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Discussions

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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I agree with the author response that there is some novelty in the way they have handled the specification and estimation of the effects of the global temperature anomaly on the contemporary global carbon budget. I'm not aware of others treating it as a linear input term estimated in this way; it is invariably handled as a nonlinear effect. However, although this may improve the fit of their simple model to the observations when compared to the case without considering this input, this does not necessarily mean it offers any insight as to the actual affects of temperature feedbacks on the active carbon cycle. For this they would first need to convince the reader of the structural efficacy of the simple model. Although I am less opposed to such simple representations than Dr. Enting and R2, this particular representation is questionable both statistically

and mechanistically.

On statistical grounds the exponential nature of the CO2 emissions input means that only a first order system should be statistically identifiable, and that this tells you little or nothing about the dynamics of the global carbon cycle for the reasons Dr. Enting articulates. So one is then left having to impose a structure and hence an aggregation scheme for the various relevant carbon stocks. Although I would be more cautious than Dr. Enting in saying what this structure should/could be, there is very little in this manuscript to reassure me that the one employed here is fit for purpose. I don't think assuming a timescale of 12 years for atmospheric CO2 stock is well supported either in this paper or in the literature. The subsequent 34 year estimate (how exactly was this estimated?) of the 'surface' stock timescale completely depends on this assumption, the structure of the simple model and exponential emissions forcing (as assumed in the manuscript). No support for this estimated value is offered. A fuller assimilation of the extensive literature on the subject would help place any such model development into context.

On the estimation of the temperature sensitivity, perhaps using the interannual variability for this is a good idea, but the reader is given no reassurance this is so. How does this method compare with alternatives such as a global optimisation of the full model to the observations, or the optimisaiton of its simple nonlinear counterpart? Investigating this in more depth could provide some novelty to this research, but unfortunately one would still be left with the difficult task of reassuring the reader of the structural efficacy of the modelling framework. This needn't be through adopting established conceptualisations of what stocks are thought to be important in this particular case; an alternative could be to demonstrate that the analysis was insensitive to such structural assumptions, possibly afforded by the exponential character of the CO2 emissions forcing themselves?

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