The Causes Of Government Budget Deficits: An Empirical Reexamination Of Partisan and Institutional Effects

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Abstract: In this paper I 1) reexamine Nouriel Roubini and Jeffrey Sachs' findings regarding the political causes of government budget deficits, and a revision of those findings by Per-Anders Edin and Henry Ohlsson; and 2) revise and extend another model of budget deficits by Robert J. Franzese, Jr. Roubini/Sachs find that "weaker" governments, meaning large coalition and minority governments, are positively related to budget deficits, whereas Edin/Ohlsson, using Roubini/Sachs' data, find that only minority governments are so related. Using the same models, but correcting coding errors in R-S's political variables and using a more accurate measurement of the dependent variable, I find that both authors' results change: Large coalition governments do run higher deficits, while minority governments do not contribute much to deficits. Franzese, using a much larger data set (which encompasses that of R-S) finds a much more subtle and varied pattern of political variables' effects on deficits. I extend Franzese's model by adding two variables, minority government and bicameralism; I use a more appropriate specification of the number of parties in government (which changes several related measures); and I test different specifications of the number of parties in government. These tests suggest that--in contrast to most of the literature, which views minority governments as weak--that minority governments do not increase deficits any more than other types of government, and can even be associated with deficit reduction. The finding is consistent with Tsebelis' hypothesis that 1-party minority governments are functionally equivalent to 1-party majority governments. I also find that bicameralism under ideologically congruent houses has no effect on deficits. Despite the considerable research already devoted to the study of deficits, much work remains to be done.

Introduction During the 1970s and 1980s, large budget deficits and public debt levels emerged in most of the advanced industrial countries, after a period of relatively low deficits during the 1960s and early 1970s. Many studies, especially over the last decade, have focused on the potential political causes of these deficits. They ask, as put succinctly by Alesina and Perotti (1995), What accounts for the within-country difference in performance over time? and, Why have some countries incurred much higher deficits than others? A number of hypotheses about political influences on deficits have been advanced: Does the number of parties matter, and under what conditions? Does bicameralism make a difference? Is there a systematic difference in how minority governments perform vis a vis majority ones? How do one-party governments and multi-party governments (of both the minority and majority type) perform?

In Part I of this paper, I reexamine and revise Nouriel Roubini and Jeffrey Sachs' (hereafter, R-S) widely cited findings (1989a, 1989b) regarding some of these political causes of government budget deficits, and a revision of those findings by Per-Anders Edin and Henry Ohlsson (hereafter, E-O) (1990). I show that both R-S's and E-O's conclusions are sensitive to 1) corrections in coding in R-S's original variables and E-O's more sensitive specification; and 2) to a more accurate measure of the dependent variable, the change in debt/GDP of central government. In Part II I compare these results with those from a study by Robert J. Franzese, Jr. (1996) which uses a much larger data set, encompassing R-S's both theoretically and empirically, using additional relevant variables,

and employing a more efficient statistical methodology, weighted least squares with panel corrected standard errors. I revise and extend one of Franzese's models of budget deficits.

PART I

In a time-series, cross-national data set, R-S find that deficits increase under large coalition and minority governments. In short, R-S's theory is "less cohesive" governments are less able to reduce spending when the situation calls for it, and they consider large coalition and minority governments in parliamentary systems, and divided government in presidential systems, to be less cohesive. According to this theory (Tsebelis 1994), in parliamentary systems, all parties in the cabinet have a veto over spending reductions, because each party is politically essential to the coalition's survival--a decision by any party to withdraw from the coalition forces the creation of a new government and possibly new elections.[1] Since each party generally represents a different constituency--each of which wishes to avoid cuts in its government benefits but also wishes to maintain or reduce overall spending levels--a greater number of partners means it is less likely that the government will reduce spending. Put more generally, the government partners are in a Prisoners' Dilemma: Party A's decision to cooperate and reduce spending demands can result, in the face of Party B's decision to maintain higher spending goals, in an overall reduction of spending, but with Party B's constituents made better off and Party A's made worse off. As shown by Axelrod (1984) and others, all parties' anticipation of such a result in a finitely repeated Prisoners' Dilemma--finite because a coalition faces eventual and frequently unpredictable votes of no confidence, termination due to internal dissension, or elections--means that each of them will defect and demand their full spending portfolio. (Analytical results for the infinitely repeated PD show that some cooperation will emerge.)

Minority governments are thought to have the same problem, because they are "weak" in the sense that they must negotiate with the parliamentary party(ies) not in the government--unlike majority governments who, via party discipline, control a majority of the seats in parliament. Again, this increases the number of partners who have a veto over spending priorities. Negotiations among parties in parliament matter more than under majority governments, whether single- or multi-party, because a minority government controls only a minority of parliamentary deputies.

The theory espoused by R-S maintains that the PD effect operates mainly in situations where there is a strong incentive for deficit spending; i.e., it implies that government partners' spending goals can be "satisficed" in times of strong economic growth.[2] In the 1960s, the industrial nations experienced high growth rates, so that all types of governments were able to spend with far fewer constraints than in more recent decades. As R-S note, "Coalition governments are not inherently prone toward large deficits. During the 1960s, no major differences in budgetary behavior are evident between coalition governments and [single party] majority governments" (R-S 1989b: 923). But after the 1973 oil shock, the industrial economies went into recession for several years, resulting in lower tax revenues. Only some of these economies reduced

[1] This is in contrast to another theory which holds that parties' relative size is positively related to their intra-coalitional power (Laakso and Taagepara 1979).

[2]The theory is incomplete in this formulation, since it remains to be explained why government partners would satisfice, and not maximize, their individual spending targets.

spending accordingly. Since most of them also developed or maintained Keynesian-style economic policies during the postwar period (Hall 19xx)--such as social security programs, health care and unemployment insurance, job creation programs, and other spending initiatives to spur economic growth--governments tended either to maintain or even increase spending during times of recession. The 1979 oil shock and the 1980s interest rate hike (in which interest rates reached double digits) deepened the economic downturn, further preventing nations from recovering. Since some countries performed better than others, R-S theorize that less cohesive governments were less able to reduce spending in the face of decreasing tax revenues.

Roubini and Sachs' Study R-S employ a times-series, cross section OLS regression, using data from 13 major industrial countries encompassing the years 1964-85: Austria, Belgium, Denmark, Finland, France, Germany, Italy, Japan, the Netherlands, Norway, Sweden, United Kingdom, and the United States. The total number of country-years is 232. The dependent variable is the change (first difference) in general government net debt-to-GDP ratio.[3] Putting debt over GDP is a way to standardize the debt measurement across countries, to control for inflation within countries, fluctuations in exchange rates (if one were to standardize by converting all currencies to dollars, for example), and the difference in size cross-nationally of debt figures (debt as a percentage of GDP is a standardized measure).[4]

R-S first justifiably posit a default model using economic variables (also known as the tax-smoothing model, whose basic idea is that temporary economic shocks affecting government spending should be deficit-financed while permanent shocks should be adjusted to by changing tax rates accordingly (Franzese 1996: 4)), that should have an impact on debt levels, and for which any model investigating political effects on debt should control. These variables are (R-S 1989b: 931; see the paper for complete discussion):

Dependent variable: **DBY** Change in net general government debt to GDP ratio.

DGR(t) Change in GDP growth rate = growth in GDP at time t minus an average of GDP growth rates in the previous three years (with a 3-year historical trend assumed to be used as a baseline by policy makers). . Positive values of GDP growth should be negatively associated with debt, since the former imply higher tax revenues.

DUB(t) Change in unemployment rate = unemployment rate at time t minus an average of unemployment rates in the previous eight years (again, with an 8-year historical trend used as a baseline). Positive values of change in unemployment should be positively associated

[3] An alternative measure is the change in government budget deficit. As Heller (1994), Borrelli and Royed (1995), and others note, there are serious measurement inconsistencies not only between the two measures but also within them: across countries and over time, and across data sources. To my knowledge, it is Borrelli and Royed (1995) who address this issue the most carefully; the upshot of their discussion however is that the several different measures available all produce very similar results (1995: 252-3).

[4] Another way to address these issues is to use the log change of real debt or the log of real deficit as the dependent variable, as done by Bawn et al. (forthcoming) and Borrelli and Royed (1995). Doing so avoids the issue of employing GDP on both sides of the regression equation, since all the models R-S, Franzese, and I use include some form of the real GDP growth rate on the right-hand side as a control variable (see below)--higher GDP growth is expected to reduce the debt, since tax revenues also increase thus reducing the debt burden. Unfortunately, testing my models using these dependent variables exceeds the resources available for this paper.

with debt, since, as noted above, most countries used welfare policies that would "kick in" in periods of high unemployment, implying, ceteris paribus, greater government spending.
DRB(t) Change in real debt servicing cost = $d(r^e - n)BY(t-1)$, where $r^e = i - e$; i = interest payments on government debt divided by gross general government debt; e = weighted average of inflation rates at time t and 3 lagged periods; n = weighted average of GDP growth rates at time t and 3 lagged periods; and BY(t-1) = 1-period lag of net general government debt/GDP. Increases in DRB should be associated with higher debt, since it means governments have higher real interest payments (adjusted for changes in GDP growth), for any level of existing debt.
The other R-S variables are:
DBYL One-period lag of DBY (see below for explanation). Positive values should be associated with positive values of the dependent variable, since debt at time (t-1) should positively affect debt at time (t).
DUB*JAPN Interaction variable, DUB times a dummy variable coded 1 for Japan and 0 otherwise.

R-S give no explanation for their particular choices of average GDP growth, average unemployment, or inflation rates; nor do they report the weights used in DRB.[5] The inclusion of DUB*JAPN also is not explained.

R-S use a 1 period lag of the dependent variable on the right-hand side of the regression equation. Debt trends move relatively slowly over time, and one should expect considerable autocorrelation between observations on the dependent variable at time (t) and (t-1). Including a 1- (or more) period lagged dependent variable(s) reduces or eliminates this serial correlation of the errors (Beck and Katz 1995: 3), which is necessary to obtain unbiased statistical inferences in least squares estimation, the technique used by all studies considered here and by the present one. (Although the coefficients estimated using least squares will be unbiased, the coefficients' standard errors will be biased unless there is no serial or (here meaning inclusive "or") contemporaneous correlation or panel heteroskedasticity. Unless corrections for these are made, the standard errors "typically...will be too small, giving a false sense about the stability of the estimates and biased hypothesis tests" (De Nardo 1996: Sec. 5, pg. 5).) Beck and Katz (1995) argue (and demonstrate using Monte Carlo experiments) that using a lagged dependent variable(s) in time-series cross-section models is preferable to transforming away the serial correlation in the residuals.[6]

R-S's political variable is:
POL, an index of political cohesion ranging 0 to 3, where

0 = one party majority parliamentary government; or presidential

[5] A possible objection raised by Heller (1994: 22, n. 24) is that politicians do not look at transformed tax-smoothing variables as used by R-S; but actually it is more likely that they do, since they make policy choices based on information given to them by economists, who certainly use such variables in their forecasts. This is an empirical matter--one could ask government officials how they create their forecasts and budgets--whose investigation is beyond the scope of this paper.
[6] There is an issue of unit roots in time-series cross-section models which goes beyond my present competence. In Part 2 below, Franzese (1996: 49) has tested for unit roots and found that Unemployment potentially contained a unit root; hence the use of the lagged level of unemployment and the first difference in unemployment on the right-hand side in all of his and my models in Part 2 (see Franzese 1996: 49 for a partial explanation).

government, with the same party in the majority in the executive and legislative branch
1 = coalition parliamentary government with 2 coalition partners; or presidential government, with different parties in control of the executive and legislative branches
2 = coalition parliamentary government with 3 or more coalition partners
3 = minority parliamentary government

Table 1, Columns 1 and 3, show R-S's main results supporting their conclusion that large coalition and minority governments run higher deficits. (Values do not match exactly with R-S's (1989b) paper due to rounding error.) Col. 1 shows that the POL variable has a small but statistically significant and positive effect on deficits: A one-unit increase in the political index, supposedly representing increasingly fragmented governments, is associated with a .44% increase in debt/GDP.

In Col. 3, R-S use an interaction variable, POL*D, which is POL times a dummy variable coded 1 for the 1975-85 period and 0 through 1974. This allows the POL regression slope to shift should the data call for it, in order to test R-S's hypothesis that the political variable matters more (or perhaps only) in the post-1973 oil shock period--"that multi-party coalition governments have a bias towards larger budget deficits, but perhaps only during periods of macroeconomic stress" (R-S 1989b: 923). The dummy variable is equal to 0 during the rapid growth period 1960-74. (Although no explanation is given, presumably R-S code the interaction as beginning in 1975 in order to allow one year for the economic shocks to affect government spending on safety net policies.) Column 3's results show support for R-S's hypothesis that the political effect is limited to the post-1973 period: POL becomes four times smaller and loses statistical significance, while a one-unit increase in POL*D is associated with a .48% increase in debt/GDP, with statistical significance.

Edin and Ohlsson's Revision E-O accept R-S's theory but show that regression results using R-S's data are sensitive to the specification of the political variables--"the choice of functional form for the political variable has strong influence on the conclusions of R&S" (E-O 1990: 7-8). While R-S use their political index as a single variable and thus impose the constraint that differences between index categories 0 to 1, 1 to 2, etc. are the same, E-O test whether there are non-linear differences between R-S's political categories, by using dummy variables for each index category in their OLS regressions. To give a motivation for this test (which E-O do not), it is possible that it becomes progressively more difficult to solve a Prisoners' Dilemma, in this case agreeing to reduce spending, as the number of players increases, because in general it is more difficult to achieve mutual trust among parties as their number increases. Thus E-O create the variables P1, P2, and P3, corresponding to index values 1,2, and 3 (with the constant now interpreted as P0, the excluded category, since the index values 0-3 exhaust all government types). E-O also test R-S's post-1973 shift hypothesis by interacting P1, P2, and P3 with the D1975-85 dummy variable, giving P1*D, P2*D, and P3*D.[7]

[7] E-O do not model the separate, post-74 effect of the 0 category in their test of the post-74 shift: They do not include P0*D. Thus they leave the constant term as the overall effect of the 0 category, not allowing a post-1973 shift in it. It is not clear whether this was intentional, since E-O provide no discussion of the matter.

E-O find that there are non-linear differences among the categories: The political effect is limited to minority governments. Table 1, Col. 6 shows the coefficient for the minority government variable, P3, is five times larger than that of majority governments and is statistically significant, while the coefficients for majority governments are insignificant (and actually negative when one adds P1 and P2's coefficients to the constant, which gives the actual level of the coefficient for those categories, -.0028 and -.0015 respectively). Col. 8, a replication of E-O's key model incorporating the R-S post-1973 hypothesized shift, shows further that the minority

Table 1: Roubini-Sachs and Edin-Ohlsson Regre

t-stats In parentheses.	(1)	(2)	(3)	(4)	(5)	(6)	(7)	
	E-O Eq1/R-S Eq2 (Replication)	Col. 1 Corr DBY CentrGv	E-O Eq2/R-S Eq4 (Replication)	Col. 3 Corr	Col. 3 Corr DBY CentrGv	E-O Eqn 3 (Replication)	Col. 6 Corr DBYCentrGv	( )
Constant	**-0.0058** (-2.62)	**-0.004** (-1.55)	**-0.0050** (-2.26)	**-0.005** (-1.84)	**-0.0026** (-1.03)	**-0.0048** (-1.85)	**-0.0048** (-1.52)	**(**
DBY(t-1)	**0.7101** (15.90)	**0.6078** (10.80)	**0.6898** (15.30)	**0.6985** (14.50)	**0.5748** (9.95)	**0.7108** (16.00)	**0.6098** (10.90)	**(**
DUB	**0.1614** (2.06)	**0.2590** (3.43)	**0.1086** (1.33)	**0.1951** (2.49)	**0.2286** (3.00)	**0.1861** (2.34)	**0.2245** (2.92)	**(**
DRB	**0.8220** (3.37)	**0.7976** (3.31)	**0.7638** (3.13)	**0.7482** (3.03)	**0.7285** (3.02)	**0.8537** (3.49)	**0.8123** (3.38)	**(**
DGR	**-0.4516** (-8.20)	**-0.2724** (-4.97)	**-0.4420** (-8.06)	**-0.4556** (-8.19)	**-0.2656** (-4.88)	**-0.4486** (-8.16)	**-0.2703** (-4.96)	**(**
DUB*JAPN	**2.747** (2.17)	**3.256** (2.66)	**2.779** (2.21)	**1.858** (1.50)	**3.062** (2.52)	**2.543** (1.98)	**3.562** (2.88)	
POL	**0.0044** (2.84)		**0.0012** (0.552)					
POL*D			**0.0048** (2.12)					
POLcorr		**0.0022** (1.69)		**0.0010** (0.492)	**-0.0008** (-.433)			
POLcorr*D				**0.0023** (1.13)	**0.0040** (2.12)			
P1						**0.0028** (0.848)		**(**
P2						**0.0033**		**(**

						(0.792)		
P3						**0.0167**		
						(3.33)		
P1*D								
P2*D								
P3*D								
P1corr							**0.0033**	
							(0.886)	
P2corr							**0.0118**	
							(2.66)	
P3corr							**0.0050**	
							(1.18)	
P1corr*D								
P2corr*D								
P3corr*D								
PRESS	0.0998	0.0828	0.0988	0.1016	0.0818	0.1004	0.0824	
	ESS=.1925	ESS=.0996	ESS=.1942	ESS=.1912	ESS=.1012	ESS=.1939	ESS=.1014	
	RSS=.0905	RSS=.0768	RSS=.0887	RSS=.0918	RSS=.0752	RSS=.0891	RSS=.0750	
R-Sqrd	0.680	0.565	0.686	0.676	0.574	0.685	0.575	

government effect is concentrated in the post-1973 period: P3*D's coefficient is .0176,[8] and just misses conventional levels of statistical significance, while the other coefficients are all much smaller and statistically insignificant. Thus minority governments add 1.76% to the debt/GDP ratio, over three times that of the overall POL variable level R-S found in their results. E-O suggest that P3*D's reduced statistical significance likely is due to high collinearity between the pre-73 and post-73 political variables, an idea supported by Col. 10, which shows that the minority government effect is much stronger with the P_i*D variables removed. (Although E-O do not report any further tests for collinearity, I confirmed its presence by checking joint confidence ellipsoids between each variable and its post-1973 shift.).

E-O therefore confirm R-S's idea that "weaker"/ more fragmented governments are unable to reduce spending in times of macroeconomic stress (leading to deficits), but conclude that only minority governments are unable to do so. Majority governments, whether of the 0, 1, or 2 type, have no effect on the level of budget deficit, either before or after the oil shock: "There is no evidence that the number of partners in the coalition in power has anything to do with the government's ability to accomplish deficit reductions" (E-O 1990: 8). They go on to suggest that "it is perhaps not negotiations within a government, as much as negotiations in parliament, that is the main obstacle in cutting deficits," but one needs to test this; and E-O give no theory about the actual role of minority governments in such negotiations except perhaps to suggest implicitly that they make no difference at all.

Reexamination of R-S and E-O's Studies However, I will show that both R-S's and E-O's conclusions are sensitive to 1) a more accurate measure of the dependent variable, and 2) corrections in coding in R-S's original political variables and thus in E-O's more sensitive specification.

Regarding No. 1, R-S's dependent variable uses the net debt of general government, which includes central, state, local, and other governments. This can lead to biased estimates of the political variables' effects on deficits, because R-S count only central-government-level political actors. To the extent that the central government controls monies collected by provincial and local governments, this measure is appropriate; but the more budgetary autonomy these units have, the less relevant are national-level actors to those monies. For example, Allen Schick (in Weaver and Rockman 1993) notes that the US has a relatively low ratio of general government debt as a proportion of GDP, but a very high ratio of central government debt to GDP--the difference is the debt of state and local governments. In Germany, the national government controls less than 50 per cent of all public budgets (Sturm 1985: 57). Granted, the US and Germany are federal states and thus can be expected to have large subnational budgets, but differences exist with other states as well. The differences are especially pronounced after 1975, from which period R-S and E-O obtain their chief results. Even though a regression of Change in General Govt Debt/GDP on Change in Central Govt Debt/GDP reveals a high correlation (R^2 = .65), a scatterplot shows that the measures track more closely together in some countries than in others.

[8] In their paper E-O do not report the constant, however, and so one cannot determine the actual level of the political variables' coefficients.

Given that discovering the extent of national control of subnational funds for each country in the sample is beyond the scope of R-S's, E-O's, and this paper, it is prudent to use central government debt figures. The data I use are consolidated (i.e., including social security funds and other extrabudgetary accounts)[9] central government gross debt figures from the International Monetary Fund's Government Finance Statistics (IMF GFS) where available, and from its International Financial Statistics (IMF IFS) otherwise.[10]

Regarding No. 2, another problem is that R-S make inconsistent use of their own coding scheme, and there appear simply to be mistakes in the coding.[11] For example, R-S code Denmark 1984 and 1985, which had 3 parties in government, as code 2, "coalition parliamentary government with three or more coalition partners." But this 3-party government was actually a minority one, and thus it should be code 3. Strom (1990) says as well that it was even a "substantive," not merely "formal," minority government (i.e., there were no parties outside the government giving it informal but long-term and reliable support, which in practice may give the government power resembling a majority government). One might argue in R-S's defense that they could believe, consonant with their veto player hypothesis, that the number of parties in government matters more than its minority or majority status when that number is greater than one. This is theoretically plausible, but in that case R-S should code all single party minority governments as 3--but they code Denmark 1964-67 as 0, "one party majority," when that party, the Social Democrats, was a substantive minority (Strom 1990: 249).[12] The corrections change some countries' coding not at all and others considerably, as Table 2 shows. The corrections come from Woldendorp et al. (1993), and were cross-checked using Strom (1990) where possible. (Strom does not cover Austria, the French 5th Republic, Japan, or the US.)

As Table 1 shows, the results of the same model change considerably when these corrections are made. In R-S's first model, using POL with no post-1973 shift (Col. 2), the POL coefficient is half as large and is just barely significant at the .10 level (no longer at the .05 level) when both corrections are made. In R-S's main model, using POL and POL*D (Col. 4), we see that with the corrections to the political variable only, the political effects are about half as big (or less) and half as significant. Curiously, when both corrections are made (Col. 5), POLcorr*D's coefficient and significance are very close to

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[9] Borrelli and Royed also recognize the problem of using general government debt and have devoted careful attention to operationalizing their debt measure (1995: 227-9; 250-3). They use central government data but argue that the extrabudgetary accounts should be excluded from the debt calculation. However, the extent to which such funds are subject to political control, and hence their relevance to the political variables considered here, is an open issue. I opt to include these accounts because it seems more likely than not that such funds are indeed subject to the same kind of political control as the rest of the budget.

[10] Unfortunately, I have not yet determined the difference between gross and net debt as defined by the IMF or the OECD, the latter which supplied R-S with the data on general government net debt. R-S do not explain what they mean by "net."

[11] Thanks to George Tsebelis who alerted me to the mistakes.

[12] A further complication is that R-S are not consistent even between their own studies. In R-S 1989a, code 1 is "coalition parliamentary government with two-to-three coalition partners" and code 2 is "coalition parliamentary government with four or more coalition partners" (115). Whereas in R-S 1989b, code 1 is set for 2 partners *only*, and code 2 is for *three* or more partners (923). Abiding by this change would make each paper's data set different--among the numerous examples are Sweden 1977-78 and 1980, which would be code 1 in the former and code 2 in the latter--but they are the same in both papers. In making corrections I have used R-S's 1989b specification.

POL*D's in Col. 3, R-S's original main model. I am not sure how to explain this; perhaps the political effects are unstable, and the

	S a m e as R-S)	corr	**F** a m e)	corr	**H**	corr	**H** a m e)	corr	**a** a m e)	corr	**F** a m e)	corr	**a**
	R-S		R-S		R-S		R-S		R-S		R-S		R-S
1964	1		1	1	0		1		1	1	1	1	2
1965	1		1	1	0		1		1	1	1	1	2
1966	1		1	1	0		1		1	1	1	1	2
1967	0		1	1	0		1		1	1	1	1	2
1968	0		1	1	2		1		1	1	1	1	**3**
1969	0		1	1	2		1		1	1	1	1	**1**
1970	0		1	1	2		1		1	1	1	1	2
1971	0		1	1	0		1		1	1	1	1	2
1972	0	0	1	1	3	3	1	1	1	1	1	1	**3**
1973	0	0	2	2	3	3	1	1	1	1	1	1	2
1974	0	0	2	2	3	3	1	1	1	1	1	1	**3**
1975	0	0	2	2	3	3	1	1	1	1	1	1	3
1976	0	0	2	2	3	3	1	1	1	1	1	1	3
1977	0	0	2	2	3	3	1	1	1	1	1	1	3

1979	0	0	2	2	3	3	1	1	1	1	1	1	2
1980	0	0	2	2	3	3	1	1	1	1	1	1	2
1981	0	0	2	2	3	3	1	1	1	1	1	1	2
1982	0	0	2	2	3	3	1	1	1	1	1	1	2
1983	1	1	2	2	3	3	1	1	1	1	1	1	2
1984	1	1	2	2	2	**3**	1	1	1	1	1	1	2
1985	1	1	2	2	2	**3**	1	1	1	1	1	1	2

NB: Columns recoded include only the years for which R-S actually have data.

similar outcome results from chance capitalization in the data. Note that other variables change in the comparison however: In the fully corrected model the constant is twice as small and no longer statistically significant; DUB is twice as large and is statistically significant; and the effect of DGR is twice as small (and remains statistically significant).

A comparison of E-O's unshifted model (Col. 6) with the fully corrected one (Col. 7) shows that E-O's main conclusion that minority governments run higher deficits is reversed: Large coalition majority governments show higher deficits (P2corr's coefficient is .0118, with statistical significance), while the other categories have much smaller coefficients and are not statistically significant. Comparing E-O's key model, using both unshifted and shifted P_i variables (Col. 8), with the corrected version (Col. 9) shows further that deficits increase with the number of parties in majority governments: P1corr*D and P2corr*D have coefficients of .0103 and .0185 respectively, with statistical significance; while the minority government coefficient is smallest and statistically insignificant. However, a comparison of Col. 10, using the shifted-P_i variables only, with the same but fully corrected model (Col. 11) shows that the collinearity of the P_i and P_i *D variables (which is Pearson's r = .66 for P1corr and P1corr*D and much higher for the other two sets of variables) may affect the results, since the minority government coefficient in Col. 11 is nearly the same as that of the P1 category and is just shy of conventional levels of significance with its t-statistic of 1.94. Thus all types of governments actually may contribute to deficits, with the largest contribution made by large coalition governments.

Although the R^2 statistic decreases by about .10 in every corrected model, this is countervailed by a more reliable indicator of the improvement in fit resulting from these corrections: the Prediction Sum of Squares (PRESS) cross-validation statistic. In every model in Table 1 the PRESS statistic--a measure of the sum of squared residuals where the residual for the ith observation is predicted by a regression on the n - 1 remaining cases for all i--is smaller for the corrected models. This means that the prediction errors of the corrected models are smaller, which is an important criterion in model selection.[13] In addition, no significant differences occur as a result of there being 14 less cases in the models using central government debt data (a decrease from 232 to 218).

However, there are at least two reasons to doubt the robustness of the conclusions of the models in Table 1. First, residual analysis for each model shows violations of the Gauss-Markov assumptions (necessary to provide the inferential power of OLS), tests or corrections for which neither R-S nor E-O report. Every model shows residuals with some heteroskedasticity (both in scatterplots of predicted vs. residuals and in added variable plots for the coefficients), which bias the coefficients' standard errors and t-statistics. An auxiliary regression using the OLS squared residuals on country dummy variables (less one to avoid the dummy variable trap) implies panel heteroskedasticity (i.e., changing residual variance across countries but constant within countries) at at least the .05 level. (The F-statistic from that regression is a test of panel heteroskedasticity against the null hypothesis of homoskedasticity; a large enough F means that we reject the null

[13] As De Nardo notes, "The PRESS residuals are true prediction errors, since the computed y-hats are independent of the y's being predicted....[T]he observation y_i is not used simultaneously for fitting and model assessment" (De Nardo 1996: Part 6, P. 10).

hypothesis (Franzese 1996: 50).) The Lagrange Multiplier test[14] shows that the residuals have no remaining serial correlation (the elimination of which was intended by inclusion of the lagged dependent variable), so this is not a problem.

Second, a lack of data discourages one from placing trust in the results. The instability of the (uncorrected and) corrected models in the R-S and E-O models calls for a larger data set to attempt to gain more robust results. For example, there are only 10 cases of = 3-party coalition government before 1975; and there are only 6 cases of minority government before 1975, and 18 cases after 1975. With the corrected POL variable, there are 13 cases of = 3-party coalition government and 10 cases of minority government before 1975, and 27 and 36 cases respectively after, but the pre-1975 figures are still discouraging. These numbers are not surprising because the number of country-years covered in R-S's data vary by country:

R-S Number of Observations per Country, by Dependent Variable (number in parentheses gives total usable number of observations):

Country	**Coverage, DBY (# of cases)**	**Coverage, DBY Centr Gv (# of cases)**
Aus	1972-85 (14)	Same
Bel	1964-85 (22)	Same
Den	1972-85 (14)	Same
Fin	1972-85 (14)	Same
Fra	1964-85 (22)	Same
Ger	1964-85 (22)	Same
Ita	1967-85 (19)	Same
Japn	1967-85 (19)	1972-85 (14)
Net	1972-85 (14)	Same
Nor	1972-85 (14)	1973-85 (13)
Swe	1972-85 (14)	Same
UK	1964-85 (22)	1972-85 (14)
US	1964-85 (22)	Same
Total	**(232)**	**(218)**

In a time series, cross-section regression setting, these small numbers can lead to unstable results. In addition, I argue below that the POL variable is not an optimal operationalization of political influence, and so these results may be misleading aside from the problem of heteroskedasticity. Finally, there are omitted relevant variables. All of these issues could affect the results.

PART II: A Re-Operationalization of the Political Variables, Franzese's Study, and Extensions

[14] To test the null hypothesis that the errors in an equation with a first-order lagged dependent variable are serially independent, "we regress the residuals from an OLS estimation [of this equation] on the first lag of those residuals and all the independent variables used in the OLS estimation" (Beck and Katz 1995: 6). If one can accept the null hypothesis that the coefficient for the lagged residuals equals 0, then one can accept that the remaining errors are serially independent (Beck and Katz 1995: 6). The test generalizes for any number of lags.

There are substantive reasons to abandon the R-S political index and to use a more natural, intuitive measure of the number of veto players: the number of government parties and/or parliamentary chambers which have a say over policy making. Of course, R-S's political index already is very close to a simple counting of parties, but it is also designed to cover presidential systems as well, which leads to inappropriate coding. An example is the coding for Germany and the United States. R-S code the US as being very cohesive in comparison to parliamentary governments, since it is given only the value 0 (during periods of unified government) or 1 (periods of divided government). But the US is a bicameral country whose two main (essentially only) parties have very little party discipline--which implies the existence of 3 veto players: President, House, and Senate. Recall for instance President Jimmy Carter's inability to get much reform passed in a fully Democratic Congress, a time coded as 0 (very cohesive) by R-S. In 1995, a single Republican vote, that of Oregon Senator Mark O. Hatfield, defeated the Senate's proposed balanced budget amendment to the Constitution. In most other countries, such party defections are rare. In contrast, R-S code Germany, a country with much stronger party discipline (although less strong than many other European nations (see Loewenberg and Patterson 1979)), as 1 during periods (such as 1964-71 and 1983-85?) of coalition government with 2 partners and when both chambers are controlled by the government's majority. Why should we believe that in these cases the US government is more cohesive than Germany's? It seems more plausible that Germany's cohesion level is equal to or greater than the US'. Put more generally, why should we believe that a presidential system under divided government (code 1) is always 100% more cohesive than a coalition parliamentary government with 3 or more coalition partners (code 2)? If the latter type of government controls a majority in parliament in a unicameral system with strong party discipline (e.g., Norway 1984-85), and the former type is bicameral with incohesive parties (the US), then there are effectively 3 veto players in both countries, although R-S code Norway 1984-85 as 2 and the US as 1 during divided government.

Another example is that R-S code Finland and France as 1 throughout the time series because they are "presidential regimes where coalition governments are usually formed" (R-S 1989: 932). The implication (R-S do not discuss the issue) is that presidents must negotiate with coalition partners over the budget. However, Shugart and Carey (1992: 155-6) show that Finland's and France's presidents have recourse only to drastic measures to affect the budget. Although France's president can impose a state of emergency and thus legislate "with little or no involvement by the assembly [parliament]" (Shugart and Carey (1992: 151; see also p. 144)), such intervention is likely to have been exceedingly rare regarding the budget. In Finland, the president does have unrestricted power to dissolve parliament (Shugart and Carey 1992: 155), and the French president can do so also but only once per year (Shugart and Carey 1992: 154). At the time of writing I had not researched the potential veto-actor roles that Finland and France's presidents may have played in budget negotiations; under such ignorance I assume that that role has been minimal or nonexistent. (See Tsebelis 1994: 758 for a justification). I welcome corrections. I have not made these changes in the corrected POL and Pi variables in the models above, however; I elected to be conservative and only to correct mistakes in R-S's coding according to their own rules. I take these issues into account in creating new political variables below, after a presentation of Franzese's (1996) study.

Franzese's Study Robert J. Franzese, Jr.'s pathbreaking study (1996) addresses many of the concerns raised above, encompassing and surpassing the two studies discussed so far. Franzese employs a time-series cross-section data set covering 20 industrialized countries for the period 1956-90 (with data missing for some country-years): Australia, Austria, Belgium, Canada, Denmark, Finland, France, Germany, Greece, Ireland, Italy, Japan, Netherlands, Norway, New Zealand, Spain, Sweden, Switzerland, the UK, and the US. The total number of country-years is 562 (slightly more or less in the different models to be considered below).

Franzese Data Set: Number of valid observations by country:

Country	Number of Obs	Missing Cases
US	35	0
JA	23	(12 missing (56-67))
GE	35	0
FR	35	0
IT	35	0
UK	24	(7 missing (56-62), 4 missing (87-90) = 11 total)
CA	31	(2 missing (56-57), 2 missing (89-90) = 4 total)
AU	18	(17 missing (56-72))
BE	35	0
DE	16	(11 missing (56-66), 4 missing (70-73), 4 missing (87-90) = 19 total)
FI	33	(2 missing (89-90))
GR	19	(14 missing (56-69), 2 missing (89-90) = 16 total)
IR	19	(11 missing (56-66), 5 missing (86-90) = 16 total)
NE	35	0
NO	24	(11 missing (56-66))
SP	25 (26)*	(9 missing (56-64), 1 missing (90) = 10 total)
SW	35	(1 missing (90))
SZ	28	(7 missing (56-62))
AL	35	0
NZ	22	(13 missing (56-68))
Total	**120 (563)**	

Table 3 provides a list of the variables Franzese uses; explanation of the models and variables follow.

Table 3: Franzese's Variables

Variable	Description	Source
Dependent variable: $DDEBTX_{i,t}$	Change in central government gross debt/GDP ratio	1
ONE	Intercept	N/A
$DEBTX1L_{i,t-1}$	1-period lag of debt/GDP ratio	1
$DDEBTX1L_{i,t-1}$; $DDEBTX2L_{i,t-1}$	1- and 2-period lags of DDEBTX	1
$DDABROAD_{i,t}$	Variable equal, for each country-year, to the average deficit in that year for all other countries in the sample	1
GR	Dummy variable for Greece	N/A
SP	Dummy variable for Spain	N/A
$DICT_{i,t}$	Dummy variable indicating non-democratic regime	6, 7, 9
$DUE_{i,t}$; $UE1L_{i,t}$	Change in (internationally comparable) unemployment rate; 1-period lag in level of unemployment	2
$GROWTH_{i,t}$	Real GDP growth rate	3
$DXRIG_{i,t}$; $DXRIGDX_{i,t}$	Difference between expected real interest and growth rates; DXRIG times lagged debt/GDP ratio	1, 2
$TRADE_{i,t}$	Trade openness (exports + imports as % of GDP)	1
$TOT_{i,t}$	Terms of trade (export prices/import prices)	1
$TOTT_{i,t}$	TRADE*TOT	1
$OILDEP_{i,t}$	Oil imports as a % of inland supply 1973-84	4
$ADWI_{i,t}$; $ADWIDX_{i,t}$	Maximum absolute ideological deviation within government (Polarization, veto-actor conception); ADWI times lagged debt/GDP ratio	5, 6, 7
$NOP_{i,t}$; $NOPDX_{i,t}$	Raw number of parties in government (Fractionalization, veto-actor conception); NOP times lagged debt/GDP ratio	5, 6
$LRGDPC_{i,t}$	Natural log of real GDP per capita	3
$OY_{i,t}$	Ratio of population 65 and older to population 16 and younger	8
$GINIXX_{i,t}$	GINI index of income inequality, expanded data	11
$CBI_{i,t}$	Central bank independence index (decade-variant)	10
$PRES_{i,t}$	Dummy variable = 1 for presidential systems: Finland, France, and US	N/

		A
$FED_{i,t}$	Natural log of number of federal districts	9
$AE_{i,t}$	Proportion of cabinet from agrarian or ethnic parties	5, 6
$ENOC_{i,t}$	Natural log of effective number of constituencies, adjusted for geographic size	
$STAX1L_{i,t-1}$	1-period lag of % of tax revenue from goods, services, income, and profit taxes (i.e., "visible" taxes as a % of total tax revenue)	6
$ELE_{i,t}$	Pre-election year indicator	5
$COG_{i,t}$	Partisan Center of Gravity of government	5, 6, 7
$RR_{i,t}$	Replacement Risk (9-year, centered moving standard deviation of COG (= **SDAPF9**) times 1 over duration in years of incumbent government (= **HRY**)	5, 6, 7
$RR*COG_{i,t}$	RR times COG	5, 6, 7

Sources: 1) IMF International Financial Statistics (1995). 2) Layard, NIckell, and Jackman (1991). 3) Penn World Tables Mach V (1991), augmented by OECD Historical Statistics (various years). 4) OECD Oil and Energy Statistics (various years). 5) Woldendorp et al. (1993); *European Journal of Political Research* Political Data Handbooks (various years). 6) Lane et al. (1991). 7) Laver and Schofield (1990), Laver and Hunt (1992), and sources cited therein. 8) World Bank World Data CD-ROM (1994). 9) World Encyclopedia of Political Systems and Parties (1987). 10) Franzese (1994) and sources cited therein. 11) OECD Social Policy Studies #18 (1995).

Table 4 summarizes the connection between the variables and the theories to be discussed below.

Table 4: Summary of Variables' Relation to Theory

Variable	Function/Theory
DEBTX1L$_{i,t-1}$	Control for near-cointegrated dependent variable and for serial correlation
DDEBTX1L$_{i,t-1}$; DDEBTX2L$_{i,t-1}$	Control for near-cointegrated dependent variable and for serial correlation
DDABROAD$_{i,t}$	Time-period fixed-effects control
DICT$_{i,t}$	Control for perverse effects of non-democratic regime
DUE$_{i,t}$; UE1L$_{i,t}$	Tax Smoothing/economic controls (in forms designed to correct for the unemployment variable's near-cointegration)
GROWTH$_{i,t}$	Tax Smoothing/economic control
DXRIG$_{i,t}$; DXRIGDX$_{i,t}$	Tax Smoothing/economic controls
TRADE$_{i,t}$	Tax Smoothing/economic control
TOT$_{i,t}$	Tax Smoothing/economic control
TOTT$_{i,t}$	Tax Smoothing/economic control
OILDEP$_{i,t}$	Tax Smoothing/economic control
ADWI$_{i,t}$; ADWIDX$_{i,t}$	War of Attrition (veto-actor conception)
NOP$_{i,t}$; NOPDX$_{i,t}$	War of Attrition (veto-actor conception)
LRGDPC$_{i,t}$	Inter- and Intragenerational Transfers
OY$_{i,t}$	Inter- and Intragenerational Transfers
GINIXX$_{i,t}$	Inter- and Intragenerational Transfers
CBI$_{i,t}$	Central bank independence
PRES$_{i,t}$	Geographically Dispersed Interests or Budgetary Institutions and Procedures
FED$_{i,t}$	Geographically Dispersed Interests or Democracy and Fiscal Illusion
AE$_{i,t}$	Geographically Dispersed Interests
ENOC$_{i,t}$	Geographically Dispersed Interests
STAX1L$_{i,t-1}$	Democracy and Fiscal Illusion
ELE$_{i,t}$	(Opportunistic) Political Budget Cycles
COG$_{i,t}$	(Partisan) Political Budget Cycles
RR$_{i,t}$	Debt as Commitment
RR*COG$_{i,t}$	Debt as Commitment

Except for those aspects of Franzese's study that I extend, I will give only the briefest summary of his very substantial work; consult his paper for a fuller treatment.

Franzese starts with a default model of economic variables which should affect deficits that is similar in spirit to R-S's. This includes the following variables:

Dependent variable: DDEBTX Change in consolidated central government gross debt/GDP ratio. Franzese used IMF International Financial Statistics data where available, and then increased the number of observations by estimating missing data using a regression fitting IMF debt using R-S's or the OECD's data (or both if possible). See Franzese (1996: 1, n. 1) for details.

DEBTX1L$_{i,t-1}$; DDEBTX1L$_{i,t-1}$; DDEBTX2L$_{i,t-1}$ 1-period lagged level of debt/GDP; 1- and 2-period lags of DDEBTX. Controls to correct for near-cointegrated dependent variable and for serial correlation (see Franzese 1996: 48-9 for details). The specification with two lagged differences of debt survives a Lagrange multiplier test for serial correlation for residuals with up to six lags (although the third and fifth lags are almost

statistically significant); a seventh lag is actually significant but "is likely spurious" (Franzese 1996: 48); indeed, there is no theoretical justification of which I am aware for such a distant lag being significant. As with R-S's models, these variables should be positively related to deficits.

$DUE_{i,t}$; $UE1L_{i,t}$ Change in (internationally comparable) unemployment rate; 1-period lag in level of unemployment. The first difference and lagged level forms are used to correct for the unemployment variable's near-cointegration (see Franzese 1996: 48 for details). As with R-S's models, positive values should be associated with increases in DDEBTX.

$GROWTH_{i,t}$ Real GDP growth rate. As with R-S's models, increases in GDP growth should be associated with lower deficits due to higher tax revenues generated.

$DXRIG_{i,t}$; $DXRIGDX_{i,t}$ Difference between expected real interest and growth rates; DXRIG times the lagged debt/GDP ratio. DXRIG is included because according to the tax-smoothing theory, governments should borrow when the "the presently expected annualized-growth-rate exceeds the analogous expected real-interest-rate" (Franzese 1996: 3-4), i.e., when future growth is expected to pay back the borrowed amount. Thus DXRIG is intended to control for the expected trend in the debt/GDP ratio (Franzese 1996: 6). DXRIGDX is used to measure debt-service shocks, analogous to R-S's DRB variable above. Expected growth rates are from estimated data using fitted values in a regression; estimated data is used to extend the available interest-rate data (see Franzese 1996: 5,6).

$TRADE_{i,t}$ Trade openness (exports + imports as % of GDP). The basic intuition of the tax-smoothing models that temporary shocks should be deficit-financed and permanent changes adjusted to "applies equally to the open economy" (Franzese 1996: 4). According to the neoclassical economic perspective, increased trade openness implies higher long-run growth and thus provides an incentive for governments to run deficits now (Franzese 1996: 38; see 37-38 for an extended discussion), and thus greater openness should be associated with higher debt.

$TOT_{i,t}$ Terms of trade (export prices/import prices). Increases in TOT, which is subject to short-term fluctuations in exchange rates, should be associated with lower deficits, since it implies greater short-run tax revenues.

$TOTT_{i,t}$ TRADE*TOT. The debt impact of changes in terms of trade should be greater in more open economies; hence the interaction term. Increases in TOTT should be associated with lower debt, since tax revenues increase.

$OILDEP_{i,t}$ Oil imports as a % of inland supply 1973-84. The oil crisis certainly was a shock expected to be temporary, and oil price increases therefore should be expected to have been deficit-financed (Franzese 1996: 4).

Other controls include:

$DDABROAD_{i,t}$ Variable equal, for each country-year, to the average deficit in that year for all other countries in the sample. Franzese reports that in all models he tested, time-period fixed-effects could not be rejected using Chow tests and/or the F-tests of auxiliary regressions of the models' residuals on time-period dummies less one. Hence he includes DDABROAD instead of 34 time-period dummies (the data set covers the years 1956-90 = 35 years) because "it does not necessarily wipe out all common variation from year-to-year" . He argues persuasively that if year-specific factors affect debt performance, it is

better to model them substantively by allowing the average deficit abroad to capture that common variation.

GR, SP, DICT$_{i,t}$ Dummy variables for Greece and Spain, and dummy variable for dictatorship (which is limited to Greece 1970-74 and Spain 1965-76). Without these dummies many of the regression results would be spoiled, but including the two countries along with the controls does not change the results much (Franzese 1996: 50).

Franzese tests a number of theories involving political variables supposed to affect deficits. Following is a brief explication; consult Franzese (1996) for fuller treatment.

Democracy and Fiscal Illusion. Buchanan and Wagner (1977) argue that voters are "fiscally illuded" and do not consider that current deficits represent future taxes, and thus will reward politicians who spend more (implying higher deficits) with reelection (Franzese 1996: 8). Non-democracies thus should run lower deficits than democracies, but the DICT variable could not be used for that interpretation and instead must serve as a control for the differences between the democracies and dictatorships in the sample (see Franzese 1996: 8-9).

Thus Franzese uses **STAX1L$_{i,t-1}$**, a 1-period lag of the percentage of tax revenue from goods, services, income, and profit taxes, as a way to capture the proportion of "visible" taxes to "less-visible" ones (i.e., as a percentage of total tax revenue) (Franzese 1996: 9). A more complicated tax structure increases this fiscal illusion, and thus deficits should be higher under complicated tax regimes (Franzese 1996: 8); if voters are not in fact fiscally illuded, there should be no relation between STAX1L and deficits. The variable is lagged one period presumably to allow time for voters to reward politicians with election who then run higher deficits in the following year.

Inter- and Intragenerational Transfers. Cukierman and Meltzer (1989) and Tabellini (1991) note that government debt represents a transfer of wealth from the future to the present (Franzese 1996: 9). Thus the poor and the old would be most in favor of higher spending (implying deficits), since the former may have little hope of becoming rich and the latter may not live long enough to have to repay the debt. In addition, "as more unequal distributions of wealth are associated with larger numbers of poor relative to the numbers of the wealthy, more inegalitarian democracies should also exhibit greater tendency to run deficits" (Franzese 1996: 9).

According to the theory, senior citizens without children should most favor debt, while those with grandchildren should least favor it (since the latter are less likely to wish to leave negative bequests to their descendants). Similarly, the extent to which the middle-aged favor debt accumulation should depend on which family link--the link of middle-aged persons to their parents versus the link of middle-aged persons to their children--is more numerous in the population. A way to capture these dynamics is to use **OY$_{i,t}$**, the ratio of population 65 and older to population 16 and younger. Increasing values of OY should correspond not only to the presence of higher proportions of older people in the population but also to the first link noted for the middle-aged being stronger; and so higher values of OY should be associated with higher debt (Franzese 1996: 10).

The average wealth of the economy can be measured by **LRGDPC$_{i,t}$**, the natural log of real GDP per capita. Countries with higher average wealth per capita should run lower deficits, presumably because on average, their citizens are less poor (Franzese 1996: 10).

One can capture income distribution using **GINIXX$_{i,t}$**, a GINI index of income inequality. Franzese has expanded the available GINI data to create GINIXX, which varies by country and by year, by filling in missing years by linear extrapolation and using auxiliary regressions to forecast back and forward in time (see Franzese 1996: 10, n. 13).

Partisan and Electoral Political Budget Cycles. Nordhaus (1975) and his followers argue that politicians will employ expansionary policies (implying higher deficits) as elections near, hoping that voters' attention will focus on those policies in evaluating whether they have been made better off under the current government (Franzese 1996: 11). This electoral (or opportunistic) political budget cycle can be assessed by **ELE$_{i,t}$**, an indicator equal to one for the pre-election year (but which is adjusted to the calendar-year debt/GDP observations and thus produces values ranging from [0,1+]. See Franzese (1996: 12, n. 15) for details).

Another version of the theory is the Partisan Political Budget Cycle (first noted by Hibbs (1977)), which argues that left and right parties differ in their desired spending levels (Franzese 1996: 11). Left governments are thought to favor Keynesian, expansionary policies and redistribution, while right governments are supposed to favor business interests and fiscal restraint. The partisanship of governments can be captured by a **COG$_{i,t}$** index, the partisan center of gravity of government. COG, which ranges from 0 to 10 (10 - right-wing ideology) and is an average of several existing left-right indices, is a weighted average of partisanship according to the number of cabinet ministers in each party in government (Franzese 1996: 12-13, and n. 16, n. 17). That is,
$COG = \Sigma_{i=1}^{k}(x_i/n)p_i$,
where i = party i; k = the number of parties in government; n = the total number of ministers in the cabinet; x_i = the number of ministers in party i; and p_i = party i's partisanship score. According to the Partisan Political Budget Cycle, higher values of COG should correspond to lower debt levels.

Public Debt as Commitment of Future Governments. A government can affect the spending constraints of future governments by varying the amount of public debt it accumulates. Alesina and Tabellini (1990) argue that "the incumbent is more likely to accumulate debt the greater the 'distance' (in terms of desired spending compositions) of potential replacements from itself and the more likely the incumbent is to be replaced" (Franzese 1996: 14). Persson and Svensson (1989) suggest a similar model but one in which it is the level of debt which varies and does so according to partisanship: low-spenders (presumably rightist governments) run deficits when facing replacement by a left government; and high spenders (presumably leftist governments) actually run surpluses when facing replacement by a right government (Franzese 1996: 14).

The variable **RR$_{i,t}$**, replacement risk, operationalizes the Alesina and Tabellini theory. RR = (a 9-year, centered moving standard deviation of COG = **SDAPF9**) times (1 over duration in years of incumbent government = **HRY**). The expected distance of a replacement government is measured by the standard deviation measure: Centered on time (t), it is intended to capture the polarization across governments, using a mixture of adaptive expectations (past governments' partisanship) and perfect foresight (future governments' expected partisanship). (See Franzese 1996: 14-5 and n. 23 for discussion.) The expected probability of losing office can be estimated in a manner also exploiting adaptive expectations, by calculating the duration of every post-war government in years

and calling the inverse of that measure the "hazard rate" (HRY), the probability of losing office in a year (Franzese 1996: 14-15). Thus the product of these two measures gives an estimate of replacement risk. Higher values of RR should be associated with increased public debt, since they represent greater ideological distance of the expected replacement government and/or greater probability of losing office.

Persson and Svensson predict that only right-leaning incumbents run higher deficits; this argues for the inclusion of COG, RR, and their interaction, **RR*COG$_{i,t}$**, RR times COG. Because COG ranges from 0 = left and 10 = right, higher values of COG represent more right-leaning governments, and thus the theory expects a positive sign for RR*COG. This theory expects a negative sign, however, for RR and COG (see Franzese 1996: 15 and n. 26 for discussion). (Actually, Franzese's final results show this variable's effect accords more with the partisan political budget cycle theory than with the debt as commitment theory.)

Distributional Conflicts and Wars of Attrition. This theory argues that the more fragmented and polarized governments are the more difficult it will be to find an adjustment scheme that fairly distributes the cost of debt adjustment on all parties. This is essentially equivalent to the Prisoners' Dilemma theory noted above in the R-S models. **NOP$_{i,t}$**, the raw number of parties in government, is a straightforward way to operationalize fractionalization (as discussed above; and see below); it allows a substantive interpretation--a 1-party increase in NOP leads to an X % increase or decrease in debt. **NOPDX$_{i,t}$**, NOP times the lagged debt/GDP ratio, is intended to discern whether the number of parties and the level of inherited (last year's) debt affects the speed of adjustment. Both coefficients are expected to be positive, according to the same logic as presented above in the discussion of the R-S models.

The polarization within governments can be measured using **ADWI$_{i,t}$**, the maximum absolute ideological deviation within government. The measure is the difference on the 0-to-10 partisanship scale of the two most polarized parties in government. Naturally, ADWI is 0 under single-party governments. **ADWIDX$_{i,t}$**, ADWI times lagged debt/GDP ratio, like NOPDX, attempts to determine whether higher polarization affects the speed of adjustment given the level of last year's debt. Both ADWI and ADWIDX therefore are expected to have positively-signed coefficients (Franzese 1996: 16-17).[15]

Central Bank Independence. Alesina and Perotti (1995) note that a government may accumulate less debt the more independent is the nation's central bank, because 1) independent central banks make it less easy for governments to reduce debt through issuing inflation, and 2) independent central banks usually are "not forced to buy government debt which the market will not absorb" (Franzese 1996: 18). **CBI$_{i,t}$**, Central Bank Independence, is a scaled average of several existing indices, and is decade-variant (Franzese 1996: 18).

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[15] Franzese also tests models substituting the following for NOP and ADWI: ENOP, the effective number of parties (where larger parties are given more weight (see Laakso and Taagepera 1979)), and SDWI, the standard deviation of cabinet ministers' polarization (again, giving emphasis to the largest parties). He finds the NOP and ADWI measures to be superior, however (Franzese 1996: 28, 35), and thus I report his results using the latter measures only.

Geographically Dispersed Interests. Politicians with geographically concentrated constituencies may overestimate the benefit from spending in their districts (relative to the national optimum a benevolent, all-powerful social planner would spend in each district), while underestimating the true cost, since that cost tends to be spread across the nation (Franzese 1996: 19). For federal systems, $\mathbf{FED_{i,t}}$, the natural log of number of federal districts, should be positively correlated with deficits if this theory is correct--countries with a greater number of concentrated constituencies should run higher deficits. (FED can also be interpreted as contributing to fiscal illusion, since in federal systems there are more taxing authorities (Franzese 1996: 19.))

Other measures are available as well that apply both to federal and unitary systems. Ethnic and agrarian parties happen to have geographically concentrated constituencies; $\mathbf{AE_{i,t}}$, the proportion of cabinet members from agrarian or ethnic parties, should be positively correlated with deficits. $\mathbf{LAE_{i,t}}$, the natural log of AE, is included because typically 100% of Belgium's cabinets are composed of A-E parties, and Belgium was found to be an outlier single-handedly influencing the regression line; including LAE is an attempt to model this non-linearity and thus to remove the bias from AE's coefficient due to Belgium.

Presidents tend to represent larger constituencies (i.e., the nation) than do the party members of cabinets or legislators, and so presidential systems may be associated with lower deficits. $\mathbf{PRES_{i,t}}$ therefore is a dummy variable equal to 1 for presidential systems: Finland, the French Vth Republic, and the US.

Finally, one can count the number of electoral districts as a basis for concentrated constituencies. $\mathbf{ENOC_{i,t}}$, the natural log of the effective number of constituencies, adjusts for geographic size by reducing the number of constituencies counted for small districts, since it is less likely to expect district-targeted benefits to remain within their boundaries than they would in larger districts (see Franzese 1996: 20-21 for details).

Franzese conducted Wald chi-squared tests for every group of variables corresponding to the theories discussed above, using the economic model as the restricted one, and concluded that for each group, one can reject the restriction (at a minimum of the .05 level)--each group of variables should be added to the economic model (Franzese 1996: 27-33). Since the variable groups do not represent restrictions of each other (they are not nested models), the question remained which one(s) to add to the economic model. Franzese used the J-test method of Davidson and MacKinnon (1981) to test competing, non-nested models, and found, with four exceptions of relatively small importance, that none of the models can be rejected, and hence he piled all the variables discussed above into one regression model (Franzese 1996: 33-5).

As in R-S's data set, panel heteroskedasticity was confirmed by plots of externally studentized residuals vs. predicted values, and by regressing the squared residuals from Franzese's main (OLS) model on a constant and a set of country dummy variables less one. The overall F statistic of that auxiliary regression is a test of panel heteroskedasticity against the null of homoskedasticity; and the F was "invariably high, implying panel-type heteroskedasticity at at least the .0001 level" (Franzese 1996: 50). Thus, Franzese used a very nice panel weighted least squares (WLS) setup, by running OLS on his main model, regressing the squared residuals on the country dummies (less one), saving the predicted values of that regression, and using the inverse of their square root as panel weights for

WLS (Franzese 1996: 50-51). The implicit theory here is that the residual variance varies by country due to left out variables (either country-specific ones or cross-country ones or both, despite the presence of 34 independent variables in Franzese's main model!), and/or to other idiosyncratic (unmodeled), country-specific factors. A scatterplot of the absolute values of the residuals vs. the predicted values shows that the country dummies used as weighting variables are a good model of the non-constant variance.

Finally, Franzese used the panel corrected standard errors (PCSE's) suggested by Beck and Katz (1995), based on the weighted residuals from this WLS specification. Beck and Katz's (1995: 4) Monte Carlo experiments showed that PCSEs are accurate in the presence of either contemporaneously correlated or panel heteroskedastic errors (not in the presence of serially correlated errors, the elimination of which is intended by using a lagged dependent variable) (Beck and Katz 1995: 4).[16] This is especially important in the present case because even the use of panel weighted least squares does not completely eliminate the panel heteroskedasticity.

[16] Since we can thus assume that the errors of such a least squares estimation are serially independent, the variance-covariance matrix of the errors takes the form (Beck and Katz 1995: 4):

$W = S \otimes \mathbf{I}_T$

where S is the NxN variance-covariance matrix of the residuals, and $\otimes$ denotes the Kronecker product.

Let **E** denote the TxN matrix of the OLS (or WLS) residuals. **E**'**E**/T provides a consistent estimate of S. PCSE's are thus given by the square root of the diagonal of

$(\mathbf{X}'\mathbf{X})^{-1}\mathbf{X}'([\mathbf{E}'\mathbf{E}/T] \otimes \mathbf{I}_T)\mathbf{X}(\mathbf{X}'\mathbf{X})^{-1}$.

Thus we arrive at Franzese's main result: **Table 5**:

Valid Cases:	562	Dependent Variable:	DDEBTX
Missing Cases:	138	Deletion	Listwise
Wtd Total SS:	1416.798	Degrees of Freedom:	527
Total SS:	4799.536	Degrees of Freedom:	527
Wtd R-Squared:	0.630	Wtd Rbar-Squared:	0.606
R-Squared:	0.484	Rbar-Squared:	0.451
W Residual SS:	524.116	Wtd Std Error Est:	0.997
Residual SS:	2476.325	Std Error of Est:	2.168
DW-Stat:	2.233	Wtd DW-Stat:	2.004

Variable	Estimate	PCSE Std Error	t-value	Prob >|t|	Standardized Estimate	Reg Std Error
ONE	9.392870	5.631270	1.667984	0.0959	3.844590	5.585674
DDEBTX1L	0.311934	0.048998	6.366216	0.0000	0.312561	0.039749
DDEBTX2L	0.150831	0.043053	3.503388	0.0005	0.158681	0.036663
DEBTX1L	-0.045296	0.009813	-4.615745	0.0000	-0.635115	0.009332
DUE	0.610319	0.103216	5.913008	0.0000	0.217040	0.111126
UE1L	0.109654	0.037847	2.897319	0.0039	0.253669	0.035865
GROWTH	-0.123075	0.032473	-3.790094	0.0002	-0.219065	0.034713
DXRIG	-0.013886	0.048784	-0.284633	0.7760	-0.018698	0.049635
DXRIGDX	0.003075	0.001384	2.222105	0.0267	0.132198	0.001349
TRADE	9.395522	3.210695	2.926320	0.0036	1.911104	3.589715
TOTT	-8.622126	3.029359	-2.846189	0.0046	-1.768032	3.357509
TOT	1.606593	0.933871	1.720359	0.0860	0.682186	1.085103
OILDEP	0.302935	0.194781	1.555259	0.1205	0.067800	0.207695
NOP	-0.479372	0.174798	-2.742436	0.0063	-0.476576	0.190194
NOPDX	0.010734	0.004726	2.271498	0.0235	0.340341	0.005011
ADWI	0.139199	0.142590	0.976218	0.3294	0.121350	0.150541
ADWIDX	0.001093	0.004385	0.249306	0.8032	0.030060	0.004290
LRGDPC1L	-0.879510	0.508641	-1.729136	0.0844	-3.265752	0.504866
OY	-0.850974	0.637005	-1.335900	0.1822	-0.194359	0.704369
GINIXX	2.217631	1.986298	1.116465	0.2647	0.324815	2.064174
ELE	0.313340	0.183560	1.707019	0.0884	0.054893	0.207055
COG	0.164619	0.067851	2.426191	0.0156	0.401136	0.076477
RR	0.911483	0.769336	1.184766	0.2366	0.147360	0.798307
RRCOG	-0.201703	0.120931	-1.667918	0.0959	-0.200115	0.130492
STAX1L	-0.027795	0.009533	-2.915606	0.0037	-0.779688	0.010630
CBI	-2.655615	1.015202	-2.615849	0.0092	-0.652680	1.021059
PRES	-0.816401	0.261674	-3.119912	0.0019	-0.159281	0.301063
FED	0.261169	0.156992	1.663581	0.0968	0.213902	0.170777
LAE	12.874233	7.509306	1.714437	0.0870	0.832105	7.983689
AE	-9.003024	5.392427	-1.669568	0.0956	-0.788996	5.701912
ENOC	0.029057	0.116849	0.248668	0.8037	0.038710	0.124522
GR	-1.827192	0.521274	-3.505245	0.0005	-0.175644	0.495564
SP	-3.999899	0.914910	-4.371905	0.0000	-0.436683	0.873401
DICT	1.663350	0.618200	2.690633	0.0074	0.144217	0.619198
DDABROAD	0.238366	0.062615	3.806856	0.0002	0.166700	0.065705

Joint Hypothesis Tests

War of Attrition: $p(\chi^2) = .0035$	Institutions: $p(\chi^2) = .0038$
Generational Transfers: $p(\chi^2) = .0000$	# Geog Const: $p(\chi^2) = .0120$
Pol./Part. Cycles + Comt: $p(\chi^2) = .0377$	#GC less PRES: $p(\chi^2) = .0957$
Fiscal Illusion: $p(\chi^2) = .0032$	F. Ill., Inst., #GC: $p(\chi^2) = .0000$

Franzese finds that the theories tested receive from mixed-to-good support in this specification. In particular, OY and RRCOG have the wrong sign and the former is insignificant (and RRCOG significant only at the .10 level); GINI just misses significance; ENOC is correctly signed but insignificant; and ADWIDX's coefficient is positive as expected but near zero and insignificant.

One possible objection (which Franzese himself noted): In testing for country-specific fixed-effects, Franzese rejected the use of a Chow test (an F-test where an

unrestricted model, the main specification plus country dummies, is tested against a restricted model, the specification without the dummies) for country-specific fixed-effects, favoring instead a test where he regressed the residuals from the WLS specification on the set of country dummies less one, and viewed the F-test of that auxiliary regression as a test of the null hypothesis that the country dummies contribute nothing to the explained variance (Franzese 1996: 49). The F was always small for all models tested, meaning one could not reject the null hypothesis. As Franzese himself notes, however, a Chow test did reject in favor of the country dummies in some specifications. Indeed, in his main model, the Chow-test F is 1.827, whereas the tabled F(.95,16,511) = 1.72--thus we can reject the hypothesis that the unrestricted model's coefficients, i.e., all the country dummies, equal 0. However, the test in this specification (inexplicably) required the use of the unweighted residuals, whereas it is the weighted residuals which are of interest, since we are investigating a weighted model. The main difference between the two specifications is that GDP per capita is not significant in the fixed-effects model.

However, I will accept Franzese's model as the new default one (excepting the changes I will make below) for the remainder of this paper. Given the overall acceptability of this model (Franzese conducted extensive preliminary testing and auxiliary regressions in arriving at this model), the rather considerable data and statistical issues involved with modifying it,[17] and not of least importance the limited resources available for the present paper (the vast bulk of which were devoted to revising some of Franzese's measures and adding others (see below) and to writing and debugging the Gauss program I used to employ the PCSEs), for present purposes it is not unreasonable to use Franzese's model as the new base model against which to make comparisons.

Extensions of Franzese's Study While the other theories certainly are important, for the remainder of this paper I will focus on the Distributional Conflicts/War of Attrition model and accept the other variables as given. I extend and alter Franzese's study in the following manner. In sum, I use what I believe is a more appropriate specification of the number of parties in government, with appropriate changes in related variables; I test for the post-1973 shifts that R-S emphasize; and I add two new variables, minority government and (two conceptions of) bicameralism.

Measurement of the Number of Parties in Government, Their Polarization, and Related Measures For the number of parties measure, Franzese (1996), Borrelli and Royed (1995), Heller? (1994) and most other studies which have as their dependent variable deficits measured on a calendar or fiscal year basis take a weighted average (according to the proportion of the time period each government was in power) of the number of parties in government during that time period, if that number changed within the time period. Alternatively, studies have simply counted the number of parties as that of the government in power for a majority of the year. In devising an alternative measure of the number of parties, the **Budgetary Number of Parties (BNOP)**, I (like Franzese) count the raw number of parties in government (as opposed to effective number of parties)

[17] For example, it is beyond my present level of competence to perform the J-tests Franzese used to determine which groups of variables could be rejected as being nested within alternative models. Hence I cannot verify the results of Franzese's J-test, nor do J-tests for my models discussed below; this issue will have to remain for future research. Fortunately, I make only relatively modest changes to Franzese's model, which decrease the likelihood that the J-test results would change significantly.

on the assumption that each party has a veto over spending priorities (Tsebelis 1994: 756). As Tsebelis has argued, a small party might be numerically excludable within a coalition, but doing so is likely to bring down the government (small parties may be pivotal).[18] However, unlike other studies I count the number of parties of the government that was likely in power at the time the budget was supposedly presented to parliament for a final vote, according to the rules regarding the budget process (Kotin 1996).[19] Where more than one government existed in any country-year and it was not clear which government passed the budget, if a government change took place near the time of supposed submission of the budget to parliament *and* the reason for the change was a lack of parliamentary support or dissension within the government, I have used the (unweighted) average number of parties measure, as an attempt to account for the possibility that both governments actually shaped the budget proposal. I assume, however, that if the government ended because of internal dissension or lack of Parliamentary support and the new government took over soon after the official deadline (within about 1 month) for parliamentary receipt of the government's budget proposal, then the new government was more likely to have had the final say over the budget presented to Parliament, and I usually count the number of parties as being that of that government. In cases where the budget came due very near the termination of a government and the reason for government change was elections, I have tended to keep the coding as that of the government in power at the time of budgetary submission, on the assumption that this budget had more legitimacy than in the previous cases in the eyes of the new government and parliament. On the other hand, in Denmark, "on several occasions, elections have been called, when governments have been unable to secure support for the Finance Bill" (OECD 1995: p. 99). In cases when more than one government likely was responsible for a year's budget, I have taken an unweighted average of the number of parties in those governments; e.g., if the debt data's time of recording was calendar year and the fiscal year began in July. Obviously, these decisions involve judgment calls; details will follow at a later date in an appendix.[20]

For example, I code BNOP for the US in 1969 as 1 (Congress and President Democratic; ideologically congruent and hence code 1) instead of 2 (Congress Democratic, President Republican), because although the latter government took office in January 1969, the budget in effect until Sept. 30, 1969--the bulk of the 1969 calendar

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[18] Of course, one would expect the political cost of excluding one or more coalition partners to vary over time and across countries; a more accurate approach would investigate empirically the intra-coalition bargaining process and outcome. Such an undertaking would be very labor-intensive, however, for any significant number of countries.

[19] Of course, a more accurate coding would specify which governments *actually* passed which budgets (see Bawn et al. (forthcoming) for an excellent effort in this regard), but such an undertaking exceeds the resources available for this paper. Obviously, governments (and parliaments) frequently do not adhere to official rules regarding the budgetary process. Also, such rules almost surely changed within each country during the postwar period; whereas my coding, due to resource constraints, assumes that they remained constant (I use OECD (1995) for the rules on the budgetary process). It is also possible that my coding, using this time-invariant source, may actually be less accurate than taking a weighted average of parties in government per year. Further research will sort this out.

[20] Note that Woldendorp et al.(1993) and Lane et al. (1991) conflict sometimes considerably on the number of ministers in government. I have used the latter source because of resource constraints (i.e., it is far easier to use for coding), but the former source is likely to be more accurate given that its authors cross-checked their work with Lane et al. and other sources--a project for the future.

year--was passed by the first (1968) government, which was all Democratic. (The 1-2 coding is used throughout the US sample.) Thus this coding sometimes results in retaining a previous year's coding one year longer than in most other studies.[21] As another example, Sweden's fiscal year begins July 1 but governmental changes frequently occurred in October. In 1952, a 1 party (SDA) government passed the budget bill covering the first half of 1952; a 2 party government (SDA + BF/CP) passed the bill covering the last half. .5*1 + .5*2 = 1.5 parties in government.

Several of Franzese's political variables must be modified as a result of the BNOP specification of fractionalization. The ADWI score changes to reflect the different counting of number of parties and becomes the **Budgetary Absolute Deviation Within Government (BDWI)**. BDWI differs also from ADWI in that the president's partisanship score is not included for Finland and the French Vth Republic, since I argue that these presidents have no role in (ordinary) budget-making. Similarly, COG becomes **BCOG**; RR becomes **BRR**; SDAPF9 becomes **BSDAPF9**; AE becomes **BAE**; RRCOG becomes **BRRXBCOG**; and DICT becomes **BDICT**.

Other modifications I eliminate PRES on the grounds noted above, that Finland and France's presidents have little say over the budget, at least in periods of normal policy making. The US president does have a veto over the budget; but I am not sure that modeling it with a dummy variable = 1 for the US and 0 otherwise is the best approach. This is an open issue, which I defer to future research. Although PRES is statistically significant in Franzese's models, it is not necessarily so that it is these countries' presidentialism that is driving these results. On the other hand, although only the US president has a veto, it is possible that Finland and France's presidents exercise "leadership" or "moral support"; or more likely, perhaps the threat of parliamentary dissolution by the president operates here (in contrast to Tsebelis 1994: xx).

I eliminate OILDEP because (as Franzese himself argued after his paper was written!) its effects should show up in Unemployment, Growth, Terms of Trade, and Interest Rates (Franzese, personal communication, Oct. 10, 1996).

Also on Franzese's and others' recommendation, I drop LAE and substitute **BAESQRD**, Budgetary Agrarian-Ethnic Parties Squared, to try to capture the non-linear effects Franzese discovered of the Belgian government's 100% of cabinet members coming from ethnic parties. (Whether the square is the best order polynomial to use is an issue requiring further research, but it is a reasonable first attempt.) Franzese's main model with these variables substituted and eliminated follows.

[21] In coding BNOP I have made corrections where necessary for differences in recording basis of debt/GDP ratios (calendar vs. fiscal year). Borrelli and Royed (1995) is the only study I have seen which also incorporates a "lag" in the NOP coding (which I discovered after having invented the idea independently); however, the main political variable they use (an index based on a number of political variables) is problematic.

Table 6:

Valid Cases:	563	Dependent Variable:	DDEBTX
Missing Cases:	137	Deletion Method:	Listwise
Wtd Total SS:	1397.908	Degrees of Freedom:	530
Total SS:	4800.269	Degrees of Freedom:	530
Wtd R-Squared:	0.622	Wtd Rbar-Squared:	0.599
R-Squared:	0.473	Rbar-Squared:	0.441
W Residual SS:	528.719	Wtd Std Error Est:	0.999
Residual SS:	2528.469	Std Error of Est:	2.184
DW-Stat:	2.214	Wtd DW-Stat:	1.964

Variable	Estimate	PCSE Std Error	t-value	Prob >|t|	Standardized Estimate	Reg Std Error
ONE	10.508364	5.564466	1.888477	0.0595	4.377858	5.545176
DDEBTX1L	0.333224	0.048039	6.936530	0.0000	0.334017	0.039283
DDEBTX2L	0.135638	0.042959	3.157371	0.0017	0.142747	0.036548
DEBTX1L	-0.045151	0.009170	-4.923962	0.0000	-0.637313	0.009255
DUE	0.582084	0.103597	5.618758	0.0000	0.210537	0.111086
UE1L	0.064464	0.034945	1.844700	0.0656	0.151453	0.033027
GROWTH	-0.123713	0.032898	-3.760479	0.0002	-0.224365	0.034595
DXRIG	0.008711	0.046852	0.185934	0.8526	0.011915	0.047921
DXRIGDX	0.002316	0.001341	1.727250	0.0847	0.099845	0.001330
TRADE	10.409543	3.212117	3.240711	0.0013	2.106281	3.539150
TOTT	-9.063759	3.001502	-3.019741	0.0027	-1.843158	3.298079
TOT	1.708006	0.944840	1.807720	0.0712	0.737711	1.080879
BNOP	-0.280547	0.187831	-1.493617	0.1359	-0.291978	0.205595
BNOPDX	0.010132	0.003898	2.599678	0.0096	0.357018	0.004350
BDWI	-0.009520	0.136971	-0.069501	0.9446	-0.008782	0.144955
BDWIDX	0.000365	0.003896	0.093681	0.9254	0.010319	0.003871
LRGDPC1L	-1.116891	0.495326	-2.254863	0.0245	-4.223385	0.494573
OY	-0.171614	0.650877	-0.263665	0.7921	-0.039640	0.693608
GINIXX	1.298138	1.971486	0.658457	0.5105	0.193421	2.001181
ELE	0.334483	0.186325	1.795153	0.0732	0.059015	0.206612
BCOG	0.193448	0.068957	2.805329	0.0052	0.477462	0.077311
BRR	0.320004	0.700215	0.457008	0.6479	0.053438	0.727957
BRRXBCOG	-0.086939	0.108327	-0.802566	0.4226	-0.089802	0.117560
STAX1L	-0.022054	0.009753	-2.261177	0.0242	-0.636317	0.010766
CBI	-2.640875	0.981677	-2.690166	0.0074	-0.661993	1.019965
FED	0.293001	0.150432	1.947723	0.0520	0.249475	0.166128
BAESQRD	-1.531292	1.669363	-0.917292	0.3594	-0.146075	1.861962
BAE	1.088436	1.635908	0.665340	0.5061	0.121815	1.820104
ENOC	0.045432	0.108476	0.418818	0.6755	0.062480	0.121647
GR	-1.698554	0.509363	-3.334661	0.0009	-0.166178	0.490671
SP	-3.136938	0.868010	-3.613943	0.0003	-0.339230	0.843878
BDICT	0.994303	0.570968	1.741434	0.0822	0.088466	0.563598
DDABROAD	0.281570	0.059851	4.704522	0.0000	0.200220	0.063092

Joint Hypothesis Tests		
	War of Attrition: $p(\chi^2) = .0036$	Institutions (no PRES): $p(\chi^2) = .0202$
	Generational Transfers: $p(\chi^2) = .0003$	# Geog Const (no PRES): $p(\chi^2) = .1944$
	Pol./Part. Cycles + Comt: $p(\chi^2) = .0122$	
	Fiscal Illusion: $p(\chi^2) = .0121$	

The results are essentially unchanged from Franzese's model except for the following. We see that the joint hypothesis for Geographically Concentrated Interests is no longer significant ($p(\chi^2) = .1944$) without PRES, but FED by itself barely misses the .05 level. This suggests that the squared BAE may be misspecified (it has the wrong (negative) sign, unlike LAE), unless agrarian-ethnic parties facing very high levels of debt (which the BAESQRD variable should capture) begin to show serious fiscal restraint. BAE is positive as expected (unlike AE). However, both variables are very insignificant, unlike AE and LAE; future work is needed to investigate other powers of BAE to determine whether it has a real relationship with deficits. ENOC--which operationalization is based

on an intuition about the effective number of electoral constituencies and does not have the constitutional backing that federal districts do--does not perform well in several of Franzese's models as well. The results more strongly support the Distributional Conflicts/Prisoners' Dilemma theory, since the main change is that BNOP (whose sign is incorrect, as was NOP's) is now significant at only the .135 level (as compared to NOP's .006). But BNOPDX remains positive as expected and essentially unchanged from NOPDX, suggesting that the level of inherited debt interacts strongly with the number of parties despite possible measurement error.

Given the instability found in R-S's model 2 (Table 1, Columns 3, 4, and 5) due to the POL corrections and the use of DBYCentrGv, it seems prudent to check the model in Table 6 above substituting actual IMF debt/GDP for the dependent variable. The results (not shown) are essentially the same, with two exceptions: BNOP is still negative but is now statistically significant at the .034 level. This jibes better than does Table 6 with Franzese's findings, that at low levels of debt (below about 45% of GDP), increasing fractionalization may actually contribute to lower deficits (as shown by Franzese's graphical representation of the confidence intervals (1996: 39-40)). The logic Franzese advaces here is that fractionalization produces inaction, and so when debt is low, it stays low; when it is high, it remains high. (This idea is in constrast to one which argues that fractionalized governments will logroll to keep everybody happy; but on the other hand, one can recall episodes where members of fractionalized governments conspired to foil the plans of their partners in power.) The other exception is that the dummy variable for Greece is three times smaller and no longer statistically significant; but this may be due to the fact that Greece has only 8 observations of actual IMF debt/GDP as compared to Franzese's 19.

Minority Governments Now we have an opportunity to confirm or to modify the revised R-S and E-O finding that minority governments contribute least towards deficits. I create a dummy variable, **Budgetary Minority Government (BMNG)**, equal to 1 for minority governments and 0 otherwise, coded according to the same criteria as BNOP. BMNG can take on the value .5, however, when more than one government passed a year's budget and only one of them was a minority government (the 1952 Swedish government example used above had BMNG = .5*1 + .5*0 = .5).

Several studies have investigated the effect of minority governments on budget deficits, but as Hallerberg and von Hagen note, "There is little consensus in the literature...about how...[minority] governments affect fiscal policy" (1996: 21). Tsebelis (1995) argues that governments in parliamentary democracies tend to have advantages vis a vis parliament with respect to the status quo because of positional and institutional weapons (agenda setting powers, etc.); and single-party minority governments are similar to single-party majority governments (Tsebelis 1994: 755; Tsebelis 1995: 11), because they usually are located centrally in the policy space created by the other parties in the legislature (Tsebelis 1995: 12). For the reasons noted above, others argue instead that minority governments are weaker.

Bicameralism Another possible influence on deficits are structural or institutional differences such as the number of chambers in the parliament. William Heller (1994) argues that bicameral countries will tend to have higher deficits, because the second chamber has (at least) slightly different interests than the first (due to electoral-institutional

and constituency differences, for example), and those interests will usually be configured such that the second chamber's agreement results in higher spending and by implication, higher deficits (see Heller 1994: 40).[22] Heller theorizes that because each chamber has constitutional backing--in Tsebelis' terms, each is an institutional veto player--the effect of a second chamber should be larger and more robust than that of the number of parties in government (Heller 1994: 27). Tsebelis (1994), on the other hand, argues that whether or not the second chamber will be a veto player--that is, whether it will influence any bargaining to take place over the budget--depends on whether or not the two houses are ideologically congruent. If they are congruent, then the second chamber will affirm the first's spending priorities, and in practice will not act as a veto player. In this case, the number of veto players is the number of parties in government, because the government controls a majority in parliament and party discipline ensures government control of its parliamentary majority. (Of course exceptions exist, such as in Italy before 1988 when the secret vote allowed members of parliament to embarrass their own government; and the level of party discipline varies and in the US is much lower.)

To address Heller's theory I create a variable **BIC**, which is coded 1 for all effectively bicameral countries, regardless of the second chamber's partisan composition.[23] Like Heller and Tsebelis, by "effectively" bicameral I mean that both chambers must have an *inviolable,* not merely suspensive, veto over financial legislation.[24] The bicameral countries are Australia, Belgium, Germany,[25] Italy, Japan, the Netherlands, Switzerland, and the US.

Tsebelis' theory calls instead for the absence of BIC and for a Budgetary Veto Players variable (**BVP**), which is the same as BNOP except for periods when an effective second chamber is controlled by a majority different than the first chamber, in which case

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[22] Heller's model allows for bicameralism to lead to *lower* deficits, depending on the preferences of the chambers and government (Heller 1994: 13, n. 13); and indeed Stewart (1991) has found that "divided government led to lower U.S. deficits in the late 1800s" (Heller, ibid.).

[23] A drawback of this approach is that it leaves unsolved the problem that BNOP + BIC does not distinguish periods in which the same majority ruled both chambers from periods of divided house control: For Germany, e.g., BNOP = 2 and BIC = 1 throughout, despite that during 1973-82 different majorities controlled the upper and lower houses (the CDU/CSU controlled the Bundesrat (by one vote (Conradt 1978: 138)) while the SPD and FDP controlled the Bundestag). BDWI is more easily adjusted; e.g., Germany's BDWI score for 1973-83 includes the CDU/CSU majority in the Bundesrat and is thus not 2.4453 but 2.7251.

[24] See Money and Tsebelis (1992) for a discussion of the veto powers of second chambers in various countries.

[25]The coding of Germany as bicameral is open to dispute. The Basic Law (German constitution) gives the Bundesrat (upper house) an overrideable (by 2/3) veto over legislation relating to the Laender (states), but not over other legislation. Thus, if there is a budget issue that does not involve the Laender, such as foreign aid, then Bundesrat approval is not needed (personal communication, Mark Hallerberg, 9/16/96). Article 77 of the Basic Law gives the Bundestag the veto over the Bundesrat (after the deliberations of a joint committee formed specifically to discuss the bill in question). Article 110 implies that the Bundestag (lower house) may ignore Bundesrat objections to the budget; on the other hand, Article 104a states that federal laws to be executed by the Laender, and which require that the Laender pay for equal to or greater than one-fourth of the expenditure involved, require the consent of the Bundesrat. Bawn et al. (forthcoming) code Germany as unicameral, but they avoid the problem altogether by considering only items in the federal, not overall, budget. Given that my data include both the federal and state components of the budget, and my lack of knowledge about their relative proportions and the breakdown of finances subject to one or the other set of laws, and in the final analysis relying on the recommendation of Mark Hallerberg, I code Germany as bicameral. The information about the Basic Law comes from Karpen (1988); see Dalton (1989: 312) and Conradt (1978: 136-39) for additional discussion.

BVP = BNOP + 1. The result is that BVP is identical to BNOP except for the cases of Australia, Germany, Switzerland, and the US. This unfortunate operationalization (the closeness of BVP to BNOP or more precisely, of BNOP + BIC to BVP) is necessitated by the present lack of any more sensitive measure of interchamber preference differences than the left-right, 0-to10 scale. BVP for Germany is coded as BNOP except during 1973-82, when BVP = 3 instead of 2. Resource constraints prevented me from obtaining upper house data for Australia and Switzerland (the data are surprisingly difficult to obtain), and so I code them as BNOP + 1 throughout (Money and Tsebelis 1992: 32-3 say that the houses are not ideologically congruent). The US is coded VP = 3 because its parties are undisciplined (relative to the other bicameral countries in the sample) and the two chambers can be expected (admittedly with a few exceptions) to vote as separate entities (and the president has a constitutional veto over budget legislation).[26] In Belgium, Italy, Japan, and the Netherlands, the chambers are ideologically congruent throughout the sample (Tsebelis 1994: 762-3), and so for these countries BVP = BNOP.

[26]As Tsebelis argues (1994: 762), the US's number of veto players will be reduced to two or one if one can argue that in certain historical periods US parties exhibited high party discipline (his examples are the first hundred days of the New Deal and certain policy areas of the 1993-94 Congress and presidency).

Table 7 shows the results of including BIC and BMNG with the BNOP variables:

Valid Cases:	563	Dependent Variable:	DDEBTX
Missing Cases:	137	Deletion Method:	Listwise
Wtd Total SS:	1401.065	Degrees of Freedom:	527
Total SS:	4800.269	Degrees of Freedom:	527
Wtd R-Squared:	0.623	Wtd Rbar-Squared:	0.598
R-Squared:	0.479	Rbar-Squared:	0.445
W Residual SS:	528.509	Wtd Std Error Est:	1.001
Residual SS:	2498.881	Std Error of Est:	2.178
DW-Stat:	2.248	Wtd DW-Stat:	1.989

Variable	Estimate	PCSE Std Error	t-value	Prob >|t|	Standardized Estimate	Reg Std Error
ONE	11.261901	5.577977	2.018994	0.0440	4.654235	5.675744
DDEBTX1L	0.335838	0.047985	6.998754	0.0000	0.336394	0.039333
DDEBTX2L	0.139254	0.043302	3.215903	0.0014	0.146128	0.036774
DEBTX1L	-0.040087	0.009959	-4.025065	0.0001	-0.566136	0.010406
DUE	0.585595	0.104115	5.624512	0.0000	0.209527	0.112671
UE1L	0.050468	0.035111	1.437390	0.1512	0.116996	0.033593
GROWTH	-0.117752	0.033618	-3.502665	0.0005	-0.211615	0.035278
DXRIG	0.018222	0.047933	0.380159	0.7040	0.024788	0.048875
DXRIGDX	0.002098	0.001345	1.559836	0.1194	0.090619	0.001344
TRADE	11.504905	3.257913	3.531373	0.0004	2.295193	3.584471
TOTT	-9.721420	3.023967	-3.214791	0.0014	-1.949832	3.325114
TOT	1.720898	0.960354	1.791941	0.0737	0.738694	1.093593
BNOP	-0.238457	0.194140	-1.228275	0.2199	-0.245402	0.210922
BNOPDX	0.008691	0.004206	2.066639	0.0393	0.307857	0.004595
BDWI	-0.018460	0.139957	-0.131895	0.8951	-0.016819	0.148022
BDWIDX	0.000661	0.003956	0.167190	0.8673	0.018730	0.003938
LRGDPC1L	-1.252363	0.500901	-2.500221	0.0127	-4.699165	0.502693
OY	0.025823	0.721944	0.035769	0.9715	0.005920	0.732164
GINIXX	1.395980	2.104903	0.663204	0.5075	0.205866	2.145428
ELE	0.306006	0.189787	1.612366	0.1075	0.053608	0.209377
BCOG	0.180268	0.070978	2.539771	0.0114	0.441004	0.078797
BRR	0.279198	0.693706	0.402473	0.6875	0.046908	0.722506
BRRXBCOG	-0.085071	0.107890	-0.788498	0.4308	-0.088333	0.117320
STAX1L	-0.023462	0.010313	-2.274958	0.0233	-0.672967	0.011387
CBI	-2.967799	1.074278	-2.762599	0.0059	-0.732576	1.069028
FED	0.281670	0.149695	1.881632	0.0604	0.237397	0.172012
BAESQRD	-2.488969	1.744101	-1.427079	0.1541	-0.232411	1.947532
BAE	1.826224	1.696905	1.076209	0.2823	0.200421	1.880301
ENOC	0.149391	0.126799	1.178170	0.2393	0.205162	0.139106
GR	-1.703803	0.520932	-3.270681	0.0011	-0.168050	0.517725
SP	-3.208801	0.884395	-3.628243	0.0003	-0.332707	0.871864
BDICT	1.450012	0.673976	2.151429	0.0319	0.125758	0.650420
DDABROAD	0.286823	0.060211	4.763671	0.0000	0.202251	0.064234
BIC	0.204465	0.250768	0.815353	0.4152	0.061140	0.268911
BMNG	0.662534	0.530542	1.248787	0.2123	0.101570	0.529257
BMNGDX	-0.027365	0.015895	-1.721595	0.0857	-0.130581	0.015495

Joint Hypothesis Tests		
	War of Attrition: $p(\chi^2) = .0144$ + BIC: $p(\chi^2) = .0144$	Institutions(no PRES): $p(\chi^2) = .0182$
	Generational Transfers: $p(\chi^2) = .0003$	# GC(no PRES): $p(\chi^2) = .0688$
	Pol./Part. Cycles + Comt: $p(\chi^2) = .0308$	BMNG & BMNGDX: $p(\chi^2) = .1804$
	Fiscal Illusion: $p(\chi^2) = .0109$	BNOPDX, BDWIDX,&BMNGDX: $p(\chi^2) = .0005$

The results for the extant variables remain relatively constant; although BNOP is half as large and half as significant as in Franzese's model, BNOPDX remains quite stable and positive. The coefficient for BIC is the correct sign, but it is highly insignificant. The same model but without the minority government variables (not shown) also has BIC at very low significance. The Distributional Conflict variables have the same level of significance ($p(\chi^2) = .0144$) either with or without the addition of BIC. In addition,

several combinations of joint significance tests, with the Distributional Conflict variables paired with BIC, all cannot reject the null hypothesis that BIC's coefficient equals zero. Hence the evidence so far shows no support for Heller's theory that second chambers with partisan compositions identical with first chambers nonetheless will make additional spending demands.

Regarding minority governments, the picture is opposite that of BNOP and BNOPDX: Minority governments in level form increase debt; but they decrease it as the inherited level of debt rises. This is not surprising given E-O's results, which suggested that minority governments' contribution to debt was small. It is also consistent with Tsebelis' idea that minority governments are similar to single-party majority ones, since the lowest levels of debt addition come from those types of government (according to BNOPDX's coefficient). The two variables are not jointly ($p(\chi^2) = .1804$) or singly significant at conventional levels; but BMNGDX is significant at the . 0.0857 level. Furthermore, additional Wald chi-square tests show that even BMNG may have some importance: BNOP and BDWI: $p(\chi^2) = .2254$; BNOP, BDWI, and BMNG: $p(\chi^2) = .0615$. Thus there is some additional support for the notion that minority governments do not contribute much to deficits and may even reduce them.

Post-1973 Shifts Given the positive effect on deficits of the post-1973 era found by R-S and E-O, one would like to see if it is confirmed in the larger data set. In the model presented below I interact the Distributional Conflict variables (viz., BVP (dispensing with BNOP + BIC now that the latter variable has been found to be insignificant), BDWI, and EMG) with a dummy variable for the 1975-90 period, called **P5** (short for "post75"). Although the oil crisis began in 1973, it is likely that its effects, on interest rates and Keynesian safety-net and demand management policies, first began to show real effect in 1974, which then was transmitted to 1975's debt/GDP ratio in the 1975 budget year. The shifted variables should all have positive signs, since all types of governments would have faced decreasing tax revenues and greater demand for safety-net policies.

Table 8: Political variables with the Post-75 Shift:

Valid Cases:	563	Dependent Variable:	DDEBTX
Missing Cases:	137	Deletion Method:	Listwise
Wtd Total SS:	1482.560	Degrees of Freedom:	522
Total SS:	4800.269	Degrees of Freedom:	522
Wtd R-Squared:	0.643	Wtd Rbar-Squared:	0.616
R-Squared:	0.491	Rbar-Squared:	0.451
W Residual SS:	529.159	Wtd Std Error Est:	1.007
Residual SS:	2445.692	Std Error of Est:	2.165
DW-Stat:	2.246	Wtd DW-Stat:	1.973

Variable	Estimate	PCSE Std Error	t-value	Prob >|t|	Standardized Estimate	Reg Std Error
ONE	17.658902	6.305247	2.800668	0.0053	7.296345	6.205161
DDEBTX1L	0.303724	0.047369	6.411858	0.0000	0.304754	0.039647
DDEBTX2L	0.112673	0.042520	2.649899	0.0083	0.119143	0.036689
DEBTX1L	-0.032469	0.010728	-3.026573	0.0026	-0.459792	0.010547
DUE	0.556614	0.103912	5.356584	0.0000	0.201103	0.111672
UE1L	0.037342	0.034858	1.071250	0.2846	0.087094	0.034023
GROWTH	-0.129880	0.033555	-3.870613	0.0001	-0.232664	0.035092
DXRIG	0.070446	0.049498	1.423222	0.1553	0.095823	0.050948
DXRIGDX	-0.000027	0.001396	-0.019137	0.9847	-0.001165	0.001447
TRADE	11.523469	3.359936	3.429669	0.0007	2.346383	3.568292
TOTT	-9.568499	3.137180	-3.050032	0.0024	-1.963409	3.318380
TOT	2.272827	1.011819	2.246278	0.0251	0.978857	1.083551
BVP	0.045425	0.224329	0.202492	0.8396	0.053810	0.242899
BVPDX	-0.007311	0.006109	-1.196677	0.2320	-0.289129	0.006726
BDWI	-0.184173	0.191710	-0.960684	0.3372	-0.172205	0.205066
BDWIDX	0.005314	0.005392	0.985591	0.3248	0.154869	0.005832
LRGDPC1L	-1.896391	0.585370	-3.239646	0.0013	-7.122295	0.576577
OY	-0.180628	0.668590	-0.270162	0.7871	-0.040825	0.729993
GINIXX	-0.015182	2.035237	-0.007459	0.9941	-0.002233	2.123183
ELE	0.330092	0.188307	1.752948	0.0802	0.057575	0.206155
BCOG	0.207972	0.069790	2.979984	0.0030	0.507617	0.078301
BRR	0.533014	0.706950	0.753963	0.4512	0.087574	0.741487
BRRXBCOG	-0.120314	0.109870	-1.095063	0.2740	-0.121763	0.119481
STAX1L	-0.026517	0.009807	-2.703776	0.0071	-0.761042	0.011186
CBI	-3.419806	1.053540	-3.246015	0.0012	-0.845947	1.079104
FED	0.462646	0.165872	2.789171	0.0055	0.400640	0.179667
BAESQRD	-1.818791	1.625688	-1.118782	0.2637	-0.170146	1.850844
BAE	1.100091	1.546767	0.711220	0.4773	0.121255	1.764311
ENOC	0.068506	0.125795	0.544585	0.5863	0.094248	0.135814
GR	-2.024897	0.534452	-3.788735	0.0002	-0.192542	0.515985
SP	-3.568559	0.898087	-3.973512	0.0001	-0.367387	0.874091
BDICT	0.880833	0.634328	1.388608	0.1655	0.075110	0.622720
DDABROAD	0.284527	0.063257	4.497926	0.0000	0.200340	0.067885
BMNG	0.001403	0.754201	0.001860	0.9985	0.000208	0.775170
BMNGDX	-0.009515	0.025473	-0.373527	0.7089	-0.043477	0.025178
BDWIP5	0.428123	0.236600	1.809477	0.0710	0.289528	0.257091
BDWIDXP5	-0.006914	0.007527	-0.918568	0.3587	-0.145076	0.007868
BVPP5	-0.488708	0.206445	-2.367254	0.0183	-0.408878	0.227834
BVPDXP5	0.018734	0.006615	2.832046	0.0048	0.564714	0.006977
BMNGP5	0.952627	0.821221	1.160012	0.2466	0.100203	0.880943
BMNGDXP5	-0.027489	0.027765	-0.990076	0.3226	-0.096296	0.028150

Joint Hypothesis Tests	War of Attrition: $p(\chi^2) = 0.3433$ War of Attrition*P5: $p(\chi^2) = 0.0006$	Institutions(no PRES): $p(\chi^2) = . 0.0051$
	Generational Transfers: $p(\chi^2) = . 0.0001$	# GC(no PRES): $p(\chi^2) = 0.0246$
	Pol./Part. Cycles + Comt: $p(\chi^2) = 0.0098$	BMNGP5 & BMNGDXP5: $p(\chi^2) = 0.5102$
	Fiscal Illusion: $p(\chi^2) = 0.0005$	BMNG and BMNGDX: $p(\chi^2) = 0.6772$
	BVPP5 and BVPDXP5: $p(\chi^2) = 0.0163$	BMNGDX and BMNGDXP5: $p(\chi^2) = 0.1114$

This model may show instability, as DXRIGDX, which has been significantly positive up to now, switches signs with DXRIG and is no longer significant. It may also (or instead) be due to the failure to include an interaction term for DXRIGDX (i.e., DXRIGDXP5). Nonetheless, there is some support for the post-1973 shift. The BVP variables are no longer significant (and switch signs) while their shifted versions are and in the same direction as the original variables. The coefficients for minority government, however, lose significance in all their permutations, a fact that may be due to collinearity among them (as encountered in the E-O models above). The signs of the level and debt-interacted variables are consistent from their pre- to post-versions; and BMNGDX and BMNGDXP5 are not far from jointly significant at $p(\chi^2) = 0.1114$. This is the only model so far in which a form of BDWI, BDWIP5, shows greater individual significance (in the previous models, it is usually significant in joint tests), and in the expected direction (positive). If polarization is a contributing factor to inaction (as fractionalization is considered to be), then the especially severe economic slowdown after 1974 may have "kicked in" this variable, as polarizad politicians could not agree on ways to reduce spending. The idiosyncrasies of this model suggest that further residual analysis is appropriate.

Government-Type Categorical Political Variables Following E-O's models examined above, one would like to investigate whether policy outputs differ systematically between single-party and multi-party governments, and whether of the minority or majority type (issues raised by Hallerberg and von Hagen (1996) and by Tsebelis (1995)). One way to begin is to test a full-fledged model using dummy variables for all four government types--single- and multi-party-majority and -minority governments--to see what distinctions emerge between or among these government types. In the table below,

ONMN (ONMNDX) = one-party minority government (ONMN*lagged debt/GDP)

G1MN (G1MNDX) = greater-than-one party minority government (G1MN*DX)

ONMJ (ONMJDX) = one-party majority government (ONMJ*DX)

ONE(=G1MJ), DEBTX1L(=G1MJDX) = greater-than-one party majority government (and G1MJ*DX or DEBXT1L) (these two constitute the excluded category to avoid the dummy variable trap)

Table 9: Categorical government-types:

Valid Cases:	563	Dependent Variable:	DDEBTX
Missing Cases:	137	Deletion Method:	Listwise
Wtd Total SS:	1455.782	Degrees of Freedom:	526
Total SS:	4800.269	Degrees of Freedom:	526
Wtd R-Squared:	0.639	Wtd Rbar-Squared:	0.614
R-Squared:	0.478	Rbar-Squared:	0.443
W Residual SS:	526.161	Wtd Std Error Est:	1.000
Residual SS:	2503.343	Std Error of Est:	2.182
DW-Stat:	2.251	Wtd DW-Stat:	1.981

Variable	Estimate	PCSE Std Error	t-value	Prob >|t|	Standardized Estimate	Reg Std Error
ONE**(=G1MJ)**	7.752661	5.407013	1.433816	0.1522	3.213936	5.398392
DDEBTX1L	0.342906	0.046552	7.366036	0.0000	0.343664	0.038541
DDEBTX2L	0.131918	0.042345	3.115353	0.0019	0.138774	0.036426
DEBTX1L**(=G1MJDX)**	-0.00667	0.01243	-0.536390	0.5919	-0.094077	0.012127
DUE	0.613534	0.102811	5.967621	0.0000	0.220935	0.110088
UE1L	0.062646	0.035831	1.748365	0.0810	0.144739	0.033745
GROWTH	-0.110557	0.032525	-3.399163	0.0007	-0.200100	0.034393
DXRIG	-0.003229	0.044670	-0.072285	0.9424	-0.004429	0.046157
DXRIGDX	0.002421	0.001331	1.818632	0.0695	0.104500	0.001310
TRADE	11.910954	3.254154	3.660231	0.0003	2.400246	3.492353
TOTT	-10.422084	3.022228	-3.448477	0.0006	-2.109483	3.245590
TOT	2.024538	0.925690	2.187058	0.0292	0.872163	1.043546
ONMN	1.540287	0.635908	2.422184	0.0158	0.214123	0.641639
ONMNDX	-0.054254	0.019329	-2.806880	0.0052	-0.220535	0.019560
BDWI	-0.075297	0.121114	-0.621703	0.5344	-0.071965	0.128818
BDWIDX	0.001411	0.003624	0.389354	0.6972	0.040613	0.003794
LRGDPC1L	-1.030214	0.494187	-2.084665	0.0376	-3.878296	0.496424
OY	0.034190	0.656772	0.052058	0.9585	0.007736	0.709013
GINIXX	2.161599	1.862811	1.160396	0.2464	0.319300	1.916460
ELE	0.321853	0.186819	1.722804	0.0855	0.056170	0.206126
BCOG	0.172783	0.071368	2.421023	0.0158	0.422303	0.079109
BRR	0.298231	0.689923	0.432267	0.6657	0.049381	0.715526
BRRXBCOG	-0.066089	0.110498	-0.598106	0.5500	-0.067480	0.116979
STAX1L	-0.027814	0.009822	-2.831716	0.0048	-0.809774	0.010997
CBI	-1.848637	1.012633	-1.825576	0.0685	-0.452339	1.058623
FED	0.140892	0.143947	0.978774	0.3281	0.119031	0.162084
BAESQRD	-2.730778	1.595430	-1.711625	0.0876	-0.270491	1.766792
BAE	2.221940	1.561283	1.423150	0.1553	0.261654	1.725479
ENOC	0.170888	0.107604	1.588122	0.1129	0.235319	0.118437
GR	-1.700892	0.521816	-3.259561	0.0012	-0.163294	0.515095
SP	-3.045823	0.857538	-3.551823	0.0004	-0.306499	0.833311
BDICT	1.556252	0.621989	2.502057	0.0126	0.131124	0.599094
DDABROAD	0.305168	0.059664	5.114760	0.0000	0.215763	0.063211
G1MN	-1.363878	0.755615	-1.804990	0.0716	-0.092119	0.845502
G1MNDX	-0.001704	0.022624	-0.075317	0.9400	-0.004056	0.023763
ONMJ	0.514753	0.516480	0.996656	0.3194	0.114739	0.531415
ONMJDX	-0.026653	0.014125	-1.886957	0.0597	-0.211089	0.014453

There are only 20 cases for G1MN, so this model is likely to be less stable than previous ones. Instability may also be shown by the fact that all categories except G1MN follow the reverse of the basic BNOP-BNOPDX pattern: Level variables are positive, while the variables interacted with lagged debt/GDP are negative. Future work will have to sort this out.

Future research. Several avenues for further research are outstanding.

With respect to the models discussed here, much work remains to be done: Detailed residual analysis to determine the stability of the regression models, and further Chow tests for fixed effects, come immediately to mind.

The most pressing issue is to expand Hallerberg and von Hagen's (1996) data on the presence/absence of strong Finance Ministers and/or negotiated spending targets. Hallerberg and von Hagen (1996) find that strong FMs or the use of negotiated targets among coalition partners (a more feasible strategy alternative for them than the use of a

strong FM, since it is much less likely that multi-party governments will be willing to delegate authority to "create" a strong FM) are both substantively and statistically significant. With these variables' inclusion, NOP became insignificant in their study. It is not clear whether strong FM and target data exist in usable form at present beyond the data set already developed by Hallerberg and von Hagen, which is limited to European countries over a relatively short time-series.

The budgetary reversion point affects the strategy parties will take in the Prisoners' Dilemma/Distributional Conflict, and in practice one or more defections (i.e., unequal distribution of benefits) may be tolerated without provoking a like response, depending on the financial impact of the defection(s) on overall spending. It is not clear whether good data on reversion points exist for a large sample such as Franzese's.

A measure of party cohesion and party discipline is badly needed. Operationalizable concepts exist (structure of party organization and leadership, party finance, campaign organization, etc.), but I am aware of no data set which systematically investigates these variables.

For minority governments having external support, one could include those parliamentary parties in the BDWI score. Future work could attempt to test whether the external support makes a difference, for example by coding governments with external support as majority coalition governments versus as one-party governments (Tsebelis, personal communication, Nov. 1996). Identifying such parties remains a data-collection task.

Conclusion

The empirical investigations undertaken here showed that the conclusions of a key study in the literature on public debt, Roubini and Sachs' (1989b), capture a part of the story on deficits (but via chance capitalization, as Table 1, Cols. 4 and 5 showed). Edin and Ohlsson (1990)'s sensitivity analysis of R-S's results was theoretically an improvement; it just so happened that their conclusions were highly distorted by bad data. Franzese's study revealed that more complex relationships were operating between and among the key independent variables and deficits, and that many more variables were related to the level of debt a country incurs. The main conclusion of this preliminary exploration is first that upper chambers in bicameral countries do not demand higher spending simply by virtue of their existence as a second chamber; Tsebelis' conception that ideologically congruent houses act as a single entity in spending demands receives tentative support. The second conclusion is that minority governments do not add any more to debt than other types of governments, and instead may contribute to deficit reduction. However, both conclusions should be viewed as quite tentative since the data warrant a more in-depth examination.

I have attempted to do two things in this paper. First, to provide the needed corrections to R-S's and E-O's studies. Second, to build modestly upon Franzese's excellent work by operationalizing more carefully the Distributional Conflict variables and their related variables, and conducting the beginnings of a deeper sensitivity analysis that was inspired by R-S's and E-O's approaches, by exploring alternative specifications and transformations of the political variables. This briefest of examinations has shown that there is considerably more to be learned about Franzese's data plus my modifications plus possible future modifications. The political economy of public debt literature is making

rapid strides in the 1990s, as researchers invent ingenious investigations and continue to find surprises in even the most well-trodden data.

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ENDNOTES:

1 This is in contrast to another theory which holds that parties' relative size is positively related to their intra-coalitional power (Laakso and Taagepara 1979).
2 The theory is incomplete in this formulation, since it remains to be explained why government partners would satisfice, and not maximize, their individual spending targets.
3 An alternative measure is the change in government budget deficit. As Heller (1994), Borrelli and Royed (1995), and others note, there are serious measurement inconsistencies not only between the two measures but also within them: across countries and over time, and across data sources. To my knowledge, it is Borrelli and Royed (1995) who address this issue the most carefully; the upshot of their discussion however is that the several different measures available all produce very similar results (1995: 252-3).
4 Another way to address these issues is to use the log change of real debt or the log of real deficit as the dependent variable, as done by Bawn et al. (forthcoming) and Borrelli and Royed (1995). Doing so avoids the issue of employing GDP on both sides of the regression equation, since all the models R-S, Franzese, and I use include some form of the real GDP growth rate on the right-hand side as a control variable (see below)--higher GDP growth is expected to reduce the debt, since tax revenues also increase thus reducing the debt burden. Unfortunately, testing my models using these dependent variables exceeds the resources available for this paper.
5 A possible objection raised by Heller (1994: 22, n. 24) is that politicians do not look at transformed tax-smoothing variables as used by R-S; but actually it is more likely that they do, since they make policy choices based on information given to them by economists, who certainly use such variables in their forecasts. This is an empirical matter--one could ask government officials how they create their forecasts and budgets--whose investigation is beyond the scope of this paper.
6 There is an issue of unit roots in time-series cross-section models which goes beyond my present competence. In Part 2 below, Franzese (1996: 49) has tested for unit roots and found that Unemployment potentially contained a unit root; hence the use of the lagged level of unemployment and the first difference in unemployment on the right-hand side in all of his and my models in Part 2 (see Franzese 1996: 49 for a partial explanation).
7 E-O do not model the separate, post-74 effect of the 0 category in their test of the post-74 shift: They do not include P0*D. Thus they leave the constant term as the overall effect of the 0 category, not allowing a post-1973 shift in it. It is not clear whether this was intentional, since E-O provide no discussion of the matter.
8 In their paper E-O do not report the constant, however, and so one cannot determine the actual level of the political variables' coefficients.
9 Borrelli and Royed also recognize the problem of using general government debt and have devoted careful attention to operationalizing their debt measure (1995: 227-9; 250-3). They use central government data but argue that the extrabudgetary accounts should be excluded from the debt calculation. However, the extent to which such funds are subject to political control, and hence their relevance to the political variables considered here, is an open issue. I opt to include these accounts because it seems more likely than not that such funds are indeed subject to the same kind of political control as the rest of the budget.
10 Unfortunately, I have not yet determined the difference between gross and net debt as defined by the IMF or the OECD, the latter which supplied R-S with the data on general government net debt. R-S do not explain what they mean by "net."
11 Thanks to George Tsebelis who alerted me to the mistakes.
12 A further complication is that R-S are not consistent even between their own studies. In R-S 1989a, code 1 is "coalition parliamentary government with two-to-three coalition partners" and code 2 is "coalition parliamentary government with four or more coalition partners" (115). Whereas in R-S 1989b, code 1 is set for 2 partners *only*, and code 2 is for *three* or more partners (923). Abiding by this change would make each paper's data set different--among the numerous examples are Sweden 1977-78 and 1980, which would be code 1 in the former and code 2 in the latter--but they are the same in both papers. In making corrections I have used R-S's 1989b specification.

13 As De Nardo notes, "The PRESS residuals are true prediction errors, since the computed y-hats are independent of the y's being predicted....[T]he observation y_i is not used simultaneously for fitting and model assessment" (De Nardo 1996: Part 6, P. 10).
14 To test the null hypothesis that the errors in an equation with a first-order lagged dependent variable are serially independent, "we regress the residuals from an OLS estimation [of this equation] on the first lag of those residuals and all the independent variables used in the OLS estimation" (Beck and Katz 1995: 6). If one can accept the null hypothesis that the coefficient for the lagged residuals equals 0, then one can accept that the remaining errors are serially independent (Beck and Katz 1995: 6). The test generalizes for any number of lags.
15 Franzese also tests models substituting the following for NOP and ADWI: ENOP, the effective number of parties (where larger parties are given more weight (see Laakso and Taagepera 1979)), and SDWI, the standard deviation of cabinet ministers' polarization (again, giving emphasis to the largest parties). He finds the NOP and ADWI measures to be superior, however (Franzese 1996: 28, 35), and thus I report his results using the latter measures only.
16 Since we can thus assume that the errors of such a least squares estimation are serially independent, the variance-covariance matrix of the errors takes the form (Beck and Katz 1995: 4):

$$W = S \otimes \mathbf{I}_T$$

where S is the NxN variance-covariance matrix of the residuals, and $\otimes$ denotes the Kronecker product.

Let **E** denote the TxN matrix of the OLS (or WLS) residuals. **E**'**E**/T provides a consistent estimate of S. PCSE's are thus given by the square root of the diagonal of

$$(\mathbf{X}'\mathbf{X})^{-1}\mathbf{X}'([\mathbf{E}'\mathbf{E}/T] \otimes \mathbf{I}_T)\mathbf{X}(\mathbf{X}'\mathbf{X})^{-1}.$$

17 For example, it is beyond my present level of competence to perform the J-tests Franzese used to determine which groups of variables could be rejected as being nested within alternative models. Hence I cannot verify the results of Franzese's J-test, nor do J-tests for my models discussed below; this issue will have to remain for future research. Fortunately, I make only relatively modest changes to Franzese's model, which decrease the likelihood that the J-test results would change significantly.
18 Of course, one would expect the political cost of excluding one or more coalition partners to vary over time and across countries; a more accurate approach would investigate empirically the intra-coalition bargaining process and outcome. Such an undertaking would be very labor-intensive, however, for any significant number of countries.
19 Of course, a more accurate coding would specify which governments *actually* passed which budgets (see Bawn et al. (forthcoming) for an excellent effort in this regard), but such an undertaking exceeds the resources available for this paper. Obviously, governments (and parliaments) frequently do not adhere to official rules regarding the budgetary process. Also, such rules almost surely changed within each country during the postwar period; whereas my coding, due to resource constraints, assumes that they remained constant (I use OECD (1995) for the rules on the budgetary process). It is also possible that my coding, using this time-invariant source, may actually be less accurate than taking a weighted average of parties in government per year. Further research will sort this out.
20 Note that Woldendorp et al.(1993) and Lane et al. (1991) conflict sometimes considerably on the number of ministers in government. I have used the latter source because of resource constraints (i.e., it is far easier to use for coding), but the former source is likely to be more accurate given that its authors cross-checked their work with Lane et al. and other sources--a project for the future.
21 In coding BNOP I have made corrections where necessary for differences in recording basis of debt/GDP ratios (calendar vs. fiscal year). Borrelli and Royed (1995) is the only study I have seen which also incorporates a "lag" in the NOP coding (which I discovered after having invented the idea independently); however, the main political variable they use (an index based on a number of political variables) is problematic.

22 Heller's model allows for bicameralism to lead to *lower* deficits, depending on the preferences of the chambers and government (Heller 1994: 13, n. 13); and indeed Stewart (1991) has found that "divided government led to lower U.S. deficits in the late 1800s" (Heller, ibid.).

23 A drawback of this approach is that it leaves unsolved the problem that BNOP + BIC does not distinguish periods in which the same majority ruled both chambers from periods of divided house control: For Germany, e.g., BNOP = 2 and BIC = 1 throughout, despite that during 1973-82 different majorities controlled the upper and lower houses (the CDU/CSU controlled the Bundesrat (by one vote (Conradt 1978: 138)) while the SPD and FDP controlled the Bundestag). BDWI is more easily adjusted; e.g., Germany's BDWI score for 1973-83 includes the CDU/CSU majority in the Bundesrat and is thus not 2.4453 but 2.7251.

24 See Money and Tsebelis (1992) for a discussion of the veto powers of second chambers in various countries.

25 The coding of Germany as bicameral is open to dispute. The Basic Law (German constitution) gives the Bundesrat (upper house) an overrideable (by 2/3) veto over legislation relating to the Laender (states), but not over other legislation. Thus, if there is a budget issue that does not involve the Laender, such as foreign aid, then Bundesrat approval is not needed (personal communication, Mark Hallerberg, 9/16/96). Article 77 of the Basic Law gives the Bundestag the veto over the Bundesrat (after the deliberations of a joint committee formed specifically to discuss the bill in question). Article 110 implies that the Bundestag (lower house) may ignore Bundesrat objections to the budget; on the other hand, Article 104a states that federal laws to be executed by the Laender, and which require that the Laender pay for equal to or greater than one-fourth of the expenditure involved, require the consent of the Bundesrat. Bawn et al. (forthcoming) code Germany as unicameral, but they avoid the problem altogether by considering only items in the federal, not overall, budget. Given that my data include both the federal and state components of the budget, and my lack of knowledge about their relative proportions and the breakdown of finances subject to one or the other set of laws, and in the final analysis relying on the recommendation of Mark Hallerberg, I code Germany as bicameral. The information about the Basic Law comes from Karpen (1988); see Dalton (1989: 312) and Conradt (1978: 136-39) for additional discussion.

26 As Tsebelis argues (1994: 762), the US's number of veto players will be reduced to two or one if one can argue that in certain historical periods US parties exhibited high party discipline (his examples are the first hundred days of the New Deal and certain policy areas of the 1993-94 Congress and presidency).