

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

T. C. Bond et al.

yark@illinois.edu

Received and published: 30 July 2010

We thank the reviewer for the attention and comments. We recognize that the topic of this paper is one that invites lively debate. We chose ACPD as a forum in order to allow public airing of the inevitable discussions, and we thank the reviewer for participating.

While many of the comments speak negatively about the manuscript, they help to clarify areas that could be confusing to other readers. It seems that this reviewer was unable to read past some of this confusion, and we hope that this reply can illuminate these issues.

We first discuss some overarching themes under “General Responses.” The reviewer’s
C5879

comments have been numbered by us and addressed point-by-point in “Specific Responses”. Occasionally, the numbering in the General section refers to points in the Specific section.

1 Major points of response

- We suggest that some of the reviewer’s comments result from a lack of familiarity with issues regarding short-lived forcers. It is, of course, our responsibility to frame the problem and we propose two introductory paragraphs to do so.
- We agree that the paper contains some mistakes in wording. For the most part, these can be remedied with the addition of one or two words, or in some cases a sentence.
- The reviewer has stated that the paper isn’t focused; we discuss how its structure is deliberate.
- The reviewer states that the paper does not treat uncertainty, but has ignored the development of uncertainty estimates, which occupies an entire section.
- The reviewer wishes to correct our definition of forcing, but often fails to differentiate between forcing and response.

2 General responses

2.1 Structure

The structure of the paper is as follows:

- **Development.** We present the concept of the Specific Forcing Pulse (SFP).
- **Rationale and Context.** We comment on potential uses of the SFP and how it relates to common knowledge.
- **Demonstration.** We demonstrate the calculation of SFP for two species, black carbon and organic matter, using results from one model.
- **Uncertainty.** We estimate uncertainties using results of multiple models.

The reviewer comments that the paper lacks focus because it contains all these elements, suggesting that only the regional analysis is of interest. The SFP, however, was presented to facilitate the regional analysis. It is customary to present development in the paper where it is used. If we had no development, but presented the measure only as a *fait accompli*, the paper would not be rigorous. If we had no demonstration, the reason for developing the measure would not be clear. And, as the reviewer points out, we really need to discuss uncertainty, so we need the ensemble evaluation too. The reviewer's suggestion that we present only the demonstration part seems to recommend an inadequate paper.

We might help the reader by adding the following sentence to the introduction: "In Sect. 4, we move toward a "consensus" or "median" value based on model ensembles. This section also examines model diversity and its causes with a view toward developing uncertainty estimates."

We do present some discussion that is not used in the paper. We link the SFP with absolute global warming potential (AGWP), global warming potential (GWP), and potential calculation of response. These are not used much (AGWP, GWP) or at all (response) in the paper. They are, however, familiar discussions for many readers. We were seeking to forestall the complaint that we use a different measure that has no relationship to anything else. We believe that readers who are looking for these common measures will appreciate the connection.

C5881

2.2 Immediacy

The reviewer says that he or she does not understand "immediate RF" (comment 1b), nor "why [we] restrict the SFP to atmospheric lifetimes less than a year" (comment 3b). The reviewer also does not understand the paragraph where we explain why we choose the time horizon (comment 7b). This paragraph is critical to the discussion. It appears that the reviewer is not familiar with the notion of rapid reductions in forcing caused by addressing short-lived climate forcers. Most earlier readers of this paper were accustomed to this idea and none of them raised this confusion, so we assume that this must be the problem. These comments coming from a less familiar viewpoint are useful because other readers will surely have the same background. Rather than attempt to explain each point of confusion, we suggest that we should provide some entirely new introductory paragraphs and discuss the lifetime early in the paper. Here is a proposal:

Atmospheric burdens of chemical species with short atmospheric lifetimes respond rapidly to changes in emission. Many of these pollutants, such as aerosols or the precursors that result in ozone, affect the Earth's radiative balance, either directly or by interacting with atmospheric chemistry. Thus, changes to emissions quickly affect burdens and climate forcing. The radiative response to emissions of these "short-lived climate forcers" (SLCFs) differs greatly from the response to long-lived greenhouse gases, for which atmospheric burdens and the consequent forcing lag emission changes by decades.

The rapid response of forcing to emission changes is not precisely instantaneous; it occurs within days or weeks after emission. However, we argue that a forcing response may be considered "immediate" when it occurs within the shortest time scale of interest. Further, we suggest that the smallest time scale of interest is one year, as modeled forcing values are

C5882

averaged over at least one year to capture all seasonal variations. With this definition, pollutants with e-folding lifetimes of four months produce immediate forcing: 95% of the forcing occurs within one year after emission. Our choice of time limit might be arguable: why not three or five years? The choice is not important. Most climate-forcing agents have lifetimes that are either shorter than one year or much longer. A one-year lifetime effectively divides pollutants that have impacts in the very near-term and those for which accumulated burdens are important.

2.3 Nature of forcing

The reviewer questions some of the ways in which we use the term “forcing,” at one point saying that we have confused the notion of forcing with that of response. Some of the reviewer’s statements also seem to us to confuse forcing with response. Therefore it is good to clarify these issues.

We agree with the reviewer that forcing is energy added to the system in the absence of any feedbacks. It appears that this clarification needs to be added to the text, although it ought to be redundant. Forcing, by definition, excludes any response of the system, redistribution within the system, or compensatory output by the system. This is such a fundamental concept in system dynamics, that a statement about excluding feedback or system response seems trivial. It is like explaining that a map of emissions is not the same as a map of atmospheric concentration: true, but unnecessary.

The forcing of a chemical species is the radiative change caused by that chemical species, disregarding the adjustment or response of the system. (Exceptions are made in atmospheric science for stratospheric adjustment.) Absorbing aerosol changes the energy flux at a particular location. This energy may turn up in the ocean, or may be ultimately re-emitted as infrared radiation by the earth’s surface (to give two examples), but this is not forcing: it is part of the system response.

C5883

Our statements here are perhaps trivial, yet the reviewer’s comments violate this principle more than once [numbers 3c, 6a, 8, 16b below]. Certainly we have confidence that the reviewer knows the difference between forcing and response. He or she is probably concerned about the interpretation by other readers. It seems that there must be a great many confused people if such guidance is needed. We can add the distinction to help them.

2.4 Uncertainty

The reviewer complains that there is no proper treatment of uncertainty. The reviewer seems to miss the point of the ensemble adjustment, the process adjustments, and the regional adjustments. These are identified as main factors leading to uncertainty and then analyzed. Therefore, the reviewer questions the very sections that were provided to support the analysis that the reviewer wishes to see. It is difficult to understand what the reviewer wants with regard to this topic.

Now, we do agree with the reviewer that there is no “proper” treatment of uncertainty. We expect that a proper treatment would address all possible factors that could contribute to uncertainty, in a statistically rigorous way. Such a treatment has not, to the best of our knowledge, been accomplished in most of the Earth science fields, and certainly not in aerosol modeling. We do not believe, however, that one should avoid discussing uncertainties until a full treatment is completed. We believe that it is useful to move forward by doing a better uncertainty analysis than has been done before, even if it could yet improve. This is what we have attempted to do with Section 4 in the paper. Specifically, rather than simply taking the model range as a measure of uncertainty, we isolate some of the processes or considerations that cause variation between models, estimate the uncertainty in these separately, and then combine them to produce a total uncertainty.

C5884

3 Specific responses

Responses to the reviewer's individual points follow. The reviewer's statements are given in italics and our responses in normal font. Numbers indicate the paragraph number in the reviewer's document (except for the first and last paragraphs which were a summary). When paragraphs have been split we have marked them [1a], [1b] and so on.

[1a] 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.

Please see the discussion under General Responses, Structure.

[1b] Furthermore I do not understand the title (what is immediate RF?) and this title only refers to part of the paper.

Please see the discussion under General Responses, Immediacy.

Referring also to the list under General Responses, Structure: The title covers each of the discussions. We use "Quantifying" to mean developing central values (Section 3) and uncertainties (Section 4). Of course this quantification has several components but these are too many to be listed in a title.

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

Please see the discussion under General Responses, Uncertainties.

C5885

[3a] 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).

We agree that this statement should be revised to avoid confusing the two. Upon reflection, it seems that sloppy language has permeated the entire field. A global warming potential measures forcing, not warming. We protest a bit about such strong criticism when we are only following convention.

[3b] It is not clear why the authors restrict the SFP to atmospheric lifetimes less than a year.

Please see discussion under General Issues, Immediacy. Note that this was explained in the paper (see comment 7b).

*[3c] 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).*

Please see the discussion under General Issues, Nature of Forcing. The forcing species causes a positive or negative energy change in the atmosphere and cryosphere. What the system does with it thereafter is a function of the system and should not be considered as part of the forcing.

[3d] On line 5-6 SFPs are provided without an uncertainty range.

Since the paper does explore several impacts and uncertainties, and the abstract should be of limited length, we struggled with the best values to include in the abstract. Should we put the uncertainties (which are regionally dependent)? Should the range of regional values (17 regions for energy-related emissions) be presented? We can certainly provide an uncertainty estimate for the global SFP.

C5886

[3e] 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).

“less deep convection” would be a more precise phrase. There are multiple controlling factors, some of which are discussed in the paper. One is the normalized direct radiative forcing. Strong convection can loft aerosol above clouds, increasing its normalized direct radiative forcing. It can also lift aerosol to an altitude where it can't be easily rained out, so rainfall is a factor, but convection is a key component. We did not diagnose the base or the top of the convection in the model runs; we simply observe that both NDRF and lifetime are greater in regions with strong convection. It would be interesting to explore the characteristics of convection that lead to higher SFP, but this should probably be done in models with better aerosol microphysics anyway, as parameterizations of convective lofting and removal in our version of CAM and other AEROCOM models are highly simplified.

We suggest that the paper (but not the abstract) could collect and clarify the relationship between SFP and strong convection as we have done above. Nevertheless, we stand by “convection” or “deep convection” as a reasonable shorthand for explaining why the value of SFP can vary.

[3f] 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?

The critical OM:BC ratio is given for the direct forcing because there is little negative forcing by OM over snow or ice (and possibly the forcing is even positive). It would be misleading to add the SFP for BC in the cryosphere to determine a critical OM:BC ratio. The cryosphere forcing often occurs in quite different locations than the atmospheric forcing, and has different efficacies. We are willing to add such a ratio to the discussion in the paper, but only with strong caveats.

C5887

The critical OM:BC does vary by region, and presenting the average is a simplification we made for the abstract (regional ratios can be derived from the tables). We could add a range to the abstract.

[3g] 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?

Again, we could say “regions with more deep convection.” See also the response to 3e.

[3h] Line 19 indicates that SFP indicates scientific uncertainty but I could not see any proper treatment of uncertainties. I could carry on.

The relative uncertainties from Table 1 could be included here, in which different sources of uncertainty (baseline, optical, regional) are combined. Please see the discussion under General Issues, Uncertainty. It was our perception that the regional uncertainties were simply too numerous to include in an abstract, but perhaps we should attempt to give a range.

[4] 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 CO₂ 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.

It is perhaps our failing because we focus on chemical composition, that we think of “the atmosphere” as the atmospheric burden of the species in question (see General Issues, Immediacy). This has now been corrected in the proposed introductory para-

C5888

graphs. We didn't say anything about the ocean, nor are we discussing the response to forcing.

[5] 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.

There is only trading for greenhouse gases under the Kyoto Protocol. However, CO₂ equivalents (using the GWP) are sometimes used for all species when action on short-lived forcings are discussed. This occurs because the currency of trading is the GWP; it's been accepted in common discussions and is therefore used even in non-trading discussions.

[6a] 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.

This should refer to page 15717, I think. Please see discussion under General Topics, Nature of Forcing. The fact that most of a pulse could be removed quickly relates to the system response, not to the forcing. The reviewer's last two sentences contradict each other. If forcing occurs in a pulse, the system can indeed respond quickly to remove the energy. Stating that this forcing is a pulse, therefore, conveys that message. It is the AGWP which does not convey the message because it carries no notion of immediacy.

[6b] 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).

First, we have already stated that we want to discuss short-lived species and not long-lived species. It's not clear which page this comment refers to—we couldn't find a line 12 to which this comment could apply, so some clarification is needed. Generally speaking, the total RF depends on the time profile of the emission rate, but the impact

C5889

measure (SFP or GWP) does not. (This is not true of Global Temperature Potential)

[7a] Equation (1) is a bit sloppy in that one does not integrate a surface from 0 to A.

We could certainly put in the spherical integral which we actually used. But this would not allow consideration of oddly shaped regions, which are also possible. Here we have tried to be general at the expense of being not quite mathematically rigorous.

[7b] 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 timescale of interest for climate policies, this does not have to be an e-folding time of 4 months.

Please see the discussion under General Topics, Immediacy. We never stated that the choice of lifetime has anything to do with climate policy interest. In fact we attempt to avoid any such choices.

[8] On page 15717, line 26, again this is the energy 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.

Please see the discussion under General Topics, Nature of Forcing. In the paper by Murphy et al. (2009), outgoing radiation and energy stored in the oceans are estimated. Feedbacks occur within the system and do not affect the energy balance. This is a consequence of the First Law of thermodynamics, which has no known exceptions.

As stated on the next page (15718, bottom), energy is a conserved quantity. Radiative forcing is not.

[9] 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

C5890

of the SFP over the AGWP are because SFP is restricted to short-lived species, rather than an intrinsic property of the SFP.

The reviewer's point is unclear. The reviewer agrees with our statements about the difference: the units and the lack of dependence on time horizon. How then does the paper overestimate differences between the SFP and AGWP?

Perhaps the reviewer means that we should use the AGWP instead, because the SFP is not different. Although this is not stated, we will address that question. We would have to present statements like this: "One can use the one-year AGWP for this purpose. One can also perform the integral over less area than the entire globe, so it is actually a regional warming potential. The units will also have to be changed so that the regions can be summed to achieve global forcing. The reader should be careful not to use AGWP found for long-lived species in the same way. Nevertheless, the revised quantity we present is still an AGWP." This is simply awkward and could easily lead to criticisms that the quantity we use is not AGWP. Furthermore, the quantity would be left with an extremely sloppy name: it would be neither global, nor must it be warming—both because it could be negative and because it represents forcing, not warming—nor is it a "potential" in the historic sense of the usage (a dimensionless ratio to the impact of a reference species). It doesn't really matter whether we call this quantity a modified AGWP, an SFP, or a Galapagos tortoise as long as the meaning and the use is clear. We happen to think it is more clear by using the term SFP and the quantity defined.

It seems that the reviewer's attitude is not entirely self-consistent. On the one hand the reviewer criticizes "sloppy" use of language. Yet we provide methods which we believe characterize the physical situation more carefully than other impact measures – for example, the SFP indicates that the radiative forcing occurs immediately instead of sometime within a 100-year time frame, and that the region where forcing occurs should matter, instead of assuming that it is globally distributed. The reviewer then implies that we have done nothing new. We are surprised that someone with such an

C5891

emphasis on precision appears to resist measures that reduce ambiguity.

[10] 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 reviewer has correctly understood our meaning. A clearer statement would be "a choice that lies in the policy domain." Nevertheless, anecdotally, I have observed policy discussions that point at the great difference between the 20-year GWP and a 100-year GWP as evidence of uncertainty. The implication that this difference reflects uncertainty in the radiative impacts of SLCFs should not be allowed to continue.

*[11] 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.*

Reviewer is correct and we apologize for the mistake. This sentence should read "This inclusion affects **AGWP** of long-lived gases, but not of SLCFs" The point is that there are some policy-relevant decisions, such as choice of time horizon or discount rate, which are necessary for long-lived gases but not short-lived gases. We would revise more of this discussion to clarify that point.

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

The reviewer's meaning is unclear. We perceive that there is some utility in communicating the radiative impact of SLCFs which happens immediately (see General Responses, Immediacy). This utility is independent of whether the same can be achieved for long-lived greenhouse gases.

[13] 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?

The discount rate is mentioned because we had discussed it in the previous paragraph. It is given to define clearly how the integrals were calculated.

C5892

[14] I suspect equation (4) is incorrect as there shouldn't be an epsilon in there (note that epsilon is named but not defined exactly anyway). If R goes from forcing to impact, then there shouldn'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).

A more exact definition would be: "R is a matrix [which gives] the response... to a CO₂-like forcing." Forcing by some agents induces fast feedbacks such as increases in cloud cover. Black carbon is a good example: many studies suggest that cloud cover increases because of changes in the vertical structure of the atmosphere, and thus the response to positive black carbon forcing is lower than the response to positive CO₂ forcing. However, the long-term response to the BC forcing adjusted by fast feedback could be similar to the long-term response to CO₂ forcing. The ratio between the effective responses is sometimes called "efficacy," and we can clarify this in the paper. If one accounts for the efficacy, then one can very nearly use the response to CO₂ in the matrix R.

It is true that we did not expand on the definition of epsilon in the paper. This is deliberate. We chose not to define where efficacy ends and response begins, which could be a lively discussion of its own. This division would certainly have to be carefully delineated to avoid the kind of double-counting suggested by the reviewer.

[14b] 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?

The reviewer is correct that the impact regions could be different than the emission regions, and that some of the matrices would no longer be square. This is a good suggestion and use of separate indices would clarify the equation.

C5893

As we state in the paper, the point of presenting Equation 4 is showing how regional SFP could be used. We had a specific reason for introducing this section. A frequent comment from early readers was that the presentation of regional forcing is potentially dangerous, because non-experts may think that forcing in a region indicates response in the same location. For example, people may believe that reducing positive forcing in the Arctic will reduce warming in the Arctic. Although one can make a contradictory statement, it seemed that presenting the forcing-response equation was a more powerful way to prevent such inferences.

[16a] 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.

Please see discussion under General Responses, Structure. The added discussion of forcing and GWP is for readers who are familiar with those quantities.

*[16b] 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).*

TOA is the standard system boundary for defining forcing. However, surface forcing is also discussed in the literature. Aerosols do indeed prevent solar energy from reaching the surface, either by scattering it or absorbing it before it reaches the surface. If the surface ultimately warms anyway (we are not committing to whether it does or not), that change would be caused by redistribution in the system. The confusion is not on our part.

[17a] 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?

C5894

Perhaps we need to change lines 18-19 to read, instead of "all potential models," "... and each Aproc is an ensemble adjustment determined from all models that can determine Eproc by running with and without that process. This ensemble that determines Eproc is often a subset of all models." We thought the explanation given was already quite verbose, and we thought it was also demonstrated amply through sections 4.2, 4.3, 4.4, where only 3-4 models could be reviewed to determine Eproc and Aproc.

[17b] 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).

Figures 3 and 5 do not have uncertainties as these are the development of the baseline values. Figures 6 and 8 show cumulative frequency distributions which are used to develop uncertainties, while Figure 7 compares model ranges for the development of uncertainties. It is difficult to understand how these figures demonstrate the failure to treat uncertainty.

Uncertainties for each A discussed are given: Atot (atmosphere), page 15729, lines 1-5; Amix, entire section 4.2 but summarized on page 15730, lines 16-18; Argn, Figure 7, summarized on page 15732, lines 2-10; Atot (cryosphere), Section 4.5; combination of uncertainties, page 15731-15732 and Table 1. As in our discussion above (General Topics, Uncertainty), we are not satisfied with this treatment of uncertainty either. The reviewer's comment, however, does not address the fundamental problems in assessing uncertainty but suggests that the uncertainty sections were simply ignored.

However, we did not propagate these uncertainties into Table 2, which was presented to show the connection with forcing and GWP. Our assumption was that it was a simple matter to translate the relative uncertainties from Table 1 to Table 2, but it would clearly be better to add the uncertainty bounds in Table 2.

[18a] 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-

C5895

radiated)?

Reject: To refuse to accept (first definition, Merriam-Webster). This is how cooling aerosols interact with solar radiation. Because of backscattering, the solar radiation is refused entry to the system. The amount of absorbed energy that is re-radiated is quite small. Perhaps the reviewer is not familiar with how aerosols interact with shortwave radiation, as opposed to the longwave interactions of greenhouse gases.

*[18b] 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 !!*

To be precise, the caption should say "a measure of impact that is independent of emission rate." The term "emission rate" is provided in the figure and the caption given merely indicated that part of the figure. We note that this independence from emission rate distinguishes SFP from forcing (addressing reviewer's comment 8). Of course the energy added depends on the species (but not through the R matrix, as R is independent of species if one can properly account for the species-dependent efficacy.) But all measures of impact per emitted mass or mole depend on the species. This comment is overly picky.

[19] Is figure 4 correct or are the two lines swapped?

The reviewer is correct and the two lines are swapped (normalized forcing in the Arctic has the greatest seasonal dependence). We apologize for the mistake.

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

C5896