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Reply to comment by T. M. L. Wigley et al. on "Climate forcing by the volcanic eruption of Mount Pinatubo"

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1. Introduction

[1] In our Pinatubo paper [*Douglass and Knox*, 2005] (hereinafter referred to as DK) we concluded that negative climate feedback and a short climate response time were required to explain the data. This is contrary to the common paradigm. The authors of the comment [*Wigley et al.*, 2005a] (hereinafter referred to as WAST) state that our "... conclusions of a negative feedback are not supported by [our] arguments or the observational evidence" and claim that our response time and sensitivity are incorrect because interchange of energy with the thermocline was unjustifiably neglected. The validity of this claim rests heavily on the rate of energy flow between surface and thermocline being large. We argue here that the rate implied in previous work is likely to have been overestimated, and that the smaller estimate here is more likely to be correct.

2. Response Times

[2] In our paper we deal with two very well-defined time constants. The forcing is characterized accurately by a function proportional to $t\exp(-t/t_V)$, where t_V (the first time constant) is a property of the eruption. Its value is 7.6 months. There is a single kinetic response time τ (the second time constant), which relates to the surface system, particularly the ocean mixed layer, whose temperature is represented by TLT or TLTm. The value of τ was to be found from the data using linear response theory:

$$\tau\frac{d\Delta T}{dt} + \Delta T = \lambda\Delta F. \tag{1}$$

We determined a value of 5.64 months. The correction proposed by WAST, and with which we do not disagree, is to provide for transfer out of the surface layer to the thermocline at a rate ΔQ, such that equation (1) becomes

$$\tau\frac{d\Delta T}{dt} + \Delta T = \lambda(\Delta F - \Delta Q). \tag{2}$$

We use the currently standard notation for sensitivity (λ) [see *Shine et al.*, 1995] and the standard symbols F (forcing) and Q (heat flow). We wish to estimate ΔQ and now make a critical assumption, namely, that the flow ΔQ is proportional to ΔT, such that $\Delta Q = s\Delta T$. When this is done, equation (2) can be written

$$\tau'\frac{d\Delta T}{dt} + \Delta T = \lambda'\Delta F, \tag{3}$$

where the primed and unprimed quantities are related by

$$\lambda = \frac{\lambda'}{1-\lambda' s}, \qquad \tau = \frac{\tau'}{1-\lambda' s}. \tag{4}$$

Since our solution of equation (3) represented the data, we were actually determining τ' and λ'. WAST's criticism can therefore be succinctly stated: the time constant and sensitivity must be corrected by dividing our numbers by $1 - \lambda' s$. They apparently assign a value 0.3 to this factor (it has the effect of their α), which results in larger time constants and sensitivities.

[3] Let us note that our formalism combines ΔQ with the relaxation term, not with "the heat capacity term," as stated by WAST. It follows that their argument based on the zero of $d\Delta T/dt$ has no force.

3. Heat Flux to the Thermocline

[4] How one justifies neglecting the mixed layer-to-thermocline flow of energy is a proper question. First, we remind the reader that the factors derived above were based on what we call the separability hypothesis, namely, that $\Delta Q = s\Delta T$. A closer look at the simultaneous surface-thermocline equations, along the lines of the approach by *Wigley and Schlesinger* [1985] and *Lindzen* [1994] is required, but we can make our point here within the separability context.

[5] We begin by observing that a value of $\alpha = 0.3$ requires that the parameter s have the value $(1 - \alpha)/\lambda' = 4.7$. Under separability, one has

$$s = c_V\kappa\left(-\frac{\partial\theta(x)}{\partial x}\right)_{x=0}, \tag{5}$$

where θ is the thermocline temperature in units of its value at the interface of the mixed layer and thermocline, x is distance (downward) and κ is the eddy diffusion coefficient. The derivative in (5) is the slope of θ at the interface and has values in the range of 1.0 to 5.0×10^{-3} m^{-1} [*Billups et al.*, 1999; *Farmer*, 2000]. We estimate s by using this slope along with $\kappa = 1.2 \times 10^{-5}$ m^2/s (the eddy diffusion

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coefficient in the thermocline [*Ledwell et al.*, 1998]) and $c_V = 4.1 \times 10^6$ J/m^3 with the result $s = 0.024$ to 0.12. Combining this with the peak excursion in ΔT, -0.48 K, we have $\Delta Q = -0.05$ to -0.25 W/m^2, small compared with the peak forcing $\Delta F \sim -21*(0.162) = -3.4$ W/m^2. Our value of α is consequently nearly indistinguishable from unity. There is clear disagreement with the WAST "$\alpha = 0.3$".

[6] The value of κ used above may be contentious in this discussion. Most earlier modeling work, including that of *Wigley and Schlesinger* [1985], assumed a value of $\sim 1.0 \times 10^{-4}$ m^2/s, which value is characterized by IPCC [*Dickinson et al.*, 1996, p. 214] as follows: "[T]hese diffusion values are ... often selected to ensure numerical stability of the simulation. A tracer experiment ... has recently indicated that the correct vertical diffusion coefficient for the ocean interior is closer to $\kappa = 1.0 \times 10^{-5}$ m^2/s, an order of magnitude smaller than often used."

[7] Contrary to the foregoing, WAST state that ΔQ (which they call ΔF) is not small. They first suggest that the model of *Raper et al.* [2001] is appropriate, but do not estimate the magnitude of ΔQ using that model. Note: This is a box-diffusion model with at least 9 parameters (one of which makes use of a value of κ that is probably 10 times too large — see above) adjusted to agree with the 1% CO_2 experiments of the NCAR/DOE PCM model – i.e., a proxy model for another model. Then they state that "a reasonable estimate of around -2 W/m^2" of ΔQ from the ocean heat content data of *Levitus et al.* [2000] may be used. It is not clear how the doubly qualified value -2 was determined. Our estimate from Levitus et al. is closer to -1.1, which would make their calculation less creditable. In addition, the uncertainties in the Levitus et al. analysis make such estimates unreliable.

4. "Disagreement With Models"

[8] As further support for their claim that our neglect of "ocean thermal inertia" was unjustified, WAST say that our results are wrong because they disagree with widely believed models and hypotheses. "Ocean thermal inertia" is clarified in a recent paper by *Wigley* [2005]: "[O]ceanic thermal inertia causes climate change to lag behind any changes in external forcing and causes the response to be damped relative to the asymptotic equilibrium response," with reference to *Hansen et al.* [1985]. In this latter often-cited paper, which uses elementary modeling, the response time is $\tau = g\tau_0$, where g is the gain, the black-body e-folding time τ_0 is proportional to d/T^3, d is the depth of the ocean layer, and T is the effective temperature of the earth. The authors chose $d = 100$ m and $T = 255$ K, obtaining $\tau_0 = 3.5$ yr. Then for $g = 3$, $\tau = 10$ yr. This calculation, of course, is not based upon observations, and it appears to be the antecedent of many subsequent statements about long ocean response times. One could easily make the case for "tropical ocean" values where climate effects originate. For this choice, with $T \sim 300$ K and $d = 30$ m, one obtains $\tau_0 = 0.61$ yr. Then if $g = 0.45$, $\tau = 3.3$ mo. This estimate of response time yields values close to those reported by DK. Quite a difference!

[9] WAST refer to "a more realistic model" [*Wigley et al.*, 2005b] that yields even larger response times (values greater than 15 months). The data show smaller response times and should invalidate the applicability of that model.

[10] WAST refer to the T_{2x} sensitivity (the sensitivity expressed in terms of a temperature change due to doubling CO_2) of a particular climate model, the NCAR/DOE PCM model, as the "true" sensitivity, concluding that our smaller value must be wrong. Why? These are two different processes that would not necessarily be expected to have identical sensitivities, because the feedbacks could be very different.

5. Summary

[11] The comment authors point out our implicit assumption that heat flow to the thermocline was small. An estimate of the effect of ocean coupling shows that this assumption may well be justified to within the error bars of our results. We therefore believe that our main conclusions need only a small correction and stand, within the error bars stated.

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