Private sector shrinkage and the growth of industrialized economies: Reply*

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1. Introduction

In my 1986 paper on private sector shrinkage, I tested the hypothesis that, *ceteris paribus*, economies with large and growing government sectors exert inverse influences on real economic growth. The data used in my paper were published in OECD (1985) and contained various measures of government spending and economic growth of 19 major industrialized economies over the interval 1960–1981. My analysis of the data concluded that there exists sufficient empirical evidence in support of the hypothesis that economies with relatively larger public sectors are associated with relatively poorer economic growth and that the results 'clearly call into question the wisdom of allowing public sectors to grow at the expense of private sectors' (Marlow, 1986: 152).

I quoted OECD's (1985: 14) observation that, 'Rather than being widely regarded as a major contributor to economic growth and macroeconomic stability, the view that the growth and financing of the public sector has, on balance, stifled growth now attracts widespread support.' Despite this 'widespread' view, I noted that, 'At the macroeconomic level, preliminary cross-country comparisons undertaken by the Secretariat have failed to reveal an inverse relationship between public sector size and economic performance as reflected in GDP growth rates ...,' OECD (1985: 15).

Saunders (1987) argues that, based on OECD's analysis and subsequent elaboration of the empirical results in Saunders (1985) and Saunders (1986), OECD's own analysis of these data are correct and that 'my conclusions appear to be unwarrantedly strong.' Saunders (1988) alleges that my analysis of the data contains numerous flaws; namely, my use of the unconventional real government expenditure series, 'extreme' sensitivity of my results to the time

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interval, my error in including Japan in the sample and the exclusion of 'appropriate' control factors in my regression equations.

In this paper, I submit the data to further scrutinity and conclude that the result that economic growth and government size are inversely related continues to be an obvious implication of the data.

2. Relative price effect

The first issue concerns the measurement of the scale of a government's participation in the economy. Saunders states that my use of the price-deflated series on government expenditures 'stands in contrast to all other work in this area, which adopts the more conventional ratios of nominal expenditures to nominal GDP.' The basic issue concerns the 'relative price effect' which refers to the possibility that the growth of the share of nominal government spending may differ from the growth of the share of real government spending. That is, the real index, GE(R) = constant dollar government spending/constant dollar GDP, will differ from the nominal index, GE(N) = nominal government spending/nominal GDP, when the price indices of government spending and GDP differ.

Saunders' comment is curious since, as discussed in Saunders (1986), there is a debate on this issue; e.g., see Beck (1976), Beck (1979), Beck (1985) and Heller (1981). In any event, it would appear that given the lack of a consensus on the issue, it might be appropriate to ask what difference the relative price effect makes on the estimation of the government size – economic growth relation. Tables 1–3 display estimations of the basic equations of my 1986 paper:

$$\text{RGDP} = \alpha_0 + \alpha_1 \text{GE} + \epsilon \quad (1)$$

$$\text{RGDP} = \beta_0 + \beta_1 \text{GE} + \beta_2 \text{RGE} + \phi \quad (2)$$

where RGDP = compound annual rate of growth of real GDP

GE(R) = ratio of real government expenditure to GDP in initial period of RGDP measurement;

GE(N) = ratio of nominal government expenditure to nominal GDP in initial period of RGDP measurement;

RGE(R) = compound annual growth rate of GE(R);

RGE(N) = compound annual growth rate of GE(N);

ϵ, ϕ = random disturbance terms.

I have deleted one set of equations from my 1986 period – those which run RGDP on only a constant and RGE. These were routinely poor fits and do not

alter the present discussion. I have also deleted the equations dealing with social expenditures for brevity and to focus on the main source of Saunders' criticism. In contrast to Saunders (1987), I have displayed the regression results for (1) as well as (2). As can be seen, either measure of GE is highly statistically significant for the period 1960–1970 and 1960–1980. As in my 1986 paper, the government scale variable is generally not statistically different from zero over 1970–1980. Moreover, as shown in Saunders' comment, the use of a nominal-based government scale variable GE(N) results in higher levels of statistical significance and relatively large increases in the equations' explanatory power. Given that my 1986 paper did not find much support for the hypothesis that the rate of change of government expenditure exerts a significant influence on economic growth, the 'relative price effect' issue would appear to offer no additional information on the validity of that hypothesis. Consequently, based on the fact that there exists no consensus on how to measure government scale and the fact that Saunders' preferred measure actually provides stronger support for my hypothesis, I view this particular criticism as curious.

3. Excluding Japan from the sample

The next criticism deals with the assertion that Japan should not be included in the sample of observations – an assertion that I interpret as implying that Japan has no relevant information for us as economists. For some reason, Saunders believes this particular member of the OECD should be treated as different from the other member countries.[1] Interestingly, Saunders offers no *a priori* reasoning to justify this deletion from the sample. Rather, it would appear that justification arrived *ex post* after Saunders observed that Japan is a major contributor to the inverse relation between economic growth and government size. Without a doubt, Japan has a relatively high economic growth rate over these periods and a correspondingly low level of government participation in the economy. I wonder if economists could infer any valuable lessons from this country? Proponents of its deletion may suggest not. Their argument may be based on the fear that its inclusion might 'contaminate' the testing of the economic growth-government size hypothesis.

Somewhat curious about the criticism, I re-estimated my equations without Japan. Using both real- and nominal-based measures of government scale, I re-estimated my equations and they are displayed in the lower parts of Tables 1–3. Saunders is correct on one count: the significance levels and goodness of fit do fall when Japan is excluded. However, this is the expected result of a scheme that seeks to limit the amount of variation in the variables being examined.

Table 1. Government growth and economic growth, 1960–1970 (regression results) – dependent variable: Real GDP growth 1960–1970

Equation	Constant	GE(R)	GE(N)	RGE(R)	RGE(N)	$\bar{R}^2$	N
(1)	12.91	−23.97				.32	16
	(4.76)	(2.87)					
(2)	14.21		−31.69			.56	16
	(6.95)		(4.46)				
(3)	13.11	−23.91		−0.40		.39	16
	(5.06)	(3.00)		(1.54)			
(4)	14.26		−31.70		−0.03	.52	16
	(6.53)		(4.30)		(.09)		
			Excluding Japan				
(5)	8.06	−9.82				.06	15
	(3.33)	(1.34)					
(6)	9.76		−16.85			.18	15
	(3.96)		(2.01)				
(7)	8.34	−10.52		−0.07		−.02	15
	(3.14)	(1.33)		(0.32)			
(8)	9.55		−16.57		0.07	.11	15
	(3.59)		(1.89)		(.28)		

One fact holds clear: exclusion of Japan does not invalidate the hypothesis. Whether one wishes to use a one-tailed or two-tailed test of significance, one or both of the measures of government size continues to be statistically different from zero. Why the difference in conclusions reached here and by Saunders? Several explantions may be correct. One explanation may be that Saunders appears to prefer to test the hypothesis using a two-tailed test. This may be an arguable point and should depend on how one formulates the alternative hypothesis, which, in turn, generally is related to some *a priori* considerations or previous empirical evidence. If one views the hypothesis in (1) as H_0: $\alpha_1 = 0$ vs. H_a: $\alpha_1 \neq 0$, then a two-tailed test is appropriate. A one-tailed test would be appropriate for the hypothesis H_0: $\alpha_1 = 0$ vs. H_a: $\alpha_1 < 0$.

I originally chose a one-tailed test, which, in part, may explain some of his questioning about my statement of levels of significance. The rationale behind this choice was my expectation that the coefficients on α_i are negative and that, at best, one could expect government size to exert a neutral influence on economic growth. On page 145, I discussed the possibility that governments could exert a 'facilitating' role on the economy, but that the public sector growth over this period was considerably above any threshold level that might be wedged in between government growth-as-facilitator of economic growth and government growth-as-impediment to growth. The observation that, based on Table 1 of my 1986 paper, the average level of the government's share

of GDP (GE(R)) had risen from .32 to .40 over 1960–1980 appeared to be ample evidence in support of the construction of my hypotheses. Based on Saunders' preferred measure GE(N), government's average share of the economy grew from .28 to .42 over the same period. Moreover, the construction of a one-tailed test would appear to be consistent with OECD's statement, '... Rather than being widely regarded as a major contributor to economic growth and macroeconomic stability, the view that the growth and financing of the public sector has, on balance, stifled growth now attracts widespread support' (p. 14). Moreover, prior empirical evidence exists in Landau (1983), Orzechowski (1984), and Weede (1984) that, to varying degrees, public sector size is inversely related to economic growth. While less than conclusive, these studies are available to Saunders and were listed in my 1986 paper. Moreover, Saunders (1986) cites Bacon and Eltis (1978), Smith (1975), Cameron (1978), Marsden (1983) and Gould (1983) as, at least, suggestive of an inverse relation between government size and economic growth. Therefore, contrary to his assertion, my results are not inconsistent 'with most of the other empirical work on the relationship between the size and growth of government in industrialized economies.' It would appear appropriate for Saunders to provide the implied long and impressive litany of empirical studies showing the opposite of my work before one can accept his assertion that my 1986 paper is inconsistent with the majority of empirical work on this issue.[2] In addition, Saunders might consider providing studies that estimate a positive relation between government size and growth before one might conclude that a two-tailed test is the appropriate hypothetical construct.

After having said all this, it turns out that the choice of a one-tailed test vs. two-tailed test is not critical to the acceptance of my hypothesis, with or without the inclusion of Japan. Using Saunders preferred measure of government size GE(N) and two-tailed tests, the coefficients on GE(N) are statistically different from zero during the intervals 1960–1970 and 1960–1980 at the respective levels of .10 and .05, or better. Curiously, Saunders lists only real-derived government scale variables, GE(R), in his comment on my paper. In conclusion, my results are not extremely sensitive to the inclusion of Japan.

4. Choice of time period

The criticism that my results are extremely sensitive to the time period chosen stems from empirical results presented in Saunders (1985: 11). He interprets his 1985 paper as suggesting that economic growth and government growth are inversely related over 1960–1973 and *not* 1975–1981. Three points are in order. One, as discussed in the section below, his empirical model includes a misspecification which casts considerable doubt on the inferences drawn from his

Table 2. Government growth and economic growth, 1970–1980 (regression results) – dependent variable: Real GDP growth 1970–1980

Equation	Constant	GE(R)	GE(N)	RGE(R)	RGE(N)	$\bar{R}^2$	N
(1)	5.05	−4.89				.04	16
	(3.82)	(1.29)					
(2)	5.05		−4.89			.04	16
	(3.82)		(1.29)				
(3)	5.27	−4.06		−0.34		.05	16
	(3.94)	(1.04)		(1.03)			
(4)	5.40		−5.39		−0.09	.00	16
	(3.62)		(1.35)		(.56)		
			Excluding Japan				
(5)	3.90	−1.76				−.06	15
	(2.29)	(.37)					
(6)	3.90		−1.76			−.06	15
	(2.29)		(.37)				
(7)	3.84	0.30		−0.46		.00	15
	(2.33)	(.06)		(1.37)			
(8)	4.15		−1.57		−0.17	−.06	15
	(2.41)		(.33)		(1.02)		

Table 3. Government growth and economic growth, 1960–1980 (regression results) – dependent variable: Real GDP growth 1960–1980

Equation	Constant	GE(R)	GE(N)	RGE(R)	RGE(N)	$\bar{R}^2$	N
(1)	9.95	−17.61				.37	16
	(5.47)	(3.14)					
(2)	10.56		−22.07			.56	16
	(7.47)		(4.49)				
(3)	10.07	−17.16		−0.27		.36	16
	(5.46)	(3.02)		(.83)			
(4)	10.94		−22.74		−0.09	.53	16
	(6.46)		(4.30)		(.44)		
			Excluding Japan				
(5)	7.23	−9.69				.12	15
	(3.92)	(1.73)					
(6)	8.33		−14.64			.23	15
	(4.39)		(2.27)				
(7)	7.30	−9.78		−0.04		.05	15
	(3.68)	(1.67)		(.14)			
(8)	8.75		−15.27		−0.13	.19	15
	(4.23)		(2.28)		(.60)		

statistical tests. Two, the coefficient on government size is negative and significant, over 1975–1981, on the basis of a two-tailed test at the .10 level and, on the basis of a one-tailed test, at the .05 level. Three, my regression results for the interval 1960–1980, in Table 3, show statistical significance for the entire period. Even if Saunders' suggestion that the period of the 1970s is sensitive to the 1973 oil shock and simultaneous decline in economic growth is true, these complications should only be of a transitory nature. As displayed in my estimations of the period 1960–1980, these short-run disruptions, if present, do not appear to be fundamental to the long-run implications of government size on economic growth. Consequently, it is difficult for me to take this comment seriously.

5. Misspecification of my equation

Saunders argues that '... none of the results are grounded in any model of the growth process, however naive this may be. By excluding other factors which influence economic growth, their impact may be wrongly interpreted to those explanatory variables which are included in the analysis.' In other words, Saunders asserts that my conclusions result, in part, from omitted variable bias. To correct my 'naive' formulation of the growth process, Saunders includes two new variables: the share of non-residential gross fixed capital formation in GDP averaged over the relevant time period and the share of civilian employment in agriculture in the initial year of each period. The extent of the economic reasoning behind their inclusion is only to 'capture the impact of investment expenditure on economic growth and the possibilities for increased growth potential in economies with larger agricultural sectors.'

It only takes a little simple macroeconomic theory to show the expected effect of including an investment variable in the equation. Standard treatment of the relation between capital K and government size G is

$$K = f(\tilde{G}) + \mu \tag{3}$$

which postulates that government crowds out capital-formation. See Aschauer (1987) for a recent paper, with supporting empirical evidence, on the effects of public expenditure on private capital accumulation. With Y = output and a production relation of the form

$$Y = f(\overset{+}{K}) + \epsilon \tag{4}$$

it follows that estimation of

$$Y = f(K, G) + \gamma \tag{5}$$

may find the coefficient on G to be zero since Y is inversely related to G.

In other words, if (3) and (4) are true, then estimating (5) is incorrect since the coefficient on G is calculated after the correlation between K and G is taken account of, and, if G exerts all influence on Y via K, then after regression of Y on K and G, there is no remaining influence left for G to exert on Y. In this case, then

$$Y = f(G) + \delta \tag{6}$$

is the true reduced form of (3) and (4), not (5). Saunders' result that G = 0 after estimating (5) is consistent with my work when (3) and (4) are true.

The above demonstrates that economic theory does not provide the basis for Saunders' findings that government size does not influence economic growth once one controls for the effects of capital formation on Y. While some may be tempted to 'throw' as many variables into an equation as one has data, the applied researcher must be careful to apply a 'little' theory so as to not include inappropriate variables along with appropriate variables. Inclusion of inappropriate variables could lead one to believe that economic growth is not related to government size when, in fact, an appropriately specified equation would demonstrate the inverse relation.

Interestingly, Saunders (1985) reports '... Of course, if a larger government sector has crowded-out private investment and thus lowered the investment share, the overall negative impact of public expenditure on economic growth will be somewhat greater than implied by the coefficient on (G/Y) itself.' Moreover, Saunders (1985) cites Smith (1975), Cameron (1978), and Marsden (1983) as providing some *empirical* evidence that, given a positive relation between investment and economic growth, '... an inverse relation between the public expenditure share and economic growth was implied' (p. 11). It is a curious matter as to why the specification problem was hinted at in earlier work but not in the recent comment on my paper.

6. Conclusion

Further tests and thoughts on the OECD data lead me to conclude that, if anything, my 1986 paper *underestimated* the magnitude of the inverse relation between economic growth and government size. If one takes the nominal-based measure of government scale, as advised by Saunders, the significance levels,

coefficient magnitudes and goodness of fits improve over what I found with my initial investigation. I would suggest that Saunders reconsider his reluctance to believe that the size of the public sector is unrelated to economic growth in OECD countries over this time period.

One additional thought appears relevant to the current policy debate concerning budget deficits and economic performance within the major industrialized economies. The empirical work displayed here and in my 1986 paper suggests serious problems associated with the various proposals urging governments to raise taxes and/or 'ease' fiscal policy. Elsewhere, I have suggested that available empirical evidence implies that plans to increase taxes as a way out of budget deficits are plans that carry the potential for raising government spending and possibly future deficits as well.[3] Coupled with the evidence presented here, we should also recognize the potential of tax increases to raise the level of government participation in a country and, accordingly, exert inverse influences on its future economic performance as well. As suggested in my 1986 paper, the empirical evidence may suggest the following irony: While political participants may crave larger and larger non-market resource allocations, their future ability to satisfy that craving may very well be severely constrained by the satisfaction of that same appetite.

Notes

1. It is interesting that Saunders (1986: 58) concludes the opposite. 'The potential insights that might follow from further study of the Japanese experience within a comparative cross-country framework would appear considerable. This is surely a more fruitful way to proceed than to discard the informational contents of the Japanese case by treating it as a statistical outlier which distorts "more reliable" relationships.'
2. The interested reader could see Barth, Keleher and Russek (1987) for a useful discussion of the available evidence on the government size – economic growth relation. Recent empirical evidence on the consistency of an inverse relation between government size and economic growth within different equation specifications is contained in Barth and Bradley (1987).
3. Marlow (1987) discusses the available empirical evidence on the tax-spend hypothesis and argues that, even if one does not accept the budget constraint type hypothesis, the hypothesis that expenditures will also rise along with legislated tax increases can not be reasonably rejected.

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