Electoral and Partisan Manipulation of Public Debt in Developed Democracies, 1956-90

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Abstract: Theoretical literature seeking to explain public-debt accumulation exploded in recent years as debt crises emerged in many nations. Empirical evaluation of political-economy theories has, however, lagged that of basic economic-conditions models. This paper joins those beginning to redress the imbalance, operationalizing and evaluating standard electoral and partisan budget-cycles arguments and their modern, rational-expectations-strategic variants. The evidence strongly suggests modification of the former and flatly rejects the latter. Electoral budget-cycles exist, but their timing is different than usually assumed. Partisan debt-effects also exist, but they run in commonly expected directions (left-deficits, right-surpluses) only when incumbents' perceived risk of replacement by ideological competitors is high. They run in opposite directions when such replacement risk is low. The pattern contradicts recent rational-strategic models but perhaps suggests alternative, equally rational-strategic, logic for partisan manipulation of the budget.

I. Introduction and Motivation

High and/or swiftly rising public-debt-to-GDP ratios *debt*) became significant issues in many developed democracies in recent years. **Figure 1** plots gross, consolidated, central-government debt for 21 OECD countries, 1948-97.[1] Fairly commonly, debt declined dramatically from 1948-72± and equally dramatically reversed thereafter. In many countries, debt doubled or more in under 20 years thereafter; in some, it now exceeds 100%. Wide and rising public concern over these developments, and the number of theories emerging to explain them, are thus hardly surprising. Beyond the common trend, however, lie large differences in postwar debt experiences of OECD nations. Shared cross-time variation comprises only 16.6% of the total, with increasing divergence apparent since 1980 while persistent cross-national differences represent 56.4%. Even removing shared temporal fluctuations and country averages, considerable country-time-unique variation, 27% of the total, remains.[2]

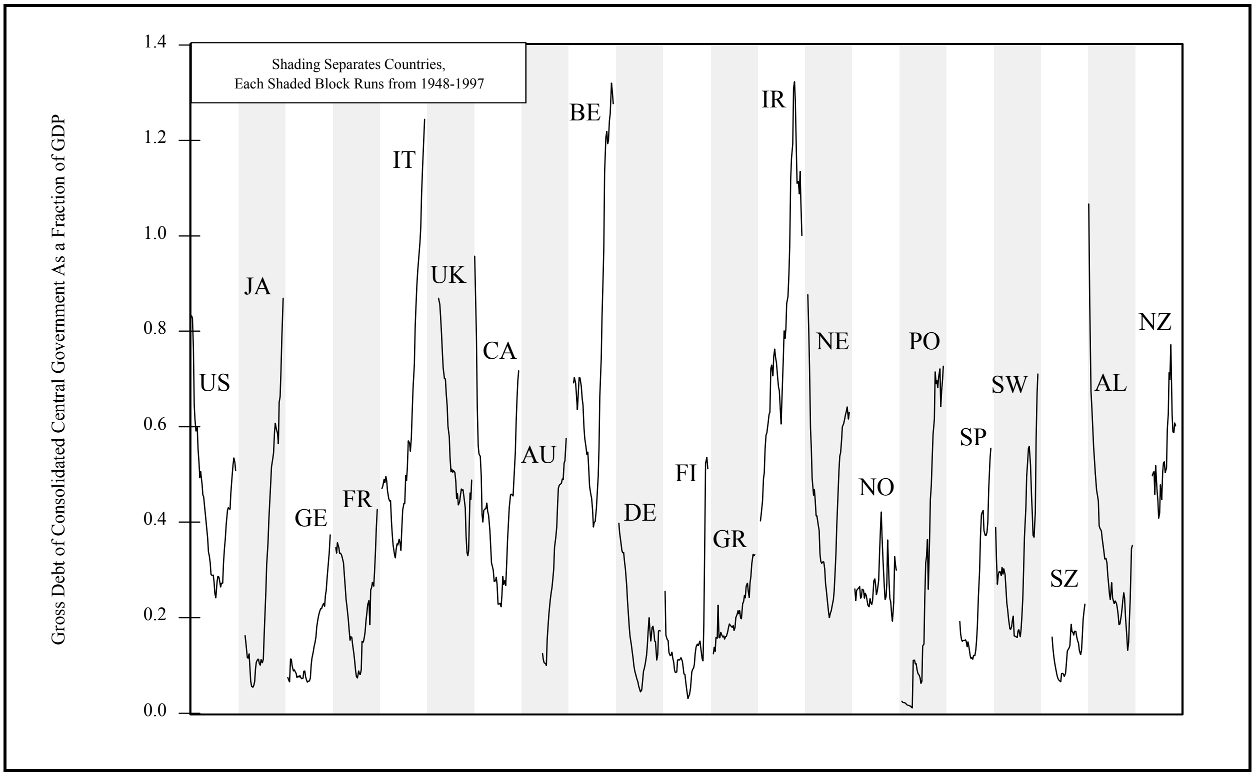

Figure 1: Gross Debt of Consolidated Central Government in 21 OECD Countries, 1948-1997

However, excepting empirical work exploring the tax-smoothing model of Barro (1979) and Lucas and Stokey (1983), few have tested the many theories purporting to explain **Figure 1**. Recent empirical attention (Roubini and Sachs 1989ab; Edin and Ohlsson 1991; de Haan and Sturm 1994, 1997; Borrelli and Royed 1995; de Haan *et al.* 1998) focuses on theories of weak government and delayed stabilization. Others (von Hagen 1992; von Hagen and Harden 1994,1996; Hallerberg and von Hagen 1996; and de Haan *et al.* 1997) stress budgetary rules and institutions or bicameralism and partisan differences between the chambers (Heller 1998). This paper joins those in starting to redress the imbalance between theoretical development and empirical testing, evaluating traditional and rational-expectations-strategic electoral and partisan budget-cycle arguments regarding political manipulation of debt, using the postwar (1956-1990) experiences of 21 OECD countries as the data base.[3] The analysis unfolds thus. Section II introduces the electoral- and partisan-budget-cycle arguments to be evaluated, identifies the necessary control variables, and operationalizes them. Section III conducts the econometric analysis, and IV concludes, evaluating electoral and partisan budget-cycle theories in light of this new evidence.

II. Reviewing and Operationalizing the Theoretical Literature

Descriptive statistics for all data and simple correlations of independent variables with debts and deficits appear in the data appendix.

II.A. Standard Electoral and Partisan Budget-Cycles

At least since Nordhaus (1975) and Tufte (1978), political economists have suspected that politicians systematically attempt to manipulate the economy for electoral purposes. At simplest, the hypothesis is that governments employ expansionary policies (here: higher deficits) prior to elections in order to boost the economy in time to earn an electoral boon from myopic voters. At least since Hibbs (1977), political economists have argued that parties of the right and left differ in their fiscal-policy priorities. Specifically, left governments favor larger public economies, greater redistribution, and more Keynesian expansion and so are expected to run greater deficits than right governments.

To examine electoral budget-cycles, a variable summing to one over the year preceding an election is created (ELE), using Mackie and Rose (1991) plus *European Journal of Political Research, Political Data Annuals* for recent years. More precisely: in election year *t*, $ELE_t = M/12 + (d/D)/12$ with M complete months before the election, *d* the day of the incomplete month, and D total days in the incomplete month. $1\text{-}ELE_t$ is allocated to the year before the election (if pre-election years overlap, ELE can exceed 1).[4] To examine partisan budget-cycles, first, every party that has been in government since 1945 in all 21 countries is coded, 0=far-left to 10=far-right, using indices from Laver and Hunt (1992) and those compiled in Laver and Schofield (1991). With these codes and party composition of cabinet ministers in each government (Lane *et al.* 1991; Woldendorp *et al.* 1994, 1998), an average left-right position of each government is obtained: the *political center of gravity* (CoG).[5] For comparison: US Democrats and Republicans 2.8± CoG-units apart (4.8-7.6) while UK Labour and Conservatives are 4.9± CoG-units apart (2.8-7.7).

II.B. Rational-Strategic Budget-Cycles: Replacement Risk and Using Debt to Commit Future Governments

Alesina and Tabellini (1990) note that incumbent governments can affect the fiscal situation subsequent governments will inherit by accumulating debt and thus constraining their fiscal options. In their model, the more different the alternative governments' desired spending-composition, the more incumbents will accumulate debt to constrain potential oppositions. The greater the ideological distance (in terms of desired-spending composition) of potential replacements from the incumbent and the more likely such a replacement, the more debt incumbents accumulate.[6] Persson and Svensson (1989) offer a similar model in which governments differ in desired spending-level. In this model, the *low-spender* (presumably the right) accumulates debt when faced with the prospect of replacement to constrain *big-spending* (presumably left) replacements. The left behaves oppositely. These models, then, emphasize the strategic use of debt to increase or decrease fiscal constraints on the opposition.

Aghion and Bolton (1990), Milesi-Ferretti (1993), and Milesi-Ferretti and Spolaore (1993 and 1995) stress, instead, that incumbents can affect their re-election probability through debt policies that alter the partisan preferences of the population. Specifically, if left parties are known or suspected greater default-risks than right parties, then right governments can run deficits to increase the amount of voter-held debt and so decrease electoral support for the suspected default-prone left.[7] The left, conversely, can reduce debt to alleviate default-risk concerns about it among the electorate. However, these models do not specify testable conditions under which such incentive would dominate the more familiar (opposite) incentives emphasized in standard partisan theory.

All these arguments identify ideological polarization across governments or, more precisely, the expected ideological distance of future governments from incumbents as an explanator of public debt. To measure such *replacement risk* (RR), first CoG is used to calibrate variations of government partisanship over specified time periods. The standard deviation of annual CoG-scores across, say, nine years centered on the present (SDaG) gives a simple measure, comparable-across-countries, of the typical distance from itself an incumbent might reasonably presume its potential replacement to be.[8] Next, the expected probability of losing office to such a replacement must be estimated. Ideally, one would model such expectations-formation explicitly, but comparable means to do so across 21 democracies awaits future research. The inverse of the actual duration of the incumbent government (*i.e.,* the hazard rate, HR, or probability of losing office), gives a simpler, reasonable estimate under the assumptions that HR is constant within government and that incumbents know or can estimate it well. HR times SDaG then emerges as a cross-country-time comparable estimate of the expected ideological distance from next year's government to this year's: *replacement risk*≡RR = HR·SDaG.[9]

Models emphasizing replacement risk vary in their expectations of its effects. Alesina and Tabellini argue that partisan governments, left or right, accumulate debt when facing high replacement risk. Thus, only RR is needed to model their argument empirically; a positive coefficient is expected. Persson and Svensson expect positive relationships between debt and RR under right incumbents and negative relationships under left incumbents. Therefore, RR, CoG, and their interaction (RR·CoG) captures their model. The pattern of coefficients implied is positive on RR·CoG and negative on RR such that *d*DEBT/*d*RR is positive (negative) for sufficiently right (left) CoG. Finally, though the remaining arguments do not specify conditions under which right and left behave counter-intuitively, presumably they do so when weakening electoral support for the opposition is deemed especially valuable, which is likely when threat of replacement (RR) is high, or when weakening the opposition by counter-intuitive debt-policy without losing its own supporters is especially low-risk, which is likely when the government is electorally secure (*i.e.* when RR is low). Including CoG, RR, and CoG·RR, therefore, should cover most possibilities and leave it an empirical issue.

II.C. Economic Controls[10]

All economic theory suggests unemployment rates (representing spending shocks), real-GDP growth rates (revenue shocks), real-interest less real-growth rates times outstanding debt (servicing costs), terms-of-trade movements (open-economy shocks),[11] and differences between expected real-growth and real-interest rates (giving expected future ability to repay debt) as independent variables.

Internationally comparable unemployment rates (UE) are from OECD sources.[12] Real-GDP growth rates (GROWTH) are changes in natural logs of internationally comparable real-GDP from *Penn World Tables v.5.6.* Expected growth rates are from OLS regression on two lags, country dummies, and lagged *per capita* GDP (also *Penn World Tables*). Inflation rates are for GDP-deflators, and expected inflation is from OLS regression on country dummies and two lags. Trade openness (OPEN) is export-plus-import GDP-share. Terms-of-trade (ToT) are export-import price-ratios. Terms-of-trade will have more impact in more open economies, so ToT·OPEN is also included. Price and trade data are from IMF sources.[13]

Interest-rate data are from IMF and OECD sources and proved trickier to compile than the other economic data. Nominal interest-rates are estimated from long-term government-bond yields (LTGBY), discount, T-bill, deposit, lending, and money-market rates, consumer- and wholesale-price inflation, and short- and medium-term government-bond yields thus. If LTGBY is available, it is used. If not, all available LTGBY are regressed (OLS) on the largest available subset of other interest rates to estimate LTGBY for country-years in which data on that subset of interest rates exists.[14] Differences between expected real-interest and real-growth rates (DXRIG) are then straightforward to calculate. Finally, the debt-service costs of interest payments (INTPAY) are calculated by multiplying nominal interest-rates minus actual inflation minus actual real-GDP growth ($DRIG_t$) by lagged debt (D_{t-1}): $INTPAY = DRIG_t \cdot D_{t-1}$.

II.D. Other Socio-Political Controls

Roubini and Sachs (1989ab), Alesina and Drazen (1991), Spolaore (1993), and Drazen and Grilli (1993) develop *war-of-attrition* models of public-debt stabilization. Governing parties will likely dispute who must bear necessary fiscal adjustments' costs even if they agree that current debt-levels or persistent deficits require adjustment. If a single party controls government, costs might be easily shifted to outsiders. Multi-party governments will certainly also try to shift costs to outsiders, but policies that neutrally distribute adjustment costs among governing parties will be more difficult to devise. Neutral adjustment plans will be harder to find the more fragmented and polarized the coalition. So, given uncertainty among parties over how long others will tolerate steadily rising debt before capitulating to stabilization plans whose distributional implications they dislike, more polarized and fragmented governments will produce deadlock and delay stabilization longer than would unified governments.[15] War-of-attrition theories thus highlight fractionalization and polarization of partisan interests within governments. To measure these, use the party left-right codes described above to code the maximum distance between CoG scores of coalition parties for polarization (ADwiG), and simply count the number of parties in government for fractionalization (NoP).[16,17]

Cukierman and Meltzer (1989) and Tabellini (1991) note that, while imperfect capital markets do not allow negative bequests, public debt effects just such transfers from future to present. Therefore, the poor, old, and especially old-poor, being most desirous of negative bequests, will most favor public debt. Thus, countries with relatively poor and old populations should have higher or faster-rising debts; and, since greater wealth-inequality usually implies more poor relative to wealthy, more inegalitarian democracies should also amass greater debt (controlling for total wealth).

One demographic statistic reflecting the relative weight of positive- and negative-bequest incentives is the ratio of population over 64 to that under 15 (OY), from OECD sources.[18] The economy's aggregate wealth is as easily measured: the natural log of real GDP *per capita* (LRGDPC) from *Penn World Tables v 5.6.* Cross-country/cross-time comparable data on wealth distribution, however, are non-existent, and income-distribution data are notoriously problematic and spotty at best. To achieve some annual indicator of income distribution comparable across 21 countries and 35+ years, then, consider the ratio of GDP *per-capita*, indexed to 100 in 1986, to a manufacturing-workers'-wage index, also 100 in 1986. This measure compares the ratio of economy-average income to blue-collar wage-income[19] to the same income-disparity index in the same country in 1986. Then, OECD *Social Policy Study* #18 (1995), which reports cross-country comparable income-inequality measures for various years, can be leveraged to make the index comparable across countries. Taking the reported GINI index from each country as near 1986 as possible, standardizing it relative to the

US 1986, and then multiplying by the within-country, across-time comparable index above, produces a cross-time *and* cross-country comparable index of income disparity. This *relative real-wage position of manufacturing workers* (RWPMW) compares each country-year to the US 1986 where RWPMW = 1. Higher (lower) RWPMW values imply greater (lesser) disparity. Measuring joint age-and-income distributions is still more difficult, but, minimally, multiplying the old-young ratio and the income-disparity index (OY·RWPMW) will distinguish country-years with high old-young ratios and high income-disparity, from those moderate in both or low in one, from those low in both.

Weingast *et al.* (1981) argue that policy makers representing sub-national constituencies tend to weight excessively the benefits of spending in their constituency relative to the aggregate cost of financing it because spending-benefits often accrue largely within constituency while tax-costs are usually spread more equally across the polity. If majorities for fiscal legislation are assembled by log-rolling compromises, then budgets will overspend on constituency projects proportionally to the number of constituencies.[20] Overspending does not necessarily imply deficits, but fiscally-illuded voters may imply deficits proportional to spending.[21] Velasco (1995) shows, moreover, that dynamic multiple-constituencies models imply deficits proportional to the number of constituencies, even absent fiscal illusion. Empirically, comparing the number of constituencies across democracies is difficult, partly because the substantive meaning of *constituency* varies across country-times (Franzese and Nooruddin 1999). To triangulate on the underlying concept, consider several measures. FED counts the number of regions in effectively federal systems.[22] AGRETH is the fraction of government composed of agrarian- or ethnic-party members, whose support is usually geographically concentrated.[23] PRES (0-1) indicates political systems with strong presidents (US, France V, Finland); presidents have national constituencies while legislators represent more localized interests.[24] ENED is the effective[25] number of electoral districts. Singly, each measure is problematic, but jointly they might adequately control for empirical manifestations of the multiple-constituencies problem.

Many scholars argue that voters incompletely understand their government's intertemporal budget constraint and, so, tend simply to reward spending and punish taxing. If voters misapprehend the relation between current deficits and future taxes, opportunistic politicians could simply spend more than they tax for electoral advantage. Buchanan and Wagner (1977) suggest further that such *fiscal illusion* should be more evident where complicated fiscal structures make cost-benefit analysis of public taxing and spending more difficult for voters. By implication, countries with more complicated fiscs should be associated with higher/faster-rising debt.[26] OECD sources provide data on tax structure divided by source of revenue into direct and indirect taxes and other receipts of general government, and by level of government into central, local, and social-security government revenue. Some of these and other subdivisions are available for very few country years,[27] leaving only three useful measures: general-government indirect taxes and total taxes as shares of total current revenue (ITTCR and TTTCR), and central government total current revenue as a share of general government total current revenue (CGRGGR). Indirect taxes are likely more difficult to assess than other taxes, while taxes generally are likely easier to assess than other revenue sources (*e.g.*, seigniorage). Also, public-revenue generation by one central authority rather than multiple local authorities and separate (e.g., social-security) administrations likely facilitates cost-benefit analysis of public endeavors. Thus, fiscal illusion should be more apparent in country-years characterized by relatively high indirect taxes and low total taxes as shares of revenue and low central government revenue as a share of general.[28]

Finally, Alesina and Perotti (1994) note that, historically, governments reduced massive debts largely *via* inflation. An independent, conservative central bank, however, would hinder this relatively easy escape, so central bank independence (CBI) may dissuade prudent governments from debt-accumulation.[29] Contrarily, imprudent or recalcitrant governments that issue excessive debt despite high CBI, will find financing that debt more costly.[30] Capitalizing on the most commonly used (and therefore presumably the best) of the many CBI indices in the literature, the measure here averages two each from Cukierman (1992) and Grilli *et al.* (1991) and one from Bade and Parkin (1982).[31]

III. Empirical Estimation of Electoral and Partisan Budget-Cycles

The usable empirical sample is 618 country-years: US, Germany, France, Italy, Belgium, Denmark, Finland, Ireland, Netherlands, Norway, Sweden, Australia 1956-90; Japan 1958-90; UK, Switzerland 1963-90; Austria 1973-90; Greece 1961-62, 79-90; Portugal, Spain 1981-90; New Zealand 1969-90.[32] The regression model takes error-correction form, with change in debt (ΔD_t) regressed on the complete set of country dummies ($\mathbf{B_0}$), two lagged changes in debt (ΔD_{t-1}, ΔD_{t-2}), lagged debt-level (D_{t-1}), average change in other countries' debt in that sample-year ($\Delta D_{\{\sim i,t\}}$), the set of controls just discussed (C), and the electoral and partisan budget-cycle variables described in Sections II.A and II.B:[33]

$$\begin{aligned}\Delta D_{i,t} = \mathrm{B}_0 + \beta_1 \Delta D_{i,t-1} + \beta_2 \Delta D_{i,t-2} + \beta_3 D_{i,t-1} + \beta_4 \Delta D_{\sim i,t} + \mathrm{B}_5{}' C \\ + \beta_6 ELE_{i,t} + \beta_7 ELE_{i,t-1} + \beta_8 CoG_{i,t} + \beta_9 RR_{i,t} + \beta_{10} CoG_{i,t} \cdot RR_{i,t} + \varepsilon_{i,t}\end{aligned}$$

Further methodological details are discussed in Appendix I. **Table 1** reports the core estimation results; a fuller report appears in the Appendix II. **Figure 1** above provides scale for interpreting the substantive magnitude of the estimated effects discussed below.

<u>Table 1:</u> Core Regression-Estimation Results; Dependent Variable: $\Delta D_{i,t}$

INDEPENDENT VARIABLES	COEFFICIENTS	STD. ERRORS	p-LEVELS
$\Delta D_{i,t-1}$	.2783	.0527	.0000
$\Delta D_{i,t-2}$	.0625	.0437	.1527
$D_{i,t-1}$	-.0502	.0098	.0000
$\Delta D_{\sim i,t-1}$	+.2077	.0546	.0002
$ELE_{i,t}$	+.4891	.1677	.0037
$ELE_{i,t-1}$	+.5757	.1707	.0008
$CoG_{i,t}$	+.3183	.0928	.0007
$RR_{i,t}$	+2.280	.8296	.0062
$RR_{i,t} \cdot CoG_{i,t}$	-.4008	.1386	.0040
Number of Observations (Degrees of Freedom)	618 (555)	**S.E.E.: Weighted (Unweighted)**	1.006 (2.174)
Adjusted R^2: Weighted (Unweighted)	.685 (.502)	**Durbin-Watson: Wtd. (Unweighted)**	2.019 (2.166)

NOTES: Results for controls suppressed to Appendix II. Equation estimated with panel weights and panel-corrected standard-errors. P-levels are two-sided t-test results.

Note first that debt adjusts very slowly. The adjustment rate also depends critically upon real-interest net of real-growth rates (DRIG) and upon fractionalization (NoP) and, less so, polarization (ADwiG) within governments (see Appendix II). At sample means (-1.3, +2.1, and +1.3 respectively) the estimates imply that the long-run effect of any permanent shock is 75± times its immediate impact and that 50± years pass before half of its total long-run effect is observed in debt/GDP. All discussion below assumes DRIG, NoP, and ADwiG at sample means, debt initially stable, and all else constant.

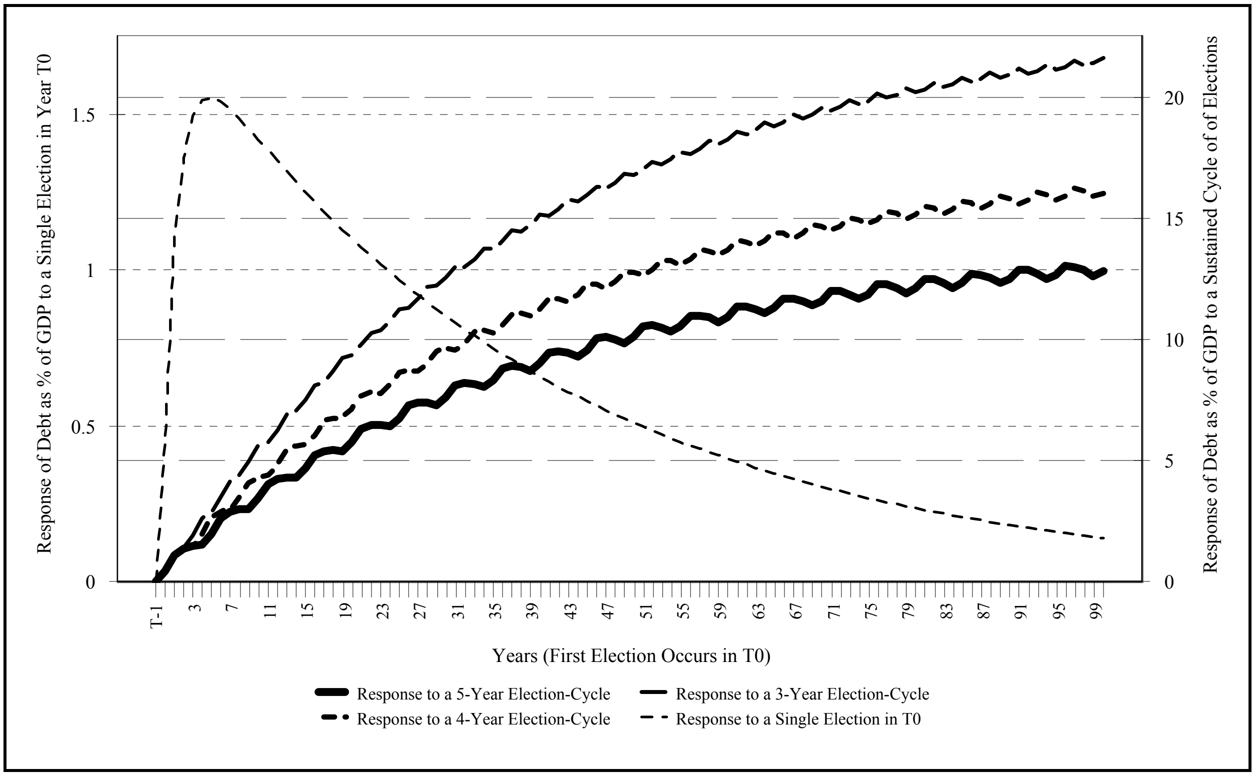

Figure 2: Estimated Debt-Responses to 3-, 4-, and 5-Year Electoral-Cycles and to a Single Election

Regarding electoral cycles, note several striking results. First, *ceteris paribus*, the year before and the year *after* elections are both significantly positively associated with deficits (p≈.0026 jointly), combining to give +1% of GDP stimulus to debt. Second, because debt adjusts so slowly and exhibits such strong short-term momentum, the effect compounds, growing to a maximum +1.56% five years post-election. Even this one-election effect (left y-axis in **Figure 2**) is sizeable, but, third, since these democracies have elections at least every 5 years, the debt-impact of one election is still accumulating when another arrives. Debt response-paths to electoral politics in 3-, 4-, and 5-year-cycle countries are plotted against the right y-axis of **Figure 2**. As shown, a country averaging elections every 3 years would, *ceteris paribus*, have long-run debt +5.8%± of GDP higher than one averaging elections every 4 years, which, in turn has long-run debt +3.5% higher than one averaging elections every 5 years. Finally, electoral-cycle oscillation is most visible in 5-year-cycle countries and more muted in higher-frequency cycles; the estimated temporal dynamics produce this effect.

Important theoretical insights also follow from these empirical findings. First, electoral politics seem to produce pre- and *post*-electoral fiscal stimulations. Perhaps this merely reflects stickiness and slippage in fiscal-policy implementation. Incumbents seeking pre-electoral stimulation may experience delays or flatly fail in attempted retrenchments. More interestingly, it may reflect that pre- and post-electoral policy-makers differ when challengers win. Challengers likely frequently make exorbitant promises, yet the electoral-cycle literature largely ignores challengers. If their brazen promises induce similar ones from incumbents, or if challengers win, post-electoral years would see further stimulus as those promises are fulfilled (and electoral promises usually are fulfilled: see Klingemann et al. 1994, Keech and Lee 1995, and Gallagher et al. 1995). More mundanely, this may simply reflect calendar-year measured ELE *versus* different (and varying) fiscal-year measured debt (though greater care than usual was taken to dynamic modeling and to election-timing within calendar years). Further, the US, on which most previous work focused, has partial elections every two years and therefore exhibits among the least-accentuated electoral cycles. All of this suggests that previous failures to find strong electoral cycles may be greater condemnation of empirical research design than of electoral-cycle theory. Regardless, "reports of electoral political-budget-cycle theory demise have been greatly exaggerated;" their study certainly appears to warrant reopening. Second, electoral-cycle theory emphasizes fiscal fluctuations, but glacial debt-adjustment rates imply that electoral frequency has important impact on cross-national differences in long-run debt *levels*. 5-year cycle democracies tend to accumulate almost 10% of GDP less long-run debt than 3-year cycle democracies, *ceteris paribus*.

Partisan debt-effects may also exist; if so, they depend on the replacement risk incumbents face (coefficients on CoG and CoG·RR are moderately significant: p≈.04 and .10 individually, p≈.09 jointly). As **Figure 3** reveals, the effect of 1-CoG-unit rightward shifts in partisanship at medium to high replacement risk is in the traditional, lower-deficit direction and can be appreciable, -.38%± of GDP at sample maximum RR (2.55±), but large standard errors render it marginally insignificant over most of this range. Right governments, contrarily, are significantly ($p < .1$ one-sided) associated with moderately higher deficits when replacement risk is below 0.32± (*e.g.*, 25% chance of replacement by government 1.28 CoG units to the left) which occurs in almost 70% of the sample. Thus, more usually, partisan effects are relatively small and operate opposite simple partisan-theory expectations.

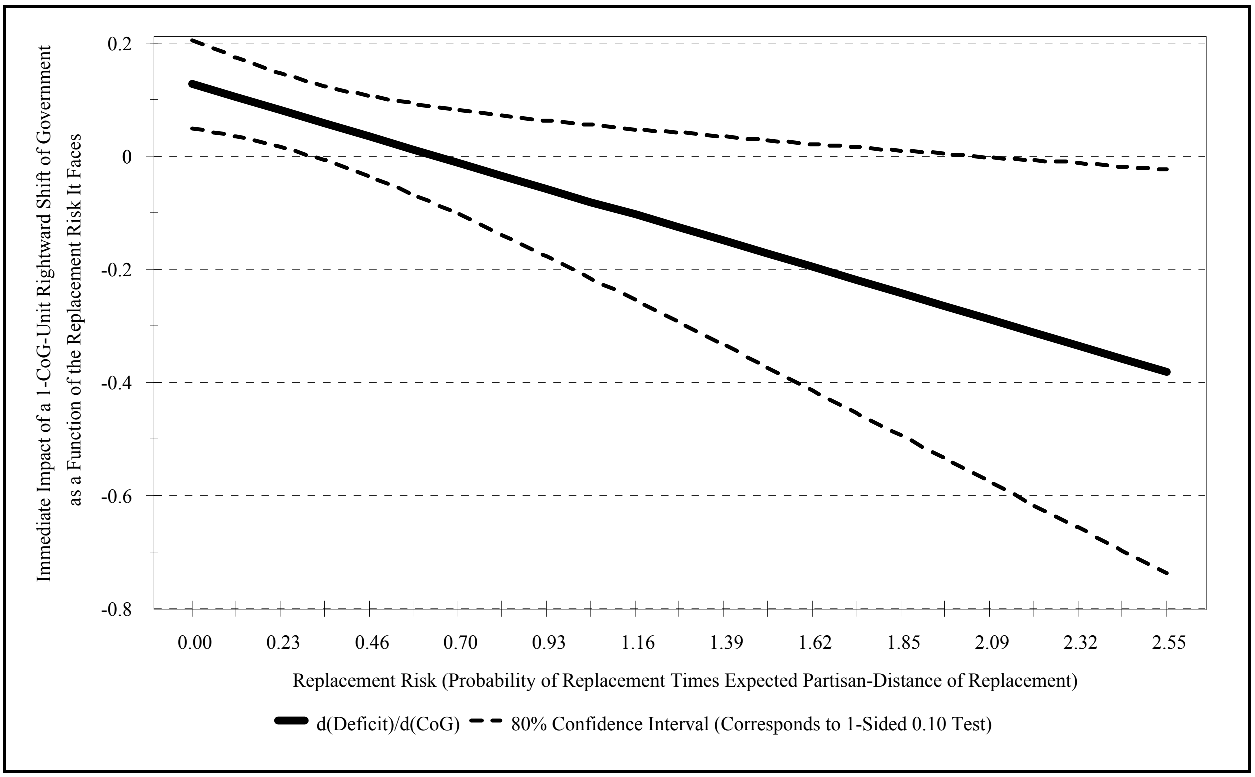

Figure 3: Immediate Deficit-Impact of 1-CoG-Unit Rightward Shift of Government as a Function of the Incumbent's Perceived Replacement Risk

Debt-as-constraint arguments receive still less support from the data. Replacement risk (RR) and its product with partisanship (RR·CoG) are only marginally significant individually (p≈.17 and

.10 respectively) and less significant jointly (p≈.225). Thus, replacement risk matters, if at all, by inducing government pursuit of standard partisan objectives *via* strategic debt-policy manipulation.

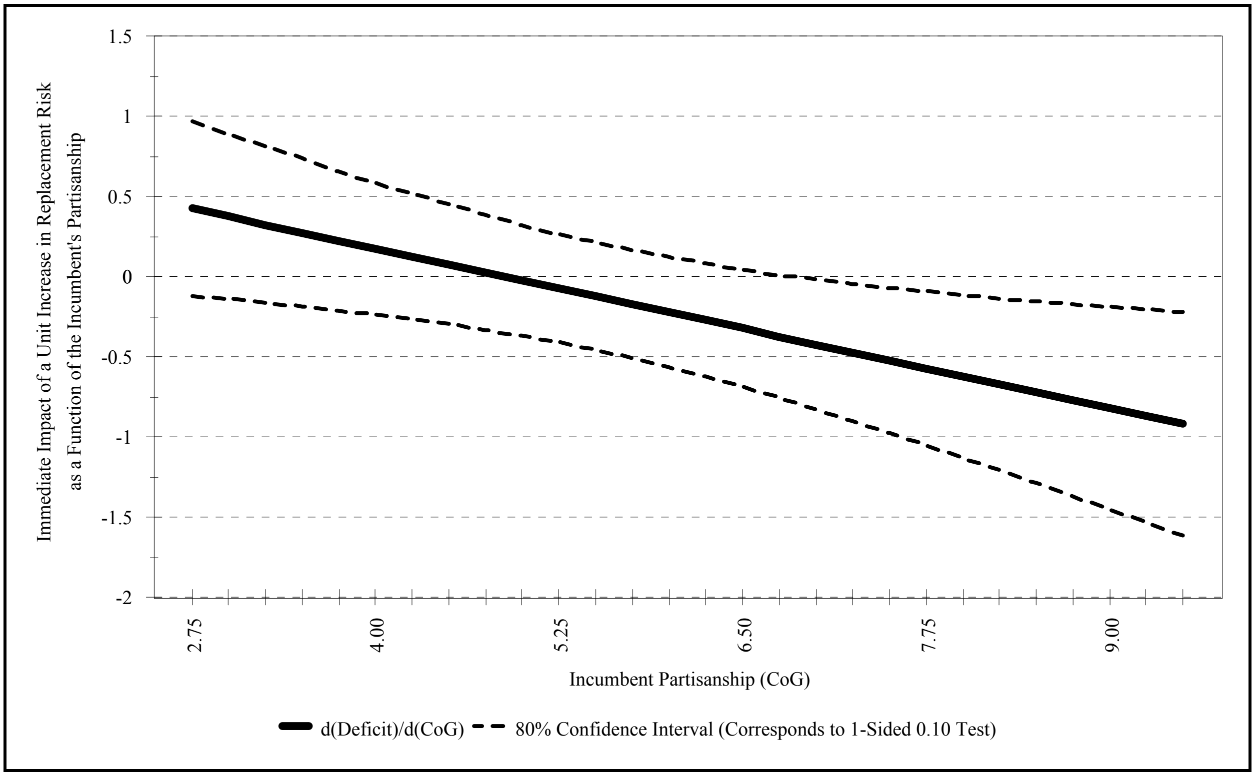

Figure 4: Estimated Immediate Deficit-Impact of a Unit Increase in Replacement Risk (RR) as a Function of Incumbent Partisanship (CoG)

The pattern is opposite that which Persson and Svensson predict and differs from that which Alesina and Tabellini predict. Specifically, as shown in **Figure 4**, increases in replacement risk are estimated to increase deficits when the incumbent is left of center, not right as argued by Persson and Svensson. Moreover, whereas Alesina and Tabellini argued that replacement risk should increase deficits under both left and right governments, the positive relationship holds only for incumbents left of CoG≈5 (about 38% of the sample) and never quite significantly so in the sample-range of CoG. Conversely, the negative impact of replacement risk on deficits raised by right incumbents is at least marginally significant ($p < 0.1$ one-sided) at CoG≳6.875 (about 18.5% of the sample). Replacement-risk effects are appreciable, though, especially for extreme-right governments. A standard-deviation increase in replacement risk (+.33) would bring the right-most Japanese LDP (CoG=8.9) to reduce deficits 0.8%± of GDP. The same increase would bring the left-most incumbent (UK Labour, CoG=2.78) to increase deficits 0.42%± of GDP. At sample-mean, CoG=5.54 (roughly a typical Democratic-president-led US government), the effect is small and insignificantly negative (-.13%). Thus, partisan governments do not appear to use debt to constrain potential oppositions and produce strategic partisan-cycles as Persson and Svensson or Alesina and Tabellini expect.

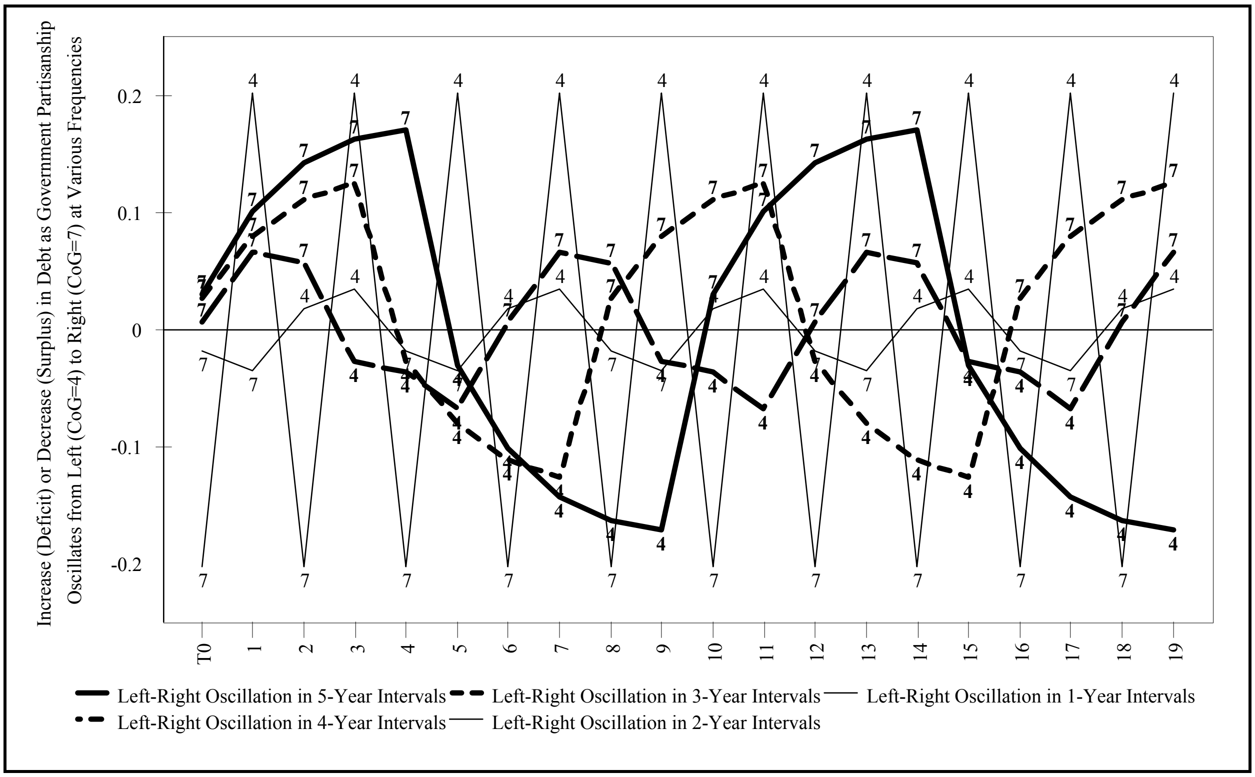

Figure 5: Deficits or Surpluses Created as Governments of Differing Partisanship Oscillate at Different Frequencies

Figure 5 demonstrates the estimated replacement-risk-augmented partisan-budget-cycles most clearly, plotting estimated deficit-paths for government partisanship oscillating regularly from CoG=4 to CoG=7 (mean ± 1 standard deviation) with 1-to-5-year frequencies (the sample-range). When governments oscillate relatively infrequently (every 3 years or less), the right runs deficits and the left surpluses, contrary to simple partisan theory. Conversely, when governments alternate frequently (every 1-2 years), partisans behave as commonly expected. Thus, greater replacement risk induces the left toward deficits and the right toward surpluses, but more usually they act oppositely. *I.e.*, both venerable partisan-budget-cycle and newer debt-as-commitment theories warrant revisiting.

Specifically, the evidence suggests merging simple partisan theory and rational-strategic-debt-manipulation models thus. First, debt-to-constrain-oppositions versions of strategic models seem to underestimate debt-persistence radically. Given slow debt-adjustment, right incumbents who expect considerable partisan oscillation in government may be unwilling to increase debt to constrain the left because they expect to govern again soon enough, and it would then constrain them. Conversely, a secure right might think its hold on government safe enough to expend some security trying to alter the electorate's partisan preferences by increasing voter-held debt. If, instead, the left expected to govern long, it may be *less* willing than the right to tolerate structural deficits precisely because it has standard partisan interests for greater policy-responsiveness so that it must remain able to respond fiscally to any economic difficulties that may arise. A currently secure left may also try to bolster that security by maintaining or reducing voter-held debt-levels. An insecure left, contrarily, might respond more quickly and even to relatively small or imagined economic slumps because it would lose office more especially surely if it does not and because it expects that the right will react insufficiently (by left standards) to any downturn that may occur in or persist into the right's quite imminent term.

This *replacement-risk-augmented partisan-budget-cycle theory* accords with the findings here and with certain stylized facts about OECD postwar debt experiences. For example, Sweden's long-secure left kept debt relatively low while it was in power, whereas Italy's equally long-secure center-right produced unrestrained structural deficits. Meanwhile, the left (right) was more (less) associated with deficits (surpluses) in countries like the UK where left and right oscillated more frequently and with some regularity. While evidence for or against this reconsideration and broadening of partisan theory should not be drawn from the same sample used to derive the argument, these findings certainly recommend further theoretical development and empirical evaluation of the conjecture.

III. Conclusions

Summarizing, the specific conclusions are two-fold. First, election-year politics are important both in that there is a statistically strong pre-*and-post*-electoral deficit cycle and in that debt adjusts so slowly that electoral-cycle frequency has sizable *long-run* impact on debt-to-GDP levels. Second, partisan budget-cycles are, typically, of rather less importance in explaining the OECD post-war debt-GDP experience (especially near sample means), and often run in directions opposite of conventional wisdom. A political system with frequent, large shifts of government partisanship, though, can have sizable partisan-budget-cycles according with common wisdom (see **Figures 3** and **5**). Thus, both traditional and rational-expectations-strategic partisan theories of fiscal-policy manipulation need revision. The evidence suggests that policy makers may indeed be partisan and strategic, but previous theories seem to have under-estimated the slowness of debt-adjustment and so to have miss-assessed the actual interests of such strategic, partisan actors. High (low) incumbent replacement-risk appears to induce traditional (counter-intuitive) left-right policy-making behavior.

More generally, the results clearly call for theoretical and empirical reconsideration of both electoral and partisan budget-cycles. Much recent eulogizing notwithstanding, evidence for electoral budget-cycles is strong once the temporal dynamics of the instrument being modeled are given careful consideration. The study of partisan effects has also, generally, under-appreciated the empirical and theoretical importance of the policy-instrument's temporal dynamics. Even recent theories that stress strategic policy-maker manipulation of stock variables have apparently failed in this regard. Here, too, the evidence suggests fruitful avenues for theoretical and empirical reconsideration.

Appendix I

In choosing the precise model specification, many auxiliary regressions and preliminary tests were conducted. This appendix details the process.

First, Augmented Dickey-Fuller tests of all variables that might reasonably contain unit roots were conducted. The null that debt contains a unit root cannot quite be rejected (.10 critical value ≈ -2.57, ADF statistic ≈ -2.14). Unemployment, trade openness, trade openness times terms of trade, the old-young ratio, and its product with the income-disparity index all also potentially contain unit roots by ADF tests, but OLS residuals of debt regressed on various permutations of this set still potentially contained unit roots. Employing the usual, 2-stage, error-correction model directly would have been unwise at that point. As Beck (1992) reviews, though, a 1-stage error-correction-type estimation might nonetheless be appropriate cases like these where several possible cointegrating or near-cointegrating factors are among the regressors to be considered. He suggests differencing the dependent variable and including its lag and appropriate lagged differences among the regressors along with differences and levels of all potentially cointegrating variables. Only the economic controls were significant in both levels and changes. Other variables were significant only in levels or neither in levels or changes, the exceptions being the income-distribution variables, which were significant only in changes, and the old-young ratio, which was significant in levels and changes. Thus, the reported results correspond to 1-stage error-correction where the economic controls and the old-young ratio enter in levels and changes, the income- and age-and-income-distribution variables enter in changes, and all other variables enter in levels only. In the model of **Table 1**, the coefficient on lagged debt has a t-stat around -3.7, which would likely satisfy any ADF test, so inferences should now be safe with respect to any lingering unit-root concerns.

Second, this specification also survives LaGrange Multiplier tests for autocorrelated residuals up to six lags and Ljeung-Box tests (any lag length). Thus, the specification including two lagged differences in debt and one lagged debt-level satisfies all autocorrelation concerns.

Third, neither a linear trend nor two trends allowed to differ from pre- to post-1973 (*i.e.*, a trend variable, a post-1973 indicator, and their interaction) were individually or jointly significant. (Including them makes little substantive difference to the other coefficient estimates.) Therefore, they serve little purpose beyond increasing R^2 and multicolinearity and reducing degrees of freedom. Moreover, the estimated trend was upward until 1973 and downward thereafter, opposite the trend observed in **Figure 1** and therefore substantively odd. As a principle, linear trends are atheoretical variables for which theoretical replacements should be sought, therefore linear trends are gladly omitted from the reported model. (Substantive conclusions are not affected by this choice.)

Fourth, in alternative models with country-specific intercepts, Wald-tests of equal coefficients on the set of country dummies nearly rejected in favor of the country fixed-effect model ($p \approx .1$). There was one important difference in the fixed-effects model from those reported. The general shape of the results on replacement-risk-augmented partisan-budget-cycles were unchanged but became much *more* significant. The more conservative results eschewing the atheoretical country dummies were nonetheless reported. (Other differences were minor; full details available from the author.)

Fifth, time-period fixed-effects were supported by analogous tests. However, rather than include 34 atheoretical dummies for years, a variable equal, for each country-year, to the average deficit that year in the other countries in the sample ($\Delta D_{\sim i,t}$) was devised. Using $\Delta D_{\sim i,t}$, time-dummies were more nearly rejected, and the key substantive results were unchanged, so the reported results include it instead. The justification for time-dummies must be that there might be year-specific factors omitted from the regression; modeling such factors implicitly by including the average deficit abroad that year as a regressor is preferable to doing so completely atheoretically with 34 year-dummies.

Sixth, the reported regression was estimated first by OLS. OLS residuals were then squared and regressed on a constant and the set of country-indicators less one. The F-statistic of that auxiliary regression tests panel heteroskedasticity against the null of homoskedasticity and was extremely high, implying panel-type heteroskedasticity at least at the .0001 level. Panel WLS was therefore applied and then the panel-corrected standard-errors suggested by Beck and Katz (1995, 1997).[34]

Appendix II

Coefficient Estimates Suppressed from Table One

INDEPENDENT VARIABLE	COEFFICIENT	STD. ERROR	p-LEVEL
ΔUE_t	+.5335	.1005	.0000
UE_{t-1}	+.0570	.0261	.0294
$\Delta GROWTH_t$	-.0592	.0394	.1330
$GROWTH_{t-1}$	-.0730	.0487	.1346
$\Delta DXRIG_t$	-.0314	.0458	.4931
$DXRIG_{t-1}$	-.1082	.0467	.0207
$\Delta INTPAY_t$	+.0046	.0007	.0000
$INTPAY_{t-1}$	+.0039	.0009	.0000
$\Delta OPEN_t$	+22.49	5.597	.0001
$OPEN_{t-1}$	+10.83	3.316	.0012
ΔToT_t	+6.749	1.888	.0004
ToT_{t-1}	+1.387	.9579	.1480
$\Delta(ToT_t \cdot OPEN_t)$	-23.12	5.598	.0000
$ToT_{t-1} \cdot OPEN_{t-1}$	-9.599	3.125	.0022
$ADwiG_t$	+.1122	.1275	.3794
$ADwiG_t \cdot D_{t-1}$	-.0025	.0039	.5151
NoP_t	-.3043	.1698	.0736
$NoP_t \cdot D_{t-1}$	+.0129	.0045	.0046
$LRGDPC_{t-1}$	+.5506	.3628	.1296
ΔOY_t	-46.48	10.94	.0000
$\Delta RWPMW_t$	-27.01	5.931	.0000
$\Delta(RWPMW_t \cdot OY_t)$	+47.63	11.52	.0000
OY_{t-1}	-1.905	.6468	.0034
CBI_t	-1.277	.6793	.0607
PRES	-1.333	.4472	.0030
$ENED_t$	+.0064	.0070	.3608
$ENED_t^2$	$-2.2e^{-5}$	$2.0e^{-5}$	.2696
$AGRETH_t$	+.8158	.5089	.1094
FED_t	-.1013	.0347	.0037
FED_t^2	+.0022	.0006	.0003
$TTTCR_{t-1}$	-3.913	3.072	.2032
$ITTCR_{t-1}$	+3.987	1.824	.0292
$CGRGGR_{t-1}$	-4.859	1.033	.0000

NOTES: As in Table 1.

Sample Descriptive Statistics

VARIABLE	Min	Max	Mean	Std Dev	Corr. w/ Debt	Corr. w/Deficit
Debt (D)	3.02	132.30	34.96	23.73	1.00	0.22
Deficit (ΔD)	-14.66	15.94	0.35	3.08	0.22	1.00
UE	0.00	20.94	4.53	3.64	0.44	0.33
GROWTH	-8.72	13.50	3.47	2.78	-0.06	-0.32
DXRIG	-10.54	10.05	-0.97	3.07	0.14	0.17
DRIG	-16.06	10.68	-1.32	4.32	0.15	0.32
(DRIG·D_{t-1})	-825.01	644.32	-34.36	176.86	0.09	0.34
OPEN	0.07	1.40	0.47	0.24	0.43	0.21
ToT	0.70	1.82	1.01	0.15	0.01	-0.29
ToT·OPEN	0.09	1.33	0.47	0.25	0.43	0.14
ENoP	1.00	5.21	1.66	0.79	-0.13	0.01
ENoP·D_{t-1}	4.44	295.36	55.05	44.46	0.80	0.14
SDwiG	0.00	2.51	0.60	0.61	-0.06	0.05
SDwiG·D_{t-1}	0.00	194.06	19.65	29.75	0.54	0.15
NoP	1.00	5.92	2.09	1.21	-0.19	0.02
NoP·D_{t-1}	4.44	480.22	66.74	62.13	0.68	0.14
ADwiG	0.00	5.41	1.34	1.47	-0.09	0.06
ADwiG·D_{t-1}	0.00	400.31	43.22	66.76	0.53	0.16
LRGDPC	7.75	9.80	9.12	0.37	-0.11	0.06
OY	0.18	1.05	0.52	0.19	0.01	0.15
RWPMW	0.33	1.39	0.80	0.17	0.25	-0.03
OY·RWPMW	0.06	0.97	0.42	0.17	0.10	0.10
ELE	0.00	1.41	0.31	0.34	0.02	0.00
CoG	2.78	9.40	5.54	1.54	0.06	0.06
RR	0.00	2.56	0.27	0.33	0.02	0.01
RR·CoG	0.00	14.83	1.42	1.84	0.02	0.02
PRES	0.00	1.00	0.17	0.37	-0.26	-0.07
CBI	0.15	0.93	0.48	0.20	-0.24	-0.06
FED	1.00	50.00	6.59	12.08	-0.12	-0.06
ENED	1.00	328.93	67.28	87.79	0.08	-0.16
AGRETH	0.00	1.00	0.06	0.17	-0.33	-0.01
TTTCR	0.81	0.98	0.91	0.03	-0.19	-0.19
ITTCR	0.15	0.71	0.35	0.09	0.11	-0.18
CGRGGR	0.26	0.89	0.61	0.15	0.44	-0.10

All statistics computed in the 618-observation sample.

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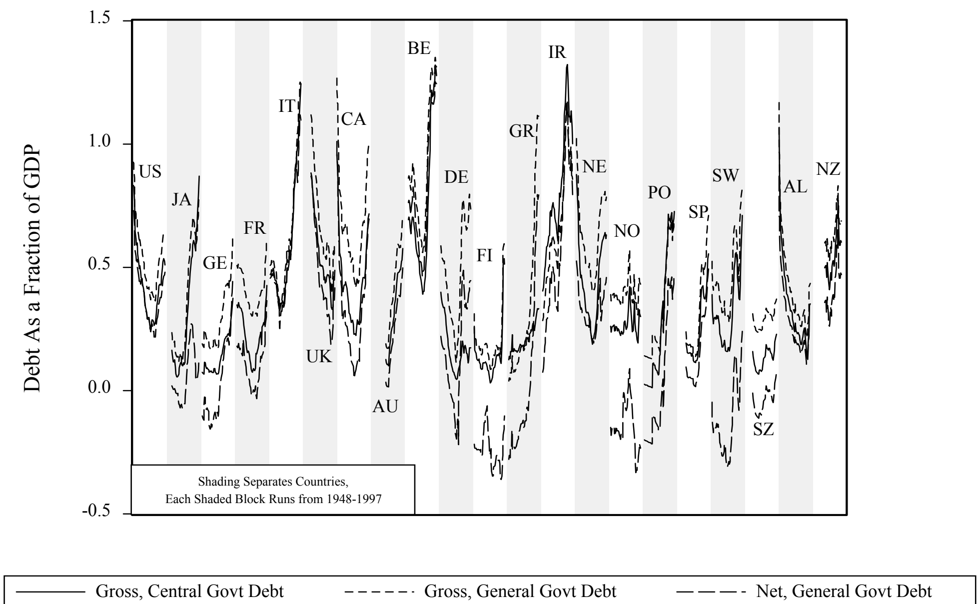
Notes

1. Data are from IMF *International Financial Statistics* (6/96 CD-ROM). "Consolidated central government" includes separate central administrations (e.g., social security). Where the CD-ROM gives no data but tape or print editions or OECD sources do, usable data are extended by country-specific fitting regressions employing all available other series. R^2 from fitting regressions usually exceeded 0.9. This procedure also maximizes available OECD gross and net general-government (*i.e.*, including sub-national) debt data. IMF data are employed because (a) they cover far larger sample prior to extension, (b) the political variables introduced below generally refer to central government, and (c) OECD general-government data double-count in some countries. Nonetheless, correlations across series are comfortably high:

Correlation Matrix	Debt Levels			Change in Debt		
	IMF Gross, Central	OECD Gross, General	OECD Net, General	IMF Gross, Central	OECD Gross, General	OECD Net, General
IMF Gross, Central	1	.891	.858	1	.869	.743
OECD Gross, General	.891	1	.896	.869	1	.838
OECD Net, General	.858	.896	1	.743	.838	1



2. R^2 from regressing debt on complete sets of country or time-period dummies gives variance breakdowns by country or time. One minus their sum is the remaining, country-time-unique, variance.

3. Franzese (1996b, ch. 3) more thoroughly explores empirically each of the several political-economic theories reviewed by Alesina and Perotti (1994).

4. Such observations are very rare, so capping ELE at 1 yields no appreciable difference in the results.

The US, Finland, and the French Vth Republic are problematic here, having strong presidents. The simplifying assumption applied throughout is that Finnish and French presidents and cabinets are each ½ the government and that the US president, Senate, and House are 1/3 each. Thus, years prior to presidential elections only are ½ in Finland and France V as are years prior to parliamentary elections only. In the US, presidential and House elections each score 1/3 while the *whole* Senate scores 1/3, implying that each senate-election year scores (1/3)(1/3)=1/9. All US elections are assumed to occur the end of the first week of November.

Schultz (1995) argues that manipulating the economy is costly in terms of lost reputation for sound policy and/or future detrimental economic repercussions so incumbents would likely employ pre-electoral manipulation only when it is most necessary, *i.e.* when the upcoming election is expected to be close. Moreover, a government obviously cannot manipulate the budget in the year prior to an election if the election is unforeseen during that year. Therefore, an indicator varying with the expected closeness of a *foreseen* coming election would be still more ideal.

5. Years in which more than one government holds office are scored as the weighted (by the fraction each held office) average of those governments. Finnish and French presidents are treated as N cabinet ministers (N is the number of ministers). US senators are N/S representatives and presidents are N representatives (N is the number of representatives and S the number of senators). The term *CoG* is Thomas Cusack's. Gratitude also extends to Duane Swank who long ago shared a similar data set which was the inspiration for this one even though those data were not directly used here.

6. Tabellini and Alesina (1990) obtain similar results with explicit voting; there the median voter accumulates debt to shift future spending-composition in her direction.

7. Whether outright default or partial default on nominal debt *via* inflation is irrelevant; the latter is more plausible in developed democracies.

8. I also considered periods of 5, 7, and 9 years, centered on the present, from the present forward, and from the present back. None behaved very differently in empirical analyses.

9. SDaG is insufficient to characterize RR because, *e.g.*, it does not distinguish annually alternating CoG=4 and CoG=6 governments from the same two alternating every 4.5 years. Replacement risk is truly higher in the former situation, incorporating the higher HR reflects this. HR for Finland and France V is half the president's and half the cabinet's. In the US, it is 1/3 each the president's, the house's, and the senate's, which is constant: 1/[4/3+2/3+6/3]=1/4.

10. Alesina and Perotti's (1994) excellent review of the theoretical literature identifies several appropriate economic and socio-political controls. See Franzese (1996b, ch. 3) for more thorough empirical treatment.

11. Unemployment, growth, debt-service costs, and terms-of-trade effects should be net of their permanent values in the rational-expectations tax-smoothing model. If insisting upon that model, the implicit assumption here, as elsewhere in the empirical literature, is that the permanent values are constant.

12. “OECD sources” are *OECD National Accounts, Volume II: Detailed Tables*, diskette (1996), *OECD Economic Outlook and Reference Supplement* #62, diskette (1998), and hardcopy versions thereof and of *OECD Labor Force Statistics* (various issues).

13. “IMF sources” are *IMF International Financial Statistics*, CD-ROM (June 1996), supplemented by hardcopy where necessary and possible.

14. Three considerations allay concerns about this procedure. First, theoretically, interest rates should move together; second, empirically, they do so overwhelmingly: R^2 from fitting regressions invariably exceeded .8. Third, the resulting series performs significantly according to expectations. (Concern here is only to have controlled properly for economic conditions when testing the other theories, so the third is a valid consideration.)

15. Previous evidence is quite mixed. Roubini and Sachs (1989ab) found empirical support but measured fragmentation only, and that crudely *via* a 1-4 index categorizing governments as single- or multi-party and majority or minority. Edin and Ohlsson (1991) break Roubini and Sachs’ index into four separate indicators and find correlations of deficits and minority governments only. De Haan and Sturm (1994, 1997) and Borrelli and Royed (1995) find not even this, but Alesina and Perotti (1995) conclude that coalition governments less successfully implement fiscal adjustment than single-party governments. Franzese (1996b, ch. 3) distinguishes polarization and fractionalization and competing *influence* and *veto-actor* views of these theoretical concepts and models the effects appropriately as prolonging debt-adjustment rates. The evidence there and here unequivocally favors the argument.

16. Tsebelis (1995) argues that each governing-coalition member is a potential veto-actor, thus effective numbers of parties in government (ENoP) and standard deviations of their CoG scores (SDwiG), which weight coalition members by the share of the coalition they represent, are inappropriate measures of fractionalization and polarization. (Franzese 1996b, ch. 3) demonstrates that NoP and ADwiG empirically dominate ENoP and SDwiG as explanators of public debt.

17. Finnish and French presidents are members of government for veto-actor purposes. In the US, each house and the president are potential veto actors. Thus, when the president’s party controls both houses, the number of partisan veto actors, NoP, is one; when it controls one or neither house, the number of veto-actors, NoP, is two. When it controls both houses, ADwiG is 0; otherwise it is the distance between Democrats and Republicans (2.7887). For country-years with multiple governments, NoP and ADwiG scores are averaged according to each government’s share of the year.

18. *OECD Labor Force Statistics* contain annual estimates of population and breakdowns by age from 1955 to present in most cases. The data are extended where necessary by linear extrapolation of quinquennial estimates of breakdowns from *UN Age and Sex Demographics* coupled with annual estimates of total population from IMF sources.

19. This assumes hours worked are not an important source of income fluctuations. Since unemployment is controlled in subsequent analyses, the assumption may not be too dangerous.

20. As stated, the argument conflates constituencies and electoral districts. However, determining the effective number and relative importance of constituencies formed along dimensions other than geography is beyond this paper. Franzese and Nooruddin (1999) begin such an analysis (*n.b.*, also, the measures of government fractionalization employed in war-of-attrition models might represent some rough summary indication of the number of constituencies).

21. Suppose, *e.g.*, voters reward spending and punish taxes and deficits with votes, V, by $V=a(\ln(G))-b(\ln(T))-c(G-T)^2$. Then, politicians maximize votes by setting $a/G=b/T$, implying deficits of $G-T=G(1-b/a)$. If $a > b$, deficits are directly proportional to spending. Estimates in Franzese (1996b, ch. 2) could be interpreted to imply 25-50% proportionality.

22. Of those considered effectively federal, the US has 48-50 regions over the sample period, Canada 12, Germany 10 (16 post-unification and post-sample), democratic Spain 17, Switzerland 23.91 (cantons and half-cantons weighted appropriately), and Australia 8. All unitary systems have 1 federal region. The raw number of federal regions, its natural log, and a simple dummy variable distinguishing federal from unitary systems were considered, but including the number of federal regions and its square (see below) was superior in all empirical estimations.

23. Lane *et al.*'s classification of agrarian and ethnic parties employed. Belgium's split parties are not considered ethnic; only the explicitly ethnic RW, FDF, and VU are. Germany's CSU is regional and is assumed, crudely, to have 10% of the CDU's cabinet seats (unfortunately, CSU and CDU are not distinguished in all the cabinet data).

24. Excepting countries with a single, national, district.

25. Effective numbers differ from raw numbers because some countries employ multiple electoral tiers/rules, suggesting weighting by the proportion of government elected by each system. Presidential and effectively bicameral systems are viewed differently than unicameral systems. Details of their weighting schemes available upon request, but no scheme considered (many, ranging from lower-house exclusive to equal weights) performed noticeably differently.

26. Democracies should also run larger deficits than non-democracies (or at least than non-democracies of the right), but, empirical leverage on democracy's impact is very small in this sample. Only three non-democracies (Portugal and Spain through the mid-1970s, Greece from late-1967 through mid-1974), and only four changes in democratic status (three to democracy, one to non-democracy) exist in the postwar OECD sample. Furthermore, other theories required measurement of incumbents' left-right partisanship and of their replacement risk; coding these accurately across democracies and non-democracies, seems impossible. Thus, excluding non-democratic periods seemed safer. Four years prior and after non-democracies are also excluded so the replacement-risk measure would not include non-democratic periods.

27. Even direct and indirect taxes, and central- and general-government total-current-revenue had to be extended by careful splicing of data from hardcopy into the diskette series.

28. Absent fiscal illusion, if all taxes have the same effect on long-run growth, deficits should be independent of tax structure. If, instead, the effects of different taxes on the long-run growth-rate of

the economy vary, tax structure will correlate with debt and deficits even if voters are perfectly rational and not illuded. In that case, though, more-complicated tax-structures should hinder efficiency more, so expected long-run growth-rates are declining in indirect-tax proportions and increasing in total-tax and central-government-revenue proportions. Thus, deficits would be decreasing in indirect-tax proportions and increasing in total-tax and central-government-revenue proportions, directly opposite the predictions of the fiscal-illusion argument. The evidence (Franzese 1996b, ch. 3) supports fiscal illusion.

29. Indeed, one key indicator of CBI used empirically is whether the bank legally must buy any government debt that markets will not absorb. Central banks independent in this way make debt-issuance more costly rather more directly. Either way, CBI would dissuade governments from issuing debt.

30. *N.b.*, the analysis controls directly for debt-financing costs, so the estimated coefficients on CBI correspond only to the dissuasion effect (see Franzese 1996, ch. 3).

31.Using the scaled average of the available indices broadens the number of country-years covered and, under certain conditions, may also reduce *a priori* measurement error.

32. The usable sample is restricted by data availability and by the exclusion of non-democratic periods.

33. Elsewhere (Franzese 1996b, ch. 3), I demonstrate that each political-economy model, discussed here as controls, adds explanatory power to the basic economic-controls model (F-tests), and that each adds explanatory power to every other (J-tests). Thus, estimating this large, artificially-nested model is statistically warranted.

34. Beck and Katz (1995, 1997) show that PCSE dominates PWLS but PWLS+PCSE likely performs better still. Franzese (19996a) gives a GAUSS procedure to implement PCSE in samples, like this, which are non-rectangular and/or contain missing data and which allows PWLS+PCSE estimation. The procedure is available from the American University GAUSS program archive and from the author's homepage.