Comment on Peter Englund, Patric H. Hendershott and Bengt Turner: The Tax Reform and the Housing Market

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The paper gives a comprehensive picture of the problems connected with the tax reform and the housing market. It strikes a sensible balance between institutional detail and analytical contents. Overall, I think the conclusions of the paper are very reasonable, and there is little I would like to add. As a matter of personal preference one could perhaps ask for an analysis of the redistributional issues; they were very much discussed during the reform, which was widely criticized for being anti-egalitarian. Many people asserted, however, that the gains to high-income earners from drastically reducing tax rates at the top would be balanced by a decline in the price of real estate (which is mainly held by high-income earners). Thus the net effects on income distribution would be negligible – or at least not too regressive. One would perhaps have appreciated a thorough and scholarly discussion of this assertion now, when the data are available – but, of course, it is always easy for the discussant to ask for a longer paper.

Apart from the introductory and descriptive parts in Sections 1–2, and the general reasoning and conclusions in Sections 5–6, the paper consists of two distinct parts with rather weak connections. First we have a model in Section 3 of how the tax reform affects housing expenditure *for a given level of housing consumption*. Second, in Section 4 we have quite a different model of how the tax reform affects housing consump-

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[1] In connection with an earlier Swedish tax reform, the redistribution issue has been analyzed in Brownstone, Englund and Persson (1988).

tion *and* housing expenditure. In the next section I will discuss these models in somewhat more detail.

1. The models used

The model in Section 3 of the paper does not have any behavioral assumptions; it mechanically calculates the change in housing costs for a given housing consumption. This is what Mervyn King (1983) labelled the cash gain, usually computed by government bureaucrats. The model in Section 4, on the other hand, is more like an economic model; it is built on the assumption of utility-maximizing households with non-linear budget sets. One could then ask: what was the model in Section 3 good for? Since it does not allow for any adjustment by consumers, it is more restrictive than the one in Section 4 and could thus be dismissed. Or does it have some virtues that are absent in the latter model, for example that it permits a much more detailed representation of the actual tax schedule? This is not evident from the paper, although I suspect it could be the case. But then – to what extent can we trust a model, however detailed, which does not allow for any consumer response to changes in prices and income? Do we, for example, dare to base any policy recommendations on the results of such a model?

In contrast, the second model (that of Section 4) is based on standard microeconomic foundations. But even here questions could, of course, be raised. For example, the "proper" modelling of non-linear budget sets is a controversial field, to say the least.[2] Based on experience and judgement, the authors feel that they have used a reasonably good estimation method, and I think I agree with them – but from a more strictly scientific point of view that question is far from settled. A reader would like to see some sensitivity analysis of how the quantitative and qualitative results are affected by different model specifications. We know from studies of the effects of the tax reform on labor supply that even the qualitative results could vary depending on which state-of-the-art estimation

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[2] Cf. for example the debate on the estimation of labor supply with non-linear budget sets in Hausman (1985) and MaCurdy *et al.* (1990). Note that the way Englund, Hendershott and Turner have chosen to solve the non-linearity problem constitutes a third alternative, quite different from the ones advocated in those papers.

method one chooses, and there is no reason to believe that things are different with housing demand. The choice of estimation method thus has to be made with a great deal of sound judgement.

2. Some remaining issues

The above is not intended as a critique of the general thrust of the paper; it contains a wealth of interesting data and convincing reasoning. I will not delve into the issue of why the authors obtain smaller effects than those I obtained in Persson (1989); here I simply think the results of Englund, Hendershott and Turner are more reliable than my own crude calculations from six years ago. Instead, I will discuss two issues that could be fit into their model and that, to my knowledge, have not been studied in this context before.

First, the microsimulation model used in Section 4 gives us the changes in demand for different sizes of houses; this is reported in Figure 3 of the paper, and we see that the demand for large houses falls while demand for small houses increases. Given the existing stock in different size classes, this disaggregated demand could be used to compute the price changes for different sizes needed to equilibrate demand and supply. The authors take a much more aggregated view and report only the *average* price change; in Section 5.2 they estimate the average price of owner-occupied houses to fall by approximately 12 per cent.

With the high degree of disaggregation permitted by the model, it would have been quite possible to compute price changes for different house sizes. One would, for example, expect the prices of small houses to increase, while prices of large houses might fall by much more than 12 per cent. Such a disaggregation is interesting because the predictions of the model could be compared to actual data – and such a comparison would give us more information about the reliability of the model.

Second, houses do not differ with respect to size only. In the discussion of supply responses in Section 5, the authors assume that the housing stock will increase as soon as Tobin's q is above unity and decrease as $q < 1$. Now, some houses are expensive not because they are large, but because of location factors (land rents). The analysis in the paper applies to marginal houses, for which the price is exclusively determined by construction costs. For centrally located houses, where the main component of the price is determined by land rents, while construction costs repre-

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sent only a minor fraction of the value, supply responses to price changes will be small or non-existent; however high the price, there simply cannot be any production of new houses at some unique locations.

If we use age as a proxy for location (new houses are built on marginal land, with no land rents, while old houses are centrally located, with high land rents), this has a testable implication: The fall in prices, as a result of the tax reform, will be greater for old houses than for new. This is so because the supply response of the Poterba (1984) model is not as prominent for old (centrally located) houses as for new (marginal) ones.

I think a careful disaggregated study of the actual data on house prices could be warranted at this point. An investigation of actual price changes along these two dimensions – size and age – might yield substantial empirical support for the model.

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