Reviewer Comments on
An empirical model of global climate –
Part 2: Implications for future temperature
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Overall comments

This direction taken in this paper (and the companion) could represent an important advance in understanding of climate change over the past century and into the future. I am sympathetic to the approach and applaud the authors for identifying an important approach.

However, as I detail below, I find numerous objections in the present manuscript and the underlying calculations such that I cannot recommend the manuscript for publication without substantial revision. These are substantive structural concerns with the approach, and I am afraid, to my thinking, would require redoing the calculations. I am not sure whether such a revised set of calculations would change the conclusions all that much, but I hope I make it clear why such revised calculations are (at least in my mind) essential.

I would encourage the authors to revise to accommodate the concerns noted below. Although I was not a reviewer of the companion paper, I would note that many of these concerns apply as well to that paper, so a major revision of both would seem essential.

I present specific comments mainly on the earlier part of the manuscript to which I paid greatest attention. This is not to say that there are no other elements of the paper to which objection might be raised, but my feeling, based on the concerns I have raised with the model, is that there is not much to be gained at present by a more detailed examination of the results and discussion.

Specific comments (In the page numbers below leading digits 239 are suppressed)

Page 18. Equations 2 and 3. I have ***major concerns*** over these equations. I am initially surprised that the factor $(1 + \gamma)$ does not multiply the λ in the aerosol response term, as it does in the greenhouse gas response term. I think the argument given on p 18, lines 7-13 in no way justifies this. At minimum the failure to include the same multiplier $(1 + \gamma)$ is at variance with current and historical treatments of forcing and response, precluding comparison with results of other studies. The authors state that

> Aerosol feedbacks are implicit in the scaling terms used to define NAA RF_i, which represents global, monthly mean total RF due to all anthropogenic aerosols i*ncluding feedbacks*. [italics in original]

At minimum this approach will result in whatever aerosol forcing that is determined by the regression approach not being additive to the greenhouse gas forcing, again at variance with virtually all current practice. The authors justify this approach by stating that:

> [T]he physical processes that link perturbation to response are extremely different for GHGs and aerosols.

If the authors believe this to be the case I would encourage them to include an efficacy factor for the aerosol forcing, but that done, retain the same multiplier to represent the feedback associated with the response to the forcing (as adjusted by the efficacy). Several investigators have proposed efficacies, typically within a factor of 2 of unity. Use of the efficacy approach would also allow comparison with the values obtained by other investigators. I am wholly surprised at the adverb "extremely" in the above quotation.

In addition to the above, the factor $(1 + \gamma)$ as a multiplier in the greenhouse gas term is at serious variance with the Hansen 1984 reference cited and with other historical and current references. Staying with the Hansen reference and notation,

$$\Delta T_{\text{eq}} = f \Delta T_0$$

$$f = \frac{1}{1 - g}$$

$$g = \Sigma g_i$$

where ΔT_0 is the response in absence of feedbacks and the multiplier f denotes the effects of feedbacks; for g positive there is positive feedback. From the above

$$\Delta T_{\text{eq}} = \frac{1}{1 - \Sigma g_i} \Delta T_0$$

Compare with the first term on the RHS of eq 2 of the manuscript under review, where I substitute in the expression in eq 3:

$$\lambda = \lambda_0 \left[1 + \left(\Sigma \frac{1}{\gamma_i} \right)^{-1} \right]$$

It seems clear that the two expressions are entirely different, as would be revealed by a plot of the multiplier as a function of either of the sums. It is clear as well that the feedbacks do not add in anywhere the same way in the two treatments. So again, it may turn out that this approach works, in the sense of yielding a minimum in the cost function of equation 1, but the feedback terms can in no way be compared to those in the older literature or in more recent studies such as Soden and Held (J Climate, 08).

Because at best the parameter γ captures only response to GHG forcing and not response to total forcing, one cannot attach any physical significance to values of this quantity that are retrieved, in addition to the issue of definition. I thus cannot ascribe any credence to the discussion at page 48 beginning at line 6.

I would certainly encourage (and if I were the editor, insist) the authors to use more conventional expressions at minimum to permit comparison with the literature but perhaps more importantly to retain consistency with feedback theory as it has been traditionally and currently used in the literature. Given the fundamental differences between the more conventional approach and that employed in the present paper, I would be inclined to insist that the authors to go back and revise their equations, but I would feel that perhaps that is asking too much of them. However, given the statement in the manuscript that the model can be run in an afternoon, perhaps it would not be all that much work for the authors to revise their model.

Still with eq 2, I am concerned over the large number of adjustable parameters, the indices TSI, SOD, ENSO, AMO, PDO, and IOD and the corresponding coefficients in addition to uncertainties in aerosol forcing, climate sensitivity, and ocean uptake. Perhaps as the authors note at page 21, line 5, these additional terms end up being minor, but I fear that with so many parameters there is a risk of the solution not being well constrained. One must be concerned as well as to the accuracy of the several indices employed.

Page 19, eq 4. My read of Schwartz (2012) is that he related the heat flux into the ocean to the increase in temperature above preindustrial, not to the forcing (at the time under consideration or at prior time, 6 years as in the present equation 4. The consequences of treating this heat flux as in eq 4 would seem to need scrutiny and justification. Perhaps the intent in eq 4 is to avoid having temperature change on the right hand side; however having it on the right hand side would lead to a more or less conventional feedback expression. I am guessing that the units of the equation are W m^{-2} and that Ω is dimensionless. This would be consistent with the statement at line 24 that Ω is a fraction. But see below.

Page 19, eq 5. If I substitute Eq 5 into Eq 5 I get Q equal to OHE times the ratio of the quantity in square brackets over the quantity in angle brackets, which is an average of the quantity in square brackets, so the ratio is dimensionless. But it is stated that OHE represents a rise in ocean heat content (say J m^{-2}), which on face is inconsistent with my supposition that the units of the equation are W m^{-2} and that Ω is dimensionless. All this makes me uneasy at this stage of my reading of the manuscript.

Page 20, line 19: "mainly sulfate"; certainly recent work by Zhang (GRL, 07), Jimenez (Science 09) and numerous other groups has shown that secondary organic is comparable to if not greater than sulfate in many locations; perhaps sulfate is a surrogate; but this would need to be addressed; including forcing per emissions or mass used in the regressions. OC in Figure 2 seems extremely small compared to what I would expect based on concentrations of OC. I note (Fig 2a caption, page 58) that the sulfate forcing has been scaled such that "total RF due to sulfate aerosols in 2005 equals –0.96 Wm^{-2};" this seems high compared to other current estimates, so perhaps sulfate is a surrogate, although there is a curve in the figure labeled OC that refers to fossil fuel burning emissions that is an order of magnitude lower than the curve labeled "SO_2" [sic]. All this leads me to question both forcings. I looked at Stern (06), which is not a direct estimate by that author, but which is an expression that is a fairly complicated expression of terms that are logarithmic in sulfur emissions (not a simple linear expression as I would expect) that dates back to Kattenberg (IPCC Report 96) and Wigley (Nature, 92); I did not check those references, but all of this leads me to wonder why that expression was chosen, why the normalization to -0.96 W m^{-2} was chosen, and at the end of the day how sensitive the results are to these choices.

Page 24 line 25: "The total RF due to aerosols is much larger than direct aerosol RF, due to many feedbacks." It is not clear what the authors are considering feedbacks, but in the conventional sense forcing does not depend on feedbacks; it is response that depends on feedbacks. Perhaps the authors are including cloud interactions in what they refer to as feedbacks, but that would be a very unconventional extension of the term. Page 25, line 3 refers to AR4 (table 2.12) and Storelvmo 09 in support of this. The AR4 table gives the direct forcing of sulfate aerosol as -0.40 ± 0.20 W m^{-2}, but goes on to give an estimate for first indirect forcing of all aerosols of –0.70 [–1.1, +0.4] W m^{-2}; perhaps it is this effect that the present authors are trying to capture, which seems appropriate, but this is not a feedback as conventionally understood. Likewise Storelvmo deals with the aerosol indirect effect.

Figure 5. If I compare the panels Total RF it seems that Net at 2010 is about the same in the two panels, but that GHG forcing is very different, much higher in the panel on the right. This seems at variance with my understanding that the authors considered GHG forcing to be well known.

Some comments on the presentation

Abstract: I do not get a real sense of the paper from the abstract. I get a vague, nonquantitative sense of the results but very little sense of what was done and what was found. I think most of what is stated in the abstract could have been stated already before the present work was done.

Generally. I take exception to the use of acronyms to represent quantities in algebraic equations, e.g., SOD, and even more so acronyms with spaces in them such as RF GHG.

I also found it somewhat frustrating that the terms in Eq 2 were not defined near that equation, but only two pages later (albeit it would be less distance in the published article format).

Page 17, lines 1-3: "Can be done on a workstation in an afternoon". Not a fair comparison. The present model does not give anywhere near the detailed picture that comes from GCM; very limited picture. From the companion paper: "We work exclusively in a global, monthly mean framework." As opposed to a GCM which works at, typically, 3-hourly with high 3D spatial resolution. The approach taken in the present paper has many strengths but this sort of comparison with GCMs is not one of them. The approaches have very different objectives.

Page 19, eq 5. Having the entire phrase "TIME INITIAL TO TIME FINAL" as a subscript is unconventional and cumbersome; could it be omitted as a subscript and let it be taken care of in the definition of the averaging of the angle brackets?

Page 19, lines 24-25: "the fraction of anthropogenic RF perturbation to climate (AF) exported to the ocean". The quantity that is transported to the ocean is heat, not forcing. I understand what the authors mean (I think), namely the ratio of the heat transport rate to the forcing, but I think the reader and the authors would be better served by more precise definition.

Page 21, line 5: Terms in the regression for PDO and IOD are "extremely minor". Is this because of little covariance of temperature with these quantities, or is there some physical reason?

Page 31, line 6-8. I would say better here an exposition of results before compare/contrast with Lean-Rind.

Figure 5. I do not understand the term "ladder plot". Is this just a term for stacked graphs or is there some sense of one building on the next.

Page 29, line 26: AMO. I call authors' attention to Booth (Nature 12) that argues that the AMO is driven by anthropogenic aerosols. I would not hold the present paper hostage to dealing with that result, but it seems potentially pertinent.