We thank Alan Robock for submitting a comment on our paper, even though he is critical of our conclusions, because this provides a forum to delve further into key aspects of volcanic cooling. We also applaud ACPD for allowing on-line dialog during the review process. If our paper proceeds to ACP, it will be considerably stronger due to the effort expended in the response to critical comments such as those offered by Alan.

> Alan wrote: I think this paper has fundamental errors, and the conclusion is wrong.

Our paper provides strong scientific evidence that the statement on page 97 of IPCC (2007) "major volcanic eruptions can thus cause a drop in global mean surface temperature of about half a degree Celsius that can last for months and even years" is not correct. The response of climate to the eruption of Mt. Pinatubo is commonly used as a surrogate for how climate might respond to geo-engineering via injection of sulfur to the stratosphere. For instance, Crutzen (2006) states "enhanced reflection of solar radiation to space by the particles [resulting from the stratospheric injection of sulfur by Mt. Pinatubo] cooled the earth's surface on average by 0.5°C in the year following the eruption" and uses the mass of sulfur deposited into the stratosphere by Pinatubo as starting point for his estimate of the cost of geo-engineering.

Our analysis shows the maximum reduction of global mean surface temperature following the eruption of Mt. Pinatubo **that can be attributed to excess sulfate loading in the stratosphere**, termed $\Delta T_{PINATUBO}$, actually **lies between about 0.12 and 0.30°C**. The global lower atmosphere many have cooled by 0.5°C following the eruption of Mt. Pinatubo (e.g., Fig. 2 of Robock, 2003 as well as Fig. 11 of Canty et al., 2012), but the heat capacity of the lower atmosphere is less than the heat capacity of the global surface (which includes the ocean skin). Surface temperature over land may have fallen by 0.5°C (Fig. 7, Canty et al.), but the heat capacity of over land is smaller than heat capacity over the ocean. If, as we suggest, **global surface temperature** did not drop by 0.5°C following the eruption of Mt. Pinatubo, then the use of Mt. Pinatubo as a proxy for geo-engineering of climate must be re-calibrated.

We address all of Alan's comments, but in a different order than they were provided to facilitate a more natural sequence for the new figures in this response.

> 2. Equation (2) is wrong. Lambda needs to be multiplied by (1+ gamma) for the aerosols, too. The sensitivity of the climate system is an expression of how it responds to radiative forcing, no matter what the source. They are assuming no feedbacks in the climate system for aerosols, and this is clearly wrong.

It is debatable whether radiative forcing (RF) due to GHGs should be multiplied by the same feedback term as radiative forcing due to aerosols. We'll address this debate below. Regardless, our scientific conclusion that the effect of volcanoes on globally averaged surface temperature has been overestimated is not affected by whether NAA RF (net anthropogenic aerosol radiative forcing) is multiplied by $(1+\gamma)$. Here, γ represents feedbacks that occur in response to a perturbation of RF at the tropopause and is formally related to the climate feedback parameter used by Bony et al. (2006) and Section 8.6 of IPCC (2007) via:

$$1 + \gamma = \{ 1 - \lambda / \lambda_p \}^{-1}, \text{ where } \lambda_p = 3.2 \text{ W m}^{-2} \text{ °C}$$

All of the figures included in this reply include best fit values of λ found by allowing the term $\{1 - \lambda / \lambda_p \}^{-1}$ (or equivalently $1 + \gamma$) to multiply **<u>both</u>** GHG RF and NAA RF.

The debate regarding whether NAA RF should be multiplied by the same feedback term as GHG RF is centered on two issues:

> i) the perturbation to RF of climate induced by well mixed GHGs is vastly different in physical nature than the perturbation induced by aerosols, which exhibit strong spatial variability in both the horizontal and vertical dimensions [e.g., Leibensperger et al., 2012; section 7.5 of IPCC (2007)]
>
> ii) the largest uncertainty in the radiative forcing of climate due to aerosols is the magnitude of the various aerosol indirect effects, which are represented by our scaling parameters α_{COOL} & α_{HEAT}. These scaling parameters have values considerably larger than $(1+\gamma)$. If one were to write the governing equation as:
>
> $$\Delta T_{MDL\ i} = (1+\gamma_{GHG})\ (\text{GHG RF}\ _i) + (1+\gamma_{AEROSOLS})\ (\text{NAA RF}\ i)$$
>
> Then clearly the term $(1+\gamma_{AEROSOLS})$ could be folded into the scaling parameters used to define total RF due to aerosols, which is what we assumed for our submitted paper.

Bony et al. (2006), the source of Section 8.6 of IPCC (2007), state (our emphasis):

> On the other hand, **we will not consider the feedbacks associated with the response to temperature** of the carbon cycle or **of aerosols** and trace gases, nor those associated with soil moisture changes or ocean processes, although these processes might have a substantial impact on the magnitude, the pattern, or the timing of climate warming.

The values of the climate feedback parameter shown in Section 8.6 of IPCC (2007) were found by considering the **response of climate within GCMs to GHGs <u>and not aerosols</u>.**

For our submitted paper we had decided to not multiply NAA RF by $(1+\gamma)$ because the largest uncertainty in RF of climate due to aerosols is the magnitude of the various indirect effects, handled by our scaling parameters α_{COOL} & α_{HEAT}. We do not understand how Alan could write "they are assuming no feedbacks in the climate system for aerosols, and this is clearly wrong" because in the submitted paper we described in considerable detail that α_{COOL} and α_{HEAT} represented both indirect effects and feedbacks due to the RF of climate by aerosols.

The review of Mascioli et al. (2012) (available on line) also pointed out that NAA RF should be multiplied by $(1+\gamma)$. Therefore, we have revised the model formulation used in Mascioli et al. (2012) (to be returned to ACPD prior to the 24 Dec 2012 deadline) such that NAA RF is now multiplied by $(1+\gamma)$. We shall make the same change to Canty et al. (2012) should this paper progress to ACP. We question the validity of treating feedbacks due to GHGs and aerosols with the same term, but this detail does not affect the scientific conclusions of either study because both α_{COOL} and α_{HEAT} are considerably larger than $(1+\gamma)$ and α_{COOL} and α_{HEAT} are varied over a wide range of values.

> 3. Douglass and Knox (2005) got it terribly wrong. This was shown by Wigley et al. (2005b), which is referenced, and by Robock (2005), which is not. There are multiple time scale responses to volcanic eruptions, so a simple linear regression like done in this paper does not do a good job of simulating the climate response.
>
> Robock, Alan, 2005: Comment on "Climate forcing by the volcanic eruption of Mount

Pinatubo" by David H. Douglass and Robert S. Knox. Geophys. Res. Lett., 32, L20711, doi:10.1029/2005GL023287.

4. The volcanic lag is wrong. The forcing develops faster than 6 months.

Comments 3 and 4 are related so we shall reply to both.

Douglass and Knox (2005) is cited three times in our paper. The first two citations, on line 16 of page 23840 and line 14 of page 23841, are for their determination of a 6.8 ± 1.5 month lag between the response of global mean surface temperature to the perturbation of stratospheric optical depth (SOD) caused by the eruption of Mt. Pinatubo. Use of a 6 month delay between SOD forcing and global temperature response is consistent with other published regression analyses (e.g., Lean and Rind, 2008; Foster and Rahmstorf, 2011) and is supported by an analysis of the temporal response to volcanic forcing (Thompson et al., 2009). Use of a ~6 month time for the response of the climate system to a volcanic perturbation is not "terribly wrong".

The third citation of Douglass and Knox is on page 23867, where we state (emphasis added):

> Douglass and Knox (2005) conducted a regression analysis of MSU lower tropospheric temperature measurements and concluded the atmosphere exhibited a negative feedback following the eruption of Pinatubo. This paper has been discussed in a series of published comments and replies following initial publication. We stress our model always requires positive feedback, as reflected in values of always exceeding zero, and **hence our findings are inconsistent with those of Douglass and Knox (2005)**.

We had clearly stated that our model results are inconsistent with the negative feedback on surface T after Pinatubo suggested by Douglass and Knox (2005). This was the source of controversy discussed in the comments by Robock (2005) and Wigley et al. (2005).

We have cited Wigley et al. (2005) on line 25, page 23868 for a matter other than whether "Douglas and Knox got it terribly wrong". Our submitted paper also states "this paper [Douglass and Knox] has been discussed in a series of published comments and replies following initial publication". We prefer to not change our discussion of Douglass and Knox because the series of comments and replies to this paper is not central to our hypothesis. If our paper proceeds through the ACP process and we receive editorial guidance to delve more deeply into comments and replies related to Douglass and Knox (2005), we will gladly modify our paper to included detailed discussion of these 4 additional papers, including Robock (2005).

We shall now turn our attention to the gist of the matter: Alan's comment that "volcanic forcing develops faster than 6 months" and his criticism that Douglass and Knox (2005) "got it terribly wrong" because, as stated in Robock (2005), they "ignored energy exchange with the deep ocean" and "confused response with the forcing."

As described in an erudite manner by Robock (2005) and quantified by Thompson et al. (2009), the response of the global mean surface temperature anomaly to a sharp perturbation such as the rapid rise in SOD induced by a major volcano is governed by a first order differential equation (Thompson's eq. 5) that leads to a response (Thompson's $T_{VOLCANO}$) that is asymmetric and broader than the forcing. Thompson et al. (2009) also computed the response of global temperature to the forcing by ENSO.

Figure 1 shows results of a regression analysis, using Eq. (2) and (4) of the modified formulation described above (i.e., where $\{1-\lambda / \lambda_p\}^{-1}$ multiplies both GHG RF and NAA RF) and in the revised version of Mascioli et al. (2012), where instead of using SOD and a multivariate ENSO index in the regression, we instead use $T_{VOLCANO}$ and T_{ENSO} from Thompson et al. (2009). No lag has been applied to either term because the temporal response is inherent in Thompson's calculation. As described in the revised version of Mascioli et al. (2012), our model has a physical representation of the steady rise in ocean heat content, including the fraction of radiative forcing of climate expended to heat the deep ocean.

The results shown in **Figure 1** are nearly identical to those in the submitted version of Canty et al. The maximum value of $\Delta T_{PINATUBO}$ is 0.28°C cooling for a regression that neglects the AMO (left side Fig. 1). When the Atlantic Multidecadal Oscillation (AMO) is included in the regression model as a proxy for the effect of variations in the strength of the Atlantic Meridional Overturning Circulation (AMOC) on global temperature, maximum $\Delta T_{PINATUBO}$ is 0.12°C (right side Fig.1), much less than the IPCC (2007) statement that elevated SOD following major volcanoes causes **global mean surface temperature to drop by ~0.5°C.**

The scientific community should not be surprised by the results of our regression analysis. Figure 2 of Lean and Rind (2008), Figure 9 of Thompson et al. (2009), and Figure 7 of Foster and Rahmstorf (2011) all show maximum values for $\Delta T_{PINATUBO}$ of no more than 0.3°C. All three studies were focused on the removal of the influence of effects of ENSO and volcanoes on the global mean surface temperature anomaly so the anthropogenic influence could be properly discerned. None of these studies drew attention to the disparity between their estimate of $\Delta T_{PINATUBO}$ and the statements regarding the magnitude of this term given by Crutzen (2006) and IPCC (2007).

In Canty et al. (2012), we have taken the analyses of Lean and Rind (2008), Thompson et al. (2009), and Foster and Rahmstorf (2011) one step further by suggesting the maximum value of 0.3°C for $\Delta T_{PINATUBO}$ could be too high, due to consideration of variations in the strength of the AMOC as represented by the AMO. This brings us to the following comment by Alan:

> 1a. What the authors call the AMO is actually the climate response. So they are just double counting the response. No wonder they get less of a response to volcanoes. And it is not correct to argue that using only the land record eliminates this problem. Land and ocean temperature time series are highly correlated and are connected by the atmospheric circulation. There may be a long-term oscillation of the Atlantic Ocean, but if the period is 70 years you would need 500-700 years of observations to prove it from observations. What they did is just to use SST and label it an oscillation. There is no proof that there is a physical mechanism with a 50-70 year period, because the data record is not long enough. Occam's Razor tells me they are simply counting climate change twice, and have the term on both sides of equation (2).

First and most importantly, we do not claim to have proven anything. Rather, we consider Canty et al. (2012) to "connect the dots" and offering a **suggestion** that prior estimates of $\Delta T_{PINATUBO}$ may be in error due to neglect of the climate response to variations in the strength of the AMOC. If our paper proceeds, we will scrub the text to be sure it read as if we are offering a plausible suggestion, and not "proof".

The title of Alan's comment "Conclusions wrong, since temperature is being double counted" as well as the first two sentences above strike at the key uncertainty in our suggestion. If SST in the North Atlantic was determined mainly by the eruption of Mt. Pinatubo (and other major volcanoes), then our use of the AMO as a proxy for variations in the strength of the AMOC would be in error. In this case, we would be confusing volcanic cooling with the climate response to variations in the strength of the AMOC.

Potential confusion of the cooling due to volcanoes and AMOC was discussed extensively in the submitted paper. We presented several figures that show temperature in the North Atlantic had entered a negative phase (i.e., North Atlantic SST tended to be lower than average) **prior to** the eruptions of Santa María, El Chichón, and Pinatubo. We had stated, but not shown, that "the regression coefficient for AMO is driven by data collected at times other than the SOD perturbation, which we have confirmed by removing data collected during times of peak SOD and repeating the analysis" (this is shown below). We also summarized the extensive debate in the scientific literature on whether the strength of the AMOC and ocean heat content is altered by major volcanic eruptions.

Figure 2 is designed to address Alan's contention that our conclusions are wrong because temperature has been double counted. The left hand side shows a regression computed in the same manner as Figure 1 (T_{ENSO} and $T_{VOLCANO}$ from Thompson et al. 2009; no lags for these terms), except data collected during the time of elevated $T_{VOLCANO}$ have been excluded. The similarity of the regression coefficient for the AMO in this calculation, 0.336 °C °C^{-1}, to the value of 0.352 °C °C^{-1} found for the full regression (Figure 1), shows this model parameter is driven by the low frequency, high amplitude portion of the AMO signal and is not driven by the value of the AMO during volcanically perturbed time periods.

The right hand side of **Figure 2** shows a calculation where the value of the AMO has been held constant during time periods when the climate has been altered by major volcanic eruptions, at the value that existed prior to each of the four major eruptions since 1900. The maximum value of volcanic cooling following the eruption of Mt. Pinatubo is 0.19°C for this regression. If we conduct a regression with AMO "flat lined" during the major volcanic eruptions using the approach in the submitted paper (forcings for volcanoes from reconstructed SOD, with response lagging forcing by 6 months; forcing for ENSO from the multivariate index of Wolter and Timlin (2011) with a 2 month lag), we find the maximum global cooling attributed to the eruption of Mt. Pinatubo is 0.22°C.

We find the portion of cooling attributed to the eruption of Mt. Pinatubo is either 0.19°C (using Thompson's response curves) or 0.22°C (using the approach in our paper) with the AMO "flat lined" during times of high SOD. Both values are more than a factor of two below the 0.5°C value for the drop in global mean surface temperature following Pinatubo quoted by Crutzen (2006) and IPCC (2007). If a portion of the actual decline in North Atlantic SST after the eruption of Pinatubo was indeed a continuation of the downward trend that had begun prior to the eruption, then volcanic cooling would be less than our estimates of 0.19°C or 0.22°C found using an AMO index "flat lined" during volcanically perturbed periods.

Alan's statement "land and ocean temperature time series are highly correlated and are connected by the atmospheric circulation" is certainly correct and is the basis for our suggestion that volcanic cooling has been overestimated! We disagree, however, with Alan's statement "it is not correct to argue that using only the land record eliminates this problem [of double counting]."

The global mean surface temperature anomaly is composed of ocean (SST) and land temperature records. A portion of the SST record is contributed by the North Atlantic. Hence when the global mean surface temperature anomaly is examined, SSTs in the North Atlantic appear in both the forcing and the response. In the submitted paper, we provide comprehensive analysis of the temperature anomaly over land. If the AMO is detrended using either a linear fit or anthropogenic radiative forcing of climate and if either of these detrending methods represent a true proxy for the strength of the AMOC, variations in land temperature attributed to the AMOC have about twice the amplitude as variations due to ENSO. Of course this is due to the fact that land temperatures are connected to the ocean by atmospheric winds. And there is no double counting when the land temperature is examined: the AMO is derived solely from SST and the land temperature record is computed solely from surface stations.

The North Atlantic likely exerts strong influence on global climate because this is the region of strongest deep water formation. During periods of time when deep water formation is high, oceanic heat is advected poleward in the NH, leading to a strong transfer of heat to the atmosphere at mid-latitudes (Srokosz et al., 2012). Surface temperature over North America and Europe responds to multi-decadal variations in the strength of the AMOC as revealed by the AMO, particularly during summer (Sutton and Hodson, 2005). Alan states "there is no proof that there is a physical mechanism with a 50-70 year period". While have noted above we are not claiming to have proven anything, we shall point out the presence of a 50 to 70 year periodicity in the 230 year long sortable silt (SS) grain size record that Boessenkool et al. (2007) invoke as a proxy for the strength of the Iceland-Scotland Overflow (Figure 2 of their paper). A 180 year long reconstruction of ice export in the Fram Strait, which influences the salinity of the North Atlantic, shows a similar 50 to 70 year periodicity (Figure 5 of Schmith and Hansen, 2003). The SS record of Boessenkool et al. (2007) as well as the ice storis record of Schmith and Hansen (2003) provide a physically consistent picture that the strength of the AMOC does indeed vary with a 50 to 70 year period.

Alan's comment began "what the authors call the AMO is actually the climate response", continues "there is no proof that there is a physical mechanism with a 50-70 year period" and concludes "Occam's Razor tells me they are simply counting climate change twice". **Figures 3** and **4** are offered to support our contention that the AMO represents **internal variability** of the climate system that is **critically important for understanding the global temperature anomaly**, particularly in the NH. These figures are identical to Figure 1, except **Figure 3** shows results of a regression for the NH temperature anomaly and **Figure 4** shows results for the SH.

Srokosz et al. (2012) state:

> an AMOC weakening will lead to a cooling over the North Atlantic and adjacent land regions, or to a reduction in the rate of temperature increase associated with global warming. A weakened AMOC is typically accompanied by a slight warming of the Southern Hemisphere, though details differ between models.

The regressions shown in **Figures 3** and **4** show a stronger contribution of the AMO to the NH temperature record than the SH record. Contrary to the statement in Srokosz et al. (2012) regarding the behavior of models, these regressions show the response to AMO is "in phase" between NH and SH (both regression coefficients are positive) (note: Srokosz et al. state details of the NH / SH response differ between models). In our analysis we have not addressed hemispheric asymmetry in RF due to aerosols (beyond the scope of this reply), so these regressions should be interpreted with caution. Nonetheless, these figures show more rapid warming in the NH than the SH, as has been noted in other studies, and a stronger influence of the AMO on the NH temperature record, as one would expect from GCM simulations due to smaller heat capacity of land relative to ocean and greater fraction of land in the SH (Xu and Ramanathan, 2012). The influence of volcanoes is stronger in the NH than the SH, perhaps for the same reason that the NH is warming more quickly than the SH. The Earth Radiation Budget Experiment (ERBE), available only for Mt. Pinatubo, shows that for the SH poleward of 20°S, the trapping of long wave radiation by volcanic aerosol nearly balanced reflection of solar radiation (Fig. S7, Canty et al., 2012). This factor likely also contributes to the hemispheric asymmetry in the response to the eruption of Mt Pinatubo

Occam's razor, invoked by Alan, states "among competing hypotheses, the one that makes the fewest assumptions should be selected". The regression on the left hand side of **Figure 3** has fewer regressor variables. The contribution of Mt. Pinatubo to the NH temperature anomaly maximizes at 0.44°C. Large excursions in temperature, such as pre-WWI cooling and post-WWII heating, are simulated poorly by the model. The regression on the right hand side includes AMO, PDO, and IOD: the only new regressor variable that matters is the AMO. The model simulates rather well the pre-WWI cooling and post-WWII heating. For this regression, the contribution of Mt. Pinatubo to the NH temperature anomaly maximizes at 0.20°C. Earth's climate is complex, consisting of a variety of external forcings that lead to responses on different of time scales as well as internal variability due to atmospheric and oceanic interactions that can, we suggest, lead to considerable fluctuations of global mean temperature. Srokosz et al. (2012) and Dima and Lohmann (2007) provide nice overviews of the atmospheric and oceanic interactions related to the AMOC. Those who have read this far can decide for themselves if they favor the simulation guided by Occam's razor (left hand side of **Figure 3**) or the simulation that attempts to represent the complexity of the climate system (right hand side of **Figure 3**).

1b. This was also addressed by Mann and Emanuel (2006):

ftp://texmex.mit.edu/pub/emanuel/PAPERS/EOS_mann_emanuel_2006.pdf

The submitted version of our paper is **entirely consistent** with the arguments put forth by Mann and Emanuel (2006). Like Mann and Emanuel, we criticize the use of linear detrending for the AMO. Like Mann and Emanuel, we recognize there is a strong correlation between the AMO and global SST. We present results throughout our paper using global SST to detrend the AMO.

Mann and Emanuel (2006) stated "yet the only direct oceanic measurements available suggest a decrease, not an increase, in the THC (thermohaline circulation) between the late 1950s and the past decade (Bryden et al., 2005)". Willis (2010) critiques the Bryden et al. (2005) study and states "A small trend (in the strength of the AMOC) is present in the altimeter-based estimate, with the overturning increasing by 2.4 ±1.6 Sv over the 16-year period" of observation.

We have quantitatively assessed the same "end points" (detrending using a linear regression; detrending using global SST) that Mann and Emmanuel (2006) state are significant for the science regarding the AMO. We offer a third possibility, detrending using anthropogenic radiative forcing of climate, that leads to regression results similar to those found when the AMO is detrended using a linear fit.

5. The uncertainty in indirect aerosol forcing is huge. How does this affect the results?

The uncertainty in the response of the volcanically induced decline of globally averaged surface temperature ($\Delta T_{VOLCANO}$) to the indirect aerosol effect was addressed in Figure 9 of the submitted paper, where NAA RF_{2005} is used as the abscissa, and in Figure 10, where "the thin blue lines are a root sum of squares combination of the statistical uncertainty and the variation in C_1 due to uncertainty in NAA RF_{2005}."

The uncertainty in $\Delta T_{VOLCANO}$ due to indirect aerosol forcing is considerable, but is not "huge". Most importantly, this uncertainty was addressed in two figures and many paragraphs of our submitted paper.

6. the correct spelling is "Krakatau" and not "Krakatoa.

Two spellings are given at http://en.wikipedia.org/wiki/Krakatoa. Alan has provided the Indonesian spelling, which we are happy to adopt.

7. the word "data" is plural, but in many (but not all) places in the paper you treat it as singular.

We are aware that the word "data" is plural. We have found two instances where the tense was misused, which we shall correct. Most instances when the word "data" appeared, it had been used as an adjective, in the form of "data set" for instance. In this case the use of singular tense is correct. The plural tense should only be used when "data" appears as a noun.

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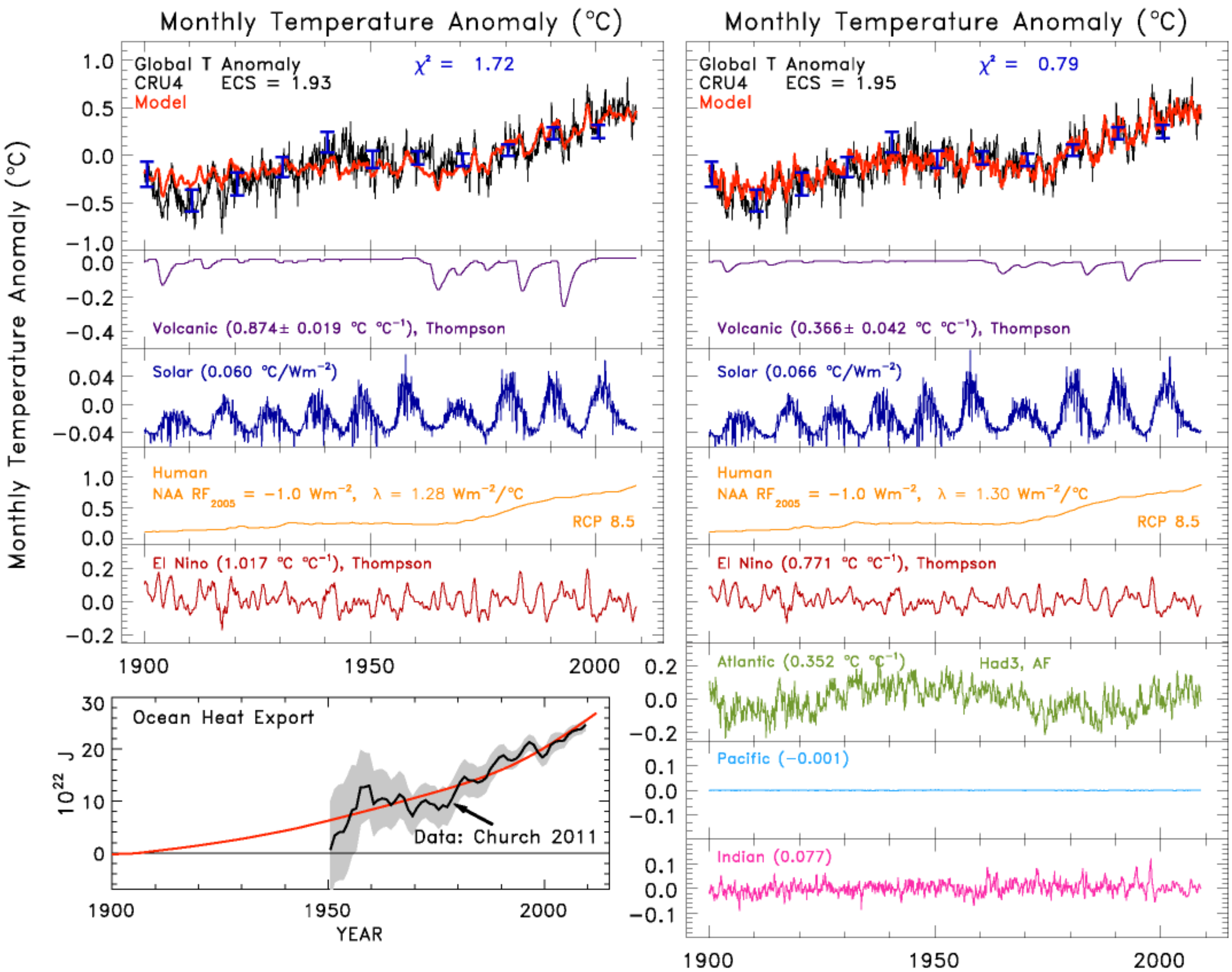

Figure 1. Same as Figure 6 of Canty et al. (2012), except GHG RF and NAA RF have both been multiplied by the same feedback term, $\{ 1 - \lambda / \lambda_p \}^{-1}$, where λ_p = 3.2 W m^{-2} °C, and rather than using time series for SOD and ENSO forcings in the regression, we use the T_{ENSO} and $T_{VOLCANO}$ response terms computed by Thompson et al. (2009), available on the web at:

http://www.atmos.colostate.edu/~davet/ThompsonWallaceJonesKennedy

No lag has been imposed for T_{ENSO} and $T_{VOLCANO}$, since the temporal response is included in the calculation of these response functions. We have imposed a 1 month lag for total solar irradiance, as in Lean and Rind (2008) as well as Foster and Rahmstorf (2011), but results are nearly identical if there is no solar lag. The best fit value of λ is noted on each ladder plot. The ladder plot on the right includes GHGs, Aerosols, and Solar Irradiance (in addition to ENSO and Volcanoes) in the regression. The ladder plot on the right adds the AMO (detrended using anthropogenic RF of climate), the Pacific Decadal Oscillation (PDO), and the Indian Ocean Dipole (IOD) to these terms. Both regressions are constrained to match the time series of ocean heat content reported by Church et al. (2011). The maximum value of volcanic cooling following Pinatubo is 0.28°C when the AMO, PDO, and IOD are neglected and is 0.12°C when these terms are considered.

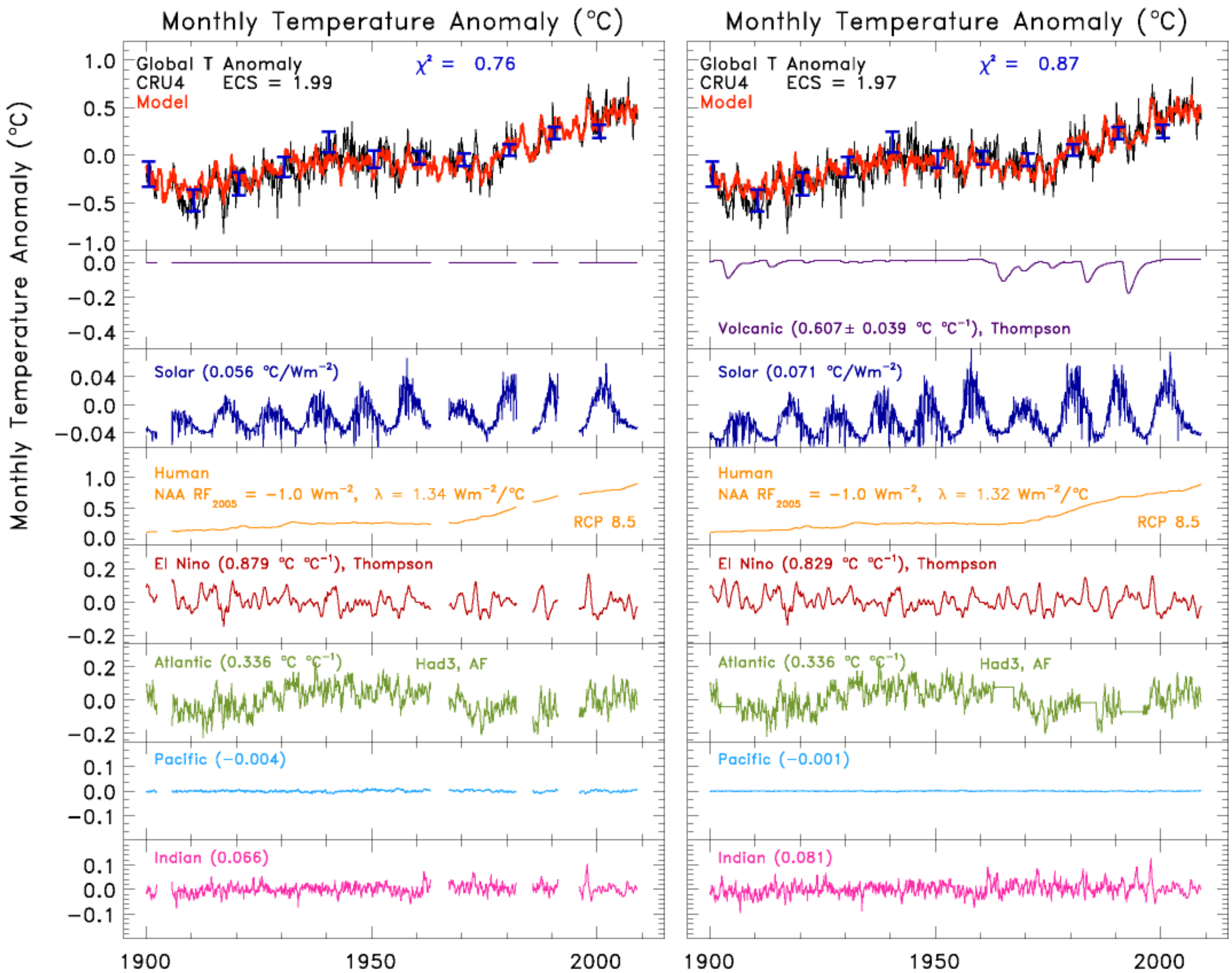

Figure 2. Both ladder plots show regressions similar to Figure 1 except:

(*left ladder plots*): data collected during times when $T_{VOLCANO}$ is elevated following the four major eruptions since 1900 are excluded from the regression and $T_{VOLCANO}$ is otherwise "flat lined" for this regression;

(*right ladder plots*): AMO is held constant when $T_{VOLCANO}$ is elevated following the four major eruptions since 1900, at the value that prevailed prior to each eruption. The maximum value of volcanic cooling following Pinatubo is 0.19°C for this regression.

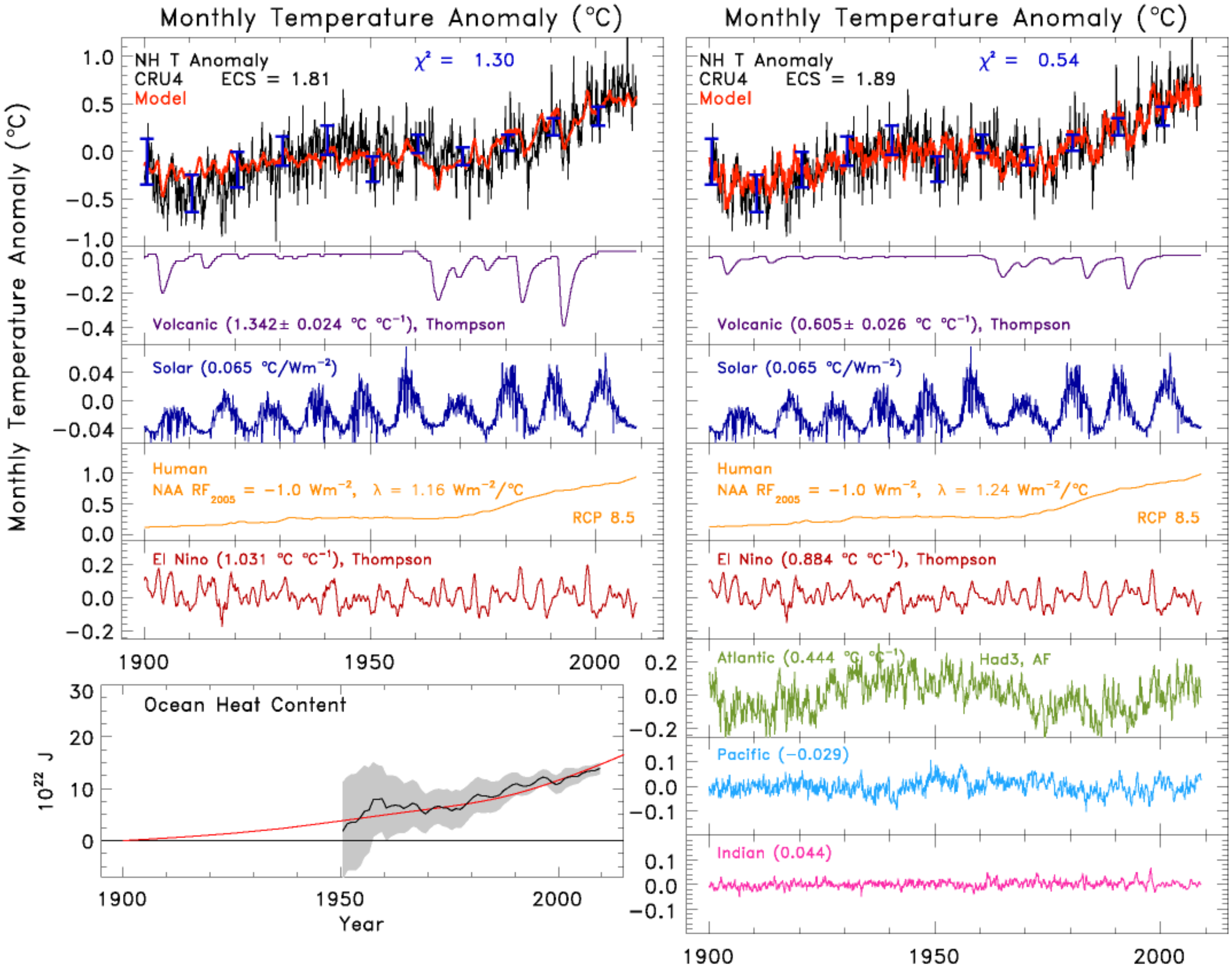

Figure 3. Same as Figure 1 except the regression has been computed for the Northern Hemisphere (NH) mean surface temperature anomaly. The regression coefficient for the solar cycle has been fixed at the same value, 0.065 °C W m^{-2}, found for the regression of the global mean surface temperature anomaly and the modeled Ocean Heat Export (OHE) term (Eq. 6 of the revised version of Mascioli et al., 2012) has been cut in half, to reflect in a gross manner that about half this flow of energy occurs in the NH. Model results are insensitive to OHE provided λ is allowed to adjust (Mascioli et al., 2012). The NH temperature record was obtained from Met Office Hadley Centre at:

http://www.metoffice.gov.uk/hadobs/hadcrut4/data/current/time_series/HadCRUT.4.1.1.0.monthly_nh.txt

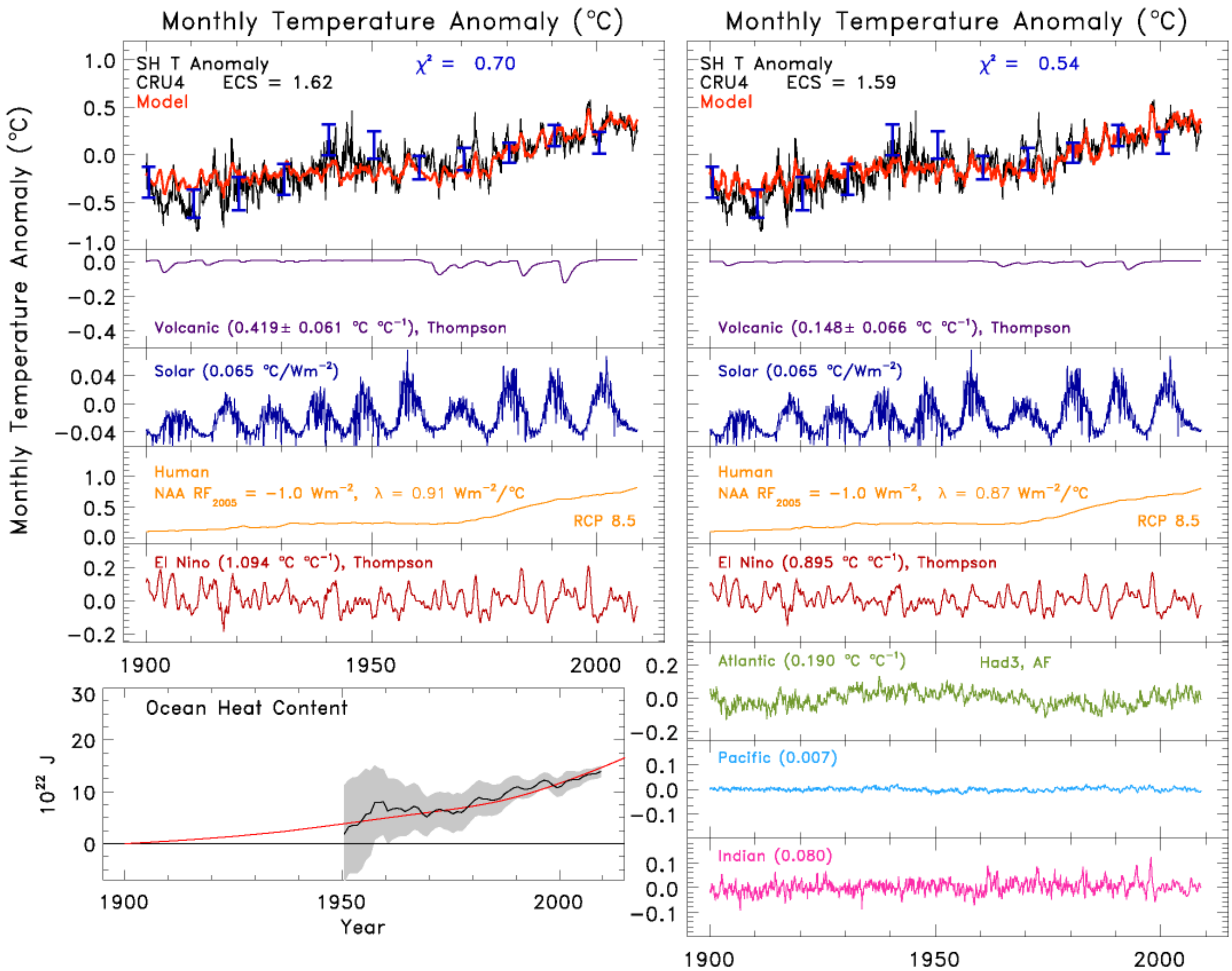

Figure 4. Same as Figure 3, except for the Southern Hemisphere (SH). The temperature record is from:

http://www.metoffice.gov.uk/hadobs/hadcrut4/data/current/time_series/HadCRUT.4.1.1.0.monthly_sh.txt