Reply to comments, acp-2012-611, "An Empirical Model of Global Climate – Part 2: Implications for Future Temperature" by N. R. Mascioli, T. Canty, and R. Salawitch

We thank the reviewer for very helpful comments and will make significant changes to the manuscript in response to these comments. We have repeated all of the calculations using the modified model formalism suggested by the reviewer. Some of the details have changed, but the overall message remains the same.

The reviewer expressed two concerns over the equations used in our regression model:

1) we had treated feedbacks in response to GHG forcing (model parameter γ) separate from feedbacks in response to aerosol forcing; we had folded uncertainty in the feedback that occurs in response to radiative forcing by aerosols into the scaling parameters α_{COOL} and α_{HEAT}

2) when relating Radiative Forcing perturbations to temperature (ΔT), we had used the product λ_p $(1 + \gamma)$ (units of °C / W m^{-2}) in an expression that has ΔT on the left hand side of the equation:

$$\Delta T = \lambda_p (1 + \gamma) \text{ (GHG RF)}$$

rather than an expression detailed by the reviewer, which is traced back to Hansen et al. (1984).

Also, in our submitted paper we had used a value for λ_p (this term had been written as λ) to represent the response of surface temperature to a RF perturbation in units of °C / W m^{-2}, which is the numerical evaluation of the term $1/4\sigma T^3$. The reviewer's comments are written assuming that λ_p is in units of W m^{-2} / °C, which represents $4\sigma T^3$.

We have altered our model equations and figures to address all of these points.

For comment 1), we will now allow the feedback term to multiply NAA RF. We believe is it debatable whether NAA RF should be multiplied by this feedback term but we have made this change based on the comments of this reviewer as well as a private email we have received from a climate modeler following the posting of this manuscript and the companion paper on the ACPD website.

For comment 2), we will use the expression suggested by the reviewer and express results as a function of the Climate Feedback Parameter so that our results will be directly comparable to Section 8.6 of IPCC (2007). In the submitted paper we had used the dimensionless quantity γ, termed the sensitivity parameter, in numerous figures. In the revised paper, we will use the Climate Feedback Parameter λ, units of W m^{-2} / °C, in all figures that had previously used γ.

Our revised model formalism will be consistent with the comments of the reviewer as well as Bony et al. (J. Clim., 2006) and Section 8.6 of IPCC (2007).

Our old parameter γ (climate sensitivity) and the new parameter λ (feedback parameter) are related according to

$$1+ \gamma = \{ 1 - \lambda / \lambda_p \}^{-1}$$

where $\lambda_p = 3.2$ W m^{-2} / °C

$$\lambda = \Sigma(\text{Feedback Parameters}),\ \text{i.e., } \lambda = \lambda_{\text{Water Vapor}} + \lambda_{\text{Lapse Rate}} + \lambda_{\text{Clouds}} + \lambda_{\text{Surface Albedo}}$$

Hence our governing equation will now be written as:

$$\Delta T_{\text{MDL}\, i} = (1 / \lambda_p) \{ 1 - \lambda / \lambda_p \}^{-1} \{ \text{GHG RF}_i + \text{NAA RF}_i \}$$

$$\text{where } \lambda_p = 3.2 \text{ W m}^{-2} / {}^\circ\text{C}$$

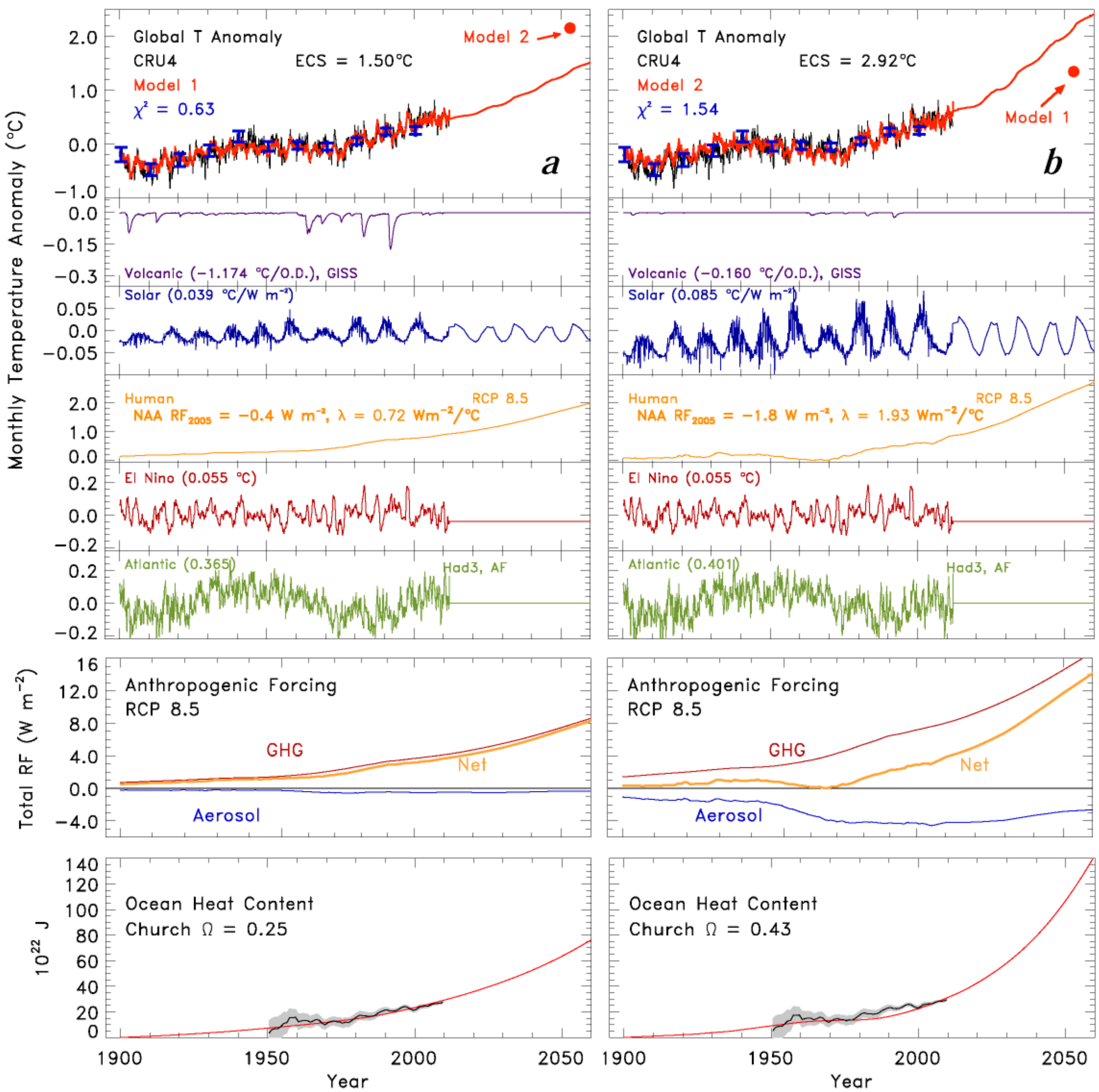
as suggested by the reviewer. In the revised paper, each ladder plot will now contain the numerical value of λ and figures 7, 8, 10, and 11 will now use λ for the y-axis. Our results will be presented in a manner that allows straightforward comparison to numerical values of feedback parameters given in Section 8.6 of IPCC (2007) and Bony et al. (2006).

The prior formulation of climate feedback used in our submitted paper was not in error. However, Eq (3) in the submitted paper, which was used to relate γ to λ, was incorrect. We apologize for this error and we believe it was the source of confusion. The figures and computations in the submitted paper had been exclusively based on γ. Upon revision, results will be exclusively based on λ. This revision leads only to a change in the numerical information printed on the "Anthropogenic Rung" of the ladder plot. Another revision, allowing NAA RF to be multiplied by $(1 / \lambda_p) \{ 1 - \lambda / \lambda_p \}^{-1}$, causes a slight change in the appearance of the ladder plots. The most important change introduced by the decision to multiply NAA RF by the same feedback term that scales GHG RF is that we can no longer achieve values of $\chi^2 < 2$ for the full empirical range of NAA RF. However, this has minimal impact on our overall scientific conclusions.

We would like to proceed with two additional revisions to the model.

Since we can no longer achieve values of $\chi^2 < 2$ for the full empirical range of NAA RF when allowing the aerosol radiative forcing term to be multiplied by $(1 / \lambda_p) \{ 1 - \lambda / \lambda_p \}^{-1}$, we would like to use the AMO detrended by anthropogenic RF, rather than AMO detrended by global SST. Use of AMO detrended by anthropogenic RF provides a much better fit to the global temperature anomaly during the pre-WWI cooling phase and the WWII warming phase of the climate record. By better fitting the climate record during these periods of time, we can access nearly the entire range of NAA RF using $\chi^2 < 2$. Another alternative, which we do not prefer, would be to relax the χ^2 constraint to something like $\chi^2 < 3$. Finally, upon careful consideration of the simulations and based on conversations with experts in ocean heat content, we propose to revise Eq. (5), the definition of Ω, to account for the fact that about 70% of the rise in OHC occurs in the top 700 m of the world's oceans (i.e., Sect 5.2.2.1 of IPCC 2007).

The updated version of Figure 5, accounting for these changes, appears below:



Proposed revision to Figure 5, Mascioli et al, ACPD, 2012 that is based on the model framework suggested by the reviewer, includes notation for the value of the Climate Feedback Parameter (λ) on the Human Rung, and has slightly higher values of Ω due to consideration that the OHC measurement of the upper 700 m of the oceans represents ~70% of the rise in OHC for the entire ocean.

Below, we reply to each comment. The reviewer's comments appear in blue.

> 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.

Thanks for these encouraging words !

> 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.

We will make substantial changes to the paper to accommodate these concerns, as outlined. We will adjust the companion paper in the same manner, should these papers be allowed to proceed. The scientific conclusions of both papers are not affected by these changes, but they are nonetheless extremely important because our model results will now be presented in a manner directly comparable to results in the literature such as Bony et al. (J. Climate, 2006) and Section 8.6 of IPCC (2007).

> 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.

We welcome a detailed examination of the results and discussion, should the paper proceed.

> 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….

For brevity we have not repeated the entire comment, as it involved numerous equations.

As noted above, we will revise the model equations such that:

a) the feedback term will now multiply the aerosol radiative forcing term;

b) model results will be expressed and illustrated as a function of the Climate Feedback Parameter

Upon revision, our model framework will be entirely consistent with Section 8.6 of IPCC (2007).

We had not initially allowed the feedback term to multiply the aerosol radiative forcing term because:

i) the aerosol driven perturbation to radiative forcing of climate occurs over such different spatial (e.g., Mickley et al., Atmos. Environ., 2012; Leibensperger, Atmos. Environ., 2012) and vertical scales (e.g., Kahn, Surv. Geophys., 2012) than the perturbation to radiative forcing due GHGs, which are well mixed geographically and vertically;

ii) the numerical values of the scaling parameters α_{HEAT} and α_{COOL} are much larger than the numerical value of the term $1+\lambda$ and, since we vary α_{HEAT} and α_{COOL} over such large ranges to "sweep" aerosol radiative forcing parameter space (i.e., to account for uncertainty in the various aerosol indirect effects), we had considered aerosol feedback to be implicit in our treatment of NAA RF.

iii) the GCM feedback parameter terms shown in Figure 8.14 of IPCC (2007) are found by methods that do not represent aerosols. Derivation of one set of these feedback parameters is described by Colman (Clim. Dyn., 2003); in this paper, the top of the atmosphere radiation field from GCMs is examined for nominal CO_2 and $2\times CO_2$, without any consideration for aerosols as far as we can discern. Figure 1 of Bony et al. (J. Clim., 2006) is the basis of Figure 8.14 of IPCC (2007); Bony et al. (J. Clim., 2006) state "Cloud feedbacks have long been identified as the largest internal source of uncertainty in climate change predictions, even without considering the interaction between clouds and aerosols[3]" and footnote 3 of their paper reads:

> In this paper, we will not discuss the microphysical feedbacks associated with the interaction between aerosols and clouds. As Lohmann and Feichter (2005) say: "The cloud feedback problem has to be solved in order to assess the aerosol indirect forcing more reliably".

In other words, the Bony et al. review paper on feedbacks in GCMs is based entirely on response of models to GHG forcing.

Nonetheless we will proceed as the reviewer has suggested. In the revised paper the same feedback term will multiply GHG RF and NAA RF. We understand a valid argument can be made for this approach. Our scientific conclusions are not affected because: i) the various aerosol indirect effects (i.e., Table 7.10a, IPCC 2007) are so much stronger than this feedback term; ii) we sweep parameter space of the aerosol indirect effect, using the scaling parameters α_{HEAT} and $\alpha_{COOL.}$

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.

The submitted manuscript had an erroneous equation, which is the source of this comment. We apologize for confusion resulting from the erroneous equation.

The original expression for Equation 3, which represented the decomposition of γ into component terms, had been wrong. This equation will be replaced with the correct expression:

$$1+\gamma = \{1 - \lambda / \lambda_p\}^{-1}, \text{ where } \lambda = \lambda_{\text{Water Vapor}} + \lambda_{\text{Lapse Rate}} + \lambda_{\text{Clouds}} + \lambda_{\text{Surface Albedo}}$$
$$\text{and } \lambda_p = 3.2 \text{ W m}^{-2}\ {}^\circ\text{C}^{-1}$$

Equation (3) had been for informative purposes only and did not affect any of the computations or figures. In the submitted paper, we had not broken the feedback term into its components: i.e., we had never used Equation (3). All other uses of γ in the submitted paper had been correct in the given context, even though this context was not well connected to the primary literature since the literature is based mainly on λ rather than γ.

Our overall model formulation is not "entirely different" than the one given by the reviewer. Rather, the simple substitution:

$$1+\gamma = \{ 1 - \lambda / \lambda_p \}^{-1}$$

converts our expression to that given on page 631 of IPCC (2007). The IPCC (2007) expression is the same as that given by the reviewer, except the IPCC (2007) expression uses Climate Feedback Parameter (λ) rather than gain (g).

As noted above, we will change the paper to use the IPCC (2007) expression and use the Climate Feedback Parameter λ in numerous figures, so that our results will now be presented in a manner that permits direct comparison with the literature.

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.

Use of indices to regress for total solar irradiance (TSI), stratospheric optical depth (SOD), and quantities such as ENSO or the quasi-biennial oscillation of stratospheric winds in a regression model is extremely common.

For instance, Figure 3-1 of the 2006 WMO/UNEP Ozone Assessment report, available on line at:

http://www.esrl.noaa.gov/csd/assessments/ozone/2006/images/Fig3-01.jpg

shows a regression of total ozone versus 5 parameters.

Our regression model of the global temperature anomaly versus TSI, SOD, ENSO, etc. builds on the work of Lean and Rind, GRL, 2008, Lean and Rind, GRL, 2009, and Kopp and Lean, GRL, 2011.

We have conducted many more simulations than shown in the paper, including alternate formulations for TSI, SOD, and ENSO, and our results are quite insensitive to which formulation is used. In the Canty et al. (2012) companion paper, we focused on possible inaccuracies of indices for SOD and AMO. Fig. S1 of Canty et al. (2012) shows regression results found using a different time series for SOD; many figures throughout Canty et al. (2012) explore the sensitivity of volcanic cooling to how the AMO index is formulated. We never see any expression of the PDO and the IO in any record of the global temperature anomaly, so we are confident these terms "being minor" is a robust result of the analysis. This is discussed more fully in Canty et al. In the Mascioli et al. paper, we have used chosen to use single representations of SOD and AMO and explore the sensitivity of future temperature to NAA RF, climate feedback, and ocean heat export.

> **Page 19, eq 4.** My read of Schwartz (2012) is that he related the heat flux into the ocean to increase in temperature above preindustrial, not to the forcing (at the time consideration or at prior time, 6 years as in the present equation 4. The consequences 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.

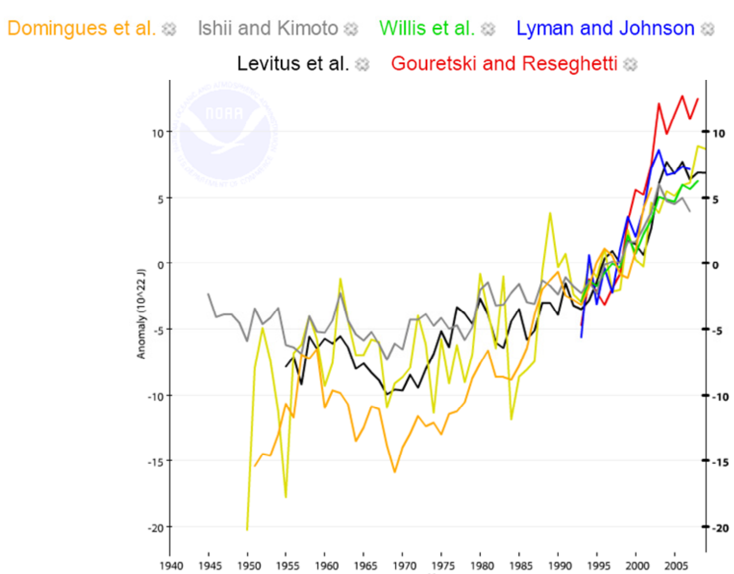
The decision to relate heat flux into the ocean to radiative forcing or temperature is reflective of how well two key quantities are truly known:

a) interannual variations in OHC

b) time delay between atmospheric perturbation and oceanic response

If both quantities were known very accurately, then it would be appropriate to base heat flux into the ocean on the atmospheric temperature rise above pre-industrial. However, we do not believe either of these quantities are well enough known to use this approach.

The figure below, from http://www.ncdc.noaa.gov/bams-state-of-the-climate/2009-time-series/ohc, shows 7 observations of OHC:



There are significant differences in the long term trend in OHC as well as interannual variations in OHC, which is the subject of many papers in the oceanographic literature. Even if the oceanographic community could precisely determine OHC, to relate heat flux to the ocean to the ups and downs of globally averaged temperature, we would need precise knowledge of the temporal delay and atmospheric response.

The submitted version of Schwartz (2012) had detailed the delay between atmospheric perturbation and ocean response for 5 GCMS: 6.3 year, 6.5 year, 5.0 year, 9.2 year, and 4.1 year for GISS, RCP, GFDL, MIROC, and PCM respectively (this level of detail was dropped in the published paper, which instead gives the median value of 6.3 year, and the range 4.1 to 9.2 years). This is too large of a range to have confidence in relating a particular OHC variation to any specific atmospheric perturbation.

In our submitted paper, we used an equation for ocean heat flux that is proportional to anthropogenic radiative forcing, because this formulation captures the general trend in rising ocean heat content without being encumbered by representation of inter-annual variability. Since the rise in temperature since pre-industrial time is proportional to anthropogenic radiative forcing, i.e.:

$$\Delta T_{ANTH} = (1 / \lambda_p)\,(1 + \gamma)(\text{GHG RF} + \text{NAA RF}) = (1 / \lambda_p)\,\{ 1 - \lambda / \lambda_p \}^{-1}\,(\text{GHG RF} + \text{NAA RF}).$$

The expression we are using for Ω does indeed vary in direct proportion to ΔT_{ANTH}.

Our expression captures the essential physics of the problem without addressing the interannual variations in OHC, which is too uncertain at present time to be represented in a model such as ours. In our model framework, results are extremely insensitive to the lag time between atmospheric perturbation and oceanic response. We get essentially identical results using any of the time lags reported by Schwartz because anthropogenic RF changes slowly over time. On the other hand, if we had related OHE to the anthropogenic rise and interannual variation of ΔT, then our simulations would be somewhat sensitive to time lag. Such a formulation is worth pursing once the oceanographic community has confidence in the interannual variation of OHC.

We address the units of Ω 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.

We regret the reviewer became uneasy reading our paper. The phrase "the term OHE in Eq. (5) represents the rise in ocean heat content (OHC)" that appears on page 19, lines 19 and 20, could have been the source of confusion. This phrase will be changed to read "the term OHE in Eq. (5) represents the time derivative of ocean heat content (OHC)"

The units of OHE are in W m^{-2}, as can be seen from Eq. 6 and 7.

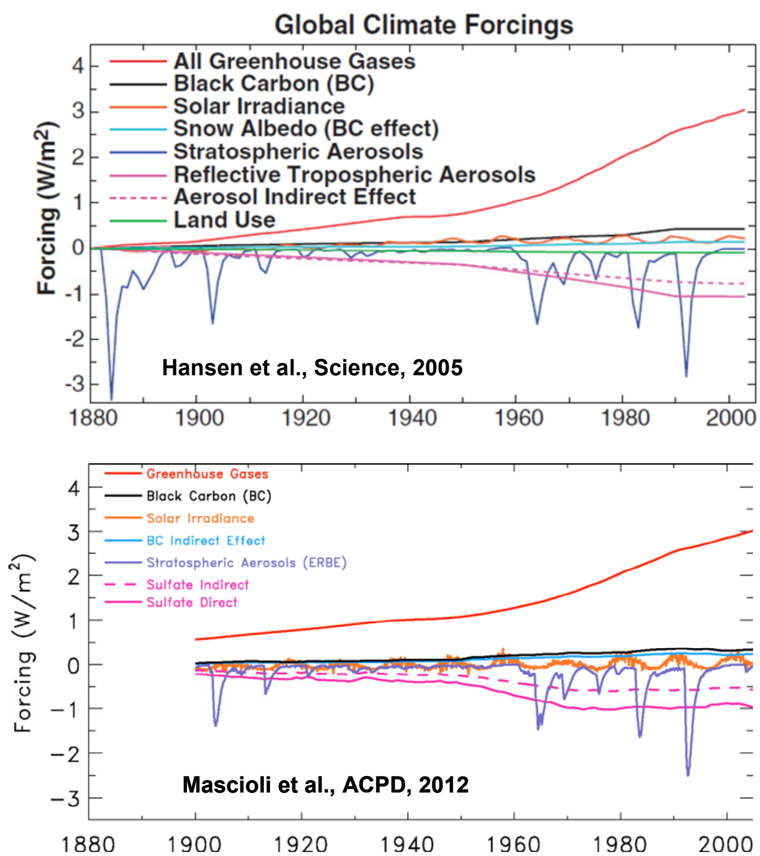
Also, appendix A of Canty et al. (2012) states:

> OHE: Ocean Heat Export (the derivative of OHC); units:W m^{-2}

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 "SO2" [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.

We will address this comment in two parts. First, we will address sulfate. Then, we will address SOA.

Sulfate: The radiative forcing used in our model for anthropogenic aerosols, particularly sulfate, is consistent with how this term is presently handled by the GCM community. The figure below compares radiative forcing terms used in our model (lower panel) to radiative forcing terms from a GCM, published by Hansen et al. (Science, 2005).



We have matched line types and styles so that the curves are directly comparable. This figure was formed after getting the review; we did not use this figure to guide our model development. However, we will probably add this figure to the Supplemental Material of Canty et al. (2012), given its importance.

The reviewer raises three issues regarding sulfate RF. The most pertinent, in our opinion, is our use of 0.96 W m^{-2} cooling for the total RF due to sulfate in 2005. We regret that we keep referring to our companion paper Canty et al. (2012) in this reply, but Figures 2 and 3 of Canty et al. were designed to document the validity of this numerical value for sulfate cooling. One can see, by inspection of the figure given above, that sulfate is handled nearly the same in our model as reported by Hansen et al. (2005). We have based sulfate cooling in 2005 on three papers, Stern (Comput. Stat. Data An., 2005), Stern (Chemosphere, 2006), and Smith et al. (ACP, 2011), as described in Section 3.2.2 of Canty et al.

The second concern is the fairly complicated expressions that relate sulfate RF to sulfur emissions. We agree these terms are complicated and prior to submission we had exchanged a considerable amount of email with Malte Meinshausen and David Stern to try to understand the physical reasons for the non-linear relation between sulfate RF and sulfur emissions. Apparently the models take into account the effect of smoke stack height on the relation between these two terms. Regardless, we feel compelled to follow the established relations, even though they are complicated. The established relation is also used, for example, by Kaufmann et al. (PNAS, 2011).

The third concern we picked up from the use of “SO2 [sic]” in the review. We had used a label of SO_2 for Fig 2a of the submitted paper. Clearly, this curve should be labeled Sulfate due to the chemical transformations that take place in the atmosphere. We will change the label of Fig 2a to Sulfate, upon revision.

SOA: The reviewer is certainly correct that Zhang et al. (GRL, 2007) and Jimenez et al. (Science, 2009) and other groups have shown that organic aerosols can have a large, previously unappreciated affect on radiative forcing.

In our model framework, we used only the Radiative Forcing from Organic Carbon provided by the RCP Potsdam website, which does not include Secondary Organic Aerosol. No estimates of RF due to SOA are given in Chapter 2 of IPCC (2007), a point emphasized in a paper entitled “Anthropogenic Influence on SOA and the Resulting Radiative Forcing” by Hoyle et al. (ACP, 2012). Paragraph 15 of Zhang et al. (2007) states SOA is only explicitly simulated in 1 of 16 models from the AeroCom initiative.

Most models as well as IPCC do not consider the RF due to SOA. This could be due to one of two factors: either the difficulty of representing the chemical transformations needed to simulate SOA and/or the uncertainty as to the anthropogenic contribution to SOA.

Zhang et al. (2007) characterize Organic Aerosol into two classes: OOA (oxygenated organic aerosol) and HOA (hydrocarbon-like OA). They note HOA “has been linked to primary combustion emissions (mainly from fossil fuel)”. Figure 2 of this paper shows that HOA is present only in urban regions. Samples obtained in remote, rural regions show little or no HOA. Hoyle et al. (2012) have attempted to estimate the change in SOA RF due to industrial activity. They reported a rather small global effect, between 0.06 and 0.09 W m^{-2} cooling, for pre-industrial time to present day. However, Figure 3 of Hoyle et al. does show significant RF due to SOA on small regional scales, from pre-industrial times to present, near centers of either industrial activity or biomass burning.

The global anthropogenic component of SOA estimated by Hoyle et al. (2012) is too small to warrant inclusion in our model. The natural component, if nearly constant over time (i.e., if not strongly altered due to either land use change or ecological change), should have no affect on the global temperature anomaly. On the other hand, regional models of climate change should address RF by SOA in the future.

> **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.

The sentence in question had read:

> The total RF due to aerosols is much larger than direct aerosol RF, due to many feedbacks.

We shall change the sentence to read:

> The total RF due to aerosols is much larger than direct aerosol RF, due to various effects of aerosols on the formation of clouds and precipitation (i.e., Table .7.10a, IPCC, 2007).

We note that the passage quoted above, from Bony et al. (J. Climate., 2006), does make use of the word "feedback" in the context of aerosol indirect effects. However, we agree with the reviewer's concern. We will no longer use "feedback" in the context of "indirect effect" anywhere else in the paper.

> **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.

The red line on the panel labeled "anthropogenic forcing" on Figure 5 shows $(1+\gamma)$ ×(GHG RF): since the left hand side had used NAA $RF_{2005} = -0.4$ W m^{-2}, this regression required small feedback ($\gamma = 0.16$ in the submitted paper) and the red line shown in Figure 5 was not too different than the total GHG RF term plotted in Figure 1. The right hand side had used NAA $RF_{2005} = -2.2$ W m^{-2} and the regression needed strong feedback ($\gamma = 0.73$ in the submitted paper) to simulate the global temperature anomaly. As a result, the term $(1+\gamma)$ ×(GHG RF) on the right hand side of the "anthropogenic forcing" rung of the ladder plot is quite different than the red line on the left hand side of the plot.

The caption to Figure 5 did state "The Anthropogenic Forcing rung shows $(1+\gamma)$ (GHG RF) (red), NAA RF (blue), and Net RF (gold).

Upon revision, we will be sure the meaning of the red lines is clear in the text as well as the figure caption.

Some comments on the presentation

Abstract: I do not get a real sense of the paper from the abstract. I get a vague, non quantitative 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.

We agree the abstract was not particularly well written. We would like to use the following text for our abstract that better conveys what was found in our study:

We use an empirical model of climate based on a multiple linear regression analysis of the century-long global surface temperature record to quantify the impact of three factors on future warming due to rising levels of greenhouse gases (GHGs): net anthropogenic aerosol radiative forcing (NAA RF), feedback (water vapor, lapse rate, clouds, surface albedo) in response to radiative forcing of climate (λ), and ocean heat export. We show that the future rise in temperature at the time CO_2 is projected to double is inherently uncertain due to the cantilevering of aerosol radiative forcing and climate feedback coupled with the projection that aerosol radiative forcing will diminish in the coming decades due to air quality concerns. If anthropogenic aerosols presently exert small net global cooling, climate feedback must be weak and the future rise in global average surface temperature when CO_2 doubles could be moderate, less than 1.0°C. If aerosols presently exert large net global cooling, climate feedback must be large and future temperature when CO_2 doubles could be substantial, more than 2.0°C. In our model, warming at the time CO_2 doubles is nearly independent of ocean heat export. Nonetheless, ocean heat export is important because the relation between anthropogenic aerosol radiative forcing and climate feedback is extremely sensitive to ocean heat export. However, analyses of output from various global climate models (GCMs) commonly do not reveal a strong relation between aerosol radiative forcing and climate feedback. We suggest this relation has been obscured by the failure to examine aerosol radiative forcing, climate feedback, and ocean heat export in a common framework. We also critique the use of Equilibrium Climate Sensitivity (ECS) as a metric to evaluate GCMs. In our framework, if ocean heat export is small then modeled temperature at the time CO_2 doubles lies close to computed ECS, whereas if ocean heat export is large then temperature when CO_2 doubles is much smaller than ECS. Future use of ECS to evaluate GCMs should consider the sensitivity of this metric to ocean heat export.

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).

Excellent point! Upon revision, we will define all of the terms used in Eq. 2 right below where the equation is given. We had included a glossary of terms in the appendix of Canty et al. (2012), in recognition of the frustration readers could have with so many terms. Hopefully the two papers will be published together, so that the glossary will serve both papers.

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.

The sentence reads:

Since the calculations shown in our paper can be conducted in an afternoon on a modern work station, a MLR model used as described below could provide a useful complement to ensemble GCM runs that require enormous, community-wide effort.

Later in the paper we had provided a proper caveat to the notion that "the calculations could be done in an afternoon on a workstation", and expressed the obvious importance of GCMs. On page 38 it had been written:

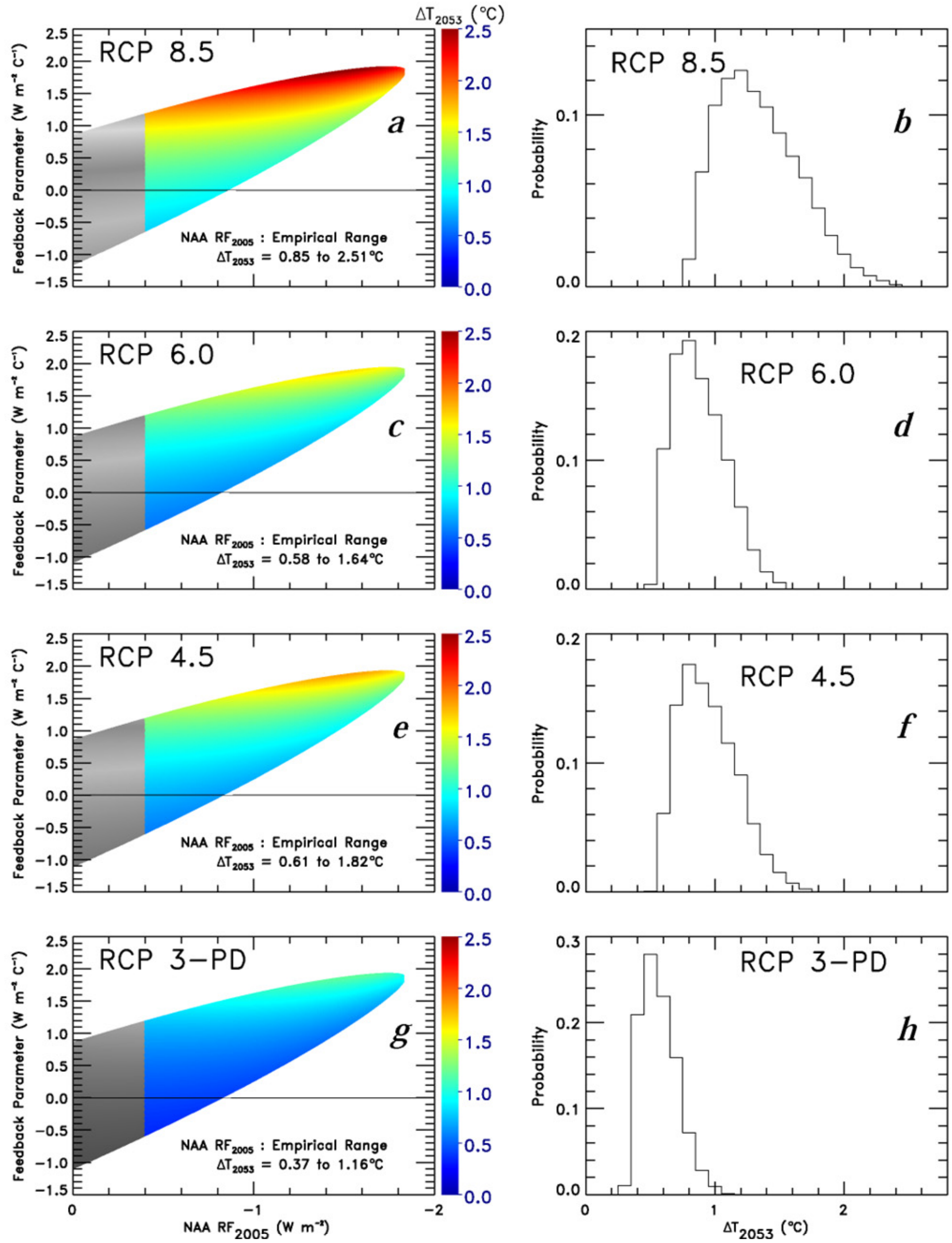
Advantages of the regression model are that all calculations shown in this section can be carried out in an afternoon on a modern work station and we have ready access to, and control over, model parameters. Of course, GCMs represent many quantities of societal interest, such as precipitation, drought, sea ice extent, sea level, etc that are not represented by our model.

Upon revision, we will move the caveat sentence to the Introduction, which will hopefully address this concern.

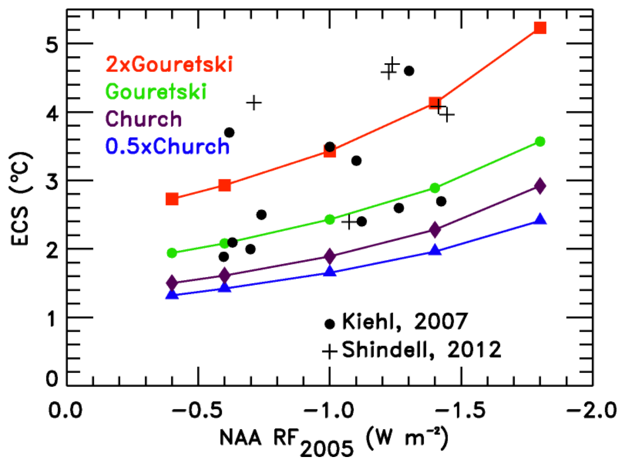
We prefer to leave the notion of calculations being able to be done in an afternoon in the paper, because we think it is perhaps the most important reason why our work does indeed offer a "useful complement to ensemble GCM runs". Climate projections shown in Figures 10, 11, 14 and 15 involved on order 100,000 simulations sweeping parameter space of anthropogenic aerosol radiative forcing and climate feedback. Figure 9 sweeps parameter space of ocean heat content.

Our new version of Figure 10 (next page) includes probability distribution functions of future temperature for year 2053, the time CO_2 will double according to RCP 8.5. The model framework we have developed can quickly generate plots such as these for any combination of future GHG abundance or aerosol precursor emission scenario the community may want to investigate. While this would of course never replace GCMs, an ability to quickly explore model parameter space could serve as a useful complement to climate models.

Finally, our new version of Figure 9 (page after next) will now include values of Equilibrium Climate Sensitivity (ECS) and Net Anthropogenic Aerosol Radiative Forcing (NAA RF) from GCMs. The sweeping of parameter space afforded by a computational efficient model reveals the existence of a family of curves for ECS vs NAA RF, dependent on how Ocean Heat Export is handled. We suggest the neglect of the OHE dependence of the ECS vs NAA RF relation is why the cantilevering of climate feedback and NAA RF is not found by studies as Shindell et al. (ACPD, 2012). We think therefore that Figure 9 is complementary to GCMs.



Proposed revision to Figure 10, Mascioli et al, ACPD, 2012, would include panels showing the probability density function (PDF) of future anomalies of global temperature for year 2053, the time CO2 will double according to RCP 8.5. The PDFs are simply histogram representation of the data in the acceptable fit ellipse for each emissions scenario.



Proposed revision to Figure 9, Mascioli et al, ACPD, 2012, would now include data points showing values of Equilibrium Climate Sensitivity (ECS) versus Net Anthropogenic Aerosol RF (NAA RF) for the contemporary atmosphere from GCMs.

> **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?

While this is perhaps not what the reviewer had requested, upon revision, we would like to change the denominator of Eq. (5) to read:

$\langle (1 + \gamma) \{\text{GHG RF} + \text{NAA RF} \}\rangle_{\text{Time Init to Time Final}}$

which is slightly more compact. The $\langle \ldots \rangle_{\text{Time Init to Time Final}}$ notation is common in physics and the use of actual numerical values for "Time Init" and "Time Final" in Eq. (6) and Eq. (7) is and important part of our model calculation that must be specified for readers (i.e., since the Church et al. (2011) and Gouretski and Reseghetti (2010) observations of OHC are for different time periods, this information must be conveyed and can not be incorporated into the definition 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.

The sentence in question reads:

> Equation (5) is used to determine Ω, fraction of anthropogenic RF perturbation to climate (AF) exported to the ocean over the time period of OHC observation.

We will change the sentence to read:

Equation (5) is used to determine Ω, fraction of anthropogenic RF perturbation to climate (AF) that is expended by heating the ocean rather than the atmosphere.

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?

Yes, PDO and IOD show little covariance with global temperature. The physical reasons why the PDO and IOD show little covariance with global T are discussed on page 23838 and 23839 of Canty et al. (2012). Briefly, the PDO is a response to local wind patterns, rather than an indicator of major release (or uptake) of oceanic heat at a magnitude important for global climate. There is little effect of the IOD on global climate due to the size of the Indian Ocean as well as the fact that oceanic deep water does not originate from the Indian Ocean.

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

We are not sure what the reviewer is suggesting. Our work truly does build on that of Lean and Rind (2009), so the second sentence of Section 3.1 is solid. We feel we have taken Multiple Linear Regression analysis of the climate record much further than had been done in Lean and Rind (2009), which is what the next few sentences are trying to convey.

While contrasting our work to that of a prior study may seem like an unusual way to being a section entitled "Overview of Calculations", it affords us a chance to list the most important new aspects of our study: examination of uncertainties in aerosol radiative forcing and ocean heat export and use of a statistical quantity, reduced chi-squared, that provides a metric for the examination of "acceptable fits" to the climate record. We would therefore like to retain this compare/contrast to Lean and Rind at the top of Section 3.1. We will gladly move the compare/contrast the end of Section 3.1 if given editorial direction to make this change.

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.

The term "ladder plot" is our terminology we have used to define the stacked graphs that constitute the numerical depiction of a multiple linear regression calculations. The sense of building on the next comes from the fact that, for Figure 5 (illustrated above), the modeled curve for ΔT is simply the sum of the various components of ΔT shown in the rungs labeled Volcanic, Human, ENSO, and Atlantic (plus the constant term).

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.

We had been aware of this paper, which is cited four times in the Canty et al. (2012) companion paper.

We sincerely thank the reviewer for the time and effort placed into the comments, and for providing pointed, constructive criticism that we are confident will improve the final product. If the paper does proceed, we will certainly provide an acknowledgement to the reviewer. Should the reviewer decide to be acknowledged by name, we would be happy to comply.