−.02	.24	−.01	.42	.00	.22	−.02
Maximum p-value	0.10	0.07	0.01	0.61	0.02	0.36	0.11	0.52
Country excluded	FRA	IRE	FRA	CHE	FIN	DNK	FIN	FIN
Maximum p-value, Joint Significance								
Coefficient estimate	.19	−.03	.24	−.02	.42	−.03	.22	−.03
Maximum p-value	0.05		0.02		0.07		0.33	
Country excluded	BEL		FRA		FIN		FIN	
Greece, Portugal, Spain, United States Excluded								
Coefficient estimate	.18	−.03	.19	−.02	.28	−.02	.14	−.02
p-value	0.10	0.02	0.01	0.04	0.11	0.41	0.26	0.31
p-value, joint	0.03		0.01		0.27		0.44	
Greece, Portugal Excluded								
Coefficient estimate	.20	−.03	.20	−.02	.32	−.03	.19	−.03
p-value	0.05	0.03	0.00	0.04	0.06	0.22	0.13	0.11
p-value, joint	0.02		0.00		0.14		0.17	

Note: First panel: each column reports the maximum p-value (second line) and the corresponding point estimate (first line) of the coefficient indicated at the top of the column, out of the 19 regressions estimated by dropping one country at a time. The third line reports the country excluded. Second panel: same as in the first panel, except that the reported maximum p-value is that of a test of a joint significance of the two coefficients. Third panel: Greece, Portugal, Spain, and United States excluded. Fourth panel: Greece, Portugal excluded. See also note to table 4.1.

sion of France causes the *p*-value of the coefficient of NSM in the deficit regression of the seventies to become significant only at the 10 percent level, and so does the exclusion of Finland to the coefficient of NPC in the expenditure regression of the eighties. But the coefficient of NSM in the expenditure regression of the seventies and the coefficient of NPC in the deficit regression of the eighties are always well below the 5 percent significance level. The point estimates of the relevant coefficients are also remarkably close to the benchmark estimates of table 4.2.

The picture is slightly different for the interactive terms. The exclusion of just one country makes the coefficient of NSM*DY in column 4 or the coefficient of NPC*DY in column 8 lose its statistical significance, with *p*-values of .61 and .52, respectively. Although we report only the highest *p*-value, corresponding to the exclusion of Finland, the interactive term NPC*DY becomes insignificant when just one of several other countries is excluded.

Although the interactive terms do appear to be sensitive to the inclusion of individual countries, one could argue that what matters is really the joint significance of the coefficients of the fragmentation variable and its interactive term. The second panel of table 4.3 reports maximum *p*-values for tests of the joint significance of the two coefficients listed at the top of the table, together with the associated excluded country. The coefficients of NSM and NSM*DY are always jointly significant, with a maximum *p*-value of .05. By contrast, when Finland is excluded, the coefficients of NPC and NPC*DY have a joint significance level of about .07 and .33 in the deficit and expenditure regressions. Thus, the analysis of the joint significance gives a considerably more robust impression than the analysis of the individual coefficients.

Recall that, in constructing our data set of political and institutional variables, for four countries—Greece, Portugal, Spain, and the United States—we had to use a different source than for all the others. In addition, one could question the inclusion of the first three countries, on the ground that they were not democracies until the mid-1970s, and of the United States, on the ground that it is not a parliamentary regime and therefore the notion of coalition size is somewhat less clear cut. The criticism concerning Greece, Portugal, and Spain hardly applies to the regressions we have presented, since they cover the period 1974–95, and therefore include very few years of nondemocratic regime in these countries. However, the role of these countries still warrants further investigation, albeit for a different reason: the fiscal data of Greece and Portugal are widely regarded to be somewhat less reliable than those of the other countries, particularly in the early part of the sample.[10]

When the basic benchmark regression is estimated without the four countries (see the third panel of table 4.3), one finds that the coefficients of NSM and NSM*DY are robust, but once again the coefficients of NPC are much less so: both the coefficient estimates and their significance drop drastically.

10. For instance, Portugal revalued its gold reserves in the mid-1970s, causing a large change in the deficit.

On further investigation, one finds that this result is mostly due to precisely the two countries mentioned above: Portugal and Greece. It is sufficient to exclude these two countries (as in the last panel of table 4.3) for the *p*-value of NPC in both the expenditure and deficit regressions to rise to .06 and .23, respectively, and for the *p*-value for the joint significance of NPC and NPC*DY to rise to .14 and .17.

Whether these are considered gross violations of the benchmark results with the whole sample is largely a subjective matter. Two countries represent more than 20 percent of the sample, and they might contain useful information that should not be wasted. Nevertheless, a conservative conclusion from this preliminary robustness analysis is that the role of cabinet size appears much more robust than the role of coalition size, in particular as concerns the asymmetric role of fragmentation in good and bad times.

4.5.3 Alternative Definitions of the Dependent Variables

In tables 4.4 and 4.5 we explore the robustness of our results to alternative constructions of the fiscal variables on the left-hand side. Some sensitivity to the specific construction of the dependent variables has emerged in the literature on government weakness.[11] In addition, in a large part of the existing literature the dependent variable is defined as the change in net or gross debt, rather than the deficit or its components.

We start in table 4.4 by considering three alternative constructions of the aggregate expenditure variable on the left-hand side, and in all cases we focus on aggregate expenditure in the last two decades, where, as we have seen, most of the action is concentrated. In the first two columns the dependent variable is in levels, and we obviously control for the lagged value of the dependent variable on the right-hand side.[12] Not surprisingly, given the very high persistence of the data, the coefficient of the latter is very close to 1, with only a marginal change in the coefficients of the variables of interest, NSM in the seventies and NPC in the eighties.

In the next two columns, the dependent variable is defined as the change in the real per capita values of expenditure divided by the previous year's real per capita GDP. One advantage of this procedure is that it eliminates movements in the dependent variable that are due exclusively to changes in GDP. For instance, suppose in year *f* the government takes no action on expenditure. Still, if there is a fall in GDP, the change in the expenditure/GDP ratio would show a substantial increase in the dependent variable, although the government did not intend to make any change to its expenditure policy.[13]

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11. De Haan and Sturm (1994) survey these results.

12. As is well known, including both a lagged dependent variable and country-fixed effects in panel regressions generates inconsistent estimates. We do not address this problem here.

13. Because the numerator—particularly when the variable in question is revenues—is also sensitive to changes in GDP, this definition of the dependent variable makes even more sense when the fiscal variables are cyclically adjusted: see Kontopoulos and Perotti 1997.

Table 4.4   **Sensitivity Analysis of the Expenditure Definition**

	ΔEXP 1974–83 (1)	ΔEXP 1984–95 (2)	ΔEXP 1974–83 (3)	ΔEXP 1984–95 (4)	ΔEXP 1974–83 (5)	ΔEXP 1984–95 (6)
NPC	−.03	.42	−.05	.27	−.14	.66
	(−0.23)	(2.58)	(−0.36)	(1.88)	(−0.38)	(1.74)
NSM	.35	.04	.28	.12	.84	.27
	(3.68)	(0.30)	(3.30)	(1.07)	(3.48)	(0.91)
NPC*DY	.01	−.09	.02	−.03	.06	−.06
	(0.25)	(−3.62)	(0.86)	(−1.49)	(0.91)	(−1.14)
NSM*DY	−.02	.01	−.02	−.00	−.05	−.01
	(−1.49)	(0.29)	(−1.59)	(−0.15)	(−1.53)	(−0.35)
DY	−.32	−.40	.07	.08	.19	.29
	(−1.89)	(−1.85)	(0.47)	(0.44)	(0.46)	(.59)
DU	.08	.20	−.05	−.04	−.06	−.10
	(0.49)	(1.72)	(−0.41)	(−0.37)	(−0.14)	(−0.39)
INFL	−.13	−.16	−.12	−.05	−.35	−.16
	(−2.86)	(−2.94)	(−2.49)	(−0.09)	(−2.98)	(1.29)
EXP_{t-1}	.23	.81				
	(24.19)	(30.21)				
R^2	.99	.99	.32	.17	.38	.15
N	195	234	224	239	195	239

Note: Columns 1 and 2: dependent variable is level of expenditures, EXP_t, instead of first difference; columns 3 and 4: dependent variable is the change in the real, per capita values of expenditure divided by the previous real, per capita GDP; columns 5 and 6: dependent variable is the change in the logarithm of expenditures. See note to table 4.1.

As one can see, once again the basic picture remains unchanged relative to the benchmark estimates of table 4.1, even though the coefficient of NPC in the eighties is no longer significant at the 5 percent level. Exactly the same conclusion applies to the last two columns of table 4.4, where the dependent variable is defined as the change in the logarithms of aggregate expenditure. Thus, overall table 4.4 confirms both the robustness of the coefficient of NSM in the seventies, and the picture of a somewhat less robust coefficient of NPC in the eighties.

In table 4.5 the dependent variable is the change in the debt/GDP ratio. In principle, the overall budget deficit should correspond exactly to the change in net debt. In practice, the difference between the two measures can be substantial, for many reasons. One can argue that the change in net debt better captures the overall stance of fiscal policy as it is actually realized, independently of how it is recorded in the official accounts. This is presumably the motivation for using this variable in Roubini and Sachs 1989a,b. On the other hand, one can also argue that the flow variable, the deficit, better captures the developments in fiscal policy that are under the control of the current policymaker; for instance, the net debt differs from the deficit because of, among other things, changes in arrears. In addition, to evaluate net debt directly (rather than from

Table 4.5   **Sensitivity Analysis of the Fiscal Deficit Definition**

	ΔDEBT 1960–95 (1)	ΔDEBT 1974–83 (2)	ΔDEBT 1984–95 (3)	ΔDEBT 1960–95 (4)	ΔDEBT 1974–83 (5)	ΔDEBT 1984–95 (6)
NPC	−.23	−.62	.46	.18	−.34	1.36
	(−0.95)	(−1.71)	(1.06)	(0.68)	(−0.91)	(2.97)
NSM	.15	−.15	.31	.11	−.06	.05
	(0.93)	(−0.61)	(0.93)	(0.60)	(−0.24)	(0.14)
NPC*DY	.00	.09	.00	−.08	.00	−.13
	(0.11)	(1.20)	(0.02)	(−1.72)	(0.01)	(−1.88)
NSM*DY	−.05	−.06	−.07	−.01	−.01	−.03
	(−1.72)	(−1.54)	(−1.46)	(−0.19)	(−0.27)	(−0.56)
DY	.47	.24	.86	−.05	−.18	.31
	(1.37)	(0.49)	(1.39)	(−0.14)	(−0.42)	(.48)
DU	.46	−.02	.75	.90	.62	1.02
	(1.98)	(−0.06)	(2.51)	(3.80)	(1.71)	(3.18)
INFL	−.28	−.26	−.46	−.17	−.39	−.30
	(−3.28)	(−2.28)	(−2.36)	(−2.25)	(−3.67)	(−1.94)
R^2	.33	.37	.40	.32	.45	.39
N	381	157	188	423	163	239

Note: Columns 1 to 3: dependent variable is change in the *net* debt/GDP ratio; columns 4 to 6: dependent variable is change in the *gross* debt/GDP ratio. See also note to table 4.1.

the flow) one must measure changes not only in liabilities but also in the assets of the government, often a highly speculative exercise, with the result that the amount of noise is likely to be much higher than for the deficit. Finally, using the change in net debt rather than the deficit causes a drastic fall in the number of observations available for our regressions—a total of 381 in the first column of table 4.5, against the 641 of the deficit regression in the first column of table 4.1.

In fact, when we use the change in the net debt/GDP ratio we find that the coefficients of both NPC and NSM lose all significance in the whole sample and in each of the last two decades (columns 2 and 3 of table 4.5). In view of our previous discussion, we believe these results simply suggest that one should not use the net debt/GDP ratio as a dependent variable in this kind of regression.

Some authors (e.g., de Haan and Sturm 1994) also use the change in the *gross* debt/GDP ratio as dependent variable. The advantages of this variable are that it is slightly more widely available than net debt, and especially that it avoids the type of measurement problems involved in any measure of government assets. On the other hand, the change in gross debt does not correspond to *any* meaningful variable under the control of the policymaker: theoretically, any change in gross debt is consistent with a given deficit, and vice versa. Columns 4 to 6 of table 4.5 report estimates with the change in the gross debt/GDP ratio as the dependent variable. We still find mostly insignificant coeffi-

cients everywhere, except the coefficient of NPC in the last decade, which is positive, significant, and extremely large. In fact, the point estimate is suspiciously large, which in our view reinforces our position on the problems with this variable. As an example, the OECD data on gross debt that we use imply a change in the gross debt/GDP ratio of 20.5 percent in 1993 in Finland, a figure that we find it hard to attribute any macroeconomic significance to since in the same year the measured budget deficit was only 5.8 percent of GDP. This and other similar values in the nineties in several countries contribute to the implausible estimate of 1.26 for the coefficient of NPC in the last decade.

Once again, in view of the lack of theoretical motivation for the use of gross debt, and in view of the serious measurement problems it involves, we consider these results mostly as a warning against using this variable.

4.5.4   The Definition of Difficult Times

One novelty in our approach is a systematic investigation of the importance of political factors in periods of macroeconomic distress. In the basic results of table 4.1 we found some support for the notion that fragmentation is a particularly important determinant of fiscal outcomes in difficult times. Table 4.6 explores the robustness of this finding.

In the first two columns, we interact NPC and NSM with a measure of the GDP gap rather than with the rate of growth of GDP. This variable, provided by the OECD, measures the percentage deviation of GDP from some measure of potential output. The results confirm the usual pattern—the coefficients of NSM in the 1970s are much more robust than the coefficients of NPC in the 1980s. In the former case, the interactive term if anything becomes more significant; in the latter case, both coefficients of NPC become insignificant.

Columns 3 and 4 present a different specification. We now divide the sample into "good" and "bad" times; the former are defined as years where growth was more than one standard deviation above the country-specific average; the latter as all other years. We then allow for a different coefficient for NSM and NPC in "bad" and in "good" times. The results broadly confirm our previous findings: in the expenditure regression in the 1970s, only NSM is significant, and there is virtually no difference in the coefficients of this variable in "bad" and "good" times. In the expenditure regression in the 1980s, the coefficient of NPC is significant only in bad times, confirming the earlier result that there seems to be a significant role of NPC in bad times.

4.6   Conclusions

Our main purpose in this chapter was go beyond existing investigations of the effects of political variables on fiscal outcomes by distinguishing and testing two alternative notions of fragmentation, which have equal theoretical status. To do so, we strove to define all our variables quantitatively, so that we could measure them as objectively as possible.

Table 4.6   **Sensitivity Analysis of the Definition of Difficult Times**

	ΔEXP 1974–83 (1)	ΔEXP 1984–95 (2)	ΔEXP 1974–83 (3)	ΔEXP 1984–95 (4)
NPC	.01	.25		
	(0.10)	(1.61)		
NSM	.20	.10		
	(2.20)	(0.87)		
NPC*DY	−.00	−.00		
	(−0.46)	(−0.14)		
NSM*DY	−.00	−.00		
	(−1.74)	(−0.68)		
NPC*GOOD			−.08	.15
			(0.49)	(−1.14)
NPC*BAD			.10	.36
			(.46)	(−0.35)
NSM*GOOD			.22	.08
			(0.00)	(−1.14)
NSM*BAD			.21	.04
			(0.01)	(−0.35)
DY	.00	.00	−.32	−.36
	(1.46)	(0.82)	(0.00)	(.59)
DU	.45	.54	.04	.03
	(2.62)	(4.64)	(0.76)	(−0.39)
INFL	−.03	.02	−.11	−.05
	(−0.73)	(0.38)	(0.00)	(1.29)
p-value, difference between NPC*GOOD and NPC*BAD			0.34	0.05
p-value, difference between NSM*GOOD and NSM*BAD			0.01	0.15
R^2	.36	.36	.59	.59
N	185	228	195	239

Note: Columns 1 and 2: DY is the GDP gap from potential output, OECD definition; columns 3 and 4: GOOD is a dummy variable with a value of 1 for years with growth more than one standard deviation above the country-specific average; BAD is a dummy capturing all other years; columns 3 and 4: values in parentheses are probability values of the *t*-statistics. See note to table 4.1.

Of the two notions, the first one is fairly traditional, although we measure it differently from the existing literature and identify fragmentation with the number of parties. The second is new in the literature and identifies fragmentation with the number of spending ministers.

Our empirical investigation reveals that this distinction was worthwhile: the number of spending ministers has a strong and very robust effect on expenditure, particularly during the period that includes the large macroeconomic

shocks of the seventies and early eighties. The number of parties in the coalition has also a statistically significant association with expenditure, but it appears only in the last decade and it seems also to be far less robust.

We believe our results are plausible and have one potentially important policy implication. The size of the ruling coalition is the result of the electoral system and of the characteristics of the whole political system; for these reasons, even if one found that coalition size is an important determinant of fiscal outcomes, realistically this finding would have limited policy implications because its causes would be very hard to modify.

By contrast, typically the number of spending ministers is not fixed in the constitution, and within certain limits it is unlikely to be a politically charged issue. In fact, in many circumstances reducing the number of spending ministers is likely to be a very popular move, with the public at large if not with the party apparatus. Thus, our findings suggest that reducing the number of spending ministers could be a feasible and even popular institutional reform with a potentially significant impact on expenditure.

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