

**Comment on Joel Slemrod:  
How costly is a large, redistributive public sector?**

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For many Swedish readers, this may be a provocative paper. Joel Slemrod concludes, after having reviewed the U.S. experience, that the efficiency costs of taxes are probably quite low. Slemrod suggests that the marginal excess burden in the U.S. is probably on the order of no more than 20-25 cents per dollar of tax revenue. This appears in stark contrast to the prevailing view in Sweden that we have reached a point where further expansion of the public sector, accompanied by increasing tax wedges, would be excessively costly in efficiency terms. I think it is fair to say that this general view is shared by friends and foes of the public sector, although the former may argue (putting a high value on redistribution and public services) in favor of *status quo*, while the latter group favors a rapid reduction of the Swedish public sector. The thrust of my comments will be to argue that there is no necessary conflict between Slemrod's reading of the empirical literature, which I agree with, and the current Swedish view, which is also my own.

How can one assess the costs of the public sector? Slemrod distinguishes two main approaches: *bottom-up* and *top-down*. The bottom-up approach starts by recognizing that the costs are due to disincentive effects caused by tax wedges, that is, one has to know the size of the relevant behavioral elasticities. Having estimated these, one can plug the estimates into a suitable equilibrium model and out comes a measure of the marginal cost of public funds to be compared with one's assessment of the marginal benefits of public expenditures. This is the approach that most of the literature has taken, but still it has not been very successful in giving us precise estimates of the excess burden. Certainly the numbers that Slemrod ends up with are subject to considerable uncertainty. There are several reasons why this is so. The basic problem is that the behavioral evidence is invariably imprecise and conflicting. This holds even for key parameters that are subjected to many studies, such as the wage elasticity of

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hours worked or the interest elasticity of consumption. Further, the micro-evidence is often quite narrow-minded; there are thousands of studies on the supply of hours worked, but very few studies on education and career-track selection. Finally, even if one were equipped with good estimates of the crucial micro parameters, these would have to be embedded into an equilibrium model, and any conclusions are bound to be controversial as long as there is no consensus on the appropriate model.

Given the poor achievements of the bottom-up approach, one is tempted to look elsewhere. Taking a top-down perspective by simply comparing macroeconomic performances of high- and low-tax countries seems obviously attractive. Monetarists need not know what is inside the black box that connects the money stock and prices (at least Milton Friedman does not). Similarly one could take a stand on the appropriate tax burden without knowing the exact nature of all disincentive mechanisms that are at work. (But the design of an optimal tax *structure* would have to be based on knowing the relative strength of various channels.) It is very natural that economists armed with good cross-country macro data, which have been available only in the last few years, have recently attacked the problem from this angle. Looking back on a decade of (sometimes) clever econometric studies, I think one can conclude that this approach has come to a dead end without yielding many insights. With the benefit of hindsight, I do not think we should be surprised.

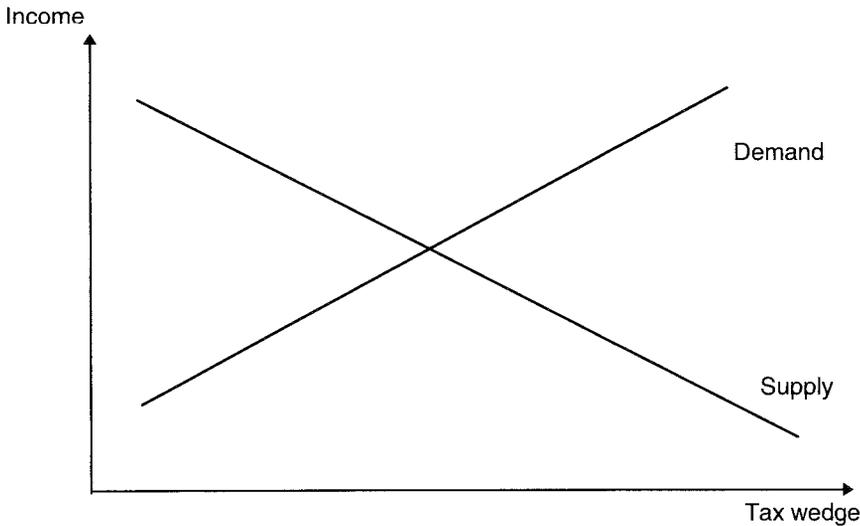
There are at least three fundamental problems with the top-down approach, which should temper the initial enthusiasm that one may otherwise feel:

1. Figure 1 illustrates a fundamental identification problem. We are interested in the negative effect of high taxes on efficiency and, hence, on the level of income (or the growth rate). The negatively sloped *supply curve*, which relates income to the tax wedge that equals the size of public expenditure as a share of income under a proportional tax system, illustrates this hypothesized relation. But real data are generated by the interplay between this and *demand* factors that can be represented by a curve that is drawn upward-sloping on the presumption that the income elasticity of the demand for publicly provided goods and services (including income redistribution) exceeds unity. Short of good instrumental variables, one would not be able to interpret regressions of income (or growth) on tax rates; a failure to find a hypothesized negative

relation could always be ascribed to failing to solve the identification problem.

2. The supply relation is not likely to be linearly decreasing as shown in Figure 1. Parts of public expenditure, such as education, are not pure consumption but are likely to have positive efficiency effects. Further, the disincentive effects of taxation are strongly non-linear, according to most reasonable equilibrium models (see Table 1). Combining these two factors, the slope of the supply relation may be positive at low tax rates but should be increasingly negative at high tax rates. Depending on the exact nature of the non-linearity, any econometric results may depend heavily on the few countries at the top of the tax league (Scandinavia) and bottom (the U.S., Japan, or non-OECD countries, depending on the sample).
3. The tax structure differs a lot among countries and if one wants to peek inside the black box, this may be important to take into account.<sup>1</sup>

**Figure 1. Income and the tax wedge**



<sup>1</sup> Indeed, recent results by Widmalm (1996) suggest that a large fraction of income taxes may be bad for growth. Clearly this result is also potentially sensitive to problems of identification and the choice of functional form.

I conclude that the aggregate cross-country research has come very close to a point of no return. There is still a multitude of regressions that could be run. In some cases they will, no doubt, illuminate interesting patterns, but it seems very unlikely that they will give us any deeper insights into the costs and benefits of a large public sector. With all its problem, I think the odds of getting insights are better with the bottom-up approach. Before turning there, let me comment briefly on the approach initiated by Feldstein (1995), which looks directly at the elasticity of taxable income regarding tax rates not caring whether the elasticities reflect changes in saving, labor supply, the timing and classification of taxable income, or outright tax fraud. In a sense, this type of study is half way between a micro-based structural approach that focuses on behavioral elasticities and a macro approach that looks at aggregate reduced forms. Feldstein's results are challenging, and a corresponding study for Sweden should be high on the research agenda. But given the problems of disentangling retiming effects from deeper behavioral responses, it would be surprising if this line of research would resolve the issues about the cost of high taxes.

This leads us back to the micro-based evidence—disappointing as it may be. Focusing on a key dimension, hours worked, I claim that we “know” at least a few things:

- Compensated after-tax wage elasticities are significantly positive<sup>2</sup>
- The wage elasticities are measured rather imprecisely with the lower end of the confidence bound being close to zero
- The elasticities appear to be heterogeneous across household types, possibly with larger effects among women

In the evaluation of the Swedish 1991 tax reform, reported in Agell et al. (1998), we conclude, based on available studies for Sweden, that the compensated wage elasticity for prime-age men is around 0.1 with a typical confidence interval that ranges from close to zero to 0.2 or beyond. One might have hoped that the natural experiments provided by recent tax reforms should have enabled econometricians to narrow down the uncertainty. Unfortunately this is not so, at least

<sup>2</sup> Given the study by MaCurdy et al. (1990), which indicates non-utility maximizing behavior, this statement may be controversial. But recent work by Eklöf and Sacklén (1997) suggest that the findings by MaCurdy et al. may reflect measurement errors in the wage variable.

judging from the studies for the U.S. TRA86 reform reported by Slemrod. The only corresponding study for Sweden, by Klevmarken (1997), reports quite strong responses, particularly for women, but still estimated with sizable confidence bounds.

Table 1 illustrates the extent of the implied ignorance about the cost of public funds. It draws on Agell et al. (1998, Table 8.1). It shows the marginal excess burden computed within a very simple, static, general-equilibrium model where the only tax wedge is on labor income. It is computed for three different elasticities—a high value (0.25) corresponding to what was believed to be typical a decade or two ago, an intermediate value (0.11) corresponding to our current best guess, and a low value (0.05)—and at three different tax wedges—79% applicable to an average white-collar worker before the tax reform in 1988, 71.5% applicable to the same white-collar worker after the reform in 1991 and finally 62% applicable to the average blue-collar worker in 1991. Post-1991 tax hikes have brought high income groups back close to the 1988 level.

**Table 1. Marginal excess burden per unit of extra tax revenue (%) at different compensated elasticities of hours worked, regarding after-tax wages.**

| Marginal tax wedge (%) | Wage elasticity |     |      |
|------------------------|-----------------|-----|------|
|                        | .05             | .11 | .25  |
| 62                     | 8               | 19  | 55   |
| 72                     | 13              | 34  | 139  |
| 79                     | 22              | 65  | 2280 |

The table emphasizes the strong non-linearity of the excess burden both as a function of the tax wedge and the wage elasticity. The degree of non-linearity is related to the proximity to the top of the Laffer curve, where the excess burden becomes infinite because no further tax revenue can be raised by increasing the tax rate. This non-linearity explains why an American may be less concerned about distortionary taxes than a Swede; at tax wedges around or below 50%, this model yields excess burdens in line with the 20-25% that Slemrod suggested. Looking in the middle column, it may appear that the costs should not be excessive at Swedish tax rates either. Even with a fraction of the tax payers, say 10 or 20%, with tax wedges close to 80%, the average marginal tax wedge for men would not ex-

ceed 40%. This way of reading the table emphasizes the *vertical* non-linearity, that is, the importance of heterogeneity in tax wedges among the tax payers. Perhaps more important is the *horizontal* non-linearity, which may be interpreted in terms of heterogeneity across individuals facing the same tax wedge or in terms of uncertainty about the true elasticity. Tax policy should be directed by the expected excess burden, which may be very different from the excess burden at the point estimates. For a back-of-the-envelope calculation, let us disregard heterogeneity and focus on uncertainty. Assigning a 10% probability to each of the high and low elasticities and 80% to the mid-value 0.11 leads to expected excess burdens of 22, 43, and 282% at the three tax wedges in the table. Put differently, when one assigns a reasonably large probability to being very close to the top of the Laffer curve, there is every reason to be extremely cautious in raising the tax burden further. Arguably this is the case for Sweden but not for the U.S.

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