Earth Syst. Dynam. Discuss., 2, C128–C134, 2011
www.earth-syst-dynam-discuss.net/2/C128/2011/

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

T. Garrett

tim.garrett@utah.edu

Received and published: 13 May 2011

My responses to the reviewer are enumerated below. Reviewer comments are in italics.

1. *For example some IAMs do represent physical flows.*

   It is possible that the reviewer is thinking of physical flows in a different way than I do, and this is a point that can be clarified. In my mind physical flows are those that involve material fluxes down pressure gradients (or equivalently gradients in the density of available potential energy). This is the basis for everything that happens in the universe, and indeed climate models work on this basis to the best extent that they can. Of course all IAMs include consumption of energy, which might be thought of as a physical flow. Nonetheless, the important thing here is

   that physical flows should be represented as a response to pressure (or potential energy) gradients for representations of civilization and the earth system to be self-consistent. In the revision, the text will be rewritten as:

   Modern IAMs are based on neo-classical economic models that, unlike EaSMs, do not explicitly represent physical flows as a material flux down gradients in potential energy.

2. *Several statements (e.g., about the "capacity of hurricanes to cause damage") are not supported by citations or evidence.*

   In the sentence prior to the one highlighted is the relevant reference to the detailed work by Pielke Jr et al. (2008) on this subject "Rather, damages have increased because economic wealth has become increasingly concentrated at the coasts (Pielke Jr et al., 2008)". This is a nice, clearly written article that I found quite convincing.

3. *The exposition is wordy and imprecise: units are missing, it is unclear who are the subjects in statements such as "what we normally term", what precisely is meant by an "economic signal that can be meaningfully distinguished from noise", etc.*

   I double-checked the manuscript, and I am actually quite explicit about the units throughout. Is there a particular place where mentioning the units again would be helpful? I will change the text stating **"Thus, what we normally term as "economic growth""** to "**Thus, what is normally termed as "economic growth"".** The sentence was only intended to be helpful to a reader who, I believe, would normally think of economic growth as an increase in GDP. As for "Figures 2 and indicate that there is, as yet, no global warming signal in γ that produces an economic signal that can be meaningfully distinguished from the noise". Is this not clear from the Figures? Perhaps the sentence could be written more skillfully to read "**Figures 2 and 3 show no clear trends in the decay coefficient γ that**

can easily be attributed to accelerating climate change. Up until this point, the dominant signature remains interannual variability in γ.

4. *The ms discusses a hypothesis, but does not provide a careful test of this hypothesis.*

   There is only one key hypothesis on which this paper rests. If this hypothesis is wrong, then so is pretty much everything else in my article. This hypothesis is that there exists a constant λ defined by Eq. 8, i.e.

   $$\lambda = a / \int_0^t P\left(t'\right) dt'$$

   where a is the global rate of primary energy consumption and P is the global GDP, adjusted for inflation and expressed in MER units. The hypothesis wasn't developed after the fact in a fishing expedition, but arose from thinking about how the economic system might be represented thermodynamically. I discuss the original reasoning on pages 318 to 321. The test of the hypothesis is described on p. 322, and illustrated in Table 1, which summarizes a more detailed exposition described in Garrett (2011). What was found was that, allowing for some uncertainty in the observations, available data does indeed seem to support the fact that λ is a constant. I only show select years in Table 1. The rest are illustrated in Garrett (2011). Regardless, year after year, the value of λ does not deviate much from the calculated mean of 9.7 milliwatts per inflation adjusted 1990 dollar, over a time period when global GDP has more than tripled and energy consumption has more than doubled. The largest deviation in the full time series is for 1970, which has a value 8.8 milliwatts per inflation adjusted 1990 dollar, and is one of the select years I show in the Table. Even here the deviation is only 10%. This is the oldest data point, so I suspect that there may have been an underestimation of total energy consumption due to contributions from poorly quantified biomass burning. Even with the deviation, the standard error in the mean for the 39 year

time series is just 0.3 milliwatts per 1990 dollar. It is this apparent constancy in λ that enables the economics to be tied to the physics, and is the basis for the prognostic model that is developed in this paper.

5. *The ms bases very long-term projections on a much shorter calibration period. What are the effects of this out-of-range forecast?*

   All forecasts, regardless of discipline, are "out of range", and inherently uncertain. It is impossible to make forecasts based on anything other than our prior experience with the past. For example, the range of future scenarios that are illustrated here might be hopelessly off if civilization is suddenly ravaged by some unexpected disease, Earth is struck by a giant asteroid, or we uncover some miraculous new source of energy. Essentially the future is unknowable. What we *can* do is perform hindcasts based on what we do know now. These can be used to make sure our models are at least able to reproduce the past with some faithfulness, and with as little reliance on tuning as possible. With fingers crossed, this gives us some confidence about what the models can say about the future. It is this approach that I have taken, and is illustrated in Figure 5. In any case, what the article describes isn't so much forecasts, but rather a physically-constrained range of scenarios, depending on how sensitive civilization is to climate change. No probability is assigned to any of described trajectories. What is useful about these trajectories is that they are physically based, and do not allow for the global economy to become magically decoupled from energy consumption. The range of scenarios informs the article's conclusion that we are in a double-bind with no obvious way out.

6. *The ms claims that the model can "make accurate multi-decadal forecasts for growth of the global economy and atmospheric composition". How is this evaluated formally using standard methods of forecast evaluation?*

   As described above, the standard way to evaluate the accuracy of a forecast model is to do hindcasts. The hindcast of the model described here is illustrated in Fig. 5 and discussed on p. 332 and 333. Effectively the model is initialized with current conditions in 1985 and a prior trend in the coefficient of nominal growth β, and the hindcast made for the 23 year period between 1985 and 2008. What I show is that the model "faithfully reproduces both the timing and magnitude of observed changes in atmospheric CO2 concentrations and global economic production P. The implication is that, even though the model that is used is extremely simple, it is nonetheless able to make accurate multi-decadal forecasts for growth of the global economy and atmospheric composition."

7. *The ms makes very strong conclusion about the real world (e.g., the last sentence of the paragraph) based on a very simple (and some would argue very poor) model. What would be the effects of considering an alternative model? How could one test which model provides more skillful forecasts?*

   In Figures 6 and 7 I do show a comparison with some of the more commonly referenced SRES scenarios. The model I describe is in a very different region of a parameter space of GDP and CO2 concentrations than all of the SRES scenarios. I would consider the region of parameter space occupied by the SRES scenarios to be based on an unphysical decoupling of the economy from energy consumption. As I state in the conclusions "Foremost, it looks as if the SRES scenarios make unphysical underestimates of the amount of energy consumption and CO2 emissions that is required to sustain prosperity growth". The true test of which is more right will be to wait and see. But in the meantime, the model I introduce here is evaluated and tested based on prior data and hindcasts. For a discussion of the validity of hindcasts from the SRES scenarios, please see Manning et al. (2010). Thus far, most of the illustrative scenarios have done rather poorly in reproducing recent rates of CO2 emissions, but not all.

8. *Why take only two estimates of ocean and land carbon uptake?*

   I'm not quite sure what the reviewer is asking. The sink rates taken from Le Quéré et al. (2003) are based on a broad range of estimates using a variety of modelling and measurement approaches. See their Table 1.

9. *How are the uncertainties calculated and displayed in Fig. 5?*

   As described in the text "The hindcast is initialized in 1985 and, based on results shown in Fig. 2, it is assumed that $d\gamma/dt = 0$ and that $d\beta/dt$ evolves on a linear trajectory that is consistent with what is observed for the period between 1970 and 1984. A linear fit for $d\beta/dt$ is 0.017% yr^{-1} per year with a 95% confidence limit of $\pm$0.01% yr^{-1} per year. A second source of uncertainty is associated with the CO2 sink coefficient, which is estimated to have a value of 1.55$\pm$0.75 % yr^{-1} (Appendix A)."

10. *Why show only a subset of the SRES scenarios in Fig 6?*

    The number of SRES scenarios is very large. I chose a few of the most relevant and commonly referenced "illustrative marker scenarios" for comparison with the CThERM model in Figures 6 and 7. In Figure 6, CThERM scenarios that do not include decarbonization are compared with SRES scenarios that also are heavily fossil fuel intensive through the remainder of the century. In Figure 7, decarbonization plays a strong role in both approaches.

References

Garrett, T. J.: Are there basic physical constraints on future anthropogenic emissions of carbon dioxide?, Clim. Change, 3, 437–455, 2011.

Le Quéré, C., Aumont, O., Bopp, L., Bousquet, P., Ciais, P., Francey, R., Heimann, M., Keeling, C. D., Keeling, R. F., Kheshgi, H., Peylin, P., Piper, S. C., Prentice, I. C., and Rayner, P. J.: Two decades of ocean CO_2 sink and variability, Tellus B, 55, 649–656, 2003.

Manning, M. R., Edmonds, J., Emori, S., Grubler, A., Hibbard, K., Joos, F., Kainuma, M., Keeling, R. F., Kram, T., Manning, A. C., Meinshausen, M., Moss, R., Nakicenovic, N., Riahi, K., Rose, S. K., Smith, S., Swart, R., and van Vuuren, D. P.: Misrepresentation of the IPCC CO_2 emission scenarios, Nature Geoscience, 3, 376–377, 2010.

Pielke Jr, R., Wigley, T., and Green, C.: Dangerous assumptions, Nature, 452, 531–532, 2008.

Interactive comment on Earth Syst. Dynam. Discuss., 2, 315, 2011.