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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

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Reviewer's original text appears in italics; our responses are in normal font.

This paper is an important contribution, particularly for its presentation of a straightforward metric, the specific forcing pulse (SFP), for the climate-heating impacts of short-lived climate forcers (SLCFs). As the authors explain, current metrics such as GWP and GTP, which express warming relative to CO2 over an arbitrary length of time, are unsatisfactory for SLCFs for a number of reasons. The short-term, spatially heterogeneous climate effects of SLCFs are not adequately expressed with a global ratio to CO2 forcing.

Thank you for your nice words and for your clear statement of the situation.

Section 4 of the paper is very complicated and needs clarification...

The early part of this section has been re-written to include more equations, which lead to the equation that was given in the original paper.

It may be best to split the present manuscript into two parts, with Part I consisting of sections 1–3, and Part II consisting of sections 4 and 5. Section 4 contributes to the evaluation of SFP, but may be better dealt with as a separate publication, as both SFP and the best estimate approach in Section 4 are new, challenging concepts.

We considered making this division even before submitting this paper. However, we felt that the uncertainty really had to be expressed. Comments from the other reviewers support that viewpoint.

A. Handling of time scale: pollutant lifetime and integration time horizon

There are two time scales chosen by the authors in this manuscript: the time horizon of integration (authors choose infinity) and the maximum lifetime of a pollutant to be assessed with SFP (authors choose 4 months). Mathematically, these choices are arbitrary, so policy needs inform the choice of these time scales.

The word "pulse" specifically invokes warming that occurs rapidly in comparison to the timescales of climate (or climate policy): as the authors state in the beginning of Section 2.2, a "burst" of energy on the timescales of interest for climate change. This concept is very useful, and should be stressed more directly in the manuscript (and the authors need be clearer that the name SF Pulse describes a 'pulse' of energy added to the climate, not a 'pulse' of emissions).

[Our response here applies to this comment as well as to the remainder of the Timescales discussion from these reviewers. We have therefore not responded to individual paragraphs of the reviewer comment.]

It's true that we presented the equations with an infinite integration time-limit and limited the use of SFP. However, the use of equations to describe the situation is somewhat limited. We would really like to say that for SLCFs, **nobody will argue that the *entire* amount of warming (actually, forcing) should be included in policy discussions.** This statement is not arbitrary, but the method of representing it might be. The reviewers will note that we have altered the integral limits and the discussion, but the general principle just stated has not changed.

We hope that we express both ideas more clearly with our new Fig. 1.

We disagree that the maximum lifetime should be guided by a policy framework, as the reviewers suggest. As we state in the revised manuscript, there are a limited number of climate-active species with lifetimes less than one year, and other climate-active species have much longer lifetimes. This physical fact sets a natural division that we wish to exploit.

Seasonality of SFP is critical to consider, but it does not amount to a judgment about what is a minimum "time scale of interest in anthropogenic climate change." We stress that we are not disputing the authors' choice of a maximum 4 month lifetime, or implied minimum 1 year time scale of interest for climate change, rather, we believe that the choice of this time scale should be justified using the authors' judgment about the time scales relevant to climate change.

We think that the term "of interest" is the problem here and have expunged it from the revised paper with regard to time scales. We do *not* want to impose a subjective judgment to determine the time scales that are of interest. However, the *physical* reality is that just a few time scales describe forcing in the present-day Earth atmosphere. These are "of interest" for analysis and we focus on the shortest ones in this paper.

B. Warming on regional vs. global scales

Metrics such as GWP and GTP are poor for SLCFs because of time-scale problems

and because they express heating relative to CO2, which is globally well-mixed. . . However, it is also important that the heating from SLCFs emitted from any location occurs in somewhat limited regions in contrast to the global effects of LLGHGs (e.g., Shindell & Faluvegi 2009). The current paper de-emphasizes this, and in fact it asserts that SFP should be used just to look at the global effects of any emission both implicitly (including in the units of the metric) and explicitly. [Some of reviewers' other comments on regionality are not repeated here]

SFP is in fact developed to demonstrate impacts on particular regions. This is shown in the original Figure 3, which shows forcing within latitude bands. We also cite regional impact as one of the values of SFP in the paper. In the discussion of SFP calculation we refer to the paper that reviewers cited. Because we agree with the reviewers, we assume that the presentation was not clear and the reviewers could not discern the regional nature of SFP. We have introduced a new Fig. 2 to demonstrate this idea.

Because of the need for such regional information and the need to investigate regional effects with regional models, statements such as "total global forcing should always be provided" are not appropriate.

We disagree with this statement. We believe that for evaluating global climate change, impacts by an emission on all regions of the world should be considered. We agree that impacts can be separated between regions for analysis, as we have shown in the paper. However, they should not be completely removed from the global total.

C. Comparison vs AGWP

Authors should be clearer in previous sections that a) SFP, by definition, is integrated until t = 1, for pollutants with a clearly defined maximum lifetime and b) SFP can be assessed over a region (not just for emissions from a region, but forcing over a region). If this is done, the differences between SPF and AGWP will be clearer. Although they are mathematically similar (albeit different units), they are conceptually distinct.

As stated earlier, we have added new discussion and figures to demonstrate this concept. However, we also refer to our comment posted on 5 Sep 2010 ("Metric or measure?"). We view the link with AGWP as an advantage. We have therefore added text to provide this context.

Specific comments

p. 15714 line 7: suggest dropping "combination"– it suggests that the source and the region are different, which we believe is not intended.

The source region and the forcing region can be different, as pointed out by Reviewer 1 and as demonstrated in the original paper. We hope that the revised text, equation, and new figures clarify that. (In fact, the source, forcing and temperature-change regions could in principle all be different.)

p. 15715 line 11: suggest adding text here that GWP also implies well-mixed pollutant, not one with impacts varying with location of emission. Time-scale is not the only problem with GWP that SFP can address.

The dependence of SFP upon the region of emission *and* the region of forcing are now discussed at greater length.

p. 15715 line 23 – 15716 lines 3: S0 is defined as a concentration, but in the discussion, it is given units of mass. Likewise, on line 1, F is the energy captured per time per mass. It would be helpful if variables were not given more than one meaning. E is used differently elsewhere (c.f. section 4). Also, on p. 15721 lower-case e is used for emissions, here it is upper-case.

The variables have all been reviewed and unique symbols have been chosen. In some cases this means that the symbols used here are not consistent with previous literature.

p. 15717 line 9: Strike "emitted" since fs is a function of the mass presently in the atmosphere, not the mass emitted.

Units have now been changed. Specifically, the terms in the equation have been altered to agree with the terms in the figure demonstrating calculation.

p. 15717 lines 16–22: As noted above, the very brief discussion of the infinite time horizon in the MS is not sufficient. . .

Please see our discussion of timescales above.

p. 15718 lines 10–15: This paragraph is presenting an important limitation, but uses imprecise arguments. Mathematically it is incorrect to state that SFP cannot be calculated for CO2 (and it can very easily be accurately calculated for a very long-lived gas with a well defined lifetime, such as N2O). The point is that SFP leaves out too much information about time for CO2. If the discussion of maximum appropriate lifetime (15717 lines 16–22) is improved (see General comments, section A), this discussion can be clarified easily.

As stated above, the important feature of SFP is that all of the warming is captured within any time horizon. If a 100-year integral is required to capture a significant fraction of the warming, it is not a pulse. The infinite integral limit was introduced in the original paper to indicate that the time horizon does not matter for short-lived species. This presentation has now been revised. New text on the issue raised by reviewers is: "For example, it is safe to say that CO2 has a negligible value of SFP compared to its total integrated forcing, so its forcing does not occur as a pulse."

The statement that "short-lived and long-lived are very nearly orthogonal" should be changed. 'Orthogonal' has a very specific meaning, and impacts from short-lived and long-lived forcers are not orthogonal. We recognize the importance of differentiating short-term and long-term impacts: a different phrasing is needed.

The behavior of short-term and long-term forcing *are* nearly orthogonal in the strict mathematical sense, i.e. the integral over the two unit functions is very close to zero. Figure 1 now demonstrates this. In this discussion (but not in the paper) we will be so bold as to say that forcing by the multitude of climate-active species has a very few functions which could be serve as a basis set for climate change. (These are immediate, methane-like, CO2-like, and longer.) However, we will not push the term "orthogonal" in the paper and just say "very different."

p. 15719 lines 1–3: "SFP can be multiplied by emission rate to obtain annual forcing": is this true if the SFP changes depending on the time of emission?

SFP, if derived from typical global models, can't be disaggregated by time of year. We have now added this clarification to the discussion of deriving SFP from global models.

p. 15719 line 13 – 15720 line 5: As noted above, the discussion of the time horizon, and the appropriateness of SFP for short-lived forcers, but not for LLGHGs, is not sufficient. That critique applies here, also.

The discussion has been changed, but we don't know if the reviewers will like it better.

p. 15720 lines 25–26: "temperature response to a pulse emission." Since SFP is part of the temperature response to a pulse emission, we believe this should be replaced with "temperature response to radiative forcing (climate sensitivity)."

We have made the change to "temperature response to radiative forcing," as reviewers are correct about that part. However, climate sensitivity implies a change at equilibrium and we do not use that term.

p. 15721 line 2: Suggest replacing "deposition" with "removal"

We have taken out the term "deposition." It was included here to indicate that deposition on snow, and hence forcing, depends on the region of emission ("removal" does not convey that). However, the notion of forcing includes cryosphere impact, anyway.

p. 15721 line 5 to end of section: Eq. 4 is a challenging expression of the complexity of atmospheric processes. It adds little to the concept of SFP, and it may be better to remove Eq. 4 and the accompanying discussion.

This section is solely a "way-forward" discussion. There was concern among some early readers of the paper that the presentation of regional forcing would lead to inferences about regional temperature change from less-informed readers. We agree that the presentation of regionally-dependent forcing should be handled cautiously. For that reason, we provide these more complex equations as a barrier to inferring regional temperature change with a simple scaling of regional forcing.

Having said that, the epsilon diagonal matrix seems to be redundant with the R matrix. And, it would be best to replace "response" in line 12 with "response in m"

We hope the revised discussion is clearer. However, we do leave an efficacy=like term in the equation. As we wrote to Reviewer 4: "Inclusion or exclusion of the efficacy term depends on whether R is response to a forcing that behaves like CO2. If so, the efficacy term, which represents additional responses that are particular to a given substance, should be included. If R is just the response to the forcing of the substance, then of course the efficacy term should be excluded. However, the second formulationâĂŤwhere R is species-specificâĂŤallows only the use of R values that have been developed for that species."

p. 15723 line 2: Suggest replacing "it" with "Organic matter" (just for clarity)

Done

p. 15723 line 15: "20% of the total occurring in the Arctic" Is this 20% of the cryosphere forcing? It is surprising that this fraction is so low, so it is worth clarifying. Move definition of 'Arctic' (60–90 N) from p. 15724 line 3 to here.

Yes, it means that 20% of the cryosphere forcing occurs in the Arctic. This can be seen partly with a review of the regional SFP figure (now Fig. 5). Former USSR emissions are half below 60N and half above 60N. East Asia and South Asia, both with high emissions, have cryosphere forcing largely at 30-60N. This is also partly apparent from the figure showing cryosphere forcing (now Fig. 4). Reviewers should recall that the area at high latitudes is smaller than the area at lower latitudes by a factor of cos(lat).

p. 15724 lines 15 – 16: While the low emissions from Japan justify excluding emissions from Japan in discussion of the variation of the SFP over the regions considered, the low BC and OC SFP from Japan is interesting and the reason for it should be noted. Does this suggest that there may be more variation when examining smaller regions, and it isn't seen in other regions because they are large enough that there is more averaging across heterogeneous SFP values, or is Japan unique for some reason?

Japan is unique not because of its small size, but because it is an island nation in a particular location. Its emissions immediately travel over the Pacific Ocean and are washed out quickly. This is interesting but, as the paper is already long, we didn't think it worth discussing in detail.

p. 15724 line 18: Replace F with fs

Done, and all other terms are checked

p. 15725 line 3: Replace "Snow albedo" with "BC reduction of snow albedo (SFPcryo)"

Done

p. 15725 line 5-9: There is an interesting result here: BC SFP from northern latitude regions are similar to BC SFP from southern latitude because cryosphere and convection effects cancel. Earlier, it is mentioned that OM effects are smaller in the Arctic. There seems to be an obvious extrapolation here that is not specifically stated, which is that the SFP of combined BC+OM from a given source will therefore be higher at high latitudes, even if the BC-only SFP is not. If this is correct, this should be stressed.

We actually had more discussion of BC versus OM in an early version of the paper, but felt that the paper was too long and so deleted it. We've now added one of the figures back to the paper, along with a short discussion of BC vs OM. There is obviously more that we could explore, but we feel this should be done in a separate investigation that should include more examination of cloud changes, which are very important.

p. 15726 line 9 – 15727 line 2: This section is currently difficult to follow and understand. It likely needs both clarification and expansion. Perhaps a diagram of the multiple adjustments performed on the various model results would help, with explanations of what, conceptually, is occurring in each stage of adjustment. A simplistic example would be very helpful.

The section has been rewritten. We hope that the core-shell example is now serving as the simplistic example, which it was intended to do in the original document. We did not include a diagram but added more steps to produce a final equation.

It is not sufficiently clear how adjustment of the models . . . are handled. . . While E is clearly defined, A is not. The definition of A would benefit from being expressed as a formula, instead of sentence form. Atot is not a clear variable name: it implies a "total" effect. However, it appears to only relate to the "baseline" model versions (with all processes turned off): therefore, it would perhaps be less confusing if it was named "Abase."

Thanks for the suggestion. We have now provided a longer introduction. We use Abase as the variable name and have added more equations to clarify its meaning.

Atot is additionally confusing in that, as the manuscript currently reads, it appears that in those models without the capability of turning off individual processes, these processes will sometimes be included in the baseline, possibly explaining some of the baseline variation. Except that in some cases, the authors make an effort to correct for those processes that can't be separated, as in the case of UIO-GCM and SPRINTARS and their inclusion of internal mixing in the baseline. The various Aproc are better defined, as it is mostly clear that one process is being addressed in each.

The process adjustments are introduced because many models don't represent some processes. The baseline models do not include these extra processes. We have clarified that in Section 4.1: "Even if a model cannot represent all of the desired processes, it can provide a value of SFPbase and perhaps some of the Eprocn. Our goal is to combine as much information as possible from the ensemble of models to obtain a best estimate and uncertainty for SFP. Examining changes in forcing caused by particular processes should also isolate reasons for uncertainty so that field studies can be designed to diagnose these factors."

Moreover, the discussion of OC:BC ratios within this section seems to imply that the authors are considering the baseline model version to be the equivalent of "direct forcing:" if that is the case, then maybe this should be described as Adirect, not Atot or Abase. Or, perhaps, the OC:BC ratio discussion should be removed from section 4.1 and placed in a separate section for more clarity.

The OM:BC discussion has been moved to a new section, "Best estimates," so that it is clear that the OM:BC ratio discussion includes all adjustments.

p. 15727 line 27 – 28: It is not clear how regional variability can be parameterized as a multiplicative factor, as mixing can. Please clarify using the formalisms of previous section.

This paragraph has been expanded for clarification.

p. 15728 line 16: Figure does not show a factor of four difference between min and max for BC.

Apologies; this is a factor of two (text has been changed). There is one model that differs by a factor of four from the maximum, but in the end we excluded it because it did not provide all-sky values.

p. 15729 line 22 and p. 15730 line 2: Appropriate to call out Fig 6 again here. Is open square result on fig. 6 from Jacobson or Chung & Seinfeld? If it is from Jacobson, it is not mentioned in text. If it is from C & S, figure needs clarification.

Discussion of mixing in old Fig 6 (new fig 8) has been moved to this section. Open square has been removed. We felt that a discussion of homogeneous mixing (which isn't realistic anyway) was excessive and removed it before submitting the original pa-

per, but neglected to take it off the figure.

p. 15730 lines 5 – 7: "lower" and "higher" values of Emix: how much lower? How much higher?

These have now been quantified in the manuscript.

p. 15749 Figure 4. Several issues with this figure: - The colors appear to be reversed: Blue appears to be within the Arctic; Red outside of the Arctic.

The figure has been fixed.

- The figure apparently shows watts absorbed per gram of BC currently in the atmosphere. This is in contrast to the units of SFP (J / g emitted). This change from g emitted -> g present in the atmosphere should be clearly noted in the text and the caption.

That's correct. The box-model discussion of SFP now discusses the implications of seasonally-dependent emissions and removal rates. One of these implications is that SFP derived from global models represents an annual average but cannot be used to obtain SFP from emissions at a particular time. The mass in the atmosphere during a single month resulted partly from emissions in a previous month. Different model experiments would need to be devised to derive a seasonal SFP.

However, we have now drawn a direct link with forcing-per-mass in the equations and referred to it in the discussion. The figure is intended to illustrate the discussion point, that forcing-per-mass can be used as one diagnostic of SFP differences.

- It would be more in the spirit of this paper, and easier for readers, if the seasonal cycle of SFP were presented. We realize that this would look quite different.

This would be interesting but is not possible with these model results, as stated above.

- For the extra-Arctic data, we presume this data is somewhat artificially flattened by the inclusion of southern hemisphere data, which is, of course, off by 6 months

in its seasonal cycle. (If the southern hemisphere data has been shifted, this ought to be stated!) It would be analytically clearer to separate out the NH and SH data in the graph. At a minimum, the text (p 15724 lines 23 and 26) needs to say "northern hemisphere summer" instead of just "summer."

We like the reviewers' suggestion and have incorporated it in the revised graph.

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