Atmos. Chem. Phys. Discuss., 10, C5378–C5382, 2010
www.atmos-chem-phys-discuss.net/10/C5378/2010/

Atmospheric Chemistry and Physics Discussions

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

Anonymous Referee #1

Received and published: 16 July 2010

This manuscript has many issues and I think it is a long way before it can be published in ACP. My recommendation to the Editor is to reject the present manuscript as a manuscript suitable for publication would have to be quite different from this one.

For a starter it is not clear what the objective of the manuscript is. There seems to be multiple objectives which keep cropping up as one reads the manuscript and this brings confusion. First there is a new metric (which has its own issues and is over-interpreted in my opinion), then there is equation (4) which is potentially interested (if it was correct) but not used, then there are the regional estimates, the ensemble adjustment, and so on. The manuscript needs more focus in order to provide more in-depth analysis of

what the authors would like to cover. Furthermore I do not understand the title (what is immediate RF?) and this title only refers to part of the paper.

There is no proper treatment of uncertainties in this manuscript, which is a major shortcoming.

I have big issues with the abstract which uses sloppy language throughout. A "forcing" (unit W or J) cannot measure warming or cooling (unit K). It is not clear why the authors restrict the SFP to atmospheric lifetimes less than a year. The definition of SFP is incorrect: it is not the amount of energy added to the Earth System (it would be the amount of energy added to the system *in the absence of any feedback*). The combination of lines 4 and 5 reads like if only the energy added to the system that goes in the atmosphere and cryosphere is considered whereas most of the energy ends up in the ocean (I know this is not what the authors mean but it is confusing). On line 5-6 SFPs are provided without an uncertainty range. On line 11, it is not clear what is meant by lower convection (lower amounts of convection or lower base or top of convection); moreover it is not clear that it is convection that is the primary factor controlling BC lifetime (I would think it is rainfall). The sentence commencing on line 11 is unclear: I thought SFP was about the direct and the snow effects, not just the direct effect, moreover the critical OM:BC ratio is the same for RF than for SFP but why does it not vary by region? As far as I know most regions of the world experience convection (except regions of subsistence) so it is not clear what the authors mean by "regions with convection" or are they talking about deep convection? Line 19 indicates that SFP indicates scientific uncertainty but I could not see any proper treatment of uncertainties. I could carry on.

The first sentence of the abstract is correct but it would be as correct to say that the atmosphere responds rapidly to the emission of long-lived greenhouse gases: it is well understood that there is a rapid response to CO_2 through thermodynamic adjustment of the atmosphere. Moreover the ocean responds slowly to short-lived climate forcers.

The main difference between short-lived and long-lived species is that it takes a much longer time to build a radiatively significant burden for long-lived species than for short-lived species (we're quite fortunate that this is the case), but the response time of the atmosphere and climate system to different forcings are not significantly different.

One line 9 the GWPs are the currency of trading but only for greenhouse gases in the Kyoto basket (there is no other trading). It is correct that GWP does not communicate explicit information on rapid climate impact but I don't think SFP does neither.

On line 1, page 15716, as mentioned above, this is the energy added to the system before feedbacks take place. For a pulse forcing, most of that energy will be evacuated from the system pretty quickly. The SFP does not convey that message. One line 12, RF depends on the time profile of the emission rate, not just the emission rate (if you want to generalise the statement to long-lived species as well, which is what IPCC does).

Equation (1) is a bit sloppy in that one does not integrate a surface from 0 to A. Paragraph on lines 16 to 22 is unclear. I can't see in principle why equation 1 can't be applied to long-lived species (it is nothing else than a absolute GWP with an infinite time horizon). I guess in practice the authors want a lifetime that is small in comparison to a typical imescale of interest for climate policies, this does not have to be an efolding time of 4 months.

On page 15717, line 26, again this is the enegy added in the absence of feedback and feedbacks would have to be accounted for in the energy-balance model but I don't see at all what the added value of the SFP is as compared to RF.

As far as I can see the authors grossly overestimates the differences between SFP and AGWP. They're more or less the same thing (but expressed in slightly different units - fair enough), the only reason why SFP does not depend on the time horizon is because

the authors have restricted it to short-lived species. All the alleged advantages of the SFP over the AGWP are because SFP is restricted to short-lived species, rather than an intrinsic property of the SFP.

I think it is incorrect to say that the choice of the time horizon is a "policy uncertainty". A policy uncertainty would be an uncertainty related to the effects of a given policy. The choice of the time horizon is a value judgement not an uncertainty per se.

The sentence on line 24 is incorrect. The GWP of a short-lived species *does* depend on the inclusion of a discount rate because of the denominator.

On page 15720, line 4-5, this is true but it is irrelevant for long-lived greenhouse gases so what is the benefit???

Line 8, I can't see a discount rate in Eq 3 and there isn't any in GWP so why mention a zero discount rate at all?

I suspect equation (4) is incorrect as there shouldn't be an epsilon in there (note that ϵ is named but not defined exactly anyway). If R goes from forcing to impact, then there shoudn't be an epsilon. I guess the integral refers to the integral of each component of the vector (note that there are other mathematical definitions for matrix integrals). Anyway I think eq 4 can be useful (but certainly not at the scale of a city for I), the regions of impact do not have to be the same as the region of emissions and the matrix does not have to be square (hence the indexing is also confusing because index n is used for both I and e). But you're not applying Eq 4 in the paper so what's the point really?

I must say that at this point I somehow have lost interest for the paper. Clarity and focus of the manuscript continue to be an issue. The authors keep swapping between SFP, forcings and GWPs without this adding much. On page 15725, line 16, it says “each gram of BC adds 1 GJ to the system when a boundary is drawn at the TOA”, why do the authors need to draw a boundary at the TOA? BC doesn’t *prevent* 1.5GJ from reaching the surface, it actually brings some energy to the surface (again confusion between forcing and response).

I have tried to understand the concept of Eq 5 but had to give up. This must assume that all models of the ensemble have all processes so why not take the multi-model mean/median? or is it that the size of the ensemble varies for each process? Is Eq 5 dependent on which model is taken as the fully sensitive model? After having said that the A "should have an uncertainty" (line 23, page 15726), the authors fail to provide a proper treatment of uncertainties (this is also apparent in all figures).

Caption of figure 1 uses the same sloppy language than the text: what is "rejected" solar radiation? why is all absorbed energy dissipated as heat (and not re-radiated)? Energy added is *not* an emission-independent measure of impact: it depends on location of emission and it depends on the species through the R matrix of Eq 4 !!

Is figure 4 correct or are the two lines swapped?

In conclusion I would say that the regional analysis of the BC/OM forcing is potentially the most interesting part of the manuscript but would require a proper treatment of uncertainties. It should be reviewed separately (my review is only cursory after section 3).

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