Earth Syst. Dynam. Discuss., 2, C76–C78, 2011
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Earth System Dynamics Discussions

Interactive comment on "No way out? The double-bind in seeking global prosperity along with mitigated climate change" *by* T. J. Garrett

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Received and published: 19 April 2011

In response to the reviewer's comments, I have several points. First, contrary to what the reviewer states, the review on rebound effects by Sorrell (2007) is in fact referenced within this paper. I did not go into depth on the content of the review because my paper is not explicitly an examination of rebound and backfire. Also, the empirical studies that were referenced in the review by Sorrell considered rebound effects for explicit economic sectors, whereas here, the work focuses on physical flows through the global economy as a whole. The statement I made that "increases in energy efficiency lead to a higher rate of return and accelerated growth of the consumption of primary energy supplies", is one based on a fundamental thermodynamic analysis that is tested empirically using available statistics for global scale flows. The statement follows as a

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direct consequence of the observation that measured values of $\lambda = a/\int_0^t P dt'$ are very nearly a constant.

While both Scher and Koomey (2010) and Cullenward et al. (2011) did provide criticisms of my earlier paper (Garrett, 2011a), I didn't reference them because they both misrepresented my prior article at a very fundamental level. In any case, neither article appears to have been peer-reviewed. The editorial board at *Climatic Change* accepted both articles on the same day they were received. That said, a separate article I have written does address these two critiques (Garrett, 2011b), and is currently available on arXiv. As stated in the arXiv article:

"Cullenward et al. (2011) and Scher and Koomey (2011) argued that there cannot be a constant relationship between energy consumption rates a and wealth C because the relationship between a and P is highly dynamic, both temporally and between sectors/nations. This misrepresents the arguments in Garrett (2011) because the discussion was explicitly referenced, not to nations or economic sectors, but to civilization as a whole. More importantly, (the derived constant) does not apply to P/a, but rather to the integral quantity $C/a = \int_0^t P dt'/a$. Certainly, there has been past discussion among economists that there exists a strong correlation between rates of energy consumption and economic production at the national level. However, P and $\int_0^t P dt'$ are not at all the same thing, and they have no obvious relationship to one another. They might be statistically correlated, but only if P is growing exponentially. "

As a final remark, the reviewer argues that some IAMs do in fact represent physical flows, contrary to what I state in the introduction. This sounds great, but I am unsure which ones these are, as none are referenced. The traditional economic models that are normally employed in IAM studies use production functions that are a function only of labor and capital. If there are IAMs that do explicitly represent thermodynamic flows of matter down gradients in potential energy, then hopefully these models are subjected to the same tests that all physical models are. Specifically, they would need to be consistent with the first and second laws of thermodynamics. Second, they would need

ESDD

2, C76–C78, 2011

to provide falsifiable hypotheses that can then be subjected to empirical validation.

It is difficult to place my article in the context of prior studies when they don't appeal to these two basic principles, as I have tried to do. I hope the reviewer might reconsider this manuscript on the basis of the arguments it presents.

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