

Interactive comment on “Multimodel emission metrics for regional emissions of short lived climate forcings” by B. Aamaas et al.

Anonymous Referee #2

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Traceability for Climate Metrics is Essential

This discussion paper by Aamaas et al. on climate metrics for emissions of short-lived climate forcings (SLCFs with general atmospheric decay times of < 20 yr) is interesting and holds some lessons for us all, from scientists to the policy arena (for which it is clearly intended). Using results from 4 different chemistry-transport/climate models is an important step and the authors talk about robustness. The work is well written and would provide a useful ACP-appropriate contribution except for two serious flaws (correctable) and some minor fixes.

(#1) Having searched the Supplementary Material, I realized that there is no information given as to the model results, how the four models varied (perhaps even in sign), and even no scientific explanation / justification for the large “aerosol effect” from CH₄

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and CO emissions, but no stratospheric H₂O effect. This is all quite different from the recent IPCC summary and such differences need better explanation at least. I was interested in the shipping results which only come from 2 models and just how different they are. Given the recent problems with representation of ship plumes (e.g., Vinken et al. ACP 2014-1353; Holmes et al. ACP 2014-6801; Eide et al., ACP 2013-4183) I do not see how these results could be promoted without a better comparison with what people are doing with shipping. It seems that all this is in a Bellouin et al (2015) manuscript that has not even been written, much less co-submitted and available for review. This ACPD manuscript simply cannot go forward until the Bellouin 2015 paper is publicly available.

(#2) The approach here lacks a traceability that would be necessary for others later to reproduce and at least compare results at a level to understand what drives the model differences. For example, the paper gives a well founded and thorough review of previous work on short-lived RF agents but it misses the early work on short-lived ODPs such as n-propyl bromide (Olsen et al GRL 2000; Bridgeman et al JGR 2000; Wuebbles et al JGR 2001) which explored indexing for these ozone metrics. The work itself is not essential but it led to the mathematical codification (e.g., Prather, GRL 2002) of the relationships between steady-state impacts from sustained emissions (easy to calculate); the pulse-response function; the steady-state lifetime (burden/emission rate). The pulse response of (e.g.) RF is not a simple e-fold decay but is quite complex for short-lived species. Nevertheless the steady-state lifetime and impact (e.g., RF) are shown to be an exact integral of a pulse function. This applies also to integrated CH₄ impact (12 yr) from a shipping NO_x (1 day) emissions. This paper confuses these simple relationships with the problem that they do not separate the clear components. Do models differ because of different lifetimes or different steady-state RF or because of the mix of time scales (mix of e-folds as in ship NO_x example here). If this paper is to be used in the future or compared with new results from the next generation of models, traceability is essential.

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(#3) This includes some minor fixes and just plain questions. The GWP/GTP figures for CH₄ show a short-lived ozone component – is this long-lived ozone? The CH₄ aerosol affect is presumably through lowered OH reducing aerosol formation (warming) by pushing more dry-deposition of SO₂ – how robust is this? In Figure 8 and some others, it seems odd that the SO₂ impact (5 days?) decays away more slowly than CO or NO_x (12 yr from indirect CH₄ perturbation). Overall, the interaction between the gases and aerosols is not clear – are connections one-way or two-way? The text implies that maybe some of these components are only from one model? “Not all processes, nor species, have been modeled by all models, and hence, the average for a process can be based on anything from only one model to four models.”

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 26089, 2015.

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