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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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[General Responses]

We first thank Dr. Enting for taking the time to review our paper. We further appreciate his willingness (and courage) in revealing his identity with his comments. The opportunity for the authors to have open discussions with the peer reviewers highlights the advantages of Biogeosciences, which we also appreciate very much.

In his comments (referred to as "Enting2014" hereafter), Dr. Enting clearly expressed his dissatisfaction with our paper (referred to as "W&N2014" hereafter). Indeed, some of his comments are so critical (e.g., "superficial manner", "inadequate model", "logically inconsistent", "erroneous claim", etc.) that we had to do some serious soul-

searching. However, after a careful reading of Enting2014, we found out that **Dr. Enting's criticism is most likely induced by his misunderstanding of the analyses that we worked diligently to explain in the paper**. Here is our turn to explain where Dr. Enting may be mistaken.

Dr. Enting's criticism to W&N2014 can be summarized in two statements:

S1) W&N2014 lacks scientific novelty and value. "This paper adds nothing to an extensive literature on linear analysis of the carbon cycle." (Enting2014, Page C6420, Summary)

S2) The temperature-CO_2 modeling in W&N2014 is flawed and "exhibits a gross failure to agree with the observed CO_2-temperature relations." (Enting2014, Page C6416, Overview)

Dr. Enting's specific explanation for Statement S2 is as follows: "The influence of temperature on CO_2 is parameterised as a flux β_TT' with an estimated $\beta_T \approx$1.64 ppm yr$^{-1 o}C^{-1}$, based on fitting interannual variations. ... This claim seems highly implausible given the relation between CO_2 and temperature through the little ice age (see for example Scheffer et al., 2006). A depression of temperature by say 0.5^o (or more) for a century or two did not lead to a CO_2 reduction of 40 to 80 ppm (assuming $\gamma \approx 0.5$)." (Enting2014, C6418, the effect of temperature)

Historical CO_2 records from ice core measurements (Etheridge et al. 1998) indicate the reduction of atmospheric CO_2 during the Little Ice Age is about -9 ppm. Therefore, a value of -40 to -80 ppm would indeed be a poor estimate. However, **it was Dr. Enting who made this estimate, not us**.

Using an airborne fraction γ of 0.5 in his estimation, Dr. Enting made a mistake by neglecting that **atmospheric CO_2 has different characteristic responses to anthropogenic CO_2 emissions versus changes in temperature, a subject we particularly emphasized in W&N2014 (Section 5, Page 13965-13968)** . We explicitly derived the long-term response of atmospheric CO_2 to a step change in temperature in Eqs. (6a,b) (Page 13698), clearly stated that "atmospheric CO_2 may rise by $\sim$15 ppm for an increase of 1 ^oC in temperature within a few decades" (Page 13968, Line 8-9), and illustrated the results in Fig. 3 (Page 13982). Based on our analysis, therefore, it is straightforward to estimate that the temperature anomalies -0.5^{o}C to -0.7 ^{o}C (Scheffer et al., 2006) lead to a reduction in atmospheric CO_2 by -7.5 ppm to -10.5 ppm during the Little Ice Age, consistent with the measurements. Indeed, our other results (Figs. 2 and 4) also clearly demonstrate the agreement between the simulations and the observations over the 150 years period from 1850 onwards. **These results indicate that Dr. Enting's statement S2 is not correct**.

Furthermore, **the fact that someone as knowledgeable as Dr. Enting couldn't (immediately) understand the CO_2 dynamics discussed in W&N2014 suggests that there is at least something scientifically new in our study, contradicting his statement S1**.

A plausible reason that may have contributed to Dr. Enting's incorrect interpretation of our results may be because our modeling approach is just too simple. As Dr. Enting describes it, "since the mathematical result is simple and well known, there is really no justification for giving such an illustration in a research paper (as opposed to an introductory textbook)" (Enting2014, C6418, the 'two-box' example). Indeed, we agree with Dr. Enting on the first half of his remark here, but disagree with him on the second half. Because our goal is to study a physical system, not just the mathematics itself, any tool suitable for the job is a good tool. Physicist Enrico Fermi once talked about two approaches of calculation (i.e. modeling) in theoretical physics as follows:

"One way, and this the way I prefer, is to have a clear physical picture of the process that you are calculating. The other way is to have a precise and self-consistent mathematical formalism." (Dyson, Nature, 427, 297).

We feel the same rules should apply to our field. By using the simple two-box model to

"demonstrate our analytical framework" (Page 13960, Line 16), therefore, we were just trying to keep a clear big picture of the atmospheric CO_2 dynamics in our analysis.

The use of a simple mathematical model by no means implies a compromise of the scientific rigor in our research. The results presented in W&N2014 reflect only a small proportion of the analytical derivations and numerical calculations performed through the study, many of which deal with general higher-order systems (by "higher-order systems" we mean an arbitrary "N-box" system with N being finite). However, to explain the mathematical proof of some of the equations in general terms requires some language of linear operator theories in the Hilbert space, which may not be familiar to everyone in our field. Interestingly though, their proof in the 2-box cases is particularly easy, which also renders a clear physical picture (as explained in W&N2014). Therefore, the simple 2-box model actually has the advantage in explaining our findings about the atmospheric CO_2 dynamics to a broader audience. Since a simple approach does the job, we feel that there is no need to introduce unnecessary mathematical complexity in W&N2014. Besides, isn't it scientifically and intellectually interesting to see such a simple model captures so much of the global carbon cycle dynamics?

In summary, we argue that Dr. Enting's criticism to W&N2014 is not solidly founded and therefore should be discounted.

[Specific Responses]

On Linear Modeling (Enting2014, C6416-C6418): We don't see major differences between the general framework Dr. Enting outlined in these paragraphs and our analytical approach (except that Dr. Enting prefers describing the problem in Laplace spectral domain, which is perfectly fine). Indeed, Dr. Enting correctly acknowledged the fact that E(t) and A(t) both being exponential has limited scientists' ability to resolve the system's impulse response function (or Green's function) from them ("many functions can be fitted to pass through this one point" (Page C6417). It is our interest in W&N2014 to study the common characteristics among these "many functions". Because they have various levels of complexity, it is very legitimate for us to start the investigation from the simplest ones before generalizing the results to more complicate systems.

On the 'Two-Box' Example (C6418): see the General Responses for our explanations.

On the Effect of Temperature (C6418-C6420): We believe that these inaccurate remarks only reflect Dr. Enting's misunderstanding of our analysis. For instance, we do not believe that anyone can arbitrarily "split" one set of carbon fluxes into two to reproduce all the agreement between our model and the observations.

Dr. Enting's remarks on "process-lumping" can also be misleading. "Lumping" is essentially inevitable in research unless we have a collection of accurate observations of all the key variables of the global carbon cycle. When good observations are scarce, on the other hand, the *principle of parsimony* tells us to choose models that have fewer unknown parameters. Again, here are the words of Enrico Fermi from the same conversation:

"I remember my friend Johnny von Neumann used to say, with four parameters I can fit an elephant, and with five I can make him wiggle his trunk." (Dyson, Nature, 427, 297).

On Other Technical Errors (C6420):

1) Regarding our introductory sentence: "The full potential of anthropogenic CO_2 emissions for changing the climate has not yet reached because only 41–45% of the CO_2 emitted between 1850 and 2010 remained in the atmosphere while the rest was sequestered by lands and oceans..." (W&N2014, Page 13958, Line 26).

We recognize that this sentence may not be comprehensive but do not understand why Dr. Enting calls it "simply false". It is our understanding that the accumulation of anthropogenic emissions of greenhouse gases (mainly CO_2) in the atmosphere, which cause changes to the surface energy balance, is the main driving force for climate change. Therefore the plausible increase in the airborne fraction of anthropogenically emitted CO_2 in the future, as is predicted by many studies, will most likely further

strengthen the forcing for climate change. We would like to hear more explanations from Dr. Enting if our understanding is incorrect.

2) Regarding the number of eigenvalues: our original sentence regarding this issue is "For a two-box system like Eq. (2a), the problem is particularly simple because the only eigenvalue (λ) is ..." (W&N2014, Page 13967, Line 7).

Because Eq. (2a) (P. 13963) is a first-order ODE, this equation could have only one eigenvalue. Therefore our statement in this context is correct. However, we agree with Dr. Enting that Eqs. (1a) and (1b) have two eigenvalues, with one of them being zero.

Interactive comment on Biogeosciences Discuss., 11, 13957, 2014.