

# **Eighteen Problematic Propositions in the Analysis of the Growth of Government**

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**E**conomists, public choice analysts, political scientists, and other scholars, especially during the past 10 to 15 years, have made many studies of the growth of government. As the literature has grown, a number of conventions have become established with respect to concepts, measures, assumptions and modes of analysis. Certain contributions have been viewed as paradigmatic and hence have served as models for subsequent contributors. No analytical consensus has emerged. Indeed, one can perceive the outlines of several competing “schools”— a Chicago school, a Washington school (see Proposition 16), a mainstream economics school, a libertarian school, several distinct positions within the public choice community of scholars, and others.

Despite the diversity of approaches and conclusions, much of the work has been premised, implicitly if not explicitly, on the acceptance of propositions that are questionable at best. In what follows, I shall state these propositions and criticize them. Although I shall provide citations and examples of scholars who have advanced or accepted the flawed propositions, my aim is not to compile a catalog of sinners. The examples are intended only to provide concrete illustrations of how various analysts have proceeded and to demonstrate that I am not quarreling with phantoms.

The discussion that follows pertains mainly to the growth of government as it has occurred in the countries of Western Europe and their overseas offshoots during the past two centuries, especially

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during the twentieth century. I have specific expertise with regard to only one case, the United States, so much of my discussion relates especially to that case. This restriction of the frame of reference does little harm, because the ideas I shall criticize have themselves been employed in the same empirical domain for the most part. Further, as will become obvious, I believe that attempts to achieve universally applicable explanations of the growth of government are doomed to fail in any event. I disavow at the start any pretension of contributing to the construction of a single all-encompassing theory.

### **Proposition 1**

*Government activities can be reduced to a single variable (the "size" of government), which can be accurately measured.*

Modern governments undertake many distinct activities. They take money away from people by taxation and fines; they deliver the mail; they operate law courts where citizens resolve various disputes and tennis courts where people work on their backhands; they conduct medical research; and so forth in nearly endless variety. To sum up the various activities, one must measure each of them in a common unit—persons on the government payroll, for example, or dollars spent by the government. These methods of achieving commensurability seem to make sense until one inquires a bit deeper.

Suppose that, *ceteris paribus*, the government has added a billion dollars to its spending for operating the law courts and cut a billion dollars from its spending for farm subsidies. Has the government grown? If the changes had been reversed, would the government have grown? The answers are far from obvious. Government (as a set of activities) is what government (as a group of people) does, but because governments do so many diverse things, no common unit of account can scale the underlying reality satisfactorily.

Often government employees or dollars work at cross purposes in their impact on the economy. Many analysts have noted the prodigious "cross hauling" or "churning" associated with modern government activities (Becker 1983, p. 389; 1985, p. 341; Musgrave 1985, p. 305). On the one hand, government wheat researchers develop higher yielding varieties of the crop, thereby increasing the supply and decreasing the price. On the other hand, government acreage restrictions decrease the supply and increase the price. Such examples can be multiplied indefinitely. I do not mean to suggest that the churning is accidental or politically irrational in its inception, because interested parties set each part of the process in motion with their eyes open and their hands grasping. The implication for muddled measurement remains, however, regardless of the motives involved.

In view of the heterogeneity, incommensurability, and offsetting impacts of many government activities, the information content of any one-dimensional measure of “the” size of government verges on nil (Peters and Heisler 1983, pp. 178-81, 186; Rose 1983, p. 7). Much more informative would be an answer to the question: *What in particular* is government doing more frequently or less frequently? Most analysts of the growth of government simply ignore this problem.

## **Proposition 2**

*The best measure of the size of government is relative government spending, the ratio of government spending to the gross national product. Good alternative measures include relative tax revenues (the ratio of tax revenues to GNP) and relative government employment (the ratio of government employees to labor force).*

Many analysts forgo entirely an attempt to justify measuring the size of government as the ratio of government spending to GNP. (Often GDP and occasionally NNP or National Income serve as the denominator.) They just plunge ahead (Lowery and Berry 1983, pp. 666-67; Mueller 1987, p. 115), noting, if anything, that “everybody does it.” But choices still must be made. Should the analyst include all government spending, including transfer payments, or only the government’s “exhaustive” spending for newly produced final goods and services, which is a component of GNP as conventionally defined? Both measures are used. Frequently, however, as in the United States during the past 40 years, the two measures behave quite differently—in this case the all-spending ratio tends to rise more or less steadily while the “exhaustive” measure remains more or less level (U.S. Council of Economic Advisers 1989, pp. 78, 397, 402). Regardless of the exact measure selected, using relative government spending to measure the size of government gives rise to many curiosities.

Consider some cases. (1) Suppose the cereal makers produce and sell more corn flakes, but nothing else changes. Implication: government has shrunk. (2) Suppose people from the Defense Department sit down with people from General Dynamics and agree to pay more per unit for this year’s purchase of (the same number of) F-16s, but nothing else changes. Implication: government has grown. (3) Suppose the government switches, as it did in 1973, from a military conscription system to a volunteer military force, which will entail payment of higher salaries to military personnel, but nothing else changes. Very strange implication: government has grown. (4) Suppose that local governments across the country stop operating and

spending money for sewerage plants, mandating instead that every home or business releasing sewage into the system ensure that the effluent meets strict treatment standards, with all costs to be borne by the private sewage generators, but nothing else happens. Implication: government has shrunk. Such examples can be produced virtually without limit. Nor are the examples merely contrived. Arbitrary or counterintuitive determinants of changes in the government's relative spending are part and parcel of this measure of the size of government.

Similar observations, and many others, might be made with respect to using relative tax revenues as an index of the size of government. Whenever an index is a ratio with GNP as its denominator, all sorts of oddities may arise. In the workaday world of government fiscal reports, the repeated shuffling of various taxes, especially some or all of the Social Security tax, between on-budget and off-budget status further confuses the historical record (U.S. Office of Management and Budget 1989, p. 6).

Michael Boskin (1987, p. 60, emphasis added) recently concluded that, for various reasons, "the accounting problems are so fundamental and pervasive that federal budget figures can not be used to compile an accurate representation of our *fiscal* history." (See also the strictures of Stiglitz [1989, p. 68] on the misleading way the national accounts treat government enterprises.) *A fortiori*, these figures cannot serve as a reliable basis for measuring the overall size of government in all its significant economic dimensions.

How, for instance, should one take into account the various activities of government in the credit markets? Governments now make many types of loans on their own accounts, insure private loans, subsidize or grant tax breaks on the extension of certain loans, and insure—sometimes far beyond the explicit promise—deposits in banks and savings institutions. Joseph Stiglitz (1989, p. 63) notes that "in the US today, approximately a quarter of all lending (to the private sector) is either through a government agency or with government guarantees. . . . The magnitudes of the implicit subsidies and costs—both the total value, and who receives how much—are hidden."

Relative government employment also is a fragile index of the size of government, partly because governments hire millions of "contractors" (Hanrahan 1983). These workers are classified as members of the private labor force, even though they work exclusively on projects set in motion by governments and receive compensation entirely, if often indirectly, from government revenues. Why are they considered any more "private" than regular government employees? Only be-

cause of legal technicalities and accounting conventions that do not reflect the substance of the matter. As the composition of the total effective government workforce (regular government employees plus "private" government contractors) changes, as it often does, the standard index of relative government employment becomes a spurious indicator of whether government has grown or shrunk.

### Proposition 3

*Even if relative government spending (or one of the commonly employed "good alternative measures") doesn't properly measure the true size of government, the two are highly correlated over time, and hence relative government spending is an adequate—indeed indispensable—proxy variable for empirical analysis.*

Many analysts know that acceptance of this proposition is risky (Lindbeck 1985, pp. 314, 325; Borcharding 1985, pp. 376-77). Yet most proceed, often into extremely intricate modeling and highly sensitive econometric analysis, without further ado. Sam Peltzman, in a widely read and cited study (1980, p. 209), was commendable for his candor:

I am going to equate government's role in economic life with the size of its budget. This is obviously wrong since many government activities (for example, statutes and administrative rules) redirect resources just as surely as taxation and spending, but the available data leave no other choice. My operating assumption has to be that large and growing budgets imply a large and growing substitution of collective for private decision in allocating resources.

This rationale, accepted by many others besides Peltzman, has several defects.

It simply is not true that one has no choice. There are mountains of evidence not only about the details of spending and taxing but about the multifarious commands expressed in statutes, regulations, and judicial rulings, all of which sit in the archives and libraries awaiting researchers. Perhaps studying such nitty-gritty evidence is beneath the dignity of modern, "high-powered" economists. If so, they need only make the "operating assumption" that a single data series, which they can retrieve from a standard statistical source, provides all the information required for an adequate analysis of the complex phenomena that constitute the actual behavior in question. One is reminded of the old joke about the people marooned on an island with cans of food but no can opener. After a chemist and a physicist propose esoteric technical solutions, the economist in the group offers his way of dealing with the problem: "Let us assume we have a can opener."

As for the assumed high correlation between the observed data series and the unobserved reality, how does one know? Unless one makes an effort to establish at least the likelihood of a close correlation, one is simply making a raw assertion, a leap into the void. (Borcherding [1985, p. 377] frankly recognizes the problem but does nothing about it.)

#### **Proposition 4**

*Point-to-point or trend-rate measures are adequate explicanda for the analysis of the growth of government.*

Analysts of the growth of government often rely on only a portion of their data (forget for the moment all that is wrong with the data anyhow). They may simply compare the size of government at one time with its size at a later time. Noting that government grew  $X$ -fold between the two dates, they proceed to explain the One Big Change by relating it to Other Big Changes in explanatory variables during the interim (Borcherding 1985, pp. 362-69). Others fill in more blanks, examining measures for "selected years" (North and Wallis 1982, p. 337; Bernholz 1986, pp. 662-63; Mueller 1987, pp. 116-17). Still others compute from annual or semi-annual data a series of decade averages or a trend rate of change, making that their explicandum (Bernholz 1986, pp. 664, 676, 678). In each case valuable information is ignored, at great risk to the validity of the analysis.

For example, Gerald Scully (1989, p. 6.93) makes much of a shift from local government spending to state and federal government spending in the United States between the average for 1902-1927 and the average for 1960-1988. Had he examined all the available data, he would have discovered that almost the entire shift occurred between 1932 and 1936 (Wallis 1985, p. 5). Obviously the change had more to do with the Great Depression and New Deal politics than with the long-term changes in the focus of rent-seeking emphasized by Scully.

Aside from the inadvisability of throwing away information in an empirical analysis, one has a more fundamental reason for examining the full sequence of data: the growth of government has been a path-dependent process. Because social understandings gained from experience constrain social beliefs and actions, where the relation of government to the economy can go depends on exactly where it has gone—that is, what precisely people's experiences have been—in the past. One needs to examine the entire profile of the growth of government to discover the dynamic interrelations of ideas and events over time. (Analysts who emphasize path-dependency include Hughes [1977] and Higgs [1987a].)

Some analysts believe that it is better to smooth the data or even to omit certain deviant years from consideration (Meltzer and Richard 1983). These analysts view the unruly observations as unlikely stochastic deviations from a smoothly changing central tendency; they prefer that their statistical analysis not be contaminated by “outliers” (and maybe that their coefficients of determination not be diminished). I shall criticize the theoretical foundation of these views when I discuss Proposition 7 below. For now it suffices to observe that the crises of history, when government expanded abruptly, were real. People did not forget them. Indeed, people were deeply affected by such experiences and later behaved differently as a result.

### **Proposition 5**

*Government can be analyzed as something having an abstract “functional” relation to the economy; it is unnecessary to consider government officials as autonomous decision makers having genuine discretion and making real choices.*

The approach implied by this proposition frequently appears as what I call the Modernization Hypothesis, which maintains that a modern, urban-industrial, technologically advanced economy simply must have a big, active government. Modern socio-economic affairs are so complex. How could they possibly take place successfully without the guiding, regulating, coordinating hand of government? “The increased complexities and interrelationships of modern life,” said Calvin Hoover (1959, p. 373), “necessitate this extension of the power of the state.” Supreme Court Justice William Brennan echoed this view in a 1985 speech. “The modern activist state,” he declared, “is a concomitant of the complexity of modern society; it is inevitably with us” (quoted by Kozinski n.d., p. 6).

One doubt arises immediately. Why is government so much bigger in some countries than in other, equally modernized countries (say, Sweden vis-à-vis Switzerland)? But this is not the most fundamental problem.

Anyone who has understood the message of Adam Smith, not to speak of the more penetrating and pertinent contributions of Ludwig von Mises and Friedrich Hayek, immediately doubts the Modernization Hypothesis. Indeed it seems backwards, for whereas government might be able to coordinate a simple pre-modern economy, it certainly could not coordinate successfully a complex modern economy. The now undeniable failure of all the centrally planned economies, confirming the early insights of Mises and Hayek, clearly supports the Austrian position on this question.

But my point is different; it has to do with methodological indi-

vidualism. Even if it were true that a modern economy “requires” bigger government for its effective coordination, the Modernization Hypothesis would be virtually worthless as an explanation of the growth of government. The fatal flaw is the absence of a human actor. Just because a course of action is “necessary” in some systemic sense for the successful operation of an economy does not ensure that anyone has a personal incentive to work toward fulfilling the requirement. In the Modernization Hypothesis the process is magical: the economy “needs” bigger government—POOF!—the government grows. But who did what to make it happen? And why did these actors find it in their interest to take such actions? To these questions, the Modernization Hypothesis, like every other “functional necessity” explanation, has no answer.

Functional necessity explanations implicitly view government officials as robots who lack genuine discretion and make no real choices, automatons programmed to accomplish whatever is necessary to optimally serve the known, unambiguous “public interest.” This view is mystical and obscurantist; it is also patently, empirically wrong. (For more general observations on methodological individualism and “the false organismic analogies of scientism,” see Rothbard [1979, pp. 15-17, 57-61] and sources cited there.)

### **Proposition 6**

*Government can be analyzed as if it were a single decision maker; it is unnecessary to consider conflicts of interest within government or migration back and forth between the ruling group and the ruled group.*

The difficulties of formal modeling and the analytical attractions of simplicity have enticed many analysts to embrace this proposition. (An outstanding example is Auster and Silver [1979].) Of course, in ordinary discourse and in newspaper columns, we frequently encounter statements that “the government” did something or “the government” decided such and such, without any specification of which government officials in particular took the action. Sometimes such usage is a harmless abbreviation. But more is at stake when analysts adopt such a conception.

Even so astute a scholar as Douglass C. North (1981, pp. 20-32) built his “neoclassical theory of the state” on the assumption of “a state with a single ruler . . . a wealth- or utility-maximizing ruler” (p. 23) who can act in a way that his subjects cannot because, as a single person, “he has no free rider problem” (p. 32). However useful this conception may be in understanding a medieval lord of the manor or the court of Louis XIV, it has virtually no applicability to the govern-



ments of the western world whose growth during the last two centuries concerns us here.

Modern governments consist of thousands of important decision makers, not to mention the millions of minions who have at least a bit of discretion in carrying out delegated activities. In the United States today, for example, there are more than 80,000 separate governments, more than 60,000 with the power to tax (Higgs 1987a, p. 6). Moreover, people are constantly passing back and forth between the ruling group and the ruled group. The “revolving door” is notorious at the Department of Defense, but a similar phenomenon occurs at many other places in the government. In many instances, one would be warranted in regarding certain persons formally outside the government as more a part of it than most of those formally inside it—just think of the exogovernmental potency of such figures as Walter Lippmann, Felix Frankfurter, John J. McCloy, David Rockefeller, and Henry Kissinger, to name only a few notables among many. (For a plethora of contemporary examples, see Dye [1990].) In any event, no one person, no small group, calls the shots for the whole hydra-headed creature that is “the state.” People within the ruling circles, though they may share at least one goal (retaining their own powers and privileges) constantly engage in internecine struggles. Supposing that the government operates as if it were a single decision maker cannot take us far toward a realistic understanding of modern government or its growth.

### Proposition 7

*There exists a structure of politico-economic behavioral relations (an “underlying model”) whose workings generate the growth of government as a dynamic equilibrium outcome; and this structure does not change over time.*

Whether they think about it or not, analysts who test their theories of the growth of government by fitting a linear regression model to the time-series data for a certain period are accepting this proposition. Econometric theory admits of no exception if the estimated coefficients are to have the meaning they are supposed to have. Thus, if the theory contains the equation

$$(1) \quad G = \alpha + \beta X + u$$

and, using linear regression techniques, one estimates the parameters  $\alpha$  and  $\beta$  from time-series data for the years 1901-1989 as  $a$  and  $b$ , respectively, then one is assuming, *inter alia*, that the politico-economic world was working such that whenever  $X$  took the value  $x'$ , then  $G$  as a result took the value  $a + b(x')$ , plus or minus a purely

random amount  $u'$ , and this result was the case regardless of whether  $X$  took the value  $x'$  in 1901, in 1989, or in any other year during the time period to which the model is fitted. That is, the underlying model is assumed to be invariant as specified by equation (1). The econometric estimation is designed only to ascertain the numerical values of the parameters, not to test or otherwise call into question the functional specification of the model. The specification is presumed to be given to the investigator by his theory independent of any empirical observation—in effect, by divine revelation, though the source may well be one of the lesser deities.

Suppose that  $G$  denotes total government spending and  $X$  denotes total personal income as defined in the standard national income accounts. Suppose further that the estimated value of  $\beta$  turns out to be  $b = 0.3$ . The interpretation would be that every additional dollar of personal income gave rise to an additional 30 cents of government spending, *no matter when* during the period that extra dollar came into people's possession: the identical quantitative linkage existed for income changes occurring between 1901 and 1902, between 1988 and 1989, indeed between *any* two years in the test period, whether the pairs be 1933-1934, 1945-1946, or any other. The dates just don't matter—by assumption.

Is the assumption plausible? No. The world of 1901 differed in many pertinent ways from the world of 1989. Among other differences, people at the two dates had quite different ideas about what they wanted the government to do. In the United States in 1901 many people still thought in terms of a variant of classical liberal ideology. They wanted not much more than a night-watchman state, and they already had more than that (Higgs 1983; 1987a, pp. 77-105; 1989c, pp. 92-98). In 1989, in contrast, most Americans had relatively inflated ideas about the range of social and economic "problems" they wanted the government to "solve" (Smith 1987; Higgs 1989c, pp. 101-03). Even if the ideology had not changed—and historians may reasonably differ about precisely how and when it did shift—socio-economic and political conditions certainly had changed enormously. In 1901, a majority of the population still lived in rural areas and 43 percent of the labor force worked in farming, fishing, and mining. In 1989, less than a quarter of the population lived in rural areas (many of them with easy access to a city) and less than 4 percent of the labor force worked in farming, fishing, and mining. These differences in socio-economic conditions are but two of the many that starkly distinguish the people of 1901 from those of 1989. Wouldn't it be strange if people so differently situated, even without subscribing to different views concerning the desirable scope of government, should just

happen to get 30 cents of additional government spending every time their personal incomes rose by a dollar? Very strange indeed. If such constancy were found to have been the case, wouldn't the analyst be on firmer ground to interpret it as a coincidence, a parametric peculiarity, rather than the manifestation of a politico-economic law? After all, the meaning of 30 cents of additional government spending—the precise collection of goods and services associated with it—was dramatically different in 1901 and 1989.

Further, given that people's behavior depends on their ideas and that people learn from their experiences, it is extremely unlikely that an aggregative "behavioral" relation between a more or less inaccurate index of government activity and any of the usual "explanatory" variables would have remained invariant over nearly a century of tumultuous experience—wars, depressions, deflations, labor upheavals, inflations, energy crises, environmental panics, and so forth (contra Becker [1985, p. 332], who postulates a similar sort of constancy, and Peltzman [1985], who claims to have confirmed a related political stability econometrically). Can we really believe that none of these great events budged people's commitment to, or acquiescence in, spending 30 cents out of every additional dollar of personal income on government? Even regarding much shorter periods, similar doubts may be raised. Can anyone really believe, for example, that the structure of politico-economic behavioral relations did not change in the United States between 1929 and 1933?

We might well take seriously the conclusion reached by Assar Lindbeck (1985, pp. 325-26): "there is no compelling reason to model a process of an expansion of public spending [or the growth of government in other dimensions] as a series of static equilibria positions at different values of a set of exogenous variables, or even as a dynamic sequence of equilibria." We are dealing with "a disequilibrium process, the *speed* of which is determined by characteristics of political competition."

A final caveat, noted by Johan Myhrman (1985, p. 279), pertains to the example itself: "we have to avoid the temptation that many have fallen for and that is to conclude that rising income is the *cause* of the growth of government." Temporal association, no matter how close, does not establish a causal relation in any event.

### **Proposition 8**

*Which particular persons compose or influence the government doesn't matter. Only broad socio-economic changes and the relative strengths of interest groups need be considered.*

In analyzing the operation of the market system, economists are accustomed to ignoring the personal identities of the actors; and usually they are justified in doing so. We can probably understand the demand for and supply of potatoes well enough without naming consumer Jones as a demander and farmer Smith as a supplier. In markets with many small demanders and suppliers, no one in particular has any perceptible influence over the prevailing price or the volume of sales. So nothing is gained by worrying about specific people.

When economic methodology has been carried over to the analysis of political, governmental, and legal matters, the nameless quality of the analysis also has been carried over. Hence, public choice scholars speak of voters, legislators, bureaucrats, and others only as anonymous members of categories of actors. The theory is supposed to apply regardless of which particular person occupies a theoretical category. The theory is supposed to be—indeed one of its imagined glories is that it is—general in the sense of abstract. (Like physics, you see: no one cares *which* uranium atom we work with.) For some analytical purposes, this approach may serve satisfactorily, but it has limits well short of its pretensions.

One fact that should give pause to the analysts is that the political actors themselves certainly seem to have acted as if particular personalities mattered to them. Legions of Roosevelt haters seethed with animosity toward “that man”—he is said to have agitated them so mightily that they could not stand even the sound of his name! Would they have hated any other democratic president as much and acted the same if, say Al Smith had been elected in 1932? Not likely. Smith himself served as an officer of the leading Roosevelt-haters’ group, the Liberty League (Leuchtenburg 1963, p. 92). Would nothing have changed had someone other than Woodrow Wilson been president during and immediately after World War I? Would the events of the 1980s have unfolded without essential difference if, say, Howard Baker had been president instead of Ronald Reagan? In the mid-1930s, when the Supreme Court was more or less evenly divided between those eager to affirm and those eager to deny the constitutionality of major New Deal programs, did nothing of substance depend on the personal character of Justice Owen J. Roberts, the famous “swing man”?

If merely raising these questions does not indicate obvious answers to them, then it must at least create serious doubts about political explanations devoid of personalities. To most historians, the significance of particular persons in determining the course of political history seems manifest. Politics is not, in this regard, like

economics. (Maybe economics [in reality] is not always like economics [in the models] either.) In politics one person can make a difference—not that very many can or do, but the potential exists when the right person and the right occasion conjoin. To understand the growth of government, which is obviously the outcome of a political process, we may need to attend to the roles played by particular actors at critical junctures.

### **Proposition 9**

*In studying the growth of government, econometric analysis is superior to historical analysis.*

The idea that econometrics trumps history seems quite warranted if one accepts Proposition 7 (invariant structural model) and Proposition 8 (personalities are irrelevant). I have already criticized those propositions, but additional objections may be raised.

One problem has to do with the distinction between the creation of a new government power and its exercise, say, by means of government spending or employment. In the United States, authorization must precede the appropriation of public funds. Often certain political events prompt the creation of new authority, but a long time may pass before much money is spent under that authority.

Consider, for example, the Social Security system created in 1935. Clearly the program reflected the unique configuration of socio-economic and political conditions in the mid-1930s (Weaver 1983). For the next 20 years it remained a minor element in federal spending; as late as 1955, only \$4.3 billion was spent for Social Security (OASI) transfers to the aged and to eligible survivors (U.S. Office of Management and Budget 1989, p. 189). It is estimated that in 1990 these types of transfers will reach about \$218 billion, thereby accounting for a large share of the increase in federal spending over the past 35 years—a period when OASI payments grew from about 6 percent to about 16 percent of all federal spending (U.S. Office of Management and Budget 1989, p. 232; note that these data do not include other Social Security transfers, authorized later, such as disability payments or Medicare).

Of course, the increase in OASI transfers during 1955-1990 reflects the unfolding of political events during those years, as members of Congress catered to a segment of the electorate by expanding the scope of eligibility and increasing the allowable amount of payment per eligible recipient. But one who tracks the yearly pulling and hauling of events that resulted in changing amounts of aggregate spending, as the econometrician does in an abstract way, is attending to only one aspect of the growth of government, and it is a consequen-

tial or derivative rather than a fundamental aspect. The increase in Social Security transfers during each year of the 1960s, for example, resulted not simply from the playing out of the politics of the 1960s. It was also a lagged effect of the events and political actions of the 1930s. The increased OASI payments in the 1960s could not have occurred without a Social Security system in place, and that system owed its existence to much earlier events and actions. As Richard Rose (1984, p. 21) has remarked, the growth of government taxing and spending "is not so much a function of new laws as it is a consequence of the continuance of old laws."

An even more compelling example is the veterans' program. As recently described by Julie Johnson (*New York Times*, December 13, 1987),

The V.A. serves 27 million veterans and 53 million dependents and survivors, more than a third of the population. It is the largest independent agency in the Federal bureaucracy, with an annual budget of \$27 billion and more than 240,000 employees. It operates one of the largest health care systems in the world, and the number of patients it treats is expected to skyrocket as more World War II veterans age; it administers one of the largest home loan guarantee programs in the Federal Government, having guaranteed some \$263 billion in mortgage loans since 1944; and it has helped 18 million veterans go to college or get job training.

Here is a welfare state in itself. Again, one can ascertain that spending for the veterans' programs grew in connection with an ongoing political process during the past 45 years. But no one can really understand how this gargantuan complex of government activities emerged unless one understands how the G.I. Bill of 1944 gained enactment: 12 million people, most of them draftees, were serving in the armed forces, and an election was coming up (Ross 1969; Higgs 1987a, p. 229). Once the institutional apparatus of the VA had been established, its vast potential to serve as a single-agency welfare state had only to be exploited at the margin as events and political conditions permitted. To use an analogy from cosmology, none of this evolution could have occurred without the original Big Bang.

Econometric models of the growth of government typically relate the explicandum to contemporaneous events alone or to events a year or two earlier. Such models are ill suited to capture the distinction between what is essential or fundamental (creation of new powers expanding the scope of government action) and what is consequential or derivative (increased government spending within an unchanged scope of government powers). As a rule, the econometrician "falsely

assumes that the causes of government growth represent current choice rather than the inertia force of established commitments" (Rose 1983, p. 6). In modern democratic political systems, it is much easier to start a program than to terminate one; just keeping programs from growing, far from killing them, requires political courage and commitment of a sort rarely evinced.

Other problems arise because, by admitting only one aspect of reality (the quantifiable), econometric models of the growth of government, in effect, throw away information. Because no number can measure a politician's personality and its political import, the econometrician has no way to appreciate the difference in the potential for the growth of government between, say, a government headed by Franklin Roosevelt and a government (that might have been) headed by Herbert Hoover in 1933. Except as the measured variables allow, the econometrician cannot appreciate any difference between, say, 1929 and 1933. A year is a year is a year; a variable is a variable is a variable; and real people with all their quirks and fickleness don't exist at all. This quantitative homogenization squeezes all the life, blots all the color, freezes all the feeling out of human history in general and political strife in particular.

By characterizing only abstract aggregative variables linked by rigid functional relations, an econometric model of the growth of government implicitly affirms that people had *no real choice*. They could not have done otherwise but to act in accordance with fixed formulas; the only deviations allowed are stochastic, as if those who deviate from the formulaic central tendency are lunatics acting randomly. This way of representing human history is not just a simplification; it is a basic distortion, a denial of the very thing the Austrians call human action (Mises 1957; 1966; 1978; Rothbard 1979; Buchanan 1979, pp. 39-63).

### **Proposition 10**

*The process generating the growth of government is internal to each country; each one's relations with the rest of the world can be ignored.*

Virtually all existing economic models of the growth of government are models of a behaviorally closed economy, that is, an economy operating and developing independently vis-à-vis the rest of the world. Of course, external events may indirectly enter the explanatory framework. For example, the gross national product may increase because net exports increase, and the rise in GNP may be assumed to increase the public's demand for government services. But in this model an identical effect would have resulted from an

increase in GNP occasioned by a rise in domestic spending; there is nothing distinctive about external demand as such.

“Rigorous” analysts usually ignore genuinely external causes of the growth of government in part because their models exclude any role for changes in economic or political ideas, which are readily “imported” and “exported.” So, analysts of the twentieth-century growth of government in the United States suppose that the same politico-economic structure persisted throughout the past 90 years even though, roughly speaking, (a) traditional balanced-budget fiscal doctrines held sway for the first half of the period but Keynesian macroeconomic theory and chronic-deficit politics prevailed during the second half, (b) traditional “isolationist” doctrines had great influence on foreign policy during the first half of the period but virtually no influence during the second half, and (c) peacetime defense spending usually amounted to about 1 percent or less of GNP during the first half but more than 7 percent of GNP during the second half (Higgs 1988b, pp. 18-22).

Increased defense spending by itself accounts for over 35 percent (4.4 percentage points) of the increase in federal spending relative to GNP (12.4 percentage points) between fiscal year 1940 and 1988 (U.S. Office of Management and Budget 1989, pp. 46, 51). Can anyone seriously contend that this increase had nothing to do with external military and political events and hence with the ideas Americans held about international communism and the threats they came to believe it posed for their well-being after World War II? Readily available facts refute such a supposition (Higgs 1988b, pp. 11-19).

Readily available facts also attest to the power of ideas imported from abroad in various other realms of thought. Information about social and economic developments in the European welfare states, for example, has heavily influenced the political thinking and practices of Americans ever since the late nineteenth century with regard to income taxation, central banking, nationalized retirement and health insurance, public housing, and countless other matters. Keynes's ideas alone had an immense influence on macroeconomic policy in the quarter-century after World War II, an influence that is still alive today (Stein 1969; 1984; Buchanan and Wagner 1977), not to mention Keynes's and other British influences in establishing postwar institutions for the international financial system, including the International Monetary Fund and the World Bank. From the late nineteenth to the late twentieth century, western European thinking exerted a magnetic attraction pulling American thinking toward collectivism. To ignore this powerful external influence on the course of events is



to abstract from an essential aspect of the process whereby government grew in the United States.

### **Proposition 11**

*Putative "public demand," especially as expressed by voting, drives the political-governmental system. Elected officials (and hence the bureaucracy subordinate to them) may be viewed as perfect agents of the electorate.*

Adherence to this proposition characterizes the bulk of all analysis dealing with the growth of government in the West, regardless of analytical tradition or ideological leaning. (Specific citations seem unnecessary. See virtually any issue of *Public Choice* as well as the widely cited articles by Meltzer and Richard [1978; 1981; 1983], Peltzman [1980; 1984; 1985], Becker [1983; 1985], and Borcharding [1977; 1985]. The most recent and most extreme contribution along these lines is Wittman [1989].) This approach displays a professional deformity related to the economist's basic tool of analysis, the theory of markets with its component theories of demand and supply. Applying their familiar tools to the analysis of politics, economists immediately look for analogues. What is the "good" being traded? Who is the "supplier" and who the "demander"? What is the "price"? The answers seem obvious. Public policy is the good; the elected legislators are the suppliers; the voters are the demanders; votes are the currency in terms of which political business is being transacted. Thus voters "buy" the desired policies by spending their votes; the legislators "sell" policies in exchange for the votes electing them to office. (See Benson and Engen [1988] for an especially straightforward application of such analogues.) Economists view consumer demand in ordinary markets as ultimately decisive for the allocation of resources; hence consumer "sovereignty," a political metaphor imported into economics. Applying their familiar apparatus of thought to politics, economists tend to think that ultimately the political system gives the voters what they want. Therefore, if government grows, it does so because that is what the people want (Musgrave 1985, p. 306; Stiglitz 1989, p. 69). Demand creates its own supply. Voting is ultimately all that matters for determining the growth of government. As Dennis Mueller (1987, p. 142) has observed, "In the public choice literature the state often appears as simply a voting rule that transforms individual preferences into political outcomes."

It is easy—and probably healthy—to mock this view of the political process. Joseph Schumpeter (1954, p. 429) called it "the perfect example of a nursery tale." There are, after all, many significant

differences between ordinary markets and the "political market" (Higgs 1987a, pp. 14-15). Even Benson and Engen (1988, pp. 733, 741), adherents of this model, describe their output variable as "somewhat artificial and very restrictive" and their price variable as "clearly an incomplete proxy."

Not least of the problems is that voters rarely vote directly for or against policies. Rather, they vote for candidates for office. Winning candidates subsequently enact a multitude of policies, many of which neither the voters nor their representatives had thought about at the time of the campaign. It is not enough that voters know something about the general ideological reputation of office seekers (à la Dougan and Munger 1989); the devil is in the details. Besides, notwithstanding the elaborate theoretical and econometric attempts to show that politicians are perfect agents (Becker 1983; 1985; Peltzman 1984; 1985; Wittman 1989), we can easily demonstrate that political representatives frequently act in ways that must necessarily run counter to the dominant preference of their constituents. We see this in the U.S. Senate, for instance, every time the two senators who represent the same state split their votes—and such splitting occurs commonly (Higgs 1989d). Remarkably, and quite damningly for models that presume tight linkages between voters and their elected representatives, many of the vote-splitting senators are reelected time and again. So elections are reliable neither as an *ex ante* nor as an *ex post* check on the substantial autonomy of officeholders.

Perhaps the most important case in which legislators and other (including many *nonelected*) officials act independently of control by the voters concerns political action during crises. How many voters could possibly have known in the election of 1940 what the elected federal officials would do during their upcoming terms in office, which were to include, depending on the office, some or all of the years of World War II? How many voters in the election of 1972 had any idea how they wished their representatives to deal with the "energy crisis" of 1973-1974, or even that such a crisis loomed? Who anticipated that George Bush would send U.S. troops into Saudi Arabia to oppose Iraq? During crises, government officials, lacking any reliable means for discovering dominant constituent preferences, necessarily exercise more or less discretionary power. But they do act, often in dramatically important ways.

Once those actions were taken, in a world of path-dependent historical processes the course of events was changed irrevocably (Brennan and Buchanan 1985, pp. 16, 74; Higgs 1987a, pp. 30-33, 57-74). (Ratcheting growth of government spending associated with participation in global wars is confirmed statistically by Rasler and

Thompson [1985], using Box-Tiao tests.) If U.S. voters actually had preferred that the nation not go to war, it was too late to rectify the legislators' mistake in the election of 1942—the fat was already in the fire.

Further, political actions are usually followed by carefully crafted rationalizations, excuses, and propaganda emanating from the politicians and their friends who initiated or supported the actions. (How often do politicians admit policy mistakes?) In this way political preferences, public opinion, even the dominant ideology may be altered, becoming more congruent with what has been done and thereby reversing the direction of causality usually assumed in political models. (On ideology and policy as interactive, see Higgs 1985; 1987a, pp. 67-74; 1989c, pp. 96-98.)

### **Proposition 12**

*A corollary of Proposition 11: The judicial branch of government can be ignored.*

If analytical political economists have greatly *overstated* the role of legislators (too often viewed as perfect agents of voters) in the growth of government, they have to an even greater degree *understated* the role of judges, at least in U.S. history, where legislation must withstand judicial review of its constitutionality to survive and have ongoing effect. The public choice and related analytical literatures contain almost nothing empirically concrete about the judiciary's role in the growth of American government, although the literature of law and economics offers some useful insights (several chapters in the volume edited by Gwartney and Wagner [1988], as well as Hughes [1977], are pertinent) and the literature on constitutional political economy offers suggestive insights, albeit at a very abstract, quasi-philosophical level (e.g., Friedrich Hayek's *Law, Legislation and Liberty* or various works by James Buchanan and his collaborators). The index of the recent, admirably comprehensive survey of public choice by Dennis Mueller (1989) has no entry for judges or judiciary. Mueller mentions but does not dwell on an oft-cited paper by William Landes and Richard Posner (1975), enticingly titled "The Independent Judiciary in an Interest-Group Perspective." Unfortunately, the thesis of this paper—legislators tolerate an independent judiciary only to augment the longevity and hence enhance the value of the legislative products they sell—is hard to take seriously, at least for anyone who has spent much time studying the constitutional history of the United States. (Cogent critics of the Landes-Posner paper include Buchanan [1975], Samuels [1975], and North [1981, pp. 56-57].)

The U.S. Supreme Court—nine persons appointed for life, answerable to no electorate, legislature, or interest group—played a key role in the growth of American government over the past century. (The relevant legal and historical literature is enormous. For selected references, see the footnotes and bibliography of Higgs [1987a].) Evidently, no one wants to deny this fact, but many analysts seem content to ignore it. The reason, one suspects, is that it doesn't fit into the profession's standard set of puzzles or lend itself to solution by the usual methods of analysis. It requires that one pay attention to particular autonomous individuals with specific values and beliefs. As North (1981, pp. 56-57) has observed, the behavior of the independent judiciary presents us with "the clearest instance of the dominant role of ideology." That fact makes most economists either run for cover or take up arms in visceral opposition.

### **Proposition 13**

*Ideology doesn't matter.*

Indeed, the idea that people act on the basis of ideology strikes most mainstream economists, including many of those who have written about the growth of government (e.g., Becker and Stigler 1977; Peltzman 1984; 1985), as utterly anathematic. They flee from it as a vampire flees from holy water—perhaps for the same reason, too. Surprisingly, in view of his leading position in the Chicago School, Gary Becker (1985, p. 345) once wrote that "undoubtedly, the decline in laissez faire ideology contributed to the growth of government." He immediately backed away, however, issuing the obiter dictum: "but most of the decline was probably *induced* by the arguments and propaganda of the many groups seeking public largess."

More than 10 years ago, when a few neoclassical economists began to toy with the idea that ideologically motivated behavior might be the cause of certain apparent anomalies of public choice theory (e.g., why people vote), the economic literature took an unfortunate turn. Economists, political scientists, and public choice analysts began to produce an outpouring of problematic econometric studies of roll-call voting in the U.S. Congress. (Recent contributions, with many references to the earlier literature, include Nelson and Silberberg 1987; Lott 1987; 1988; McArthur and Marks 1988; Davis and Porter 1989; Dougan and Munger 1989; Lott and Reed 1989; Richardson and Munger 1990; Zupan 1990; Nollen and Iglarsh 1990.) Roll-call voting was a poor choice of observations for testing whether ideology matters—it was seized upon because it produces numbers that can be cranked through the econometric mill—though even in these studies it seems fairly clear that ideology does matter insofar

as the indexes used to measure it mean what they are supposed to mean.

Elsewhere, I have tried to clarify the concept of ideology, to show how ideology can be understood as consistent with rather than the antithesis of rational action, and to document how ideology affected and in turn was affected by the growth of American government over the past century (Higgs 1983; 1985; 1987a; 1989c). I shall not repeat everything I have written on this subject, but one point requires restatement and emphasis.

The existing thrust of the economic literature, the quest to determine econometrically whether ideology mattered in determining a certain set of political actions, seeks to answer a non-question. Of course it matters. *It always matters*, because people cannot even think about political questions, much less undertake political actions, without an ideology (Siegenthaler 1989; Higgs 1989a; 1989c, pp. 98-100).

How can I make such a claim? Economists are supposed to believe, or at least to postulate for analytical purposes, that people pursue their "economic interests." Open any mainstream text on economic theory and check the arguments of the utility function: sure enough, they consist of amounts of "goods" consumed by the individual; nothing about ideas here, just pounds of potatoes, bottles of beer, trips to the shore, hours of leisure, and so forth. In the words of Gary Becker (1983, p. 374, emphasis added), "the utility of each person . . . depends *only* on own commodities." To consume more of these things is, the mainstream economist supposes, precisely what is meant when one speaks of people's acting in their self-interest. In this context, to speak of a person's economic or material interest would be redundant, because the theory recognizes no other kind. Thus, Thomas Borcherding (1985, p. 378, emphasis added) declares it "an open question whether after the *obvious elements of self-interest* are separated from political action, scope for ideology remains."

The most charitable thing I can say about this view is that it is simply wrong. No one ever explained why it is wrong more clearly and succinctly than Mises (1957, pp. 140, 142, emphasis added):

In the world of reality, life, and human action there is no such thing as interests independent of ideas, preceding them temporally and logically. What a man *considers* his interest is the result of his ideas. . . . Free men do not act in accordance with their interests. They act in accordance with what they *believe* furthers their interests.

Nor are the Austrians alone in appreciating the dependence of interest on belief. Jon Elster (1989, p. 20), for example, recently wrote:

“What explains the action is the person’s desires together with his *beliefs* about the opportunities. Because beliefs can be mistaken, the distinction is not trivial.”

Ideologies are belief systems about social relations. Chief among their dimensions is the cognitive: ideologies structure and give meaning to a person’s perceptions of social life. They also place affective weight on those apperceptions, designating some things good or right, other things bad or wrong. They also point toward a justifiable political program and open up the potential for solidarity with like-minded comrades. Such solidarity serves as an important means of establishing and maintaining a social identity; it helps to determine people’s psychologically essential conceptions of who they are.

In myriad ways, the growth of government has involved collective action, a transcendence of the free-rider problem regarded by neoclassical theorists as more or less paralyzing. This transcendence reflects ideologically motivated action. It poses no great puzzle for those who understand that real people act on the basis of two equally propulsive—but inextricably intertwined—motives: to get something *and* to be someone (Higgs 1987b). (See also the discussions of “artifactual man” by Buchanan [1979, pp. 93-112] and “preferences for preferences” and “the role of norms” by Brennan and Buchanan [1985, pp. 68-73, 146-47]; and the discussion of self-interest and the free-rider problem by Hummel and Lavoie [1990].)

#### **Proposition 14**

*Government grows in order to correct the distortions stemming from externalities.*

This proposition, along with Proposition 5 and 15, lies at the heart of the theory of the growth of government usually embraced by mainstream economists (e.g., Baumol 1965; Stiglitz 1989, p. 57). The theory maintains that governments grew in a process of correcting emerging “market failures” associated with monopoly power, externalities, and public goods. As a positive theory of the growth of government the idea suffers, as already indicated, from reliance on magic: a market failure emerges—POOF!—government undertakes a program to remedy the associated deviation from the “efficient” allocation of resources. In the words of Richard Musgrave (1985, p. 287), “the assumption was that government, once advised of proper action, will proceed to carry it out.” But no account is given of (a) why either the public or specific government officials know or care about systemic efficiency and (b) even if they do know, what personal incentives they have to take the implied corrective action. In short, a black box stands between the alleged cause and its presumed effect.

What actually fills the black box are the Two Big Collectivist Assumptions: government officials *know* what needs to be done to promote the public interest, and they *act* on the basis of that knowledge.

Apart from the implausibility of the theory because it has nothing to say about the personal incentives and constraints of actual decision makers, the theory does not stack up empirically. A reasonable survey of how the government has grown—that is, an accounting of what it has come to do more often and what it has undertaken now and then to do for the first time—must conclude that only a small proportion of all government activities has anything to do with externalities. One need only examine an organization chart for the government, leaf through the Federal Register, or scrutinize U.S. Statutes at Large, not to mention the detailed budget documents. Evidence of the alleged connection rarely appears. Studies that have sought to find a relation between the growth of government and proxies for growing externality problems (e.g., population density, urbanization, ratio of manufacturing to agricultural activities) have found little or nothing (Borcherding 1977, p. 53; 1985, p. 368; Mueller 1987, p. 119).

The theory of government as fixer of externalities is often quite backwards. Governments themselves compose “the prototypical sector in which decision makers do *not* take accurate account of all the costs as well as all the benefits of each activity” (Yeager 1983, p. 125). In reality, the government is more likely to cause a negative externality than to reduce one. Since the recent revelations in Eastern Europe and the Soviet Union, everyone has become aware of the vast environmental destruction wreaked by government officials there, but one need not rely on these egregious cases to establish the point. According to studies of the United States cited by James Bennett and Manuel Johnson (1980, pp. 133-34),

federal government agencies emit huge quantities of pollutants into the water and atmosphere. The U.S. Department of Defense alone discharges over 335 million gallons of human waste per day, of which 30 percent received secondary treatment or less. The Tennessee Valley Authority (TVA) is the country’s largest sulfur dioxide polluter, accounting for 38 percent of total sulfur emissions in the Southeast U.S., and its compliance record with pollution laws is only 16 percent as compared to 74 percent for all utilities nationally.

Recent press reports tell us, and government spokesmen such as Energy Secretary James Watkins admit (*Wall Street Journal*, November 27, 1989), that the government’s plants for manufacturing nuclear materials have been poisoning the surrounding air, land, and water for decades while hiding behind their top-secret national security classification. The Energy Department forecasts that the future

expense of cleaning up this mess will accumulate to more than \$80 billion (*Wall Street Journal*, December 12, 1988). Nor are the nuclear plants the only problem of this sort. Already 87 military installations have been placed on or proposed for the Superfund list of the nation's most dangerous toxic-waste sites, and more than a hundred other military facilities may be added to the list. A "Newsday" study concluded that "the armed forces have been slow to move, have resisted state regulators' efforts to force compliance with environmental laws, and continue to violate anti-pollution laws even as officials in Washington, D.C., insist their bases are trying to be better neighbors" (*Seattle Times*, February 5, 1990).

Still, one should not leap to the conclusion that the growth of government had *nothing* to do with programs in response to emerging externality problems, although one ought to refrain from immediately labeling those perceived problems "market failures." Historically, for example, urbanization created severe externality problems in relation to the spread of contagious diseases, and the (mainly local) governments' public health programs responded to these problems in a fashion that in retrospect seems remarkably successful (Higgs 1971, pp. 67-72; 1979; Meeker 1974). Other examples also might be found, perhaps in other areas where public health and safety are at stake. The point, however, is that such examples cannot bear much weight as significant explanations of the growth of government. They do not add up empirically to a big part of the relevant record.

### **Proposition 15**

*Government grows in order to supply public goods that the public demands but the free market won't supply.*

As already indicated, this proposition belongs to the class of "market failure" explanations of the growth of government. Like all such explanations, it suffers from the infirmities of the behavioral black box. Empirically, however, it seems to possess greater warrant than its cousin, the externality proposition. Although many examples of public goods are problematic—they do not actually involve goods that are totally nonrival in consumption or nonpayers who cannot be excluded—at least one important case remains, namely, national defense. Especially when one conceives of defense as the deterrence of nuclear or other widely devastating attacks against national territory, it seems to be a genuine public good, one for which the free market would make insufficient provision. Nor is the necessity of government provision affirmed only by mainstream economists. Mises himself concluded that "in a world full of unswerving aggressors and enslavers . . . isolated attempts on the part of each individual



to resist are doomed to failure, [and therefore] the only workable way is to organize resistance by the government." Mises went even further, supporting conscription of people to serve in the armed forces (1966, p. 282).

As indicated above, increases in military spending over the past 50 years account for a substantial share, more than a third, of the rise of federal government spending relative to GNP. The arms industries also have become the most heavily regulated sector of the U.S. economy (Kovacic 1990). It would seem, then, that Proposition 15 has a good deal, both theoretically and empirically, to recommend it to students of the growth of government. Of course, the empirical weight that this explanation will bear needs to be kept in perspective: defense spending, for example, now amounts to only about a quarter of federal spending, less than a fifth of all government spending, less than 6 percent of GNP (U.S. Office of Management and Budget 1989, pp. 51, 364), and the defense spending shares are substantially below their levels during the 1950s and 1960s (Higgs 1988b, pp. 18-22).

Before embracing Proposition 15 fully, however, one needs to consider a rather difficult question: how much of the observed increase in the government's military activity represents a response to the public good dilemma (the free-rider problem) and how much represents self-serving exploitation of the public's insecurities by people making little or no contribution to the maintenance of genuine national security (the free-loader problem)?

No simple answer can be given, but some things are fairly obvious. Much military activity has served the interests not of the general public but of the government itself (Hummel and Lavoie 1990). The recent invasion of Panama is an example, as was the earlier invasion of Grenada. Far from seeking a "revelation" of the public's true demand for defensive actions and the derived demand for arms production, the national security establishment has engaged in a series of mendacious efforts to scare the public and stampede the taxpayers into supporting higher levels of military spending—just recall all the weapons "gaps" announced over the past 40 years, most of which were revealed in due course to be overblown or completely bogus (Higgs 1988b, pp. 11-19). Much military spending has done nothing to promote national security, for example, lavish officers' clubs and golf courses, cushy military retirement systems, and maintenance of obsolete facilities such as Fort Monroe, the fort with a moat. Members of Congress have twisted the defense program again and again to aid their quest for reelection (Higgs 1988a; 1989b; 1990a; and many of the chapters in the volume edited by Higgs [1990b]). Anyone who reads the newspapers, not to mention the literature on

military procurement, knows that the big defense contractors, in league with their friends at the Pentagon and in Congress, have siphoned many billions of dollars in rents out of the public treasury during the past half-century, never more than during the bonanza of the 1980s (Stubbing 1985; Fitzgerald 1989).

In sum, it is obvious that the growth of government via increased military activities represents far more than a straightforward effort to achieve a solution to the public good problem. To a large extent, it represents a poorly disguised form of redistributive politics.

### **Proposition 16**

*Government grows in order to reduce the transaction costs inherent in a complex modern economy, thereby facilitating a high degree of division of labor and enhancing productivity.*

I call this proposition, which is a more sophisticated variant of the Modernization Hypothesis, the theory of the Washington School. Its prime proponent is Douglass C. North, long of the University of Washington (Seattle) and more recently of Washington University (St. Louis). North draws from theoretical work on measurement and transaction costs by Steven N. S. Cheung (formerly of the University of Washington) and Yoram Barzel (still there). Collaborating with North on empirical work connected with the theory has been John Wallis, who earned his Ph.D. at the University of Washington. The thesis began to take shape more than 10 years ago and appeared in North's *Structure and Change in Economic History* (1981, especially pp. 187-98 on the United States). Later papers (North and Wallis 1982; North 1985; Wallis and North 1986) clarified and extended the argument and presented empirical materials in support of it.

An early summary conveys the essence of the argument (North and Wallis 1982, p. 336):

The wedding of science and technology in the late nineteenth century made possible a technology of production whose potential was only realizable with an enormous increase in the resources devoted to political and economic organization—the transactions sector of the economy. A substantial part of this increase has occurred in the market and through voluntary organization, and a substantial share has also been undertaken by government.

The government's part evidently has outpaced the market's part; hence the growth of government.

North's argument traces virtually everything back to a single aspect of societal modernization, the increase in specialization. That increase caused the rise in productivity, hence economic growth; it

necessitated more "contracts across time and space and with unknown second parties," hence a demand for bigger government to supply "effective third party enforcement" (North 1989, p. 113); it fostered ideological divisions, hence the proliferation of politicized interest groups (North 1981, pp. 51, 196-98; 1985); it cheapened tax collection and hence shifted outward the supply schedule of government activities. (Becker [1985, p. 345] tells a similar tale.)

Although Wallis and North's empirical exercises in creative national income accounting are not compelling, partly because the empirical categories just don't match the theoretical counterparts (for some details, see Davis [1986]), there may be something to the Washington thesis. North continually emphasizes that government has grown throughout the Western world and elsewhere over the past century; and, by conventional measures, government is bigger in the more developed countries than in the less developed. A good theory, it seems, ought to account for the apparently pervasive association of economic progress and growth of government in the West. Because rising specialization marks every case, it would appear to resolve the issue. Perhaps to some extent it does, but problems remain.

One difficulty is that the theory is *too* general. Although it seems to match the long-term trend in every Western country—and many others as well—it cannot account for the marked irregularities that have appeared in most cases. The specific shape of the historical profile must be explained by auxiliary theories or in an *ad hoc* manner. The abrupt growth of government that occurred in the United States during the world wars and the Great Depression, for instance, would seem to have little to do, in any immediate way, with changes in the degree of societal specialization (Higgs 1987a, pp. 123-236). Similar questions can be raised about the precise paths followed by other countries.

Another problem: the theory is rather vague, and the attempts to give it empirical substance only heighten one's misgivings in this regard. The concepts of "transaction cost" and "transaction sector" have been stretched to the breaking point. The distinction between "transaction" and "production transformation," though central to the thesis, is blurry at best (Davis 1986).

Further, the explanation of why remote transactions and other features of modern economic life could not be accommodated in the market, an explanation that appeals to "moral hazard, adverse selection, and the demand for public goods" (North 1985, p. 392), is offered almost in passing and needs a much more extensive argument before it can become persuasive. (See Lindbeck [1985, pp. 315-16] for trenchant criticism with regard to the alleged roles of adverse selection

and moral hazard.) Myhrman (1985, p. 277) argues that while North's thesis may explain why governments got bigger during the early stages of economic growth, it does not account for why the growth of government has continued to exceed the growth of the private sector. (Davis [1986, p. 158] makes a similar point.)

Finally, stepping back from the theory and viewing it as a whole, one may get an eerie feeling of unreality. Many of us are convinced that, all things considered, modern governments—the American and just about all the others—hugely *increase* the costs of transacting mutually beneficial exchanges in comparison with what those costs would be in a minimal or night-watchman state. In view of all the taxes, all the direct, highly politicized government participation in markets, all the regulations, all the laws infringing economic liberties on every side, how can anyone suppose that *on balance* the growth of government has reduced transaction costs and promoted economic growth? Perhaps cause and effect have been reversed in the Washington Thesis; perhaps only economically progressive societies can *afford* the deadweight costs of ever bigger governments.

### **Proposition 17**

*Government is nothing but an engine of redistribution.*

Many of the most cited contributions to the literature make this assumption their point of departure (Meltzer and Richard 1978; 1981; 1983; Peltzman 1980; 1985; Becker 1983; 1985; Benson and Engen 1988; and others cited by Mueller 1987, pp. 122-28). Evidently analysts adopt the assumption because it facilitates the construction of tractable formal models. To simplify the analysis further, investigators usually assume that the redistribution runs from richer to poorer. There is something to be said for simple models, but in this case it is not much.

These models lack even the elementary saving grace of positivism: they do not generate predictions that fit the facts. (For criticism, see Higgs [1987a, pp. 12-15] and Mueller [1987, pp. 126-28].) Peltzman, in his influential 1980 article, claims to present empirical confirmation, but the claims are too ill-founded to be acceptable. Indeed, the econometric methods employed in that article—presumptuous proxy variables, *ad hoc* substitutions for “missing” data, unwarranted specification switches, *inter alia*—fill a chamber of horrors sufficient to discredit the entire undertaking. The methods employed in Peltzman's 1985 article warrant a similar evaluation.

More fundamentally, assuming that government just redistributes wealth simply isn't true, isn't even an approximation to the truth. It's hard for the hard-core anarchists to swallow, but govern-

ments in the West do provide some desired services. Deterrence of foreign aggression, a degree of local protection of life and property, a body of property law and a system of courts for resolving civil disputes, a public health system, public water supplies and sewage disposal, the roads and the traffic rules—all seem to qualify as more or less public goods and as goods genuinely demanded by the overwhelming majority of the public. Of course, even these goods are supplied in ways that one might lodge many complaints against. But the point remains: they are not *just* means of redistribution, even though their financing, production, and distribution have many undeniable redistributive aspects.

National defense, perhaps the most important example, surely receives much political impetus from those who privately appropriate benefits from its provision (Higgs 1990a; Lee 1990; and other chapters in the volume edited by Higgs [1990b]). Still, not many citizens favor unilateral disarmament. Most people want the government to maintain a military establishment adequate to deter foreign aggression. (The evidence of public opinion polls and elections indicates that a substantial number also support military aggression against others, although one might apologize for at least some of those who maintain this position on the grounds that they have been duped to believe the aggression is actually defensive.) In any event, they do not support just giving money to the owners and employees of General Dynamics (GD) in the same way that they support Aid to Families with Dependent Children. Most of them want the military potency produced by the Trident submarines, M-1 tanks, and F-16 fighter planes; only a few of them care whether GD or somebody else supplies the weapons. For the general public, GD's rents are incidental, though of course they are far from incidental in the actual political process by which GD becomes the supplier (Goodwin 1985).

### **Proposition 18**

*The modern welfare state merely "filled the vacuum" left by the deterioration of private institutions.*

This proposition is still another variant of the Modernization Hypothesis. Modern economic development, it is said, caused socio-economic transformations (e.g., urbanization, greater personal mobility, increased survival of the aged) that sapped the vitality of private institutions. Families, churches, and voluntary associations became less and less able to accomplish their traditional tasks. Hence, government increasingly substituted for them "as the principal institution assisting individuals in time of economic or social misfortune"

(Fuchs 1979, p. 13). Government had to hold the “safety net” when others no longer could or no longer would.

Certainly government activities in immense profusion—countless programs ranging from sex education to mental health care to the federal Foster Grandparents Program—can be viewed as illustrating this thesis. Perhaps the proposition contains more than a grain of truth. But as an explanation of the growth of government, the proposition by itself does not carry us far, and even that much is partly illusory.

A serious defect of the proposition is that once the alleged process of “vacuum filling” had got under way, if not before, the direction of causality must have run in both directions. Yes, perhaps certain socio-economic changes did, say, promote the breakdown of individual responsibility among family members. But the availability of governmentally provided substitute services lowered the cost of irresponsible private actions and hence increased their frequency. Government did not simply substitute for responsible private efforts; it also crowded them out. Without narrowing the focus to a specific activity, not much more can be said. But the crowding-out theory itself is logically unimpeachable, and those with normal eyesight and a little knowledge of history can see evidence of such crowding out on all sides. (See Wagner [1989] for an extended discussion and references.)

Of course, Proposition 18, as an explanation of the growth of government, presents us with yet another case of the black box. When we fill the box in a theoretically and empirically warranted manner, the nature of the explanation changes completely. Question: How do the kinds of people who need a government safety net—presumably those who are destitute, physically or mentally handicapped, aged and infirm, or otherwise in dire straits—exert enough political pressure to elicit the creation of a safety net by those who control the political process? Short answer: They don't. But notice the millions of middle-class administrators, school teachers, social workers, lawyers, urban planners, doctors, nurses, professional and technical specialists, and all the others who act as well paid providers and facilitators of governmentally funded services for the helpless, and then the politics of the welfare state becomes a lot plainer. Also plain is that the rise of the welfare state involved far more than unvarnished altruism (Weaver 1978; Higgs 1987a, pp. 248-51).

Not surprisingly, it involved a great deal of redistributive politics: redistribution not so much from the fortunate to the unfortunate as from the taxpayers to the bureaucrats, providers, and hangers-on. As Lindbeck (1985, p. 327) puts it, “the original ‘welfare state,’ designed

mainly to provide basic economic security, has gradually developed into a free-for-all competition for favors from the state, with 'every politician trying to buy votes from everybody.'"

## Conclusion

In the literature on the growth of government, much that is misguided can be seen as unwarranted reduction. This in turn can be seen as arising almost inevitably from the positivist pretensions that underlie modern social science in general and modern mainstream economics in particular.

The drive toward reduction takes several forms. First, analysts strive to reduce empirical reality to *one measure* of the explicandum or dependent variable. Second, they strive to reduce the theory to *one independent variable* that can carry the entire explanatory load. Third, they strive to reduce historical and geographical diversity so that *one general explanation* applies to all times and places. In sum, the goal appears to be an equation of the form  $G = f(X)$ , where  $G$  is one simple measure of the size of government,  $X$  is one simple explanatory variable, and  $f$  is a fixed-coefficients functional relation connecting the values of  $X$  and  $G$  by what amounts to a law of history.

This reductive quest is nothing more than a species of scientism, the attempt to conduct the study of man with the same methods employed to study nonhuman nature. Research in political economy is being carried out as if it were research in physics or chemistry. But people are not atoms; the political economy is not a molecule; and the growth of government is not analogous to the natural growth processes analyzed in biochemistry. The prevailing reductivism, which is both positivist and historicist, founders on the reality that people are purposive, choosing actors whose actions are shaped by their (changeable) beliefs and values and whose personal and societal histories are marked by contingencies with significant consequences, including path dependencies (Mises 1957; 1966; 1978; Rothbard 1979; Higgs 1987a).

Strange to say, one can describe a large part of the recent research on the growth of government as attempts by researchers who neither know nor care much about history to discover laws of history. Small wonder that black boxes litter the field. But here, as in other areas of serious empirical research, there is no good substitute for knowing, quite literally, what we are talking about. One must, then, study history; one must comprehend the great variety of acting and interacting individuals whose actions compose our subject and the diverse and changing institutions that condition the actors' choices.

To this recommendation, a positivist might respond: If there are no laws of history to be discovered, what is the point of studying history? The answer is that there exists much valuable knowledge in the gap that separates unvariable law and utter chaos. Although no laws of history exist—indeed, as Mises (1957, p. 212) explained, “the notion of a law of historical change is self-contradictory”—the study of history can reveal patterns and probable relations. Mises (1957, pp. 264-84) called the search for this kind of understanding “thymology.” He maintained that

what thymology achieves is the elaboration of a catalogue of human traits. It can moreover establish the fact that certain traits appeared in the past as a rule in connection with certain other traits. But it can never know in advance with what weight the various factors will be operative in a definite future event.

So, even though one cannot rely on historical understanding to be apodictic, as one can rely on the pure logic of choice, which the Austrians call economic theory, one must, both in everyday life and in empirical research, constantly place bets. Although one cannot be certain that the relations on which one places the bets will (or did in the past) prevail, one confidently expects to come closer to the truth by taking thymological understanding into account than by closing one’s eyes and throwing darts at the dartboard of all possibilities.

It is no accident that many of the leading lights of Austrian economics—Mises, Hayek, Rothbard—have taken historical understanding seriously and devoted much effort to historical research. Mises (1957, p. 293) went so far as to describe historical understanding as not only essential for practical action but worthwhile in another sense as well. “It opens the mind toward an understanding of human nature and destiny. It increases wisdom. It is the very essence of that much misinterpreted concept, a liberal education.”

In view of the wide extent to which the problematic propositions criticized above have been accepted by contributors to the recent literature on the growth of government, a Misesian might well reach the following conclusion. Many of the analysts thought they were formulating and testing economic theory, but in the Austrian sense they were not. Few of them thought they were writing economic history, but in the Austrian sense they were. Unfortunately, much of this inadvertently written economic history has been deeply flawed.



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