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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 3: On inverse problems.

As part of ongoing discussions of their paper (Wang and Nemani, 2014a), Wang and Nemani have posted a second response (Wang and Nemani, 2014b).

This response has clarified one important issue. My orginal criticism (Enting, 2014) of their parameterisation of the temperature effect was based on a mis-understanding of their equations and so that criticism should be disregarded.

The second response (Wang and Nemani, 2014b) asserts, as number 2 of their 4 key points, that their temperature term enables them to fit a linear model where other

studies have used non-linear modelling to fit the CO_2 over the industrial period.

I have two problems with this:
(i): Many of us have introduced non-linear terms with the aim of extending the domain of validity of of the models for higher concentrations rather than improving the fit for the 20th century;
(ii): The paper (Wang and Nemani, 2014a) does not seem to provide evidence that they do give a better fit.

Expanding on point (ii), although the authors claim that they are achieving better fits, quantitative results to justify such a claim are notably absent from the paper. In practice what we see in figure 2 is that the author's 2-box model does not do much better than the Bern model (although the caption to figure 2 leaves it unclear as to whether this refers to the orginal Bern model or the authors' modification of the Bern model).

Expanding on point (i), it follows that the estimation of non-linear effects cannot be based on the observed CO_2 record. The estimates are primarily based on laboratory studies of sea-water chemistry and plant physiology. Of course the validity of such modelling in the non-linear regime is conditional on the assumption that all the relevant non-linear processes have been identified. I have argued elsewhere (Boschetti et al., 2010 (online) that **all** modelling is conditional on some assumptions and so I am prepared to accept this caveat.

There are, in my opinion, two distinct inverse problems in play in this discussion:
(a) the estimation of a linear response function, $R(t)$ describing how CO_2 responds to emissions;
(b) the representation of $R(t)$ as a sum of exponentials.

Problem (b) is notoriously ill-conditioned in some cases. A classic example is given by Lanczos (1956). (In my view, the proposed solution by Yeramian and Claverie (1987) does not solve the problem. Firstly it enlarged the domain used by Lanczos. Secondly, it is mapping one (potentially) ill-conditioned problem (fitting exponentials) onto a different (potentially) ill-conditioned problem (finding zeroes of polynomials)). In my view, this ill-conditioning does not (or should not) reflect uncertainty in the carbon cycle as such, even though this ill-conditioning can cause computational problems when trying to propagate model uncertainties. 1 While problem (a) is also ill-condtioned (analysis analogous to that given by Enting and Mansbridge (1987) would suggest that is should be equivalent in difficulty to numerical differentiation) the real difficulty is the specific (i.e. exponential) form of the changes which limits the precision to which $R(t)$ can be estimated. As I said in my original report (Enting, 2014), the usual way this problem is addressed is to use carbon cycle data beyond just the atmospheric concentrations. (Since the expenditure of public money obtaining such data probably runs into the billions of dollars, it seems a shame not to use these data). Such incorporation of other data can be done in (relatively) generic terms (e.g. Enting, 1990) (note that there is an exponent of -1 missing on one of the expressions in that paper). However it is more common to link various carbon cycle data via specific reservoirs. This can have the advantage of helping provide **a** solution to problem (b). However, regarded solely in the context of problem (b) this solution will (almost always) be neither minimal nor optimal.

I think that it is this last point that identifies the differences between my views and those of Andrew Jarvis (Jarvis, 2014). In a sense we are talking about different problems:
— in the restricted sense of solutions of problem (b) (or the combination of problems (a) and (b)) the ranges of uncertainty that I have quoted are undoubtedly too small;
— in the sense of uncertainy in problem (a), I think that they are reasonable.

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