Earth Syst. Dynam. Discuss.,
https://doi.org/10.5194/esd-2019-41-RC2, 2019

Earth System Dynamics
Discussions
Open Access

Interactive comment on "Estimates of climatic influence on the carbon cycle" *by* Ian Enting and Nathan Clisby

Anonymous Referee #2

Received and published: 7 October 2019

This paper addresses the important topic of using CO2 and temperature data of the past few thousand years to reconstruct carbon climate feedbacks, and adds to previous work by adopting a more complete formal framework and looking at the ability of same parameter sets to fit different time periods. The most important new messages involve the finding that the climatic controls in CO2 during the Little Ice Age may have been different than more recent periods, and that an understanding of carbon-climate feedbacks may need to consider spatial complexity in the temperature anomalies. The paper builds on the deep insights of the authors and their understanding of nuances of the science. The main issue with this paper is that is such a touch read. I expect as written it will be accessible to only a very limited audience. If the paper is to have significant immediate or lasting value, the presentation needs substantial improvements.

Printer-friendly version

Discussion paper

In the detailed comments below, I provide a list of issues that I confronted on reading the paper. While the paper needs line-by-line improvements of this sort, there are also larger issues.

In particular, the derivations using the Laplace transforms seem of secondary importance but are placed front and central, which will discourage many readers. Other than showing a formal equivalent of Eq (1) in frequency space which is not actually used, it seems the main point of the Laplace framing is to flesh out a rigorous distinction between the carbon response function R with and without feedbacks, and to show how the two forms are related. At least, that's about all I got out of it. I thus urge a simplification along these lines: Move much of Section 2 off the main outline, e.g. into an Appendix. Instead, start Section (2) right off with Eq 10, which can be understood without reference to any Laplace transform framing. Also write a new equation that is equivalent to Eq. (10), but replaces carbon pulse response R(t) with R_FB(t). These two equations then cement the idea that there are two slightly different frameworks, depending on whether R contains the climate feedback or not. Here it would be good to point out how the frameworks converge for the preindustrial case. From there, I suggest jumping to the parameterizations used for H(t), also expanding these equations in the time domain, and inserting in to Eq. (10). If I understand correctly, these few equations are sufficient to carry out the main fitting calculations and most of the discussions. If not, I still suggest starting from this basis. With this streamlined outline, the Laplace framing is mainly needed to justify the equivalency of the two versions of Eq. (10) and to show how R_FB is formally related to R(t). These topics would fit fine in an Appendix.

Other general points: If I understand the method correctly, three parameters are fit using CO2 and temperature data: theta_0, theta_s, and theta_gamma in Eq. (17). Another parameter, tau, is not fit, but rather given a seemingly arbitrary value of 100 years. Why this choice? I sense that more discussion is needed on the sensitivity to this choice and why 100 years is justified.

The conclusions need to be rewritten in such a way that the reader isn't forced to remember all the symbol definitions.

Line-by-line comments:

Line 6. Needs rewording. The language is ambiguous as to whether there are discrepancies between different types of temperature records, or whether the carbon feedback parameters optimized on "paleo" data don't fit the early 20th century CO2 record. Also, paleo records are not usually considered relevant for reconstruction of 20th century temperatures. So, on the face of it, this sentence merely states that the paleo temperature reconstructions (e.g. from some earlier period) don't fit the temperature reconstructions of the 20th century, which is akin to saying that the political history of the 19th century was different than that of the 20th century.

8. Would be clearer if this particular reconstruction was named here.

9. Needs rewording. See later comment on line 37.

21: Although Oeschger at al formulate a model to calculate the partitioning of carbon into different reservoirs, I'm quite sure they do not define factors that are direct analogues of the beta factors used here. The context for this citation therefore appears incorrect.

37: This sentence, which is also echoed in the abstract, is hard to follow. The Moberg reconstruction did not use CO2 as an input, but the wording suggests it did. I think the point is that the temperature reconstructions by Moberg do not reflect uniform weighting of land temperature. By chance, it seems that their average is weighted towards regions with larger carbon responses, yielding a larger (or a more appropriate?) CO2 response than would be obtained for an area-weighted hemispheric average.

Eq. (2). The quantity Q(t) (or q(p)) needs defining. Alpha*Q is defined to be radiative forcing, but Q itself is not defined here, leaving it to the readers imagination.

Eq. (4). Some additional explanation is needed on how this equation can be applied separately to both preindustrial and modern situations. Also, for a preindustrial application Q(t) is the not the excess above a pre-industrial level, but rather a perturbation in the preindustrial level itself, right? The text leading into Eq. (4) is thus problematic, i.e. Q(t) needs to be explicitly defined as the perturbation relative to a reference (i.e. constant) preindustrial level. This should be done on first usage (see above). A similar clear definition is needed for W(t). (Comment might be partly superseded by reorganization proposed above).

59: Confusing. If p r(p) is equal to the airborne fraction and psi(p) is also equal to the airborne fraction, then why not write an equation that sets them equal to each other? Also, are we talking about the cumulative airborne fraction or the airborne fraction based on yearly emissions?

64: Hard to follow. I sense it would help to show an additional equation of the sort q(p) = . . . between (6) and (9) to clarify how g(p) inserts into the math.

73: Partition into what?

90: Meaning of “ambiguity” unclear. I think there is no conceptual ambiguity here. Rather, the key issue is distinguishing the quantities from past records of variability.

Eq(11) and Eqs (12). Does this formulation have a simple basis in box modelling? Would be good to clarify. I notice that this formulation fixes the ratio of the responses at the high and low frequency limits. Why is this reasonable? Reading ahead, I see that Eq. (15) somewhat answered these questions. This suggests flipping the order and putting Eq. (15) ahead of (11) and (12) to help justify the approach. Also would be good to clarify whether H(t) has units of flux or units of amount, i.e. does a step change in temperature immediately release a finite pulse or does it cause a step change in flux? (This comment might be superseded if text is reorganized as proposed above).

Eq. (16). This would fit better in the next section, see below.

Eq (17). Hard to follow. What is mean by “normalization” here? What do Rref and

Href represent? Why is the "ref" subscript needed at all? The need for theta_0 is explained. But the need for theta_s and theta_gamma is not. What happened to gamma_prime? What parameters are adjusted in the fitting process? What are the data inputs? What is being minimized? If correct, it would help clarify to explicitly write: R(t) = theta_s*Rref(t), H(t) = thetma_gamma*Href(t), where Rref is . . . etc.

124: There is no hat symbol appearing in Eq. (17). The hat symbol should be defined only when first used.

Eq. (18). Why the jump back to a Laplace transform formulation? Is the regression actually done in p space? If not, why not stick to the time domain? It's not obvious how Eq. 18 is derived. Which previous equations are combined used under what mathematical assumptions? In this context it would be good to clarify why a "preindustrial" situation allows a term to be eliminated. This later point would be covered by addressing the earlier query about Eq. (4).

139. Hard to follow because the relationship between gamma' and theta_gamma has not yet been clarified. If they are equal to each other, why the need for two symbols? Eq. (19), Similar to Eq. (18), an explanation is needed for how to derive Eq (19). What Equations are combined?

208. This text could also use some expansion, similar to the related point in the Introduction. The relevant regions are those in which carbon fluxes are particularly sensitive to temperature.

209. Actually, Rafelski's model does partly account for the plateau.

238 The meaning of "correspond" is unclear.

240 Better to put the xk(p) above when introducing Xk(t).

255 having

261. The dash is confusing. Replace with "and"?

262. “notably” is dangling.

282-287. I am not overly concerned about the lack of error analysis, but the difficulty arising from non-independence could be overcome with an appropriate error model that accounted for the dependence.

287-291. Could be cut as out of scope.

300: I see no “hats” on the list that follows. This comment belongs wherever the hat is first actually used.

Table 1 and Table 2. Meaning of t1 and t2 not obvious at first glance. I suggest changing the t1 and t2 headers to “time frame”, and collapse t1 and t2 columns to one column, e.g. 500-1750. Also, don’t the numbers depend on the choice if tau? The table should clarify this.

Figure 3. Ice-core CO2 data source is not specified.

Interactive comment on Earth Syst. Dynam. Discuss., https://doi.org/10.5194/esd-2019-41, 2019.