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Comment

***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 paper is an important contribution, particularly for its presentation of a straightfor-

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ward 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 CO<sub>2</sub> 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 CO<sub>2</sub> forcing.

Better metrics are needed in the policy community. In the US, California is considering regulating black carbon emissions from light-duty vehicles.<sup>2</sup> Regulators in California have specifically asked for comments on metrics for black carbon, recognizing the difficulties in the use of GWP. In Europe, regulation of BC from small boilers is currently being considered. Numerous international groups are considering how to shape international agreements which limit BC emissions. Several of these efforts have called upon the science community for metrics to appropriately quantify the climate impacts of BC.

The logic behind the design of the SFP, and its use to evaluate forcing from BC, is quite sound and makes SFP a novel and useful tool. However, the present draft should communicate this logic more clearly. We have made some specific comments that we hope will improve the clarity of this section.

Section 4 of the paper is very complicated and needs clarification. The procedure for adjusting models which differ in the processes they include, so that the models can be compared like-to-like and the ensemble can be systematically analyzed to arrive at a best estimate, is a useful contribution. However, we found the explanation of this procedure to be difficult to follow.

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

<sup>2</sup><http://www.arb.ca.gov/msprog/levprog/leviii/leviii.htm>

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and the best estimate approach in Section 4 are new, challenging concepts. Whenever Section 4 is published, either with the rest of the paper or as part of a second paper, the explanation of the procedure and results presented in Section 4 needs considerable expansion and clarification. We have made several specific comments on this section below; however we believe that beyond these suggested changes, more general clarification and expansion are needed.

## General comments on Section 2

### 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. Discussion of these time scale choices should be in that context. Here we suggest some approaches, which we hope will clarify the approach the authors have taken.

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).

By setting the horizon at infinity, SFP accounts for all of the warming from the pollutant; arguably this makes the metric more intuitive and clear. This should justify the choice of the time horizon. However, when integrating until  $t = \infty$ , information is discarded about when warming will occur, relative to emission. For short-lived forcers, that warming occurs so rapidly (a pulse), relative to the pace of climate change, that the information lost is unimportant. We suggest that this reasoning guide the discussion of the maximum appropriate lifetime for a forcer evaluated with SFP.

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We don't find the justification of the choice of the 4-month maximum lifetime for pollutants appropriate to evaluate with SFP to be persuasive. Again, this choice should be guided by the policy framework the metric serves. 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.

### **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 CO<sub>2</sub>, which is globally well-mixed. Emissions of CO<sub>2</sub> affect the climate independent of their location, unlike SLCFs.

The authors use SFP to compare the heating caused by BC emissions from various regions, demonstrating how the metric enables a logical discussion of impacts. We agree with the authors that SFP is a more appropriate metric to use for such a comparison than a GWP.

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).<sup>3</sup> 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:

- The fact that SFP is not normalized to area implies that it is to be used globally. The paper concentrates on total energy added to the climate system: however, total energy added to a given region is also important, and the energy per unit area can be relevant to the impacts in that area and useful for comparing different areas.

<sup>3</sup>Shindell, D. and Faluvegi, G. (2009) "Climate response to regional radiative forcing during the twentieth century," *Nature Geoscience* 2 294. doi: 10.1038/NGEO473

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- At at least two points, it is stated that SPF could be calculated over a discrete area with Eq. 1, but “total global forcing should always be provided.” (p. 15717) Similarly, on p. 15718: “an exhaustive set of regions should always be provided to indicate total global forcing.”

We suggest that the authors note that regional warming from a SLCF is also of interest. In that case, SFP could be normalized to area ( $\text{J g}^{-1} \text{m}^{-2}$ ), for example to compare the susceptibility of different regions or airsheds (of different sizes) to warming from a unit amount of an SLCF. Comparing susceptibility at the regional scale would be best approached with regional models, and it would be valuable to note that.

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.

### C. Comparison vs AGWP

Authors should be clearer in previous sections that a) SFP, by definition, is integrated until  $t = \infty$ , 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.

### 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.

**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.

**p. 15715 line 23 – 15716 lines 3:**  $S_0$  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

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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.

**p. 15717 line 9:** Strike “emitted” since  $f_s$  is a function of the mass presently in the atmosphere, not the mass emitted.

**p. 15717 lines 16–22:** As noted above, the very brief discussion of the infinite time horizon in the MS is not sufficient. Also as noted above, this section touches on two issues: the time horizon (which has already been set at infinity) and the need to consider the variability of the factors in eq. 1 ( $f, m$ ) over at least one year, without clearly separating those two issues. The upper limit presented here (e-fold lifetime of four months) has a logic which is arbitrary. In fact, one needs to compare the behavior of pulses released at various times during a year, not simply consider the heating caused by a single pulse for at least one year.

Also, the discussion of a “pulse” is confusing here, since pulse is often used to refer to the instantaneous release of a mass of pollutant into the atmosphere (cf beginning of sec. 2.1), but here the draft refers to a pulse of heat generated by the pollutant in the atmosphere. This should be clarified.

**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  $\text{CO}_2$  (and it can very easily be accurately calculated for a very long-lived gas with a well defined lifetime, such as  $\text{N}_2\text{O}$ ). The point is that SFP leaves out too much information about time for  $\text{CO}_2$ . If the discussion of maximum appropriate lifetime (15717 lines 16–22) is improved (see General comments, section A), this discussion can be clarified easily.

The statement that “short-lived and long-lived are very nearly orthogonal” should be

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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.

**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?

**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.

**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).”

**p. 15721 line 2:** Suggest replacing “deposition” with “removal”

**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.

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  $m$ ”

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

**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.

**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,

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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?

**p. 15724 line 18:** Replace  $F$  with  $f_s$

**p. 15725 line 3:** Replace “Snow albedo” with “BC reduction of snow albedo (SFP<sub>cryo</sub>)”

**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.

**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.** It is not sufficiently clear how adjustment of the models for the processes that are not included and adjustment of models resulting from comparison of the individual models with the ensemble mean/median are handled.

Finally, the description of the problem that section 4 is addressing should be clearly separated from the procedure suggested (ie, the discussion of various processes in various models, which centers around  $E_{proc}$ , on line 23 and following on 15726, should probably precede the introduction of Eq. (5)).

While  $E$  is clearly defined,  $A$  is not. The definition of  $A$  would benefit from being expressed as a formula, instead of sentence form.  $A_{tot}$  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 “ $A_{base}$ .”

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$A_{tot}$  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  $A_{proc}$  are better defined, as it is mostly clear that one process is being addressed in each. 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  $A_{direct}$ , not  $A_{tot}$  or  $A_{base}$ . Or, perhaps, the OC:BC ratio discussion should be removed from section 4.1 and placed in a separate section for more clarity.

**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.

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

**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.

**p. 15730 lines 5 – 7:** “lower” and “higher” values of  $E_{mix}$ : how much lower? How much higher?

**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 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

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g emitted -> g present in the atmosphere should be clearly noted in the text and the caption.

- 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.
- 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.”

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