Energy, expenditure and consumption aspects of rebound part I

This paper sets out a framework for analysing rebound effects, disaggregating them into the original energy saving with no change in behaviour, the emplacement effect from embodied energy in a different technology, substitution effects from greater consumption of a cheaper energy service, income effects from a higher real income (given the lower "price" of energy services) and macroeconomic effects. It has been split into two parts, giving a better length compared to the original paper that I refereed last year.

The paper's main claims are that it advances on the existing literature by including the effect of changes in the capital cost, embodied energy and maintenance /disposal cost of the energy-using device; that it includes macro-economic effects (while accepting that work remains to be done in this area); that the framework can cope with non-marginal energy price changes; and that it includes empirical applications. (The paper also has a detailed model of consumer behaviour allowing the identification of income and substitution effects, but it says these are already in the literature.)

I wonder whether there needs to be a discussion of where the boundaries of what we call "rebound" should be drawn? Without checking the literature, but aware that Steve Sorrell (who is not this referee) did a systematic review for UKERC some years ago (which I read at the time), I'd think of it as "the reduction in energy savings, compared to engineering estimates, due to behavioural changes", and might add a component for the difference between "factory-measured" and "as imperfectly fitted" savings (with unchanged behaviour). Embodied energy is certainly important (and a big part of the life cycle literature), but is it "rebound"? The definition just given(with the caveat that preceded it!) would suggest not. Should maintenance energy be included when calculating the "engineering estimate"? That certainly doesn't mean it is! The authors also convert the stock variable of initial embodied energy into a flow by simply dividing it by the device lifetime – it's much more usual in economics to use some kind of discounting, certainly when it comes to costs appearing in budget constraints.

I think the macro effects, on the other hand, probably do fall into the "rebound" category, and are well within the spirit of Jevons' original remarks, although he long preceded Keynesian multipliers! However, rather than using multipliers alone, I think the authors need to think about the effects of reducing the demand for energy on its price in a world where supply is not perfectly elastic. That effect is likely to be much more direct, and hence easier to measure, than the indirect and longer-term effects of greater energy productivity or real-terms consumer spending. A partial equilibrium model of the energy sector ought to be sufficient to capture the size of such price changes, which can then be fed into the rest of the analysis.

I'm still slightly sceptical about the importance of explicitly coping with non-marginal price changes. There's something about this at the very end of the online appendix to part II, but I'd advise putting a sentence into the paper itself that says how important this is, in terms of how much it changes the rebound effect, relative to the 0-100% scale. Even without the numerical estimates that are

coming in part II, you can look forward to the result when you have the discussion of the contribution you're making. While the authors point out that other micro models of consumer behaviour are available, the one they've chosen has the serious weakness that the income elasticity of demand is inevitably equal to one. We know that a lot of energy services have an income elasticity of less than one, so this matters. There's an open question of whether getting the income elasticity or the substitution effects wrong is going to be a bigger problem. I'm not suggesting that the authors add the fully worked-out answer to this paper, but it needs to be acknowledged – and there could (should) be some basic calculations in part II.

My overall conclusion is that the paper does make a contribution, but I think it's less important than the authors claim.

I think my only "specific" point is that the budget constraint (eq. 4) would normally be written in terms of the general quantities of energy, other consumption, and so on, rather than solely in terms of the starting point (o superscript variables) – as written, it doesn't say anything about whether the other expenditure combinations have to add up to anything.