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What Limits Social Spending?*

PETER H. LINDERT

University of California, Davis

The forces that are most likely to set the ultimate limits on social spending as a share of GDP are not those usually imagined. The deadweight costs of such spending, and the taxes behind them, fail to show the predicted upward spiral. The experience of 1960–1981 shows a major role for shifts in relative age-group sizes, but with an approaching sunset to the effect of aging on social-spending patterns. In addition, the further are the middle pre-fisc income ranks from the poor, the lower the political tendency to spend on any major type of social program.

In all the industrialized countries, government spending on welfare, unemployment, pension, and health programs rose dramatically after 1930, and especially after 1960. Even in the 1980s and early 1990s, a renewed conservative drive to slash government handouts failed to reverse the tide in most Western democracies. As shown in Table 1, the rise in the share of social transfers in GDP since 1960 has in fact been very uneven, across countries as well as over time. At the start of the 1980s, it took over 26% in Belgium, the Netherlands, and Denmark, yet under 16% in Australasia, North America, Switzerland, Japan, and Greece. The contrasts remain just as strong if one includes public education spending, a less vertically redistributive kind of social spending.[1]

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[1] Two key terms used in this study deserve definition and comment here. ''Social transfers'' consist of expenditures on welfare and unemployment compensation, pensions, and health subsidies. ''Social spending'' consists of social transfers plus government subsidies to education. All expenditure measures refer to the consolidated accounts of all levels of government.

The concept of social transfers is meant to approximate the set of progressive-redistribution programs at the heart of the welfare state. Thus, the text uses ''redistribution'' to refer to these kinds of spending and the progressive taxes that usually back them. While some kinds of transfers are more progressive in their incidence than others, we must stick with expenditure aggregates that are supplied

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TABLE 1
Social Transfers as a Percentage of GDP vs Economic Growth, 19 OECD Countries, 1960–1992

	Transfers as % of GDP						
	OECD (1985) estimates			IMF accounts		Growth rate of real GDP/capita	
Country	1960	1970	1980	1980	1990	1960–1980	1960–1992
Belgium	13.1	19.3	30.4	22.3 c	19.2 c (1988)	3.7	2.9
Netherlands	11.7	22.5	28.3	25.5 c	25.6 c	2.9	2.3
Denmark	12.3	19.1	27.4	28.5	29.0	2.8	2.4
Sweden	10.8	16.8	25.9	21.2 c	21.3 c	2.7	2.1
West Germany	18.1	19.5	25.7	26.2	25.3 (1989)	3.0	2.6
Austria	15.9	18.9	23.3	24.0	24.7	3.8	3.0
France	13.4	16.7	22.6	24.1	27.4	3.5	2.8
Italy	13.1	16.9	21.2	16.9 c	22.8 c (1988)	4.1	3.2
Norway	7.9	16.1	21.0	21.0	27.1	3.8	3.1
Finland	8.8	13.6	19.2	10.1 c	14.1 c	3.8	2.8
Ireland	8.7	11.9	19.2	n.a.	n.a.	3.5	3.4
United Kingdom	10.2	13.2	16.4	18.8	16.2	2.0	1.9
New Zealand	10.4	9.2	15.2	17.2 c	19.7 c (1991)	1.4	1.2
United States	7.3	10.4	15.0	12.0	12.2	2.1	1.8
Canada	9.1	11.8	15.0	14.9	16.7	3.3	2.5
Switzerland	4.9	8.5	14.3	16.3	17.6 (1984)	2.1	1.7
Australia	7.4	7.4	12.8	11.6	12.6	2.6	2.1
Japan	4.0	5.7	11.9	n.a.	n.a.	6.2	5.1
Greece	9.3	9.0	11.1	n.a.	n.a.	5.4	3.7
19 countries	9.3	12.3	17.4			3.1	2.6
16 countries				16.6	17.4	2.7	2.2

Source. OECD (1985); IMF, Government Finance Statistics, 1993; and OECD, National Accounts 1960–1992 (1994, Table 20).

Note. c = Central government only. Social transfers = spending on pensions, welfare, unemployment compensation, and health, by national and local governments. Countries are ranked according to their shares in the 1980 OECD estimates. For the all-country averages, the country weights are 1980 GDP weights using the PPP measures.

Why should tax-based social spending have advanced so far, and why has it already risen so much further in some industrial countries than in others? What forces will set its ultimate limits?

Answers to these questions remain inadequate because the full set of questions has not been sufficiently confronted in comparative perspective.[2] Most theories of

by the OECD estimates. Fortunately, these estimates permit a division by type of expenditures that allows us to look separately at more progressive and less progressive kinds of programs.

[2] Progress has been made, however, on explaining international differences in social spending. For historical perspectives, see Flora and Heidenheimer (1981), Flora (1986), and Baldwin (1990). For studies of postwar international experience featuring social and political variables, see Hicks and Misra (1993) and Huber *et al.* (1993).

the rise of the ''welfare state'' do not account for wide international differences and lack a satisfactory explanation of how the rise will stop.

The leading theory of the limits to social spending predicts that the rising marginal deadweight costs will choke off either the ability or the willingness to go on raising taxes and transfers. The classic version warns that the marginal deadweight costs of the redistributive state rise nonlinearly on two fronts, discouraging effort both for the taxed and for the subsidized. If the political process fails to heed the warning, growth will fade, and government redistribution will be limited by stagnation of the whole economy.

Adam Smith said that. A passage from Smith leaves us as strong a warning about deadweight costs as any society could ask:

> The whole, or almost the whole public revenue, is in most countries employed in maintaining unproductive hands. . . . Such people, as they themselves produce nothing, are maintained by other men's labour. Those unproductive hands, who should be maintained by a part only of the spare revenue of the people, may consume so great a share of their whole revenue, and therefore oblige so great a number to encroach upon their capitals, upon the funds destined for the maintenance of productive labour, that all the frugality and good conduct of individuals may not be able to compensate the waste and degradation of produce occasioned by this violent and forced encroachment.[3]

A less classic recent version says that the political process has been heeding the warning all along. Gary Becker has theorized that the marginal deadweight costs of extra redistribution will tip the balance of pressure-group competition against further redistribution, because any deadweight costs either weaken the will to lobby in favor of more redistribution or strengthen the will to oppose it. Thus, even the disorderly competition among political pressure groups acts as though it were struggling to cut waste.[4]

For all the power of these ideas, they have not explained two basic facts about the rise of social spending. First, they do not explain why the share of GDP differs so greatly between countries. It cannot be true that Denmark taxes and spends vastly more than Switzerland because the Danes have discovered a cheap redistribution formula unavailable to the Swiss. Indeed, as will be argued again

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Peltzman (1992) comes close to posing the puzzle of no drag on growth from government spending, yet curiously passes it up when it emerges from his analysis. He is puzzled that governments seem to spend more than his estimated voter-preference function would predict. A footnote (p. 359n) reports that government spending has an insignificantly positive effect on permanent income, without noting how directly this result conflicts with the conventional wisdom.

[3] *Wealth of Nations* (New York: Modern Library, 1937, pp. 325–326). Of course, other passages of Smith were devoted to justifying taxes and expenditures by a government better run than the one he lived under.

[4] Becker (1983, 1985). In what follows, as in Becker's framework, the deadweight-cost rate is defined as the (average or marginal) ratio of net GDP loss to the transfers received by the favored group. The marginal deadweight-cost rate is the G, $-F'$ of Becker (1983), or the $D'_S + D'_T$ of Kristov *et al.* (1992). The deadweight costs consist of direct resource costs (e.g., administrative costs) and incentive-distortion costs.

below, the deadweight costs of devoting a given share of GDP to a given kind of redistributive program are about the same in all industrial countries. Cost curves that differ little cannot explain wide differences in the amount taxed and transferred.

Second, it is not clear that nations with greater social spending grow more slowly, or have lower income levels, than countries that keep the state in a smaller corral. In Table 1, for example, it is not evident that the bigger government countries at the top of the table grew more slowly than the lower spenders in Australasia, North America, Britain, and Switzerland. Nor does social spending correlate negatively with the level of GDP per capita, either within the OECD or for the world as a whole.

The set of theories to be tested here is shaped by the historical setting. Before the mid-20th century, there were sharp international contrasts in the advance of democracy and in religion. For such a setting, it is not difficult to see the effects of these forces on redistributive taxes and social spending.[5] After mid-century, however, these contrasts disappeared from the set of OECD countries studied here: electoral democracy prevailed (except in Greece 1967–1973), women voted (except in Switzerland before 1972), and religion had little effect on the size of government redistributive programs. Meanwhile, other forces are better illuminated by the greater size of government, and the greater abundance of data, for the period since 1960. With government so large, we can better judge its costliness and whether that costliness slowed the growth of social programs. We can also test whether government generates its own growth once it has become large. In addition, there are sufficient recent data on the pre-fisc distribution of income to explore how it might affect fiscal redistribution. These are some of the forces that can show up more clearly for the years since 1960. In particular, this paper will highlight the roles of income distribution and demography in setting the levels of social spending.

Given the vastness of the literature on the growth of the state, and the complexity of the policies and behavior under discussion, this paper can only offer an incomplete display of evidence with condensed caveats. The most efficient path here leads from a look at competing theories to some tests on international panel data and interpretation of a few main results. Section I introduces some leading hypotheses, mostly from the rise-of-the-state literature. Section II offers a test on OECD experience, 1960–1981, highlighting the roles of age and income distributions. Those results surveyed, Section III takes a closer look at the apparent weakness of the usual cost-of-government arguments and suggests a possible explanation. Section IV uses the regression results to ''predict'' several countries' social-spending trends from 1980 through 2020, arguing that demography may have set the real limits to social spending before rising deadweight costs could set in.

[5] See the tests on 1880–1930 experience in Lindert (1994), and the similar results for a 1930-and-1960 sample in WP.

I. COMPETING HYPOTHESES

Twentieth-century experience has set a large stage on which a host of social-science theories of social spending can compete for leading roles. The competing models have been well developed and need only a brief introduction before they are tested against the international patterns of recent decades.[6]

Deadweight-cost theories predict that the rising cost of expanding government is what limits spending, including social spending, in the long run. The deadweight costs rise in proportion to the square of the tax-rate wedges built into most tax-transfer combinations. The rising-cost argument implies, other things equal, that both income growth and redistribution through government would slow down and approach stasis. In the most pessimistic variant, the deadweight costs keep rising even as they choke off the growth of the whole economy. Niskanen's (1971) theory of bureaucratic growth actually implies an acceleration in the shares of labor force and national product gobbled up by bureaucracies, apparently including social-transfer bureaucracies.[7] This pessimism about bureaucratic empire building is an extreme form of an hypothesis we must confront: the *momentum* hypothesis, which argues that social programs take on their own upward momentum once they are launched.

The notion that *income* growth will raise taxes and government spending, including social spending, is the most durable black box in the whole rise-of-the-state literature. Tests often, though not always, find that higher average income raises not only the level of government spending, but even its share of national income. There is no consensus on why higher income should raise the government share, and political scientists and sociologists, with equal validity, repackage the income effect as general ''developmental'' effect.[8] Some variants are optimistic (development provides the social insurance we need), while others are pessimistic (development creates the problems that make insurance more urgent). All variants

[6] For a fuller review of most of the theories tested here, see Lindert (1994). These cannot cover all the hypotheses advanced in the vast literature on the theory of optimal taxation. For an early summary, see Atkinson and Stiglitz (1980, pp. 333–450).

[7] On the predicted nonlinear rise in deadweight costs, see Becker (1983, 1985), Tullock (1983), Browning and Johnson (1984), and Browning (1987).

Others who have suggested that aging social programs generate their own momentum include Aaron (1967), Pryor (1968), Pampel and Williamson (1989), and Peacock (1992).

[8] Examples from the economists' size-of-government literature include Borcherding (1985) and Mueller and Murrell (1986).

Even when regressions suggest an elasticity less than or equal to one, economists often hook the growth of government to some other development variable that rises monotonically with income. For example, Mueller (1989, Chap. 17) doubts that the income elasticity of demand for government is above one, but cites with approval a large literature on the ''Baumol effect'' that the rise of government is an inelastic response to secular rise in the relative price of its services. That relative price is tied to the real wage rate, itself a monotonic symptom of advancing economic development. Thus, the seemingly different price effect gives the same trend prediction as the belief in a high income elasticity of demand.

On different interpretations of development effects, see Wilensky (1975, p. 47), Jackman (1975), and Pampel and Williamson (1989, Chaps. 3, 4).

predict that the shares of government transfers and total government spending should rise with the advance of average incomes.

Recent views have shifted the focus from income levels to the *distribution of income* as a determinant of fiscal redistribution. While much of the recent writing invokes the perennial debate about ''inequality'' and growth, the variable of choice is usually not inequality as such, but income skewness, proxied by the ratio of the mean to the median income. The higher this ratio, the poorer is our stylized friend, the median voter, relative to the average income available for redistribution. The poorer the median voter feels, the more she or he will favor taxation of the rich and egalitarian spending. So argued Meltzer and Richard (1981). The same belief that a higher mean/median income ratio raises the willingness of the median voter to favor higher taxes and spending has recently been given an egalitarian twist. Both the model of Alesina and Rodrik (1994) and that of Persson and Tabellini (1994) suggest that higher inequality, measured by the same mean/median income ratio, is bad for economic growth because it gives the median voter more taste for taxing the rich, which discourages growth. Hence, pre-fisc equality, somehow achieved, is good for growth. Saint-Paul and Vervier (1991) agree that a higher mean/median ratio raises redistribution, but stress the favorable growth effects from taxes spent on education.

Other income-distribution theories are similar but more flexible, in that they abandon the mean/median income ratio and the stylized median voter in favor of more nuanced pressure-group reasoning. Peltzman (1980) predicted that progressive redistribution will be greater, the greater the dispersion of incomes below the median and the greater the mean-income gap between the rich and poor halves of society. More recently, the ''social affinity'' theory developed by Kristov *et al.* (1992) has predicted that progressive redistribution through government budgets will be greater, the wider the pre-fisc income gaps above the median and the lower the gaps below the median.

The only *electoral variables* to be examined here are those that varied greatly in OECD countries after 1960. By that time the main change, the spread of democracy, had already occurred. The rise of democracy and voting rights raised progressive taxation and social spending before 1960. Universal adult suffrage arrived back in mid-century. The power of Western socialist and social-democratic parties, and of labor unions, had also reached something like a peak in the 1950s.[9] In this paper the emphasized political variables are those that

[9] This indirect reference is as close as the present paper will come to allowing for a causal influence of labor unions and labor parties on social spending. Their role is de-emphasized for three reasons. First, I consider them endogenous intermediate variables, shaped by the spread of voting rights and the movement of socio-economics gaps, that is, by other variables already introduced here. Second, it is hard to get systematic data on the strength of unions and labor parties for the years and nations of the pooled samples used in this paper. Third, other studies have already found that the rate of union membership or union concentration is less important than the presence or absence of nationwide labor–management–government pacts that regulate labor relations, i.e., ''corporatism'' (e.g., Schmidt (1985)). The role of corporatism is quantified below.

fluctuated most within OECD countries after 1960: voter turnout and political turnover. Both can have positive effects on fiscal redistribution. Since voter turnout is more elastic among lower income voters, a high voter turnout tends to tilt sympathies toward progressive redistribution. Faster turnover of the chief executive (president or prime minister) is often thought to be a measure of the insecurity of incumbents' tenure in office. Their insecurity may raise their spending. (Ex post, each individual turnover may shift policy abruptly in either direction.)

Finally, the *age distribution* of the adult population may have been an important influence on society's priorities and its government budgets. An older population seems to vote for more government contribution to pensions, and health and welfare programs.[10] The older the adult population, the more the concerns of the elderly will mobilize both the elderly and those of middling age. There are conflicting intuitions about whether an older population would raise pensions enough to keep them at least as generous per elderly person. The usual bet is negative, predicting less generosity toward the elderly as they become more numerous, but their lobbying power might offset that. What is the net effect of aging on pension generosity, when other forces have been given their due?

Thus, we have a host of competing forces that can now be tested against each other as determinants of social spending: deadweight costs, bureaucratic momentum, income level, income skewness, electoral variables, and the age distribution. We turn to some formal tests, and then to larger interpretive questions raised by the test results.

II. THE DETERMINANTS OF SOCIAL SPENDING, 1960–1981

The relative abundance of data for the period since 1960 allows us to test these competing hypotheses in ways that complement the study of the rise of social spending between 1880 and 1930 (Lindert, 1994). To facilitate comparisons with the results of other studies, the dependent variables to be explained are the conventional ones: shares of different kinds of government expenditures in GDP.[11] The historical data set consists of a pooled cross section of 19 countries over the 22 years 1960–1981, thanks to a special OECD study that made a fair effort to measure social spending by all levels of government on consistent definitions for all 19 countries. Post-1981 data are not available in the abundance

[10] Wilensky (1975), Pampel and Williamson (1989), Lindert (1994).

[11] This is not the only choice, however. To the extent that one wants to focus on how a social-expenditure program affects the incentives and relative well being of those eligible for it, one would prefer the ''support ratio''

$$\frac{\text{(expenditures per person in targeted group)}}{\text{(GDP per person of prime working age, 20–64)}}\text{,}$$

which is equivalent to (expenditures as a share of GDP) times (share of the whole population that is between 20 and 64 years old). The working-paper version of this paper (WP, Tables 1 and 3) presents the results in terms of support ratios.

and quality of this special OECD study.[12] The following analysis reports test results from samples that grouped the annual data into five four-year averages from 1962/1965 through 1978/1981, to simplify the adjustments for serial correlation.[13]

Choosing the income measures, the political parameters, and the age distribution is relatively straightforward, guided by past literature. Less obvious is the choice of a cost-side variable. As noted, there is little likely difference, over time or between countries, in the marginal costs of tax-based social programs for each given share of GDP they claim. Thus, there is no direct way to enter a program-cost-curve parameter into any tests. The appropriate procedure involves a simultaneous-equation feedback: in one kind of equation, social spending is allowed to depend on income, and in another it is allowed to drag down income levels, which would check the social spending itself. This section views the effects of income and other forces on social spending, and Section III explores the possible effects of tax-based social spending on income growth.

The overall model to be tested posits simultaneous interaction between the log of income per person 20–64 (ln Y) and each of six government-expenditure shares of GDP (the S_i's):

$$\ln Y = ao + \sum_i a_i \hat{S}_i + \sum_i b_i \hat{S}_i^2 + cZ_0 + \epsilon_0, \tag{1}$$

and

$$S_i = d_0 + d_1 \ln \hat{Y} + d_2 (\ln \hat{Y})^2 + eZ_1 + \epsilon_i, \tag{2}–(7)$$

where i = p (pensions), w (welfare), u (unemployment compensation), e (education), h (health), or n (nonsocial spending); ˆ = predicted value from first-stage regression; Z's = vectors of exogenous variables; and ϵ's = error terms.

Any statistical test must be tailored to the ideas being tested and the likely patterns in the error terms. First, as is implied by Eqs. (1) through (7), there is good reason to explore simultaneous relations between income levels and social

[12] According to OECD staffers, the Organization has not yet been able to update the special 1960–1981 survey of total government expenditures by type.

Reasonable as the OECD estimates look, they are underexplained in the source (OECD, 1985). Cross-checking against data from the United States, for example, shows a rough correspondence between the U.S. and OECD figures, but with enough discrepancies to leave doubts about just which figures the OECD used. A major uncertainty is the extent to which reporting governments netted out the contributory part of social-insurance programs. On another front, it appears that OECD omitted insurance benefits to government employees. That omission is desirable for present purposes, since this study, like the OECD study, focuses on social programs rather than on payments mandated by contracts between employer and employee.

[13] Thus, we have a sample of 95 observations = 5 four-year period averages times 19 countries, instead of 418 = 22 annual observations for the same 19 countries. Regressions on the larger annual data set produced similar results, except that t's seemed suspiciously high, suggesting that higher order serial correlation may remain unidentified. For both the sample of 95 and the sample of 418, it was assumed that the same Cochrane–Orcutt ρ held for all countries.

spending, and these interactions may well be nonlinear, as reflected in the squared terms here. Second, in a pooled data set involving countries and time, the error terms are likely to contain both international heteroscedasticity (a different error variance for each country) and serial correlation (dependence of each year's error on that of the preceding year). Third, the errors in each of the social-spending equations, Eqs. (2) through (7), are likely to be interdependent. Any omitted influence on the national propensity toward one redistributive program will also cause errors in the estimation of the propensity toward another program. For example, an omitted influence that would raise pension spending may well cut educational or military spending.

Most of these econometric concerns about the historical laboratory we have inherited can be met by a two-stage econometric procedure. The first stage entails generalized-least-squares estimation of $\ln Y$ and the S_i's.[14] The second stage combines the resulting predicted values ($\ln \hat{Y}$ and the $\hat{S}_i$'s) and their squared terms with the other variables in a generalized-least-squares estimation. The pooled-data regression procedure deals with simultaneity, heteroscedasticity, and serial correlation.

These desirable adjustments, however, make it inconvenient to apply a seemingly unrelated regressions technique for estimating Eqs. (2) through (7) as a system. To deal with the issue of cross-equation interdependence of error terms, let us rely on two indirect tests. First, a convenient comparison of single-equation OLS and equation-system OLS estimates shows that the only effect of estimating the whole system together is slightly greater efficiency, with slightly higher significance attached to the same coefficients. By analogy, it should be true that laborious cross-equation estimation of Eqs. (2) through (7)—complete with heteroscedasticity, serial correlation, and simultaneity—would serve primarily to improve fit without changing the coefficient values much. Second, one can confirm, from the results below, that using single-equation techniques has not done violence to the implicit adding-up of coefficients from these equations to those from a direct estimate of all social spending.[15] Tentatively, the two-stage single-equation techniques used here seem to yield unbiased estimates, albeit with some understatement of the statistical significance and goodness of fit achieved by each equation.

[14] The first-stage regressions assume some prior-information restrictions on the sets of exogenous variables (Z_0 and Z_1). Specifically, the equation to estimate $\ln \hat{Y}$ uses only the Z_0 regressors that will appear in Table 4 below, and the equation estimating the $\hat{S}_i$'s uses only the shared Z_1 set of regressors shown in Table 2.

[15] Specifically, adding up each independent variable's coefficients in the equations for pensions, welfare, unemployment, education, and health (in Table 2 below) yields a coefficient for total social spending that is safely within the confidence interval of the direct single-equation estimate for total social spending for 11 out of 12 coefficients (the exception is the upper income gap) and the constant term. Similarly, adding the respective coefficients for social and nonsocial spending to get coefficients for the determination of all government spending fits the direct-equation confidence interval in nine cases (the exceptions being the two young-adult terms, the squared term on over-65s, and the constant term).

Two other necessary cross checks on any statistical results reflect two familiar kinds of counterarguments. One is the possibility that what looks like the influence of a featured independent variable is ''merely'' a fixed-time effect—a relationship of the behavior being explained to unseen forces that just happen to be time-specific. Sidetests show that adding a full set of fixed-time effects has only a negligible effect on the conclusions that follow.[16] The other counterargument, however, will require a closer look. This is the momentum argument, already introduced, which claims that government budget expansions are merely the inexorable growth of programs launched earlier. We shall return to this momentum argument after examining the results of tests that ignore it.

The regression results in Table 2 suffice to harvest most of the insights sought from history's OECD laboratory for 1960–1981. Each set of independent variables (rows) sheds light on a different set of forces. We review these in order and develop their implications.

Income effects, summarized in the first two rows, show up only faintly. That is, there is weak support for the ''Wagner's Law'' view that raising average income would raise the share of government spending in GDP. For all government spending, the estimates imply an income elasticity of 1.20 at the average income level, an elasticity that is not statistically different from unity. Only for total social spending is the underlying income elasticity (1.30) significantly above one for average OECD incomes, thanks to the squared-log term in the third equation of Table 2. The verdict on Wagner's Law in the postwar OECD laboratory is thus the same as that handed down by the 1880–1930 experience (Lindert, 1994): the Wagner's Law idea barely survives.

Electoral variables, the second set of rows in Table 2, seem to show that voter turnout and insecurity of the chief executive do raise government spending, again in conformity with the 1880–1930 experience. Interestingly, these two forces seem to raise different kinds of government spending. A stronger voter turnout seems to have raised spending on every kind of social program, as one would expect if one assumed that the social programs cater to the lower income groups whose voter turnout differs most over time and across countries. Rapid turnover of the executive, by contrast, seems to have raised nonsocial programs, perhaps because insecure incumbents try to pander on a broader front, not just to the lower income voters.

Shifts in the *age distribution* are represented by the third set of independent variables. Given the high levels of spending reached since 1960, and the relative ease of forecasting future movements in the age distribution, we should give age

[16] In addition to fixed-time effects, one might worry that the patterns explored here are ''merely'' due to fixed-country effects, representing a fixed set of unique national influences that have escaped measurement here. This issue can be set aside, however. Most of the effect of a full set of fixed-country effects has already been captured by variables already introduced into the regressions discussed here. In the case of total social spending, for example, over 71% of the explanatory power (contribution to R^2) of a full set of country dummies has already been captured by the fixed-country variables in Table 2, mainly by the income-gap variables.

TABLE 2
Determinants of Social and Nonsocial Government Spending in 19 Countries, 1962/1965–1978/1981

Independent variable	All gov't expenditure		Nonsocial expenditure		All social expenditure		Pensions		Welfare		Unemployment compensation		Education		Health	
	Coefficient	|t|	Coefficient	|t|	Coefficient	|t|	Coefficient	|t|	Coefficient	|t|	Coefficient	|t|	Coefficient	|t|	Coefficient	|t|
ln(GDP per adult 20–64)	−6.51	(0.67)	10.94	(1.62)	−10.45	(1.48)	−5.85	(1.51)	−2.89	(1.13)	0.57	(0.69)	−1.70	(0.91)	2.83	(1.28)
squared	2.88	(1.28)	−2.83°	(1.86)	3.40*	(2.12)	1.39	(1.62)	0.51	(0.90)	−0.15	(0.76)	0.78	(1.80)	−0.44	(0.85)
Voter turnout	10.71	(1.70)	5.52	(1.27)	12.94**	(2.80)	1.80	(0.78)	1.88	(1.15)	1.54*	(2.50)	2.70*	(2.04)	2.48	(1.69)
Executive turnover	0.53**	(3.22)	0.23*	(2.23)	0.18	(1.52)	0.005	(0.07)	0.03	(0.66)	0.001	(0.10)	−0.01	(0.36)	0.02	(0.37)
School-agers (5–19)	−252.55**	(3.18)	−173.50**	(3.38)	21.29	(0.45)	21.41	(1.37)	5.19	(0.34)	1.75	(0.23)	8.15	(0.58)	−53.07**	(2.98)
squared	266.65**	(3.35)	182.96**	(3.52)	−22.87	(0.48)	−28.26	(1.78)	−8.82	(0.57)	−1.23	(0.15)	−4.02	(0.29)	53.31**	(2.92)
Young adults (20–39)	−105.76	(0.45)	−189.54	(1.12)	367.02*	(2.34)	205.79*	(2.55)	222.06**	(3.93)	6.47	(0.32)	−6.23	(0.13)	−20.45	(0.33)
squared	184.25	(0.79)	202.67	(1.22)	−291.51°	(1.92)	−182.73*	(2.34)	−207.72**	(3.80)	−0.32	(0.02)	18.29	(0.39)	42.13	(0.70)
Over 65's	325.25**	(2.84)	−56.07	(0.63)	341.85**	(4.42)	122.58**	(3.65)	50.51	(1.83)	19.41	(1.74)	73.83**	(3.16)	34.62	(1.21)
squared	−396.49	(1.50)	326.68	(1.60)	−599.38**	(3.43)	−200.70*	(2.64)	−80.56	(1.28)	−32.61	(1.28)	−157.31**	(2.97)	−22.44	(0.34)
ln (upper income gap)	−4.65	(1.26)	−8.89**	(2.85)	3.99	(1.51)	−2.07	(1.18)	0.83	(0.61)	1.12**	(2.96)	0.72	(1.04)	−0.84	(0.96)
ln (lower income gap)	−4.74*	(2.12)	4.56**	(2.76)	−7.63**	(4.97)	−0.77	(1.06)	−2.27**	(3.86)	−0.21	(0.93)	−1.74**	(3.71)	0.08	(0.18)
(Plus eight lesser variables—see notes)																
Constant	84.78	(1.09)	110.28*	(2.06)	−125.36*	(2.45)	−53.06*	(2.08)	−52.80**	(2.89)	−11.55	(1.72)	−4.48	(0.28)	7.95	(0.39)
Buse R^2	0.902		0.676		0.914		0.867		0.716		0.581		0.891		0.809	
Durbin–Watson statistic	2.130		1.956		1.974		1.876		1.683		1.683		2.108		1.917	
Dependent variable mean	36.389		15.905		20.483		6.578		3.897		0.597		5.098		4.315	

TABLE 2—*Continued*

Independent variable	All gov't expenditure		Nonsocial expenditure		All social expenditure		Pensions		Welfare		Unemployment compensation		Education		Health	
	Coefficient	$\|t\|$	Coefficient	$\|t\|$	Coefficient	$\|t\|$	Coefficient	$\|t\|$	Coefficient	$\|t\|$	Coefficient	$\|t\|$	Coefficient	$\|t\|$	Coefficient	$\|t\|$
Re-expressing the income-distribution variables in terms of income inequality and skewness																
Inequality	−4.70*	(2.53)	−2.17	(1.38)	−1.82	(1.44)	−1.42	(1.81)	−0.72	(1.07)	0.46**	(2.98)	−0.51	(1.75)	−3.78	(0.91)
Skewness	0.04	(0.02)	−6.73**	(3.40)	5.81**	(3.20)	−0.65	(0.60)	1.55°	(1.91)	0.66*	(2.46)	1.23*	(2.40)	−0.46	(0.82)

Note. Dependent variables = government expenditures as percentages of GDP.

The data set is a two-stage pool consisting of 19 countries and five four-year time periods from 1962/1965 through 1978/1981. The generalized-least-squares regression technique adjusted for country-by-country heterodescedasticity and for a common rate of first-order serial correlation in the nations' time series. The endogenous variable ln(GDP per adult 20–64) is a value predicted by a separate first-stage regression using the predetermined variables from Table 4 below. Correspondingly, the social-expenditure shares used as independent variables in Table 4 below are first-stage predictions based on the predetermined variables shown here in Table 2, plus fixed-time effects and lagged dependent variables.

Definitions and sources for independent variables:

ln(GDP per adult 20–64) = the natural log of real GDP per prime age (20–64), measured in international dollars of 1980, using data from Summers and Heston (1988), and age distributions from the UN *Demographic Yearbook*.

Voter turnout = the ratio of voters to population over the age of 20 in the enfranchised genders, as of the last election. The numerator is from Mackie and Rose (1991).

Executive turnover = the number of changes in the chief executive (president or prime minister) over the preceding 10 years. The data are from Bienen and Van de Walle (1991).

School-agers (5–19) = the ratio of persons 5–19 to persons 20–64.

Young adults (20–39) = the share of adults 20–64 who are 39 or younger.

Over 65s = the ratio of population 65 and older to the population 20–64.

ln (*upper income gap*) = the natural log of the ratio of the average income for the top fifth of full-time earners (or of household income) to that for the middle fifth, relative to a comparable-data ratio for the United Kingdom, circa 1970. The *lower income* gap is the comparable measure between the third and fifth income quintiles. Appendix B of the Working Paper gives sources and methods for calculating these relatives, used in this regression and in the alternative tests of the role of the mean/median income ratio:

TABLE 2—*Continued*
(For years circa 1970, relative to UK = 100)

	Upper/middle income ratio	Middle/lower income ratio	Mean/median income ratio
Australia	92.2	216.7	0.923
Belgium	138.0	59.9	1.045
Canada	106.2	131.2	1.017
Denmark	109.3	121.7	1.011
Finland	81.8	85.6	0.901
France	135.6	117.6	1.146
West Germany	101.2	91.4	0.988
Ireland	120.2	123.1	1.090
Japan	126.9	135.8	1.104
Netherlands	65.8	79.6	0.773
Norway	75.3	136.4	0.837
Sweden	105.6	120.4	1.028
United Kingdom	100.0	100.0	1.000
United States	136.9	318.8	1.069

Here the ''median'' income is approximated by the average income of the third quintile. Comparable income-distribution measures were unavailable for Austria, Greece, Italy, New Zealand, and Switzerland.

Income *inequality* = ln (upper gap) + ln (lower gap).

Income *skewness* = ln (upper gap) − ln (lower gap).

Eight lesser variables: In addition to the independent variables shown here, each regression also included eight other variables of lesser interest. Five of these were binary variables for those countries lacking reliable fixed-effect estimates of the income gaps (Austria, Greece, Italy, New Zealand, and Switzerland). Two others were binaries for the absence of democracy in Greece, 1967–1973, and the absence of female franchise in Switzerland before 1972. The final extra variable was the approximate share of Roman Catholics among all who declared a religion. The only significant effects of these last three corrective variables were negative coefficients on both pre-1972 Switzerland and the Catholic share in the educational-spending equation.

In the absence of the coefficients for these variables, the numbers reported in Table 2 yield predicted values for non-Catholic full democracies with income-distribution data.

° Significant at 7% level.

* Significant at 5% level.

** Significant at 1% level (two-tail).

variables a chance to show that their influence on social spending is not monotonic. Perhaps their effect accelerates or perhaps it will reverse itself as the age distribution continues to shift. If a reversal is predicted by the data, and if we can think of an underlying mechanism bringing such a reversal, we would have at least a partial answer to the question of ultimate limits.

The age distribution played several roles, each of which lends itself to easy interpretation. Whatever one might have thought about the life cycle of conservatism, the older the population, the more it took from taxpayers for government spending, other things equal. Table 2 says so in several ways. Each equation contains quadratic age terms, to allow for either acceleration or reversal in the effect of each age-group share on social spending. Among the strongest effects are these:

	Sample range for the age ratios (min. $<$ mean $<$ max.)	Over the sample range, expanding this age share relative to the 40–64 population would significantly:
School-agers (per working-aged adult)	$(0.347 < 0.458 < 0.615)$	• Cut educational spending per child, while having little effect on such spending as a share of GDP
Young adults (ages 20–39)	$(0.428 < 0.504 < 0.597)$	• Raise total social spending, particularly welfare spending and even pension spending
Over-65's (per working-aged adult)	$(0.106 < 0.204 < 0.287)$	• Raise total government spending • Raise total social-program spending until there are 285 elderly per 1000 adults 20–64 • Raise total pension spending until the elderly share reaches 0.305 over-65's per person 20–64, though pension spending *per elderly person* drops after this age ratio passes 0.224

There are two main patterns in the overall set of age effects. One is that the higher the share of the elderly (over-65's) among adults, the more society devotes taxes to social, and even total government, spending. Second, a population with more young adults (20–39's) per middle-aged adult (40–64's) tends to spend more, not less, on tax-based social programs, even for pensions.

Turning to specific kinds of social spending, we pick up a story already partly told in the results for 1880–1930. A more elderly adult population gives more tax-based pension subsidies. The coefficients on the over-65 terms in the pension equation of Table 2 imply that raising the elderly share of the adult population would go on raising government pension expenditures until there are 305 persons over 65 per 1000 persons 20–64. That point had not been reached by 1981 and is only now being reached by the oldest populations, those of Sweden and Norway.

For most OECD countries, a rise in the over-65 share of adults could continue to raise the share of GDP spent by government on pensions. Meanwhile, a rise in the share of under-40's among adults would have a similar effect.

Up to a point, population aging could even raise pension expenditures *per elderly person* relative to GDP per person of working age. Eventually, expanding the size of a group lobbying for assistance reaches negative returns per recipient, as pressure-group models would predict.[17] Taken at face value, the coefficients and averages in Table 2 imply that the threshold is crossed when there are 224 persons over 65 for every thousand working-age adults. For 12 of the 19 countries, that point came before 1976. Finland and Netherlands reached this threshold by 1980, while 5 others (Australia, Canada, Japan, New Zealand, and the United States) had not yet reached it as of 1981. The implication is, though, that it will be reached eventually by every industrialized nation. American baby boomers' fears of diluted pensions seem well founded, says the pension equation, even though the aging of America had not had such an effect before 1981.

For students and their parents, growing numbers already meant diluted educational quality throughout the sample period. A greater number of children 5–19 per person 20–64 does not affect the share of educational spending in GDP very much. So says Table 2's education equation about an historical setting in which this demographic ratio ranged from 0.347 to 0.615. On a per-child basis, a school-age bulge meant *lower* expenditures per child, as some had feared on behalf of the U.S. baby boomers born between 1945 and 1963.[18] Apparently, the school-age share of the population was always above the share that would maximize expenditures per school-ager, in contrast to the situation for pensioners.

The 1960–1981 experience also sheds light on those theories of how changes in the *income distribution* might tip the political scales toward or away from higher taxes and social programs. Here the data constraints are more severe. The theories call for measures of income inequality and of income skewness toward the rich. They also call for measures of income concepts that are likely to be related to political sympathies, such as lifetime earning power or full-time earning power. Unfortunately, most income-distribution data report only incomes unadjusted for hours or age and are inconsistent in their coverage of part-timers, students, and pensioners, badly distorting the international view of the bottom income ranks. The best international studies allow us to compare snapshots of the income

[17] See, for example, the treatment of lobbying-group size in Kristov *et al.* (1992). The present argument does not assume that the elderly themselves are the only ones lobbying for the social programs that the elderly favor. Rather, an older population may be one in which more persons under 65 also feel the concerns of the elderly. The greater the share over 65, the greater the share of under-65's who sense that ''that could be me'' (longer life expectancy) or ''that could be me soon'' (fewer average years to retirement).

[18] On the baby-boom squeeze on schooling per child, see also the less systematic evidence in Lindert (1978, Chap. 6).

A rise in the under-20 share of total population also lowered nonsocial government spending over the whole range of age distributions sampled in 1960–1981. This effect, plus the social-spending results, imply that an older population is relatively more willing to raise taxes.

distributions in 14 countries for the period from the late 1960s to the early 1980s. Only a single set of snapshots is usable, however, since few countries give reliable annual estimates of the income distribution from 1960 through 1981. Fortunately, what little evidence we have suggests little change in countries' pre-fisc income inequalities and income skewness until the 1980s.[19]

The comparisons of pre-fisc income distributions must therefore be viewed as a fixed set of country effects for 14 countries. To round out the fixed-country view of income distributions, however, we must add country dummies for the other 5 countries (Austria, Greece, Italy, New Zealand, and Switzerland) whose income-distribution data could not be used here. This ad hoc adjustment seems harmless, since it supplements one set of hypothesized fixed-country effects for 14 countries with a no-hypothesis set for five others.

The effects of socio-economic inequality and skewness on social spending are represented by the natural logs of the ''upper income gap'' and ''lower income gap'' in Table 2. These are interquintile income ratios (top quintile/middle and middle quintile/bottom, respectively). The two income gap measures are a frugal way to summarize two key dimension of the income distribution, namely overall income inequality and income skewness, the tendency of middle incomes to be in closer ratio to bottom incomes than to top ones. Specifically, we can proxy inequality and skewness as

$$\text{income inequality} = \ln(\text{upper income gap}) + \ln(\text{lower income gap}),$$

and

$$\text{income skewness} = \ln(\text{upper income gap}) - \ln(\text{lower income gap}).$$

Let us first interpret the role of these variables in terms of the hypothesis they tend to support, the social affinity hypothesis cited earlier. That hypothesis predicts more progressive redistribution from rich to poor, the closer middle-income voters feel they are to the poor and the further they feel they are from the rich. A wider lower income gap means less affinity of the middle classes for the poor, and thus less social spending. A wider upper income gap erodes sympathy of the middle classes for the tax-burdened rich, leading to more social spending financed by taxes on high incomes. The social affinity hypothesis could, but need not, be narrowed to predict a positive effect of income skewness, as measured above, on progressive social spending. It makes no prediction about the effect of inequality on social spending.

The social-affinity hypothesis receives support from the results in Table 2. Its definitive prediction is that the coefficient on the upper gap is positive, and that on the lower gap is negative, for clearly progressive redistributions. Exactly that

[19] For indexes of relative income inequalities among 13 countries, see the notes to Table 2, and the details in WP, Appendix B. The main underlying sources are the United Nations, Britain's Royal Commission on the Distribution of Wealth and Income, and the Luxembourg Income Study. All measures are compared to the most comparably defined measures for Britain (e.g., distributions of full-time individual earnings in both countries, or of household pre-fisc income in both countries).

result emerges for total social spending, for welfare spending, for unemployment compensation, and for education subsidies, although the coefficients are significant only for one income gap in each case. For pensions and health, by contrast, Table 2 finds no significant effects of either income gap. All of the results would be consistent with the social-affinity hypothesis if the progressivity ranking of the different clusters of tax-based social spending were, and were perceived to be, [total-social, welfare, unemployment, and education] > [pensions and health]. Yet, it is not clear that education belongs in that more progressive category, nor is it clear that the pension and health programs are much less progressive. With this disclaimer, the overall pattern of social-spending results appears to support the social-affinity hypothesis.[20]

There is a curious split in the effects of skewness versus inequality, as shown at the bottom of Table 2. Income skewness, as just noted, raised total social expenditures. Yet, it cuts nonsocial expenditures strongly enough to leave no effect at all on total government spending. The extra social spending that seems to have arisen from the relative closeness of middle and lower income groups apparently came at the expense of nonsocial government expenditures (military, infrastructure, product subsidies, etc.). A more skewed income distribution apparently did not mean bigger government—just a government with more emphasis on redistribution through social programs.

The effects of overall inequality look quite different. Wider inequality in pre-fisc incomes significantly reduces total government spending as a share of GDP. By itself, this may be partly due to a feedback effect, in which those countries where the wealthy have successfully opposed larger government would be ones where they are able to prevent taxes that would level incomes, even ''pre-fisc'' incomes (since a heavy tax on high incomes would cut wealth inequality). Whatever the role of such feedback from size of government to pre-fisc inequality, the anti-spending effect of inequality is spread across all of Table 2's spending categories except unemployment compensation, which tends to be the smallest of these spending categories. Even more importantly, the anti-spending effect of greater income inequality casts doubt on theories predicting that greater inequality would raise taxes on the rich and propertied.[21]

The 1960–1981 experience poses surprising difficulties for the set of models that leans on the mean/median income ratio as a predictor of greater redistribution

[20] The social-affinity hypothesis also receives support in the behavior of U.S. state governments in 1985 and 1991. See Chernick and Reschovsky (1994).

[21] As predicted by Alesina and Rodrik (1994) and Persson and Tabellini (1994). As noted elsewhere in the present article, they represent ''inequality'' by the mean/median ratio, which does not exactly measure either inequality or skewness.

The negative effect of income inequality also directly contradicts a prediction by Peltzman (1980). On the other hand, Peltzman's other main prediction does receive support here. Like the social-affinity hypothesis, Peltzman's 1980 model predicted that progressive redistribution would be reduced by a wider lower income gap, a prediction borne out by Table 2's effects of the lower income gap on social spending.

through government. On the face of it, the prediction seems plausible enough: A greater mean/median ratio, which might accompany greater skewness in the income distribution, should promote social spending just as skewness does in the bottom row of Table 2, by pushing the median voter into Robin Hood's camp (Meltzer and Richard, 1981; Alesina and Rodrik, 1994, pp. 447–448; Persson and Tabellini 1994, p. 604). Using the income of the middle income quintile as a rough proxy for median income, regressions find that the mean/median income ratio fails to have the positive effect on government spending that the median-voter models predicted. In none of the eight spending categories was its effect significant in regressions that replaced Table 2's income gaps with the mean/median income ratio. And the coefficient on this ratio had the wrong (negative) sign in all the spending categories except unemployment compensation. If other tests yield similar results, we should conclude that the mean/median income ratio lacks predictive power.[22]

To judge the importance of any determinant of social spending, one should look beyond the *t* statistic and sign of its coefficient, to the size of its contribution to explaining observed differences. We need to supplement Table 2 with an accounting for actual differences in social spending over time and space. Table 3 asks ''How much does each set of forces contribute to our overall predicted differences in social spending?'' and ''How well do the predicted differences match the actual differences?'' For brevity, it concentrates on a tale of eight countries, two time periods, and aggregate social spending.

Leading countries' social spending differed in ways that the regression model would predict, according to Table 3 (top). The model makes accurate predictions on the average, either for the 7 countries contrasted with the United States in Table 3 or for all countries in the sample. Such accuracy is to be expected, of course, since we are dealing with part of the sample period. Comparing individual countries with the United States reveals one large prediction error: the model overpredicts social spending in Britain by more than five percentage points. Yet overall, the international contrasts are ordered correctly, both among the 8 countries of Table 3 and among all 19 countries in the sample.

The leading reason why other countries provide more support than does the

[22] The mean/median income ratio is not really supported by the tests previously offered on its behalf. Persson and Tabellini, for example, do not test the effect of this ratio on government redistribution, but instead test the effect of the top-quintile share on income growth, folding two separate structural relationships into a deceivingly simple reduced form.

Other hypotheses fared poorly in side-tests not detailed here. In particular, less redistributive government expenditures, such as military spending, might crowd out redistributive transfers (Feldstein, 1973, pp. 371–373). Yet side-tests found that military spending did not significantly affect social spending or income growth. By inference, it must have crowded out private consumption and government nonmilitary nonsocial spending. Openness of the economy, measured by the foreign trade share of GDP, also failed to explain patterns of social spending in this era (Pampel and Williamson, 1989, Chaps. 3, 4). The effects of Catholicism on government social spending were also less negative than in the 1880–1930 era, confirming Wilensky's (1981) interpretation of the changing social role of the Church.

TABLE 3
Accounting for Differences in Social Spending as a Percentage of GDP

	Australia	Denmark	France	Germany	Japan	Sweden	UK	Average
Predicting differences total social spending in 1978/1981, this country minus United States								
Income level effect	−2.18	−2.10	−2.17	−1.93	−3.63	−2.17	−3.64	*−2.55*
Electoral variables	4.25	4.14	1.10	3.53	2.46	4.34	2.96	*3.25*
Age distribution	−3.67	1.83	1.99	−0.43	−8.58	1.19	0.36	*−1.04*
Income gaps	1.37	6.45	7.57	8.33	6.21	6.39	7.59	*6.27*
Catholicism	0.28	0.83	−1.61	0.00	0.81	0.86	0.50	*0.24*
Predicted differences	*0.04*	*11.15*	*6.89*	*9.49*	*−2.74*	*10.61*	*7.78*	6.17
Actual Differences	*−1.19*	*14.73*	*8.15*	*10.85*	*−3.31*	*12.19*	*2.28*	6.24

	Australia	Denmark	France	Germany	Japan	Sweden	UK	US	Average
Predicting net changes in social-spending percentage, 1962/1965 to 1978/1981									
Income-level effect	2.16	2.71	3.56	3.50	3.80	2.86	1.97	2.25	*2.85*
Electoral variables	1.48	0.83	−0.57	0.05	0.56	1.80	0.55	−0.40	*0.54*
Age distribution	4.71	8.46	3.94	1.34	4.63	9.19	7.91	6.76	*5.87*
Predicted change	*8.35*	*12.00*	*6.94*	*4.89*	*8.99*	*13.85*	*10.44*	*8.61*	9.26
Actual change	*8.02*	*16.56*	*8.69*	*9.42*	*7.74*	*14.77*	*6.90*	*7.93*	10.00

Source. The regression for total social spending in Table 2, and the data used in it.

United States relates to the income distribution, and to social perceptions tied to them. The United States stands out as the industrialized country with the widest gaps between middle and bottom incomes, to judge from measures taken around 1970. Its gaps between top and middle incomes are also wide, though less outstandingly so among OECD countries. Drawing on the regression estimates, Table 3 implies that having less gap between middle and poor should have raised social spending considerably in Europe and Japan. This income-distribution effect seems to fit both the data and intuition. The wide American gap between the poor and the rest of society may also have reduced America's social spending through another mechanism, one that ranks second in Table 3 (top). The difference in electoral variables that predicts more spending in other countries than in America is driven mainly by the voter-turnout effect: Americans are less likely to vote, especially those Americans with lower income and education.[23]

International age differences also play a significant role in explaining who spends more than who on social programs. Europe spends more than America in part because a greater share of European voters are elderly (over 65). Correspondingly, Australia and Japan spend less partly because their adult populations have been younger and less interested in safety-net issues other than schooling. By contrast, Catholicism plays no real role, and income effects predict the opposite international differences from those observed.

[23] See Wolfinger and Rosenstone (1980) and Teixeira (1987).

The forces featured here also account for most of the net rise in social-spending shares in eight countries between 1962/1966 and 1977/1981, according to the lower panel in Table 3. It is remarkable that the changes over time were followed so well by a model that omitted fixed-time effects and dynamic feedbacks from lagged values of the dependent variable. Of the three causal players introduced here, the aging of populations played the greatest role in explaining the differential rises of social spending from 1962/1965 to 1978/1981.

It is time to return to the main competing hypothesis that has not yet been addressed in discussing Tables 2 and 3, namely the momentum hypothesis that once social programs get launched, they grow with a momentum of their own. Perhaps the growth, and the international differences, in social spending as a share of GDP is really due to the lobbying power of swelling bureaucracies or to some political or budgetary friction that makes any initial social demand for such programs work itself out. Perhaps such momentum was the real driving force between 1960 and 1981, and perhaps all the variables discussed thus far are accidentally correlated with an expenditure growth they did not explain.

The elusive part of the momentum argument is that it is never accompanied by a clear answer to the obvious questions ''Momentum starting when?'' and ''Momentum started by what?'' If the initial launching of a redistributive dynamic was caused by forces like those discussed above, then it is just the working out of these forces, and not a separate explanation. There could still be some interest in knowing whether the growth of social programs, and the taxes to pay for them, springs from known forces from long ago or from the same known forces just yesterday. The most convenient way to quantify the effects of momentum in regressions is by using lagged values of all variables. This does not work out in practice, however, partly because the data seldom run far enough into the past to allow consistent coverage of long lags. Since our data set begins only with 1960, there is no way to explore longer run dynamics.[24]

We are not powerless on the momentum issue, however. There is reason to doubt its separate importance. Table 2's regressions seemed to achieve excellent fits while ignoring any such dynamic. It is not true that all countries with major social programs in place by 1960 all marched in a lock-step dictated by their having launched such programs. Their levels and rates of change in social spending not only varied greatly, but varied in ways correlated with the variables already discussed.

[24] For what it is worth, the only feasible regression with a lagged dependent variable used a lag of 3.5 years, to use the earliest (1960) data in explaining the 1962/1965 levels of social spending. That set of regressions, resembling Table 2's regressions, showed that the lagged dependent variable always had an elasticity below unity. There was no suggestion of momentum in a strong form (an elasticity of present with respect to past spending above one) or of a random walk (a unit elasticity). In those regressions, other variables retained their same signs. Most retained their same levels of significance, though some of the income-gap and age-distribution coefficients shrank and lost significance.

III. WHAT HAPPENED TO THE RISING COSTS?

Thus far, it appears that aging, the income distribution, the income level, and voter turnout are prime suspects in the international differences and trends in social spending. Yet before crediting these factors with setting the real limits to social spending, we must confront the traditional arguments about deadweight costs. Won't social spending be checked by itself, by inflicting such costs on the economy as to drag down income levels, which in turn affect social spending? As mentioned, the strategy here is to look for deadweight-cost effects on the other side of the circle linking social spending and economic productivity. If it can be shown that social spending cuts productivity and incomes, then there is reason to believe that the income loss in turn lowers the generosity of social supports.

The deadweight-cost argument rests on a strong negative influence of tax-based spending on GDP, an influence that should rise with the square of the tax wedge. In the spirit of Adam Smith's warning, Browning and Johnson argue that each dollar redistributed to the poor not only costs taxpayers that dollar but entails an additional $2.49 of deadweight costs around 1976. While the Browning–Johnson estimate is atypical both in method and in magnitude, measures based on more widely accepted welfare economics, such as Stuart's estimate of $0.72 in deadweight costs on top of the dollar taken from taxpayers, also suggest substantial costs. How could countries spending a sixth of GDP on welfare alone, and taking half of GDP in taxes, defy their logic? Surely the deadweight costs should show up empirically.[25]

Four main kinds of evidence underline the absence of strong deadweight-cost effects of tax-financed social spending in postwar OECD experience:

(1) There is no negative raw correlation between such spending and overall economic growth, as noted in Table 1 above.

(2) Multivariate analysis also yields small, and statistically insignificant, estimates of the deadweight costs for 1962/1965–1978/1981.

(3) Less direct tests confirm this null result.

(4) Since about 1980, other forces are limiting the rise of social spending, so that new cost pressures would hardly be evident even if marginal deadweight costs really would rise sharply with the share of social spending in the economy. The immediate task is to demonstrate (2) and (3), and then to conjecture briefly as to how so many countries managed to defy the appealing logic of the deadweight-cost argument up to about 1980. The fourth point is taken up in Section IV.

To weigh the role of government program costs against other determinants of national product, we start by following a conventional approach to weighing growth determinants. Recent studies of growth convergence have explained the growth and level of income in terms of past income and the accumulation of human and nonhuman capital, adding political and policy variables. Table 4 does

[25] See Browning and Johnson (1984) and Stuart (1984). The atypicality of the Browning–Johnson estimate is suggested by alternative simulations by Ballard (1988) and Triest (1994) get deadweight-cost rates like those of Stuart, such as $0.50–$1.30 in certain baseline cases.

TABLE 4
Determinants of Income Levels, 1960–1981

Independent variable	Eq. (1) Coefficient	Eq. (1) $\|t\|$	Eq. (2) Coefficient	Eq. (2) $\|t\|$	Independent variable mean
ln(GDP/capita), $t-10$	0.494**	(12.22)			1.500
ln(real investment), $t-1$	0.311**	(8.90)			1.082
Primary + secondary school enrollment, $t-10$	0.476**	(3.84)	1.037**	(4.40)	0.631
University enrollments per person 5–19, $t-10$	0.325	(1.12)	2.507**	(3.93)	0.040
Pop. ages 0–19/adults 20–64	0.296**	(3.16)	0.617**	(2.84)	0.508
Adults 20–39/adults 20–64	−0.150	(0.46)	0.839	(1.21)	0.255
Adults 65+/adults 20–64	1.251*	(2.53)	4.502**	(4.25)	0.204
Corporatism	0.003	(0.45)	0.048**	(3.53)	1.632
Predicted values (instrumental variables)					
Pension exp./GDP	−0.0017	(0.11)			6.578
squared	0.0002	(0.26)			
Welfare exp./GDP	0.0057	(0.54)			3.897
squared	0.0003	(0.32)			
Unemploy. comp./GDP	−0.0229	(0.52)			0.597
squared	0.0049	(0.31)			
Health exp./GDP	0.0107	(0.38)			4.315
squared	−0.0025	(0.93)			
All social transfers/GDP			−0.022	(1.29)	15.387
squared			0.0007	(1.80)	
Education exp./GDP	0.0157	(0.45)	0.266**	(4.29)	5.098
squared	−0.0018	(0.55)	−0.020**	(3.34)	
Nonsocial exp./GDP	−0.0110	(0.50)	0.019	(0.37)	15.896
squared	0.0002	(0.34)	−0.0014	(0.88)	
Constant	0.7493	(2.51)	−0.681	(1.03)	
Buse R^2	0.967		0.804		
Durbin–Watson	1.605		1.793		
Dependent variable mean	2.479		2.479		
Dependent variable mean (unlogged)	($11,932)		($11,932)		

Note. Dependent variable = ln(real GDP per adult 20–64), in thousands of 1980 international dollars.

Same regression techniques and sample as in Table 2. Each expenditure share is a predicted value from a first-stage regression, as in the second-stage regressions shown in Table 2, except that the log-income term and its square were omitted.

Both the GDP and the real investment terms take logs of thousands of dollars.

The lagged real-investment data are meant to serve as a set of proxies for current inputs of nonhuman capital per person 20–64. Thus, each of the lagged investment values is divided by the 20–64 population in the current year, not in the lagged years.

''Corporatism'' is a crude index of national-level institutions negotiating pay, employment, and government fiscal policies among organized representatives of labor, business, and government. Use of indexes from Schmitter (1981) and from Bruno and Sachs (1985) suggests that Corporatism = 4.0 for Austria, Netherlands, Norway, and Sweden; 3.0 for Denmark and West Germany; 2.5 for Finland; 2.0 for Belgium and Switzerland; 1.5 for Japan; 0.5 for Italy and New Zealand; and 0.0 for all other countries.

''All social transfers'' = government expenditures on pensions, welfare, unemployment, and health.

* Significant at the 5% level.

** Significant at the 1% level (two-tail).

the same, exploring the determinants of the income level, measured by the log level of real GDP per adult of prime working age (20–64).[26] Past investments in human and nonhuman inputs are represented by proxies: the levels of school enrollments and university enrollments per person aged 5–19 ten years earlier, and the levels of gross nonhuman capital formation one year earlier, per member of the current (not lagged) working-age population, in 1980 international dollars. Longer investment lags gave similar results. The equations shown here are simultaneous with the social-expenditure equations in Table 2.

Table 4's results confirm standard expectations about some basic growth determinants, en route to its spotlighting the effects of government programs. The first equation adheres strictly to the conventional production-function approach, while the second takes a reduced-form approach, omitting past income levels and capital formation as merely intermediate mechanisms shaped by more basic forces, such as human capital, corporatist postwar institutions, and government spending levels.

The top row shows the effect of past income levels. As the convergence literature would lead one to expect, higher past income reflects advantages that carry over to the present, but with an elasticity that is definitely below unity. The age-group variables offer a secondary correction, showing that GDP per person aged 20–64 is raised by the presence of extra children (who also contribute labor inputs) and by persons over 65 (who also work some, and who own productive property). The share of the prime-age population that is under 40 seems to have little effect.

Inputs of human and nonhuman capital play their usual positive roles. One could argue that recent nonhuman capital should be omitted from the set of income-explaining variables, on the grounds that it is the result of prior accumulation of human skills or of the recent government policies whose full effects are at issue here. To cover this likelihood, the second equation omits the proxies for current non-human capital inputs, giving most of the extra credit to prior levels of school and university enrollment.

Corporatist institutions have been credited with raising incomes over the whole business cycle.[27] The main mechanism is their alleged ability to keep the industrial peace, lowering unemployment and giving businesses more reason to invest in both equipment and worker skills. This view finds support from the reduced-form equation here, though not from the fuller equation.

The possibly key role of government spending, and of the taxes behind it, is tested with expenditure shares in Table 4. A quadratic form is used for two reasons. First, the conventional fear of tax wedges and effort disincentives implies

[26] The regression approach used here follows such recent contributions to the convergence debate as Barro (1991), DeLong and Summers (1991), Mankiw *et al.* (1992), and Barro and Lee (1993). Additional unreported regressions added fixed-time effects for each four-year period, to pick up contributions of technological advance and the world business cycle. The fixed-time effects made little difference to R^2 or to the values of other coefficients, and the extra equations are thus omitted here.

[27] See, for example, Bruno and Sachs (1985, Chap. 11).

that their deadweight cost rises with the *square* of the wedge or disincentive, as it would in a classic deadweight-triangle diagram. Second, if we want to suggest extrapolations into higher levels of government involvement, it is important to test for curvature, whatever the underlying mechanism.

The conventional deadweight-cost view would predict that expanding any program eventually cuts national income, meaning that their squared terms should all be significantly negative in Table 4. The conventional view is not borne out by the results. None of the dozen government terms in the first equation is even close to statistical significance. Several variations on this conventional equation were tried, with the same result. One reason for turning to the reduced-form approach of the second equation is that it presents the deadweight-cost argument in the most favorable available light. The negative coefficient on social transfers is statistically significant at the 11% level. To develop this potential role for negative effects of government spending, let us first summarize how Eq. (2)'s predictions relate to the ranges of spending actually experienced by our 19 countries in 1960–1981:

Program	Sample range for expenditures/GDP (min. < mean < max.)	Over the sample range, expanding this program's spending share would:
Social transfers	(9.0 < 20.5 < 39.3)	• Reduce income when the share is below 15.3%, then raise it
Education	(1.7 < 5.1 < 8.0)	• Raise income when the share is below 6.7%
Nonsocial gov't spending	(8.5 < 15.9 < 28.5)	• Reduce income

To find serious deadweight costs from program expansion here, let us build on the negative predictions about social transfers and nonsocial spending.

Table 5 weighs the sizes of the various deadweight costs (and benefits), comparing all the predicted growth effects with the actual log differences in income levels. Table 5 (top) shows that the reduced-form equation from Table 4 overexplains the average income advantage of seven other countries over the United Kingdom. The model fits some differences well, though it overpredicts the relative incomes of Denmark, Germany, Sweden, and the United States relative to Britain. The American income advantage is attributed mainly to the greater prior schooling of its adult labor force. Within Europe, the advantages of Scandinavia and Germany over Britain are attributed to corporatism. Corporatism here represents many institutional dimensions of the cooperative solutions that have brought fuller employment, smoother reallocation of labor, and stronger investment incentives in Austria, Germany, Netherlands, and Scandinavia. In mirror image, the corporatism difference here reminds us of the more confrontational labor relations and business–government relations of postwar Italy, France, Belgium, Britain, and Ireland.

The forces featured here can also account for most of the actual income growth from 1962/1965 to 1978/1981, as shown in Table 5 (bottom). The prediction is

TABLE 5
Accounting for Differences in Income per Working-Age Adult

	Australia	Denmark	France	Germany	Japan	Sweden	U.S.	Average
Predicting differences in income levels in 1978/1981, this country minus U.K.								
Age-distribution effect	−0.34	−0.07	−0.03	−0.05	−0.52	0.03	−0.24	*−0.17*
Past education	0.09	−0.08	0.00	−0.07	0.11	0.07	0.52	*0.09*
Educ. support	0.00	0.00	−0.02	−0.04	−0.05	−0.02	−0.02	*−0.02*
Social transfers	0.00	0.06	0.02	0.10	0.01	0.04	0.00	*0.03*
Nonsocial spending	0.28	0.22	0.20	0.21	0.32	−0.13	0.26	*0.19*
Corporatism	0.00	0.15	0.00	0.15	0.07	0.19	0.00	*0.08*
Predicted differences	*0.03*	*0.28*	*0.17*	*0.30*	*−0.06*	*0.19*	*0.52*	0.20
Actual differences	*0.03*	*0.15*	*0.18*	*0.15*	*−0.09*	*0.06*	*0.35*	0.12

	Australia	Denmark	France	Germany	Japan	Sweden	U.K.	U.S.	Average
Predicting net log-change in income level, 1962/1965 to 1978/1981									
Age-distribution effect	0.02	0.23	0.10	0.23	0.03	0.31	0.30	0.01	*0.15*
Past education	0.19	0.17	0.20	0.15	0.15	0.31	0.13	0.36	*0.21*
Education support	0.17	0.08	0.12	0.22	0.11	0.04	0.12	0.10	*0.12*
Social transfers[a]	−0.03	0.06	0.02	0.09	−0.07	0.04	−0.01	−0.04	*0.01*
Nonsocial spending[a]	−0.03	−0.12	−0.03	−0.11	−0.03	−0.34	−0.14	0.04	*−0.09*
Predicted change	*0.31*	*0.41*	*0.42*	*0.57*	*0.18*	*0.36*	*0.41*	*0.47*	0.39
Actual change	*0.32*	*0.38*	*0.59*	*0.52*	*0.88*	*0.36*	*0.42*	*0.25*	0.46

Source. The reduced-form regression from Table 4.

Note. Each figure is a difference between the natural logs of the respective levels of GDP per person in the age range 20–64, in 1980 international dollars.

[a] Negative signs suggest net deadweight costs of extra spending.

near the mark for some countries, though the growth of Japan is badly underexplained and that of the United States is overexplained. The forces showing the most predictive power are the advance of education and changes in the age distribution.

The deadweight costs of social transfers and nonsocial spending do show up in Table 5. They are too small, however, to fit the expectations of those who predicted that expanding such tax-based spending would choke off growth. They play minor roles in accounting for international income differences (top), and as a negative contribution to growth (bottom). For example, the eight-country average growth of 58% (the log change of 0.46) between 1962/1965 and 1978/1981 was hampered by only an 8.6% loss (log change $= -0.09$) due to tax-based nonsocial spending, which was more than canceled by the income gains from the rise of tax-based educational subsidies. In terms of the deadweight-cost ratios cited in

past literature, each dollar of social transfers brought only $0.09 in marginal deadweight gains for the eight main countries (and a *gain* of $0.02 for the 19 countries), well below Stuart's $0.72 cost and the Browning–Johnson $2.49.

Two indirect tests of the absence of spiraling cost should be noted briefly here. The first relates to the burdens on the more heavily taxed groups. To the extent that the expanding taxes and social spending are a net cost to taxpayers, even more than to the whole economy, we should expect to see that the share of taxes and social spending in GDP rose more slowly where it was already higher, because of threatened and actual flight of capital and skills to lower tax countries. Yet the opposite trend has been evident among OECD countries: the percentage-point differences in tax rates have continued to widen.[28] The widening of the implied tax-wedge gaps between nations actually spans the whole period since 1880 or earlier, since the gaps could not have been so wide when the state claimed only a tiny share of GDP. International flight has not constrained the higher budget countries to wait for lower budget countries to catch up in terms of spending shares of GDP.

Second, what the international cross section suggests about social spending is not far from the findings of an econometric study of the interstate cross section within the United States. Helms (1985) found that between 1966 and 1979 higher state taxes did reduce state income when they were spent on transfers. However, when they were spent on state services like health or education, the net effect on state income was positive. On balance, higher taxes spent on the average mixture of transfers and services could have slightly raised aggregate state income. Even where businesses could easily flee the fiscal burden, as across states in the United States, the fiscal packages of the higher budget states were so designed as to do no clear damage to growth.[29]

Can the rise in social spending have had so little deadweight cost? It would certainly be naive to reach such an important conclusion from regressions alone. These specifications, like most others, rest on a set of assumptions that may be wrong, especially the assumptions about serial correlation or omitted variables. Deepening the evidence and exploring the possible mechanism remain a top research priority.[30] What can be added here are possible explanations for the deadweight-cost avoidance, explanations that should be clearly labeled CONJECTURE.

There may be good reasons why the cost spiral had not shown up, even in

[28] For figures calculated from OECD (1985) and World Bank (1990, 1993), see the Working Paper version of this article.

[29] Helms (1985).

[30] The results obtained by Barro and Lee (1993) reveal more significant deadweight costs of government, but on a different front. Their government variable is "government consumption" excluding transfer payments and educational and military expenditures. The data set accompanying Barro (1991) and DeLong and Summers (1991) also seems to show that the cost of government consumption arose mainly in Third World dictatorships, not in the OECD countries with high social-spending shares.

high-spending Europe, by 1981. Perhaps the main force holding down the deadweight costs of social-spending programs, and of some other government interventions, is that the process of pressure-group competition reflects more awareness of program costs than our usual parables admit. Economic theory may have erred in not pushing Becker's pressure-group argument far enough. Competing interests usually do take account of the side-costs borne by their constituencies, as Becker theorized. Their awareness need not be confined, however, to the simple fight he modeled: a one-dimensional tug-of-war over the size of a single transfer. Rather, those who shape budgets and laws may shop around, exploring many dimensions of program design that could achieve politically desired transfers at lower deadweight cost. Perhaps the larger the budget contemplated, the more urgent the imperative to cut its net cost per dollar budgeted.

The whole design of taxes and transfers may have evolved in ways that were aimed, however crudely, at containing deadweight costs.[31] The basic shifts from indirect to direct taxation and from frugal to generous poor relief have had aspects that cut administrative and incentive costs. Over the past two centuries, the shift from indirect to direct taxation has been accompanied by a drop in the share of revenue spent on administrative costs, at least in Britain and America. It also brought a decline in the rate of deadweight cost because the old indirect taxes, especially the customs, taxed behaviors that had higher price elasticities than today's labor supply elasticity of 0.25 or less with respect to direct taxes. Costs were also contained on the expenditure side. Historical evidence on transfer-payment programs aimed primarily at the poor belies the common belief in high rates of leakage into administrative costs. On the contrary, the administrative-cost share is only about 2–3% of total expenditures for today's large-scale programs, versus 12–25% for the older policies that ran workhouses and denied relief to most of the able-bodied poor, in order to maximize their work incentive.[32] The regressions may not be wrong in failing to find that the costs of welfare and unemployment compensation soar with the square of those expenditures: perhaps the higher spending countries have adapted their programs to cut effort disincentives.

Deadweight costs do exist, of course, and there is no denying that they would check social spending in the obvious extreme: a move to expand social programs by raising the rate of taxation on all productive incomes from 90 to 100% would meet fierce resistance. Yet real-world democracies have not approached that

[31] A referee has pointed out a different adaptation to huge budgets not mentioned in this paragraph. The higher social-budget countries of West Europe tend to rely more heavily on value-added taxes relative to income taxes. The VAT avoids the double-taxing of interest and dividend earnings, so that intertemporal choices are not distorted away from accumulation.

[32] On the Anglo–American history of administrative costs on both the tax and transfer-spending sides, see Lindert (1991, pp. 15–16, and Tables 3 and 4). On recent administrative-cost shares for many countries' social security programs, see Estrin (1988). For summaries of postwar U.S. evidence on the response of labor supply to taxation at different income levels, see Killingsworth (1983, pp. 398–399), Hausman (1985, pp. 238–252), Mroz (1987), Burtless (1987), Triest (1990, 1994).

TABLE 6
Actual Changes versus Predicted Changes in the Share of Social Spending in GDP, 1962/1965–1990

	From 1962/1965 to 1978/1981				From 1980 to 1990			
	Predicted effects				Predicted effects			
	Income	Age distribution	Both	Actual change	Income	Age distribution	Both	Actual change
Australia	2.16	4.71	6.87	8.02	0.18	2.04	2.22	−0.59
Austria	3.60	6.22	9.82	7.97	0.24	−0.62	−0.38	0.61
Canada	2.49	4.40	6.89	7.56	0.73	2.07	2.81	0.91
Denmark	2.71	8.46	11.17	16.56	0.51	−2.13	−1.61	−0.96
France	3.56	3.94	7.50	8.69	0.39	−1.34	−0.96	3.75
Norway	3.66	10.51	14.17	13.15	0.64	0.39	1.03	6.97
U.K.	1.97	7.91	9.88	6.90	0.20	0.22	0.42	−2.97
U.S.	2.25	6.76	9.01	7.93	0.59	1.17	1.76	0.46

Source. Table 1 and its sources for the actual changes; Tables 2 and 3 and their data sources for the predictions. Figure 1 converts the 1962/1965–1978/1981 changes into their decadal rates.

extreme. Even the net private costs to owners of movable capital have not triggered mass movements from high- to low-tax countries. Perhaps the underlying reason, beyond possible misspecifications of the present tests, is that policymakers have long known that a high-budget program of social redistribution, which creates natural enemies, could survive only if its net deadweight costs were kept low.

IV. PARTIAL TESTS AND PREDICTIONS, 1980–2020

To round out a tentative answer to the question posed in the title, the strength of the forces featured thus far should be tested beyond the sample period. In the process, we shall note the fourth and final reason why deadweight costs failed to show their power as a constraint on taxation and social spending: since 1980, they were not even given the chance to fall short in the same way as in the 1960s and 1970s.

The OECD experience of the 1980s permits a limited test of the strength of age-distribution and income effects in 8 countries. The choice of countries and of independent variables is governed by data constraints. Only 8 of our 19 countries yield IMF figures on all social spending by all levels of government. Among independent variables, we still lack sufficiently processed and proven estimates of changes in income distribution and electoral participation for these 8 countries. Fortunately, however, these constraints still allow a test of the two forces that accounted for most of the rise in social spending over time, namely, the age distribution and the income level.

Table 6 and Figure 1 show the extent to which the actual changes in the social-spending share can be explained by changes in age distribution and income

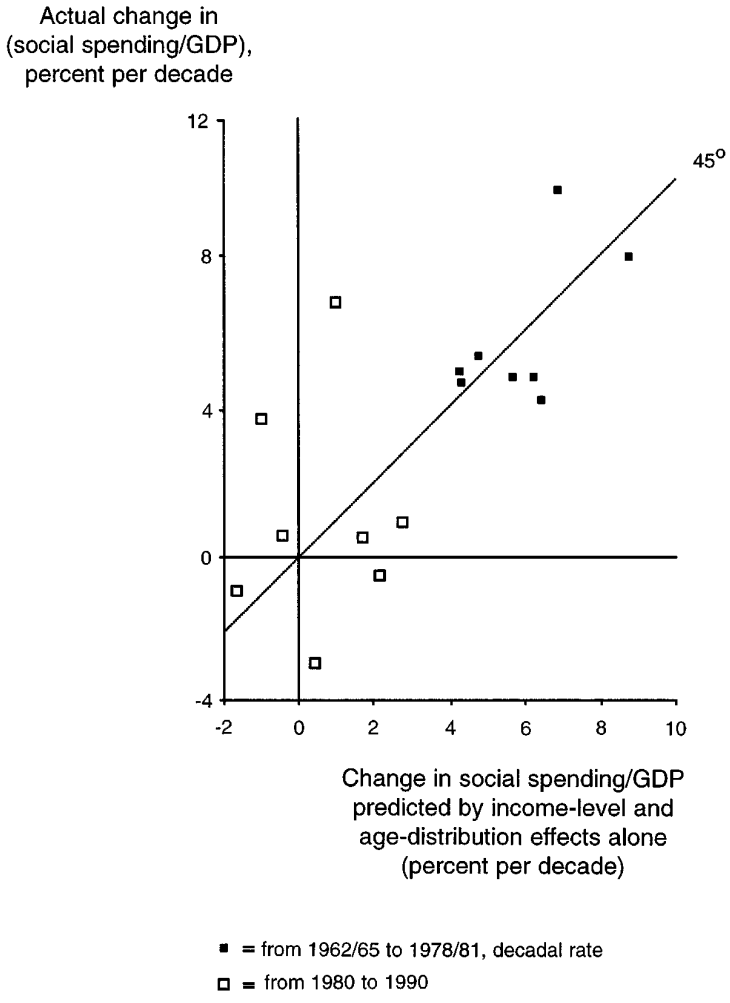

FIG. 1. Changes in the share of social spending in GDP, actual versus those predicted by income and age effects alone, 1962/1965–1978/1981 and 1980–1990.

level, across the 1980s as well as in the sample period 1962/1965–1978/1981. Relative to the sample period, the 1980s show larger prediction errors using just the income-level and age-distribution data and their coefficients from Table 2 above. The worst predictions for the 1980s were the serious underpredictions for Norway and France. That said, there is a remarkable ability of just these two forces, especially the age distribution, to predict the changes over the 1980s and to differentiate them from the faster rates of change across the 1960s and 1970s. Where comparisons can be made, each forecast for 1980–1990 in Table 6 is better than every one of the OECD's alternative forecasts (OECD 1985, p. 50). The

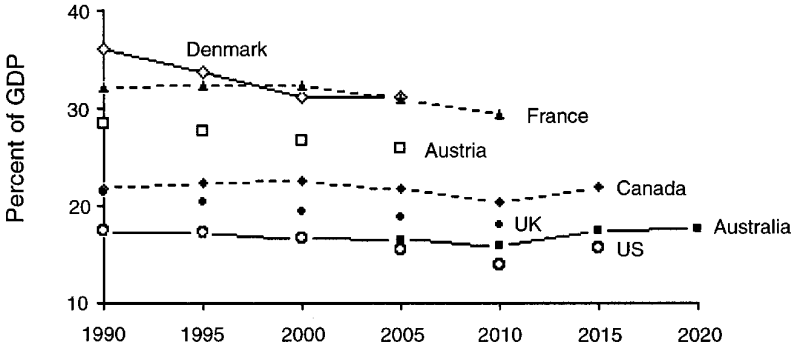

FIG. 2. Predicting changes in the share of social spending in GDP, from income and age effects alone, 1990–2020, based on Table 2's regressions and the data sources underlying them. The forecasts of age distributions are those of Keyfitz and Fliegger (1990). The projections cover only those years for which the expected age-group shares are still within the range of the 1960–1981 sample. The projections of GDP per person of working assume a continuation of the average growth rate for GDP per worker in 1980–1992.

tentative prediction, tentatively supported, is that social spending expanded more slowly in the 1980s than in the 1970s because incomes grew more slowly and because the continuing changes in age distribution implied less pressure to raise social spending and taxes than in the 1960s and 1970s.

The drop in the ''age effect'' relates to the nonlinearities of the age-spending relationship revealed back in Section II. Population aging does not simply raise social spending with a fixed slope or elasticity. The part of aging that shows up as a rise in the share of adults over 65 has a diminishing tendency to tip the policy scales in favor of extra social spending, as we have seen. By the 1980s, the over-65 shares of most countries were rising through the range over which their effect on social spending was predicted to have peaked out, judging from the statistical pattern of the 1960s and 1970s. The suggestion is again that there are diminishing returns to group size in political lobbying, though this inference obviously must await more detailed studies of the political machinery that transmits pressure from age groups.

The deceleration of the 1980s may well continue, if we are to judge from the income and age effects, combined with average expectations about the trends in income levels and age distribution up to the year 2020. This conjecture assumes that GDP per person 20–64 will grow at the annual rate of 2.3% at which GDP per employed person grew in the high-income countries from 1980 to 1992. Slower income growth would produce slightly slower growth in social spending as a share of GDP. The best available age projections suggest continued aging of the OECD adult population, at rates that may reaccelerate after 2010.

Figure 2 shows the conjectures implied by these projections and by the patterns of behavior from the 1960s and 1970s. Each country's behavior is projected into the future until its population becomes older than any population back in

1960–1981, at which date the forecast stops. The potential for wrong forecasts is obvious, but the suggestion that emerges from Fig. 2 seems fruitful: the patterns from the past predict no rise in the share spent on social programs, despite the likelihood of income growth and population aging. The reason is again, as with the 1980s, that once the population is as old as the oldest OECD populations of the 1960s–1970s, further aging does not seem to raise social spending. In fact, Fig. 2 adds the surprising suggestion that there might even be a slight decline in the social-spending share to a trough in 2010. This possible slight decline follows from the other age-distribution theme from Section II: adding more adults under 40, at the expense of adults 40–64, also favors social spending up to a point. Since the likely trend until 2010 is for a fall in the young-adult share to historically low levels, there may be less pressure for social spending from this group. Such an effect must remain highly conjectural, however. The main message of Fig. 2 is not that the share of social spending in GDP should decline, but only that it would probably not rise much, because the pro-spending pressure from aging plays out in an older population.

The limits to social spending, then, may well be set by the changing effect of aging on pressure-group competition. The anti-spending tendency of middle-age groups may stop retreating in the face of pressure from the elderly and young adults—even without any soaring deadweight costs from the welfare state.

V. CONCLUSIONS

Both simple correlations and careful statistical tests call for a rethinking of the determinants of social spending's share of the economy. The limits to social spending may be set by forces quite different from those usually imagined. There are reasons to doubt that the rise of the state, or of its redistributive social programs, will reach the self-checking point where its soaring deadweight costs stop the income growth on which it depends. Those costs were not evident even in the 1960s and 1970s, when taxed-based government spending, especially social spending, shot up rapidly.

The level of social spending may be governed primarily by the relative sizes of age groups, and by the income distribution, electoral conditions, and the income level. That, at least, is suggested by tests on the OECD countries' experience between 1962/1965 and 1978/1981. Stark international contrasts in the share of GDP committed to social spending relate largely to the distribution of income, in a way that seems best predicted by the ''social affinity'' hypothesis. The rate of growth in social spending seems best explained by the strong but diminishing effect of aging, supported by a conventional income effect. When these same patterns are tested against the 1980s and projected into the 21st century, they predict an end to the growth in social spending's share of GDP and suggest new reasons why it had already decelerated in the 1980s.

All such inferences are obviously tentative. They rely on quantitative evidence that might also support other interpretations, and this article has not peered inside

the statistical black box to examine the underlying political mechanisms in any detail.

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