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2, C164–C174, 2011

Interactive Comment

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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I thank the referee for the thoughtful review. My principal response is that the "thermodynamic baby" is inseparable from the economics I describe, provided that λ is constant, which I believe I show. The constant λ ties the economics to the physics by relating economic value to a rate of consumption of potential energy.

Below I have highlighted in bold the primary referee comments, with my responses in plain text and any associated modifications that will be made to the text included in italics.

...he does not extend his reference to thermodynamics towards the inclusion of the recent Maximum Entropy approaches...Regarding the first aspect, the Max-

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Interactive Discussion

Discussion Paper

Ent 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.

I am aware of theoretical development of maximum entropy arguments. I believe they could be useful eventually to take the modeling approach I present to a higher level of sophistication. Indeed, I am currently supported by the Ewing Marion Kauffman foundation in the United States to develop a more sophisticated expression of how non-equilibrium thermodynamic constraints might lead to improved forecasts of economic innovation and growth.

However, this is not at all an easy problem, and I feel it is beyond the scope of this paper. Implementation of MaxEnt arguments might be fairly straightforward if there were a fixed pressure gradient between civilization and primary energy reservoirs. Unfortunately, the reservoirs we access are not fixed. They are both depleted and discovered in response to civilization growth. While I am currently making some progress in describing the mathematics of civilization's response to energy reservoir depletion and discovery, the problem is really rather subtle, and I feel it is premature to include it in the article I have submitted here.

The primary result that I build on here is that there is a fixed relationship between wealth and primary energy consumption. There is quite a lot to be done based on this result alone, without extending sophistication further to include something like MaxEnt principles. A more fully developed non-equilibrium thermodynamics will hopefully come in a future submission.

ESDD

2, C164–C174, 2011

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 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.

I am well aware of the studies by Odum and by Ayres and Warr. I appreciate the referee's view that there are others who have looked into energy economics from a more thermodynamic perspective, and I will add reference to their work in the article's introduction.

The difficulty here is that, for a variety of reasons, these studies don't actually map all that well onto what I present here, and so my fear is that making an extensive comparison would come across as tedious without making my argument any clearer.

There are two primary distinctions between what these prior authors have done and what I do. First I consider the human system only at global scales with absolutely no internal resolution of people, their activities, or their possessions. Second, I treat global economic value as being a representation of either a thermodynamic potential or a flow down a potential gradient, the two being related through a constant.

As an atmospheric scientist, my goal is to present a study that can facilitate the link between human systems and atmospheric concentrations of CO_2, where CO_2 is a gas that is well-mixed globally over timescales of interest to civilization growth. There are a variety of points where I feel that the approaches taken by Odum and Ayres and Warr are insightful, but not particularly well suited for these goals. It may be worthwhile to draw a thorough comparison between our various interpretations in a separate article, but here, while I appreciate the referees concerns, I feel the central arguments I am trying to make need to rest solely on the merits of the thermodynamic analysis and empirical validation that is presented. If my approach were purely an incremental development of these authors' prior work, then a more thorough discussion of their achievements would be well-suited. But, as far as I can tell, what is presented here differs quite substantially.

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

ESDD
2, C164–C174, 2011

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.

My article really isn't intended to be on the rebound effect and backfire. It's an important topic, and I could spin the core arguments of my work in that way, but that would need to be a separate article. That said, I do provide arguments which show why backfire must be implicit at global scales, provided that it is indeed true that there exists a constant $\lambda = a/\int_0^t P dt'$, where P is the inflation-adjusted GDP and a is the rate of primary energy consumption, both expressed at global scales. The evidence for λ being constant is presented here in Table 1, and in greater detail in Garrett (2011). If λ is constant, then it follows that

$$\lambda = \frac{da/dt}{P} = const.$$

Thus, for any given value of a, if the energy productivity P/a increases, then $da/dt = \lambda P$ must increase also, since λ is constant. Increased energy productivity leads to increased energy consumption.

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 perspec-

ESDD
2, C164–C174, 2011

Interactive Comment

tives 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. Therefore, 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.

It may be basically correct to ague that Ayres and Warr are arriving at similar conclusions for somewhat similar reasons. However, unlike the work presented here, the basis for their approach focuses on the US alone (Ayres et al., 2003), and still requires tuning of exponents to match the observed data. Tuning is not even possible under the approach I introduce, which applies solely to global scale flows. Perhaps what is more applicable is work by Saunders (1992), which has also shown how backfire should be expected as a consequence of energy efficiency. Nonetheless, even then, there are some rather big differences between the approach I take and those who start with more traditionally-based economic production functions. These latter approaches internally resolve people (or labor) as separate from capital infrastructure. I treat civilization as a whole, without any distinction between the two. This is done to make the problem simple, straightforward, and most importantly, falsifiable.

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.

I realize that treating wealth as a stock rather than a flow is the traditional perspective, but perhaps the referee could elaborate on what he means by a "stock", and why he feels wealth must be treated as a stock? Is this a falsifiable hypothesis that appeals to physical laws and has been rigorously validated with empirical observations?

The reason I treat economic wealth, or value, as a flow is because nothing in the universe is perceivable in the absence of there being a flow to measure. If something were merely a stock, with no pressure gradient between itself and its surroundings, it would be invisible to an outside observer, and therefore be impossible to quantify. For us to perceive the value of something, it *must* be associated with a flow of some sort, requiring some gradient in pressure (or equivalently potential energy density). For example, an object that is perceived by many people may have economic value. But the exact same object lost to all human recognition is worthless. Gold buried unknown in the desert is worthless. It only has value at the point of observation and discovery. Sustaining observers, the process of discovery, and communication of the discovery, requires energetic flows. This is what gives gold the value it lacked previously.

I simply made the hypothesis that, at global scales, the relevant flow to compare economic value to is the flow of primary energy consumption. More details on the thermodynamic reasoning for why this hypothesis was made are in the Section 2 of this article, but what is most important is that *this is a falsifiable hypothesis*. This is really important, because we can all make assertions. That's easy. Making testable assertions is harder. And the only way I know whether or not these assertions are true is to actually test them. This is what I did, and as shown in Table 1 and in Garrett (2011), the assertion I make is one that is supported by available data.

All economic arguments presented in the paper follow directly from this core result. It is for this reason that I don't feel they should be dismissed as being flawed simply because they run counter to traditional economic thinking. Consensus is not normally the standard by which we judge scientific truth. Falsifiability is.

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.

The referee is describing how depreciation, consumption and inflation are treated in traditional economic growth models. The model presented here is different. Everything that is described rests on the falsifiable hypothesis that current rates of global energy consumption are linked to the time integral of inflation-adjusted global GDP through a constant. This hypothesis is shown to hold. If the falsifiable hypothesis ever ceases to hold, then what is presented in the paper is indeed wrong. But while the falsifiable hypothesis holds, I think it should be considered on its own merits, rather than its consistency with prior models.

As a point of discussion, could the reviewer state why he is sure that inflation is purely a monetary phenomenon? What exactly does this mean? Everything is part of the physical universe, monetary phenomena (and central banks) included. If inflation has no role in a physical argument, why would Sarel (1996) start an article by stating "It is now widely accepted that inflation has a negative effect on economic growth". And if inflation is purely monetary, why does it occur in response to natural disasters, or resource scarcity (as argued by the current chairman of the US Fed (Bernanke et al., 1997)), or global warming (as shown in its effects on crop prices (Lobell et al., 2011))? As Bernanke et al. (1997) argued, monetary policy can itself be viewed as a response to external non-monetary forces. If inflation is purely a monetary phenomenon, how does inflation exist at global scales when there is no central bank that is external to the planet?

The description of inflation I present here represents it as the ratio of the rate of depreciation of wealth (or the capacity to consume energy) to the rate of production of wealth. As discussed in this article, this leads to increases in inflation during times of accelerated scarcity or decay. Isn't this consistent with observations?

Accumulated values of flows cannot be stocks, unless there is no consumption at all, which is economically meaningless.

In physics, all stocks of internal energy, evaluated along surfaces of constant potential, are the consequence of a time integral of flows. The stocks grow and decay as a

ESDD
2, C164–C174, 2011
Interactive Comment

consequence of flow convergence and divergence due to the sum of flows in and out of the surface of constant potential. Such flows exist only as a consequence of pressure gradients between stocks of internal energy. All this is necessitated by the second law. What this means is that all stocks must be an accumulation of flows, as stocks cannot materialize from nowhere. This is what is described in the model here, and is illustrated in Figure 1. If economic systems are part of the physical universe and obey physical laws, the same principles must apply.

Second, in confusing stocks and flows, Garrett loses a big opportunity, which is to relate the physical concept of thermodynamic potential with capital.

Relating capital to a thermodynamic potential is the foundation of the model presented here. However, I realize now that this is stated more explicitly in Garrett (2011) than here. Currently the text reads

"Taking λ to be a constant, it follows from Eqs. (1), (3) and (5) that the "rate of return" for economic growth applies equally to wealth C, energy consumption rates a and the size of the interface driving flows ΔG":

This will be rewritten to read

Taking λ to be a constant, it follows from Eqs. (1), (3) and (5) that wealth C, rates of potential energy consumption a, and the size of the interface (or thermodynamic potential that drives flows) ΔG are all proportional. In this case, the "rate of return" η for economic growth applies equally to all three:

...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.

I am sorry, but I don't quite understand what is being asked. In any case, it is a very

ESDD

2, C164–C174, 2011

deliberate choice to consider civilization as a whole, and not to explicitly resolve people. If different elements of civilization are explicitly distinguished then the global problem becomes much harder. The global problem is what is most problem relevant to CO_2 emission trajectories and it is most simply treated by treating civilization as a whole.

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.

Yes, I am making a very strong claim about λ being constant. This is *the* central result on which the foundation of this work rests. λ is what links economic quantities to physical quantities, so if it's constancy is true, as I believe I show, then this is of fundamental importance for simplifying the economic problem.

There may be other quantities that are reasonably stable. I don't know about the stability of the real interest rate at global scales, but over multi-decadal timescales, the global wealth to GDP ratio and the rate of technological progress are not constant. In my model at least, these last two quantities are effectively equivalent to one another, since $\eta = P/C$, and η is related to a thermodynamic efficiency through a constant α. The value of η evolves slowly to be sure, but that is not the same as being constant. To see a multi-century statistics for η, please see Figure 3 in Garrett (2011).

ESDD

2, C164–C174, 2011

Interactive Comment

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Discussion Paper

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