

Interactive comment on “Climate Impact of Finnish Air Pollutants and Greenhouse Gases using Multiple Emission Metrics” by Kaarle J. Kupiainen et al.

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This is a straightforward application of the climate metrics of Aamaas et al. 2016 and 2017 to emissions from Finland. The methodology is appropriate so the work is acceptable to publish after minor corrections, but it would have been useful to have some more interesting conclusions.

The discussion about ARTP vs AGTP is confused, particularly since it is not clear whether this is due to latitudinal efficacies or different treatment of BC on snow. Is this discussion really necessary at all? Presumably the Aamaas et al. 2017 are the preferred metrics – so maybe stick with them to simplify the discussion?

C1

If it is going to be included the comparison with Sand et al. 2016 needs to be done better. In their figure 2 they show very much higher temperature response per unit emission for the Nordic Countries compared to Europe. It is surprising therefore that the BC response in this paper (figure 7) is so similar to that using Sand et al. It would seem much more appropriate to use Sand et al. to get the scaling between Nordic and Europe and apply this scaling to the values in this paper.

The mean(1-25) metric is not an obvious choice, and the authors have not explained how it fits in with any nationally or internationally agreed policies. Shindell et al. 2017 indeed admit: “We chose the mean temperature (rather than end-point temperature) to incentivize early action” rather than for any scientific or policy reason.

Line 32-36: I don't think there is any agreement on the definitions of SLCFs or SLCPs – maybe there should be. UNEP (2011) used SLCF when discussing warming agents, IPCC AR6 WG III report used SLCP to refer to warming agents, IPCC SR1.5 stated that SLCP was an equivalent alternative term to SLCF.

Line 51: “metrics is” should be “metrics are”.

Line 166: You are actually assuming the pattern is *exactly* the same for all GHGs.

Line 172: Re-phrase “our main pick”.

Line 188: “the original” should be “their original”

Line 195: “combining” should be “convolving”

Lines 200-202: This description is too abrupt – the reader would need to be an expert in ARTPs to follow the argument. It either needs a longer explanation to guide the less expert or removing.

Line 209: The AGTP(1-25) is presumably equal to the iGTP of Peters et al. (2011) divided by the time horizon. This should be mentioned.

Lines 240-245: Some comment on the reasons for using emission pulses rather than

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emission steps should be provided here. While the pulse gives the mathematically useful Green's function, the convolution with a step emission could be considered more representative of the climate impact of Finland continuing to emit at 2010 levels.

Lines 258-262: Again, if you really need to compare AGTPs vs ARTPs then this needs to be explained for those readers who haven't read Aamaas et al. 2017.

Line 274: If authors consider the relative importance of BC using AGTP and ARTP to be an important point they need to explain why, otherwise this just seems to be a random fact. Alternatively this sentence can be removed.

Section 3.2.4: This section seems to suggest that the main difference between AGTP and ARTP is not the latitudinal dependence of the efficacy, but the different treatments of BC on snow in Aamaas et al. 2016 and 2017. If so, then this should be made more explicit earlier on.

Lines 356-357: This seems a confusing policy message – why should a Finnish policy maker need to know which metric is being used when implementing wintertime BC control measures in Finland?

Lines 392: Why do Sand et al. 2016 have a much larger indirect effect? Is it due to a different model?

Section 4: This section needs to be structured. There is no obvious story being told here. I suggest having the Discussions and Conclusions separately so the Conclusions can be more tightly written in such a way that the reader is clear what the key messages are they should take from this paper.

Lines 445-449: This justification of the mean(1-25) metric is very weak. The argument seems to be purely that Shindell et al. 2017 suggested it.

Lines 481-483: The mean(1-25) metric wasn't "evaluated to be useful" in Shindell et al. 2017, it was simply devised "to incentivize early action".

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Lines 483-484: Surely the appropriate metrics for Arctic warming by 2040 or 2050 should be endpoint metrics for 21 or 31 years (e.g. for a start in 2019) rather than a mean over that period.

Figure 7: This needs labelling on the figure to distinguish the Aamaas and Sand values.

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