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The Welfare State Is the Wrong Target: A Reply to Bergh

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I AM DELIGHTED TO HAVE THIS CHANCE TO EXPAND ON THE links between social spending and economic growth. Andreas Bergh (2006) raises both small and large concerns that deserve further exploration. Let us go first to the largest issues of scope and methods, then to his specific criticisms of parts of my *Growing Public* book (hereafter *GP*) relating to Sweden's policies toward the poor, toward women's work, and toward retirement. The final section of this reply invites him and others to re-focus their search for flaws in large government, since the welfare state, as actually practiced, has not become a major flaw. On the contrary, the social transfers that have always defined the welfare state are indeed a "free lunch" in the sense that they have delivered more equality and longer life expectancy at an essentially zero cost in terms of GDP. Rather, it is other forms of legal and governmental interference with markets that are more likely to be anti-growth.

Two Centuries, Many Countries

We now have the benefit of a rich international history for studying the sources and effects of social programs. My book has mapped much of that history, starting with a few European countries in the late eighteenth century and widening to a couple dozen countries from the late nineteenth century on. It covers all the historical experience in which democratic

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countries devoted a large share of GDP to social spending, exploring why that social expansion came so late in history, and why it has come more to some countries than to others. The book also tackles the whole issue of the effects of tax-based social spending on economic growth. Bergh focuses on this side of the causal circle and on what he considers my “special use” of Sweden.¹

TESTS VERSUS THEORIES

Think of a bumblebee. With its overly heavy body and little wings, supposedly it should not be able to fly—but it does.... This is how so-called analysts view the Swedish economy. We ‘defy gravity.’ We have high taxes and a large public sector, and yet, Sweden reaches new heights. We are still flying, so well that many envy us for it today.

—Prime Minister Göran Persson, Opening Address on March 10, 2000 to the Extra Party Congress of the Social Democrat Party in Stockholm²

Theory and fact are complements in the production of knowledge. Like all complements, they need to be mixed in the right proportions. Excessive fact-gathering calls for fresh theorizing. Excessive theorizing calls for fresh facts.

On the crucial issue of the welfare state and economic growth, theory has gone into overdrive. For some years now, there has been a widening gap between the empirical record and a story that is re-told with increasing insistence. No longer do we just hear that there is *the danger* that tax-based safety nets and anti-poverty programs *might* bring high economic costs. Notice how often we are told that economists have “shown” and “found” that this is true.

Such assertions are often a bluff. So theory-dependent are the latest parables about the ruinous welfare state that those words—“shown” and “found”—have been abused. Rather than showing or finding this result,

¹ *GP* devotes as much detail to Britain, or to the United States, or to the contrasts among 21 core OECD countries as it does to Sweden.

² As quoted in Thakur et al (2003).

they have *chosen to imagine* it.³ The vulnerability of such theory-dependent bluffs to challenge has reached the point where a pro-state politician could rightly mock them before a partisan audience, as Prime Minister Persson did in the speech quoted above. My reading of the statistical and historical record leads me to very different tentative conclusions about the performance and prospects for the welfare state.

Andreas Bergh's critique is fortunately less theory-dependent than much of the literature pushing the high costs of taxes and social transfers. He has not been guilty of presenting simulations or pure models as fact. His arguments lean mainly on a detailed reading of Sweden's tax and benefits rules. Yet even the higher standard of evidence resting on the calculation of incentive wedges from a reading of statutes runs into a limit—a limit that has become obvious after thirty years of literature in the “I-see-a-tax-wedge” tradition. It is one thing to see a tax wedge, and quite another to show that it—and the larger package policy changes that accompany it—will bring a big effect in the real world. I return to this point below.

To compare theoretical nightmares with actual practice requires statistics and history. Given the large number of countries for which we now have the benefit of decades of fairly good data, there is no excuse for not working at length with multivariate testing of the competing hypotheses. Fortunately, many scholars have ventured the larger statistical voyage to find out how growth is affected by different structures of government taxes and expenditures. Most of the international econometric literature antedates my book. While my own work has presented what I consider more appropriate tests, my findings do not depart from the findings typical of the best-specified tests published by other economists.⁴

³ This passage's contrast between what is “shown,” “found,” and “imagined” is elaborated in *GP* (Volume 2, Chapter 18, especially 82-84) and in my NBER working paper cited by Bergh.

⁴ For the depth of the literature by other economists, see the surveys of it in Atkinson (1999) and *GP* (Chapter 18 in Volume 2, especially Table 18.1). Bergh hides the extent and thrust of this literature with some spin-control phraseology that seems designed to make it look as though my results are weak or un-representative: “Lindert has not been able to find a robust and statistically negative effect.” “Lindert acknowledges that other studies find a negative effect....” (211). The only other econometric studies he cites on this negative effect are the Fölster-Henrekson study, which has been questioned in the other studies he cites, and the Gordon-Wang (2004) comment here in *Econ Journal Watch*. Neither of these studies is really about the welfare state or social transfers. The Gordon-Wang comment deals with a global cross-section, not a panel of OECD democracies, and thus mixes in behavior from such kleptocracies as Zambia and Zimbabwe. It finds an insignificantly positive effect of “scope of public sector” on economic development.

The current state of their econometric evidence is more interesting and more revealing than a reader might catch from Bergh's attempt to set it aside by saying, "It has long been known that it is hard to produce robust results regarding the effect of government size on growth" (212). A more accurate wording would be that most serious studies fail to confirm high net costs or high net benefits of big government among OECD democracies. That fact delivers some important information. We should not push the econometric testing aside as if it were just a faulty camera that cannot take the picture Bergh knows to be true. When repeated tests keep giving non-negative results about social transfers, one should be prepared for the possibility that the tests are telling us something about the real world. One should not keep repeating past assertions about the ruinous cost of the welfare state when the data refuse to reveal it—just as one should not just cite theoretical models saying that the whole package of social transfers is good for economic growth, which is also not supported by any clear test.

The path to firm conclusions requires both solid econometrics and some thinking about what real-world forces might explain the econometric results. The econometrics must meet several criteria. The samples must actually contain different policy regimes. The samples should also pertain to OECD democracies, and not to some global sample mixing too many structures together. The authors must grapple with the thorny issues of simultaneity, heteroskedasticity, and the non-linearity of GDP costs predicted by theory. The measure of government should actually refer to the social transfers of the welfare state. Tests that meet these criteria have contradicted the assertion that the overall welfare-state package has clear high costs.

A more persuasive study in this genre is Kneller, Bleaney and Gemmill (1999), which classifies taxes by whether they discourage investments, and divides expenditures into uses that are productive (general public services, defense, education, health, housing, transport, communications) or unproductive (social benefits, recreation, "economic services"). Using macro-panels with attention to budget accounting constraints, the authors reach intuitively persuasive conclusions: distortionary taxes (on income, payrolls, property) hurt growth if they finance unproductive expenditures, while non-distortionary taxes (on buying goods and services) do not, and productive government expenditures enhance growth, while unproductive ones do not. In Allard and Lindert (2006), we note that the Kneller-Bleaney-Gemmill study does not explicitly test for the welfare state and the implicit bundle of taxes that goes with it, a bundle tilted away from taxing capital and corporations, especially in Sweden's case. Their results seem consistent with those in *GP* (Chapter 18) that explore the relationship of transfers to tax mix.

Taking the next step of reflecting on how the statistics could have given such nullish results is particularly rewarding. One wades into the differences among different kinds of social transfers, different kinds of taxes, and differences in historical context. It turns out that there are good reasons why the net effect could be near zero, as most observers have concluded. Some social transfers are better than others, and some taxes are better than others. The “free lunch puzzle” remains as I described it when exploring these issues in *GP*, though much more remains to be done.

Tax Wedges versus Work versus Output

It is a long road from calculating an apparent tax wedge to delivering a final empirical result in terms of lost work or lost GDP. Perhaps one of the main reasons why Martin Feldstein and others who have contributed so well to the incentive wedge literature have not made the full trip to empirical results is that quantifying the wedges itself is an arduous task. Tax codes and social program rules are very complex. Their complexity raises the importance of doing incentive calculations, because society might be missing something by not knowing the possibly dangerous implications of how the various tax rates, exemption clauses, and economic shocks might interact. Yet the same complexity means that at the end of the exercise, one might have missed some important features of how a whole policy regime change might play out. One common misstep is to fail to look at the full budgetary implications of a particular fiscal shift. Studies of the effects of marginal taxation talk of their “burdens” without exploring how productively taxes are spent. Studies of the distribution of social “benefits” often gloss over the effects of the taxes that pay for them. The danger of missing something is especially great if one *chooses* to look at only part of the picture and then presents it as the whole picture.

For calculated tax wedges, one must sooner or later look at actual performance of the entire national economy to determine how it affects work and output. Let us turn first to the work effect. Bergh, like anybody else worried about what look like high statutory marginal tax rates, must struggle with a severely limiting fact: There is no negative international relationship between employment rates and the main welfare-state indicators, such as the share of transfers in GDP or the statutory generosity

of unemployment compensation.⁵ Nor is the employment ratio lower in the countries for which we have reasonable estimates of overall marginal tax rates. Bergh acknowledges the same point in his two-country comparison of employment ratios in Sweden and the United States.

There are two basic ways to reconcile high statutory tax rates with the absence of work and output response. One is to conclude that people have such low elasticities that the marginal tax rates simply don't matter. The other possible resolution is that the high marginal tax rates are misleading in the first place. The second resolution seems more plausible. Many high statutory tax rates are seldom paid, often because other features of the tax system were designed to allow people to avoid those high rates.⁶ In addition, people see offsetting benefits of work within the welfare state. That the benefits of work within the high-budget welfare state are missed by those analysts intent on seeing taxes as merely taxes is well illustrated by the OECD measures of tax wedges on employment. A large share of these consists of mandatory contributions to the social security system. Yet the individual's benefits from that same system are tied to those paycheck deductions, even if the benefits are "pay as you go" in terms of entire cohorts.

Even if a given tax incentive cuts work, the output results might well be smaller than the work effect. If the higher marginal tax cuts labor supply, employers may well respond by substituting capital and shifting toward the types of labor less affected by the higher wedge. That is, the same mainstream theories that predict a work loss should predict a smaller output loss in percentage terms. The empirical elusiveness of output losses from the welfare state may relate to such productivity offsets, though the prevailing empirical tendency has already been noted: The employment effects themselves are hard to distinguish from zero.⁷

⁵ Using data for 21 OECD countries from 2001, for example, the correlation is zero (0.021) between the overall employment ratio for persons 15-64 and social transfers as a share of GDP. The correlation is slightly negative for men and slightly positive for women.

⁶ The built-in offsets to what look like high marginal rates occur both for high- and low-income households in Sweden. I return later to the ways in which the rich never faced an average effective marginal tax rate as high as the top rates before the 1991 reform. Even for low-income recipients of welfare transfers, the incentives were often less than meets the eye, as the Lindbeck Report, which wanted to stress that Sweden's tax burdens are excessive, briefly cautioned its readers: "[t]he size of public-sector spending [and taxation] in Sweden is not strictly comparable to that of many other countries, because several types of transfers in Sweden are taxed, while in many other countries they are not" (Lindbeck et al 1994, 5).

⁷ An exception is the econometric literature that uses the OECD's *employment* tax wedge as its tax variable. That literature does find a negative effect on employment, but does not follow

Tax Wedges at the Bottom and the Top

Bergh announces very high marginal rates of effective taxation at the bottom and top of the Swedish income ranks, contradicting my argument that the rates have been smoother than in Britain or America. This is an important issue well raised. Let us turn first to the single mothers at the bottom of the ranks, and then to the tax rates faced by the richest.

Bergh's case of statutory tax rates and benefit loss rules facing single mothers is well chosen, since he is talking about a passage in my book (Table 10.2 in Chapter 10 of Volume 1) where I too talk about the marginal rates, rather than about the behavioral response. He denies that Sweden has marginal rates as low as 30-35 percent (as I had said), arguing that benefit losses would give a job-getting single mother a much higher marginal tax rate. Given the complexity of quantifying all the marginal incentives, he could be right on this specific point. I had used other studies that implied lower rates, in particular the Gustafsson-Klevmarken (1993, 106-110) calculations of a 30 percent marginal rate for a single parent and for someone liable for child support payments. Other studies get varying results, and some do indeed calculate high marginal tax rates like those that worry Bergh. For example, the recent IMF study of Sweden worries about the same issue, though it does not take as firm a stand as Bergh does (Thakur *et al.* 2003, esp. 48-50). His claiming a lower marginal rate for a single mother in the United States than I had presented, based on the work of Acs and others, seems to misunderstand which era in American history I was discussing. He is talking about the United States since the generosity of the Earned Income Tax Credit (EITC) was dramatically raised in 1993, whereas I had contrasted Sweden mainly with the United States policies of the 1980s. For that earlier era, before the jump in EITC, the universality contrast between countries still looks the same, adjusted for EITC or WFTC, especially if the provision of health care, child care, and other services is less means-tested in Sweden than in America.

Higher up the income spectrum, Bergh seems to contradict a considerable literature without any solid evidence.⁸ He asserts that “the

through and explore whether such taxes specific to jobs have a negative effect on output. Unreported regressions in the project by Allard and Lindert (2006) confirm that specifically taxing employment reduces employment, but does not significantly reduce output.

⁸ He cites only his own 2004 article. That article does not present any fresh evidence on top marginal income tax rates, nor does it rebut the earlier evidence I cite in the next footnote. Rather, his 2004 article is mainly just a sensible and readable discussion of the different meanings of universality, which he concludes did not decrease in the crisis of the early 1990s.

structure of the marginal net tax rates in Sweden is exactly the opposite of Lindert's description—high for low and high-income earners, and lower for middle income earners” (219). While there lingers some uncertainty about those low-income single mothers, he surely owes us an explanation of why he passed up all the evidence that top income earners did not pay much higher rates than middle earners, either before or after the tax reform of 1991.⁹ Before the reform, they had huge opportunities to pay less than the high top rates that got into the media, and the reform slashed those top rates. Nor does he mention the important point that Sweden levies low corporate income taxes. I had pointed all this out in *GP* (Chapters 10, 11) in the hope of encouraging a new look at how tax structure relates to the welfare state. And if the marginal tax and benefit-loss rate were really so high, Bergh would again need to present evidence that it actually affects labor supply more than in other countries.¹⁰

Women's Hours

Having acknowledged that Swedish women have a higher relative pay than in other countries and a higher employment ratio than in most other countries, including the United States, Bergh goes through some contortions to dismiss that achievement and my explanation of it. My explanation was that so many of them had jobs because Sweden paid women relatively better than in other countries, and women respond with a relatively high elasticity of labor supply.

Bergh disagrees, on the grounds that the average employed woman worked shorter hours in Sweden than in the United States, so that “[w]hen actual working hours are taken into account, we find that Lindert is mistaken about Sweden having higher female employment” and “they spend more time in unpaid non-market work” (220). He thus implies that women's formally working 10 percent fewer hours on the average would

⁹ Hansson and Stuart (1990); Gustafsson and Klevmarcken (1993, 64); Aronsson and Walker (1997, 236-238); Norrman and McLure 1997; and Thakur *et al.* (2003), showing that the marginal effective rate of labor taxation did not peak at the top and that Sweden's marginal effective rate of capital taxation is low relative to other countries.

¹⁰ Denmark's case in the 1980s and 1990s was even more extreme than Sweden's. For a single mother at the bottom of the income structure, the statutory marginal tax rates looked even higher than those posted by Bergh. Yet the in-depth studies found almost no labor supply response at all, either for this sub-group or for the labor force as a whole (Pedersen 1993, especially 273-288; Mogensen 1995).

cancel the work benefits of having jobs for 10 percent more women. His overall context invites readers to think that Swedish women turn to part-time work because they face higher marginal taxes—despite the fact that from 1963 to 1992, as the statutory marginal tax rates probably climbed, women’s average hours per week did not decline as men’s hours did.¹¹

His imagery here is implausible. By equating a percentage difference in hours with a percentage difference in productivity, he implies that the value of an hour of a woman's extra home time is worth zero. Are we to imagine that women are forced into useless partial idleness because of taxes? Do they knock off formal work at 3:00 pm because taxes are too high for a full day, or quit their jobs for part of the year and get a new job later to cut taxes? Instead, should we not look at the greater part-timing and partial unpaid work for women in Sweden (and Netherlands and elsewhere) as a response to a better opportunity set? Unlike their American counterparts, they are not under pressure to keep a full-time job just to keep their health insurance.¹²

Retirement

For the age of retirement, we confront the same clash between calculated marginal tax rates and employment results as for the wedges discussed earlier. For men who would reach the ages 55-64 in 1985-1994, the Gruber-Wise calculations do imply that Sweden gives senior men a stronger incentive to retire than in the United States (Gruber and Wise 1999, *passim*).

Yet for retirement as for all other labor margins, we must compare the international differences in a synthetic tax-rate calculation with the actual work outcome. Among the 21 core OECD countries in 1999, a greater share of men 55-64 were still at work than in most other countries. Sweden ranked fifth in this respect, and the United States ranked sixth. For women, the share still working in the 55-64 age bracket is also high by

¹¹ Aronsson and Walker (1997, 259). For more up-to-date hours estimates see either OECD or the Groningen estimates online.

¹² Bergh offers no reason for saying that he is “inclined to reject Lindert’s elastic labor supply explanation of Sweden’s high female employment ratio” (221). He has not denied that the relative pay favors women more in Sweden than elsewhere. As for the elasticities of labor supply, the literature is clear that it is higher for women than for men, other things equal, in Sweden and elsewhere. See, for example, Aronsson and Walker (1997, 239-244) and Gustafsson and Klevmarken (1993, 77).

international standards, despite Bergh's claims to the contrary. Again we face that choice between two possible reconciliations: Does Sweden's favorable work effort among seniors belie the high marginal tax rates because the elasticity is inherently low, or because the actual net incentive is not as negative as the calculated tax rates suggest? Again, the latter reconciliation is close to my hunch, but either way the issue must be explored, and just calculating an apparent statutory tax wedge is not sufficient to explain actual behavior.

Public versus Private Jobs

In a revealing quip, Bergh tips off his readers that "Lindert. . . . does not tell us that in Sweden, there has been no net growth of jobs in the private sector since 1950," reproducing a graph from Davidsson and Henrekson. Bergh's point about public sector jobs stops there, as if to say "Q.E.D." (221-222).

Yes, the rise in jobs is virtually all in the local-government sector, and virtually all female. Dominant are health-care professionals, educators, and day care providers. But who is the reading audience here? Are readers supposed to shake their heads in knowing disapproval, because they all know that public employment is inferior to private? Again, we are back to that divide between those who are willing to be swayed by direct tests of effects on outputs and wellbeing and those who want to hear only what fits their pre-conceptions.

Changes versus Levels in "Freedom"

Bergh talks against himself in stressing that Sweden's system was reformed in the 1980s and 1990s. Most of the article tries to stress that Sweden has painted itself into a corner today, yet this part emphasizes that Sweden reformed its way out of problems: "[T]he reforms toward increased economic freedom seem to be a good candidate for explaining the survival of the Swedish welfare state" (233). He lists a host of reforms in separate sectors of economic life. This is somehow supposed to contradict Lindert, who "does not attempt to describe" the wave of reforms and its connection with the survival of the welfare state. Yet I did in fact discuss the reforms, and even more reforms than his final page credits me with. I also discussed some reforms in other countries, particularly the major reform of disability

claims in the Netherlands.¹³ It is hard to make out what this final section is trying to achieve, beyond putting in a plug for subjective indices of “freedom” by the Heritage Foundation and the Fraser Institute.

The Welfare State Is the Wrong Target

Bergh’s comment illustrates a puzzling tendency in the debate over big government and the welfare state among OECD democracies. For some reason many critics still prefer to attack the welfare state as if it were some all-purpose Leviathan dragging down freedom and growth. Yet it has not dragged down economic growth, and of all the forms of government intervention social transfers are not the ones that infringe on the flexibility of free markets the most.

To underline the importance of changing targets, let me share Bergh’s chosen emphasis in the trans-Atlantic contrasts in growth factors. That is, let me concentrate here on European institutions that have probably checked the advance of growth and well-being, rather than such American inefficient-policy suspects as its health care/insurance system, its under-investments in the human capital of the poor, and a tax structure that puts too much emphasis on taxing capital and not enough on taxing gasoline, alcohol, and tobacco.

Other European institutions deserve the preliminary indictments that have been misdirected at the welfare state. The same international-panel econometric method that fails to indict the welfare state does indict other institutions. One culprit in particular has been Europe’s employee protection laws (EPLs) dating from the late 1960s and 1970s. By blocking firing, these have also blocked hiring and have probably slowed the rate of re-allocating labor to rising sectors. What is especially clear about EPLs is that they have created insiders and outsiders, much as the model by Lindbeck and Snower (1988, 2001) describes. The negative effect of EPLs is likely to show up only with a lag, and in the form of eventually reduced productivity, since it will take some years for the human capital loss from outsider status to outweigh the human capital gains for the insiders. For as long as the econometric literature was testing for the effect of EPLs on the overall unemployment rate, its results were not fully robust. Yet testing for

¹³ *GP*, pages 271-282, 288-291, 298-300. As for Bergh’s implying that I overlooked school vouchers, I did discuss them and the evidence on their merits in four other European countries and Chile (*GP*, 165-167), though I did not mention Sweden.

the eventual productivity losses seems to have strengthened the case against EPLs.¹⁴

The locus of the problem of EPLs within Europe differs from the locus of the welfare state. EPLs seem to delay jobs and growth mainly in Mediterranean Europe, whereas Italy and Greece are not particularly generous with welfare or unemployment compensation or active labor market policies. While Sweden and the other Nordics have EPLs, they are not as strict as in the Mediterranean, and this seems to have shown up in productivity as well as in the relative unemployment of youths and women.

A second European institutional drawback is in the degree of product market regulation. Here the OECD measures for seven large sectors seem to tell a plausible story, even though they miss the likely importance of restrictions on retailing. Product-market liberalization has an interesting geography. The lead was taken by the Pacific Rim countries (especially New Zealand), with Mediterranean Europe again being the slowest to liberalize. Econometric tests seem to indict product market regulations as a further source of productivity loss in Europe.¹⁵

Finally, a particular American advantage over Europe (and Japan and Latin America) lies in policy toward higher education. Among the social sectors, higher education is perhaps the one where ordinary competition works best. While there is a case for subsidies to correct for knowledge externalities, there is no case for centralized work rules or for dominance of government funding. It is specifically the Americans who have made the least error in this social sector. The public share of funding is lower in American higher education, and state universities must compete with each other as well as with private universities, for research funding, for faculty talent, and for student talent. Students' consumer evaluations also play as strong a role on U.S. campuses as in any other country. Sweden's system seems to be in better shape than those of countries to the south (e.g. Italy), but it is not clear what gains Sweden gets from having centralized university education, and in having made it tuition-free in the past.

Overcoming the excessive targeting of the welfare state sets the stage for a larger inquiry plotting the efficiency boundaries between markets and governments.

¹⁴ Allard and Lindert (2006).

¹⁵ Allard and Lindert (2006, Table 2). While our product-regulation result seems plausible, I should add that it is not necessarily robust to one's choice among defensible regression specifications.

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