

Interactive comment on “Quantifying immediate radiative forcing by black carbon and organic matter with the Specific Forcing Pulse” by T. C. Bond et al.

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This document gives both a response to the reviewer's clarifications posted on 13 Aug 2010, and a follow-up to our response posted on 30 Jul 2010, explaining how we have revised the paper to address the reviewer comments.

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1 Response to clarifications

(9) By "overestimating the differences between SFP and AGWP", I meant indeed "overstating the difference between the two metrics and the value of the SFP". As far as I am concerned the SFP is very much the same as an AGWP.

The paper did not claim that SFP should replace AGWP, only that it has uses that AGWP does not. The reviewer does not argue that AGWP can be used for regional forcing. The reviewer does not argue that AGWP is independent of time horizon. The reviewer appears to have no argument with our specific statements about difference, and yet continues to claim (without specifics) that we overstate the difference.

The authors argue that AGWP is a poor name for this metric, fair enough, but SFP is not really better (if some forcings are specific, then what is a non-specific forcing?).

The term "specific" commonly means a quantity normalized to mass, e.g. "specific heat capacity" of an object differs from its heat capacity by being normalized to the mass of the object. Therefore "specific forcing" was chosen to distinguish forcing per emission it from the simple term "forcing," which is commonly bandied about without any notion of the emission rate that led to it. "Specific" has a defined meaning; "forcing" is chosen to indicate forcing rather than implying the necessity of response, and "pulse" is chosen to indicate the time dependence. Every word was chosen with intent.

The unit is confusing –this is what I was trying to convey in my previous comment on forcing vs response- the unit J usually refers to quantify the heat content of a system so it is a bit weird to use it for a forcing integrated over time. It is quite deliberate that the AGWP has unit of $W.m^2.kg^{-1}.yr$ and not unit of $J.m^{-2}.kg$; this was in order to make explicit the concept that it is a time integral of a forcing.

To obtain SFP, we integrate over area and over time. The reviewer doesn't seem to complain about integrating over area, but we added a comment about it to the paper anyway. This integration is required to express inputs that are not normalized to the

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entire area of the earth.

A delta function is a better mathematical name for the quantity that has been called a “pulse” in these discussions. A delta function is quantified by the absolute area under the curve and not by any measure relating to its width. For forcings that can be expressed as pulses- and not for any other shape of forcing- we hold that a unit expressing the already integrated nature of the forcing is the most accurate presentation.

Finally I don't really see the usefulness of a metric if it excludes long-lived species from its definition.

Please see our response “Metric or measure?” posted earlier.

(16) If you say "BC adds 1 GJ to the system", then it reads to me that the energy content of the climate system has increased by 1 GJ because of the BC. Obviously this is not what the authors mean and I admit I've been playing devil's advocate, but I think the authors are replacing a well established way of saying something by a language that could confuse many people.

That actually *is* what we mean. The energy content of the climate system does increase by 1 GJ because of the BC, and it does so rather locally. The climate system may respond with a temperature increase, and perhaps 1 GJ (or, if you like, 32 W yr-1) of increased longwave radiation is re-emitted because of this increase.

We suppose that the reviewer has in mind the *equilibrium* climate system here, when he or she is uncomfortable with discussion of energy addition. But this use of the term “climate system” is rather sloppy (if we can borrow the reviewer's term). It excludes the *transient* climate system that we shall have to live in for at least the next half-century. It excludes energy-balance approaches to the recent history of the Earth system, which inherently exclude equilibrium because the system has not been in equilibrium during the last years. Further, concentrations and forcings of short-lived climate forcers are unlikely to be in equilibrium with emission rate even if the climate system comes to

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equilibrium. When the discussion is limited to only equilibrium, one cannot discuss short-lived forcings.

Now, perhaps we should not fight against the “well established way” even if the terminology is careless. To refresh memories about this established literature, we revisited some of the literature on simple energy balances (Hartmann, 1994) used in metrics discussions (O'Neill, 2000; Fuglestad et al., 2003; Shine et al., 2005). Nowhere did we find equations or descriptions implying that “the climate system” is something other than the Earth (oceans, land, biosphere) and its atmosphere at the time of interest. Solutions to the energy-balance equation can be derived for a climate system at equilibrium, but this is a different matter.

If we understand the reviewer correctly, it appears the notion of “equilibrium” has crept in to the discussion, unstated and frequently undocumented. We find this dangerous. We suggest that the terms “equilibrium climate system” and “transient climate system” should be used in the future and we will do our part to add the word “transient” when “climate system” is used in this paper.

We also find it perfectly acceptable for a reviewer to play devil's advocate.

(18a) Why is the shortwave radiation absorbed by the aerosols all dissipated as heat and not partly re-radiated as longwave radiation? Surely if the particle is heated, it must emit more longwave radiation (Planck's law).

Yes, the particles emit more longwave radiation, but as stated in the original response, this change is small compared to the heat dissipation. Convection of heat away from the particle is fast, so the overall change in temperature is small and the resulting change in emission estimated by Planck's law is small. Furthermore, this extremely detailed discussion was conducted around a figure labeled “concept” that demonstrated a general box-model. We did not intend to include every process in the conceptual framework. Some extra energy is retained in the system, and some is rejected from the system (reflected+re-emitted). Other examples of generalities in this figure are our

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choice not to separate dry and wet deposition, and not to show that some of the emission can occur at higher altitudes rather than at ground level. We have now indicated these limitations in the caption.

References for this part

Fuglestad, J. S., Berntsen, T. K., Godal, O., Sausen, R., Shine, K. P., and Skodvin, T., 2003: Metrics of climate change: assessing radiative forcing and emission indices, *Clim. Change*, 58, 267-331.

Hartmann, D., 1994. *Global Physical Climatology*, Academic Press: San Diego, CA.

O'Neill, B. C., 2000: The jury is still out on global warming potentials, *Clim. Change*, 44, 427-443.

Shine, K. P., Fuglestad, J. S., Hailemariam, K., and Stuber, N., 2005: Alternatives to the global warming potential for comparing climate impacts of emissions of greenhouse gases, *Clim. Change*, 68, 281-302.

2 Follow-up to response

Headings and numbering here refer to our response posted on 30 Jul 2010. This document is sometimes called "our earlier response."

Structure: We have added more description of the paper sections in the introduction. We hope that this also clarifies the role and strong presence of uncertainty in the paper.

Immediacy: A modified version of the proposed new text has been incorporated.

[3] Abstract: The term "warming" is no longer used. Also, "convection" is replaced with "deep convection, which affects both lifetime and forcing per mass"

[4] Atmosphere response to emission: The first section of the manuscript has been

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rewritten.

[7] SFP equation is revised to demonstrate its use for a specific region. We have also added a longer discussion of time scales.

[10] Policy uncertainty: Language has been changed to reflect our earlier response.

[14] Relationship between SFP and response: As the reviewer suggested, we changed the equation so that the identification of emission regions and forcing regions is distinct. Three regional indices are now identified, the region of emission, the region forcing and the region of response. These can all be distinct. We also changed the term that was formerly designated epsilon, although we do keep that term in the equation. As we wrote in our first response, this term is included so that the response used could be general rather than species-specific. However, the notion of efficacy as used throughout other literature also includes some other species-dependent terms, such as the spatial distribution of forcing, which should not be included here.

[17] We have separated the idea of the ensemble adjustment into several equations. We hope this is clearer.

[18] Caption for the figure has been re-written and extended.

[19] Figure showing forcing-per-mass has been corrected. It has also been altered as requested by another reviewer.

Interactive comment on *Atmos. Chem. Phys. Discuss.*, 10, 15713, 2010.