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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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In discussing the paper presented by Garrett it is necessary to distinguish between the theoretical argument and its empirical implimentation. Seen together with the 2011 Climatic Change paper, the new and the previous paper are strong in theory but partially flawed in the empirics, especially regarding the economic part. In spite of the flaws, I think that one should not throw the thermodynamic baby out of the bathwater. Garrett presents an important idea, namely that economic systems can be seen as thermodynamic feed back engines which leverage their endogenous potential in harnessing energy for further expansion of this potential. He claims that this process involves some physical constraints and structural constants which allow to base long-run forecasts on a set of simple hypotheses and parameters. In doing this, I think that he leaves out two

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important additional theoretical arguments to support his theory. The first is that he does not extend his reference to thermodynamics towards the inclusion of the recent Maximum Entropy approaches. The second is that he does not pay sufficient attention to the field of ecological economics in which the issue of relating thermodynamics and economics has attracted considerable interest since Georgescu-Roegen published his seminal research in the 1970s. Regarding the first aspect, the MaxEnt approaches can give a more general rationale for Garrett's approach, because they explicitly argue that forcasting the behavior of complex systems can build on the hypothesis that given certain constraints, systems will approach the most probable state. Physically, the forces driving the system towards this state are covered by the laws of thernodynamics. Against this background, Garrett's model decribes the physical mechanism how the economic system contributes to the realization of the maximum entropy principles (for a related approach that would possibly establish the microfoundations, see Annila and Salthe 2009). I think that this can provide a more coherent justification for his argument. In that context, I wonder why Garrett leaves out references to potentially relevant other approaches to the literature, especially Odum's (2007) work, who over decades analyzed the role of maximum power principles in understanding the interaction between economic and physical aspects in general ecology (as Kleidon 2009, 2011 has argued, maximum power and maximum entropy relate closely). I mention this because Odum has developed an important distinction which seems directly relevant to Garrett's analysis of the relation between wealth and thermodynamic potential, which is the notion of embodied energy. If embodied energy is considered in both the flow analysis as well as in the analysis of the infrastructure of human civilisation, it is evident that empirical observations on changing relations between current energy flows and current GDP are not directly relevant for Garrett's approach (which is a major point made by his critics, see Cullenward et al. 2011), because the standard measurements of energy do not cover all relevant physical phenomena. The second additional pillar of support can be the work by Ayres and Warr (2003, 2005, 2009), who have also presented long-run analyses of the relation between exergy and economic growth. I think

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that it is worthwhile to consider their distinction between exergy and useful work, and also their specific approach to the production function, which enables them to show that useful work and the thermodynamic efficiency in transforming exergy into useful work are the drivers of growth and can explain away the Solow residual which is interpreted as technological progress in standard economic models of growth. This model is also very powerful in tracking real economic data over the long run. So, Garrett would be well advised to integrate these results into his argument. In particular, and related to my previous point, Ayres and Warr also consider the exergy equivalent of mass flows. This comprehensive input measure closely tracks GDP through time. Against the background of these general considerations, I think that the single most important empirical observation in support of Garrett's approach is the rebound effect. Unfortunately, the discussion between his reviewers and Garrett does not really go into the details here. The so far authorative report by the UKERC (2007), with Sorrell as a lead expert, is very careful and comprehensive and allows to make a number of points that actually support Garrett, in spite of the opposing views of his critics. First, we can say that no partial result on lower rebound effects in particular industries and technologies are relevant to Garrett, because he refers to civilisation in its entirety. Second, the report makes the clear point that we do not have much reliable data about rebound effects on the level of the global economy, but that those effects can be very strong, if one considers the role of catch-up processes in developing economies and the impact of general-purpose technologies which affect both productivity and consumption. Third, although the evidence in support of the Khazzoom-Brookes postulate is relatively weak, this is also true for counter-evidence, which, at the moment, means that the theoretical arguments in favour of this postulate remain strong. In this context, the UKERC report points out that the Ayres and Warr research is highly relevant and shows that ecological economics can offer fresh perspectives on the issue, but is neglected so far. Indeed, Ayres and Warr argue that the rebound effect is nothing but a manifestation of the feedback mechanism that drives economic growth. Then we can also conclude that the Garrett model refers to the same mechanism, but in purely physical terms. There-

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fore, I think that the very rich empirical data presented by Ayres and Warr could be of great value to Garrett. This leads me to the drawbacks of Garrett's approach. Unfortunately, Garrett presents several flawed economic arguments which are also pointed out by his critics. I think that a major revision is absolutely necessary in that respect. First, Garrett confuses stocks and flows. Wealth is a stock, GDP is a flow. In the appendix of his 2011 Climatic Change paper, he presents a discussion of the economics which reveals the difficulties. He argues that the distinction between nominal and real values is the same as depreciation, which is simply wrong. Inflation is a purely monetary phenomenon and should not play any role in a physical argument. Depreciation is a flow that relates with a stock, namely the capital stock. Accumulated values of flows cannot be stocks, unless there is no consumption at all, which is economically meaningless. Second, in confusing stocks and flows, Garrett loses a big opportunity, which is to relate the physical concept of thermodynamic potential with capital. Capital can be treated by accumulated net investments through time (Ayres and Warr have a discussion on that), and Maddison also has data on that. So Garrett should revise his approach in considering the capital stock (which could also include human capital, hence population data). Third, nonetheless one can think of alternative measures of wealth which might involve GDP. One simple idea is in that standard growth models GDP per capita is directly determined by the capital endowment per capita, so that one could think of the former as an in direct proxy of wealth in the sense of Garrett, taken as the instantaneous value. Fourth, Garrett makes a very strong claim about the λ. I think that he should search for possibly related constants in the established theory of economic growth, over the long run. He will not need to search for long, as there are several interesting observations, such as the stability of the real interest rate, the near to constancy of the capital-output ratio or the long- run stability of the rate of technological progress (which is the rate of the growth of total factor productivity). It is evident that these values closely relate to his model, so that the assumption of constancy might not appear as far-fetched as it seems on first sight. To sum up, I think that Garrett's fundamental point is correct. In the spirit of the Maximum Entropy approach,

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it is possible to simplify forecasting models by means of concentrating on the long run evolution of the physical constraints under which economic systems operate. However, his economics is flawed and should be reviewed in the light of recent contributions to ecological economics which also focus on the thermodynamics.

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