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Interactive comment on "Dynamics of global atmospheric CO_2 concentration from 1850 to 2010: a linear approximation" *by* W. Wang and R. Nemani

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Additional comment 1.

Having reviewed the paper Wang and Nemani (denoted W&N2104, and denoted W&N in my review) I have now read their response (denoted W&N2104a) and see no reason to change the substantive conclusion of my report (denoted Enting2014) that the paper is not suitable for publication in *Biogeosciences*.

Given the constraints on the length of comments in the Copernicus system (with trail and error being apparently the only way to test the length) and the desire to provide a review promptly, my response may have been less than an ideal length. To the extent to which I am able to make additional comments, each will be largely confined to a

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Interactive Discussion

Discussion Paper

single topic.

When considering approaches to modelling, one needs to recall George Box: "all models are wrong. some are useful", with the understanding that 'useful' is context-dependent and that the context will change with time. My assertion is that the two-box carbon model is no longer useful for carbon cycle research (and I have some degree of scepticism as to how useful it would be for teaching). Neither (W&N2104) nor (W&N2104a) convince me that they have found a new use.

Their argument (W&N2104a) that 'it must be new because Ian Enting doesn't understand it' seems like an exercise in desparation. While the arguments in (W&N2104) are at times poorly expressed, I feel that if have obtained an adequate understanding of the issues by reading the work of others who have done it better over the last 40 years or so (not to mention working on these things myself – e.g. the identification of simple linear system models of CO_2 that can be used in the Kalman filter (Trudinger et al., 2002a,b)).

One way of thinking about model simplcitiy vs. complexity is to rank models in a spectrum (Enting, 1987) from descriptive through to mechanistic with an intermediate stage being what are sometimes called phenomenological models. (think of the sequence from Brahe to Kepler to Newton).

One can build up a sequence of models for CO_2 which have (or have had in the past) various uses:

- Statistical model as concentrations = linear trend plus cycle plus noise — useful (in the past) for establishing the statistical significance of trends. Various refinements such as investigating an ENSO signal (Bacastow, 1976) can be added. An analysis of CO_2 using standard techniques from time series modelling is given by Surendran and Mulholland (1987).
- Concentration modelled as concentration plus exponential with known growth

rate. This sort of modelling was used by myself and others in the early to mid 1980s when there were no ice-core data to establish the pre-industrial concentration which was then highly uncertain (e.g. Schneider, 1983) (and when early ice core data indicated values around 250ppm due to losses that occured when extracting air by melting the ice rather than crushing it).

- Explicitly relate concentrations to emissions by assuming a constant airborne fraction. This approximation was extensively analysed by Laurmann and Spreiter (1983).
- Generalise this to relate emissions to concentrations via a linear response relation (Oeschger and Heimann, 1983).
- Build a box model that represented carbon reserviors explicitly and allowed the comparison of multiple species (especially carbon isotopes). A comparison of such models was gived by KILLOUGH and EMANUEL (1981).
- A model that derived ocean tracer transport from and ocean GCM (e.g. Maier-Reimer, 1993).
- Coupled carbon-climate models as in CMIP5.

One characteristic of such a heirarchy is that going towards the more mechanistic end of the spectrum tends to increase the domain of validity of the model. Thus in the multi-model study of CO_2 projections (Enting et al., 1994), two highly simplified models (model C and model Z) were judged as not having an adequate domain of validity for inclusion in the CO_2 chapter of the IPCC Radiative forcing report. In particular, model Z was developed to represent the longer timescales of glacial-interglacial changes. My view is that when (W&N2104) progress from illustrating what is well-known through to actual analysis of the carbon cycle then their model is too simple to be useful.

BGD

11, C6933–C6938, 2014

Interactive Comment

One example of explicit exploration of model simplification in terms of a small number of boxes is the work by Sundquist (1985). Here the small number of boxes is acheived by contraction from notionally a large number of boxes, by concentration on distinct timescales. At each stage, all the modes associated with faster timescales are taken as having achieved equilibrium (and thus represented as a well-mixed reservoir) and all the slower timescale modes are taken as defining fixed forcing.

A example of simplification (more closely related to W&N2014) in terms of linear models is the use of time series analysis methods to provide systematic estimation of the parameters of a linear representation of the carbon cycle by Young et al. (1996). Note however that the specific numerical conclusions of that study were are questionable, particularly for the confidence intervals of their parameter estimates. This is because Young et al. (1996) appear to have treated the concentration data as about 200 independent values when actually the concentrations were a highly-smoothed spline fit as described in (Enting et al., 1994, appendix B) (note the plot of residuals in (Young et al., 1996, fig10)).

While linear modelling with 2 or 3 boxes was useful in the early studes by Revelle and Suess etc, the limitations of small numbers of boxes were explored by KILLOUGH and EMANUEL (1981) and the significance of exponential growth discussed in various papers by John Laurmann, particularly (Laurmann and Spreiter, 1983).

This discussion represents only a small fraction of past work – it is (in my opinion) not the task of a referee to make up for inadequate literature surveys. However, I can find nothing in (W&N2014) to show that they have made any advance on what was done decades ago.

The paper (W&N2014) states that they “propose that the response of atmospheric CO_2 concentrations to the disturbances of anthropogenic CO_2 emissions since 1850s can be properly approximated by a linear dynamic system with constant parameters.” Their stated purpose is to explore this proposal/hypothesis. Such exploration has been done

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11, C6933–C6938, 2014

Interactive Comment

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Interactive Discussion

Discussion Paper

decades ago both through explicit analysis and modelling practice.

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Interactive Discussion

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