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Discussions

Interactive comment on "Evaluating the climate and air quality impacts of short-lived pollutants" *by* A. Stohl et al.

Anonymous Referee #2

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The authors have chosen an interesting topic and provided many valuable results in their analysis. A particular strength of this study is that it comes from a large multi-model project. Some of the results such as the characterization of regional and seasonal metrics for species other than BC are fully multi-model and therefore of great value to the community. Some of the primary findings, however, do not come from the multi-model results but rather depend upon single models. In particular, all the human

health impacts come solely from the GAINS model's source-receptor relationships, and the net climate impact of the BC-related measures is highly dependent upon the estimated importance of processes such as the semi-direct effect of BC and the BC albedo forcing that were also simulated by just one model. The reliance on single models for some of the main results suggests that those conclusions may not be as robust as others from this project and this should be made clearer to the reader I feel.

Much of the paper reads very well and provides detailed descriptions of the simulations and results. Two sections in particular could use improvement in my opinion. The first is section 3.2 on model evaluation. Given that aerosol forcing is key to much of this study, and although not directly observed it is most readily evaluated by examining aerosol AOD and AAOD, I was quite surprised that there is no evaluation of the models against AeroNet and/or satellite AAOD. Capturing surface BC (Fig 5) is a good thing, but tells us little about how aerosols influence climate in these models. The lone comparison in Figure 4 is with total AOD, so does not tell us how the models simulated absorbing aerosols vs reflective aerosols or even natural vs anthropogenic aerosols. The models should be compared with observational constraints for absorbing aerosols (AAOD), as was done in Bond et al (2013) for example, in particular since that study found that all models were systematically biased low (as had been shown in prior studies). Fine-mode aerosol AOD might also be a good test though admittedly the observations are not without their own issues.

Secondly in this section, and along similar lines, the models should be compared with observationally-constrained estimates of forcing by absorbing aerosols (showing values of about 0.9 W/m2 for all present-day (natural plus anthropogenic) BC (e.g. Ramanathan and Carmichael, 2008; Bond et al., 2013). The modeled values should also be compared with values reported in recent comprehensive assessments (IPCC AR5 and Bond et al., 2013 in particular) to give the reader some context for the models used in ECLIPSE. For example, for the direct radiative forcing of preindustrial to present-day BC increases, the reported values from recent Assessments are 0.45 (0.30- 0.60) W m-2 in UNEP/WMO (2011), 0.71 (0.08-1.27) W m-2 in Bond et al. (2013), and 0.54 (0.10-1.03) W m-2 in IPCC AR5 (2013).

Section 3.6 on climate impacts is the other section that is lacking a substantial amount of information. In particular when it comes to describing the model results for the impact of BC, the paper does not give enough information for the reader to judge the credibility of the results. The authors discuss two possible reasons for the small values and wide range in the climate response, unforced variability and differences in processes. For the former, the authors refer to the possibility of "unforced responses of climate system components, especially sea ice, that happen to counteract the small temperature response". These results come from models, not observations, so one can and certainly should test this to find out. The models need to be run long enough and have enough ensemble members to reduce unforced variability to a small enough magnitude to see the forced response or at minimum bound it rather than presenting a response that may consist largely of internal climate 'noise'. Given the difference in the two CAM4 ensemble members shown in Figure 12, it seems that the model sampling may be inadequate, in which case the conclusions of the study in this regard need to have substantially expanded caveats. For the question of processes, the authors write that differences in the response to BC "could be due to the different sizes of the indirect and semidirect effects of BC in different models". Previously the paper described how only one model included the semi-direct effect, one included BC albedo forcing, and 3 included the first aerosol indirect effect. Given the strong differences in the response to BC shown in Figure 12, it is important to state more clearly which processes are included in which of these models. It is also important to present the effective radiative forcing due to BC in these models as without this it's very difficult to understand how, for example, the HadGEM model produces a warming in response to BC removal. It seems likely that HadGEM has an overall negative forcing from BC given its response, which is a surprising result, and ECHAM seems to have positive forcing from OA (maybe due to strong 'brown carbon' asbsorption) but without diagnostics of the forcing from various components, a clear description of which processes are included and a comparison

of total present-day forcing with observationally-constrained estimates it's difficult to have an understanding of how credible these results are. There is clearly a strong negative BC semi-direct effect in the one model that simulated this in ECLIPSE, yet in the Assessment of Bond et al (2013) multiple BC-cloud impacts were analyzed including indirect effects on liquid and ice clouds, semi-direct effects, cloud inclusions, and mixed-phase clouds. In particular, Bond et al estimated that, like the one ECLIPSE model with results, the semi-direct effect was likely negative but that the effects of both BC cloud inclusions and BC's mixed-phase cloud forcing were positive and larger than the semi-direct effect (the latter without inclusions, as I'm assuming was done here). Thus the overall indirect BC forcing used in this paper appears to come from only a subset of known processes and hence to be of the opposite sign to the Assessement of Bond et al (summing over all their quoted values), so it's important to know if this is in fact the case by specifying clearly which processes are included and their forcing values. Including only negative indirect effects and neglecting positive ones might be a reason that the net climate impact of BC in the models is so small, though without a clearer description of the models' processes and forcings it's impossible to be certain.

The obvious way to figure out the question begged by the results as to whether the BC forcing in the models is relatively small or the response to BC is relatively weak compared to prior literature is for the authors to add a table with forcing values (ERF) and responses per unit forcing for their equilibrium removal of individual aerosols simulations and compare with prior results. It seems a major oversight not to have this and I strongly recommend such an addition.

Finally, it seems likely that these models have smaller BC forcing than that in the recent Assessments of UNEP/WMO, Bond et al and IPCC AR5 at least in part because those three all used a blend of observations and models (adjusting modeled values upward to account for systematic biases relative to observations) whereas ECLISPE used native model results. If indeed that's the case, that deserves greater prominence in the discussion of these results which currently only mentions the semi-direct effect (not noting other indirect effects) and reduced BC lifetimes as possible reasons for differences relative to prior studies (the comparison with observations suggested previously would clarify if these models, with BC lifetimes and emissions that differ from prior studies, are likewise still biased low in AAOD). The prior CICERO groups' study (Hodnebrog et al., Nature Comm., 2014) indicated that lowering BC lifetime while simultaneously increasing emissions gave a better match to observations of the BC distribution, but had little effect on radiative forcing. It is difficult to reconcile those results with the suggestion here that the impact of BC is comparatively small due to the shorter lifetime used unless the reader assumes that the emissions are too small to allow the models to reproduce observations.

I reiterate that there is a lot of good material in this paper, but I think it could nonetheless be substantially improved by addressing these larger issues as well as some additional specific comments listed below. I believe it would be well worth the additional effort required.

Additional comments:

P15158, L23-26: The text states "The climate response from BC reductions in our study is smaller than reported previously, largely because our study is one of the first to use fully coupled climate models, where unforced variability and sea-ice responses may counteract the impacts of small emission reductions." It is not correct to say that unforced variability may counteract the impacts of forcing as that confuses forced and unforced climate change. Unforced variability doesn't reduce the impact of forcing, rather it may mask it if statistics are inadequate to remove it. This should be analyzed (see further comments below).

P15160, L8-9: The text states "Methane is a greenhouse gas roughly 26 times stronger than CO2 on a per molecule basis at current concentrations." This is an incomplete description as this is assuming integrated RF over 100 yrs, and with a different physical quantity or time horizon the ratio would be quite different.

P15164, L8: I provided additional analyses supporting the claim that even cooling agents can cause damaging climate effects via changed in precipitation in Shindell, Climatic Change, 2015.

P15167, L5-7: Reference to later and broader studies such as Anenberg et al., Env. Health Pers., 2012 or Lim et al, Lancet, 2012 would be better than our earlier, simpler model-based study (Unger et al.) P15176, L26-28: There is substantial debate about how much of the difference be- tween the regional temperature changes during these time periods is due to aerosols vs oceanic circulation changes. This makes this a fairly weak test of a model's ability to capture the climate response to aerosols.

P15178, L9-11: The text states "Quantifying the semi-direct effect has large uncertainties, however, because internal variability in the climate system masks tropospheric adjustments to BC perturbations." So how long were the simulations run to diagnose this and what is the resulting uncertainty? Please also explain how this uncertainty is incorporated into the total SRF uncertainty (shown in Fig 7) given that you only had one model diagnosing the semi-direct and BC albedo forcings vs multiple models for direct forcing?

P15179, L7-9: The text states "ECLIPSE is therefore able to state with confidence that NOx exerts a negative SRF, because the O3 response is not sufficient to offset the combined CH4 and nitrate response." I think it'd be worth pointing out that this conclusion is consistent with that of the IPCC AR5 (Myhre et al., 2013).

P15179, L11-13: The text explains that the SRF for methane differs across models due to the different lifetimes for methane in the models. These should be compared with observationally-constrained values (the Prather et al values cited previously) and if some models are unrealistic then they should be excluded.

P15181, L24-26: Discussing the RTP coefficients, the text states "Even though the coefficients are likely model-dependent, we had to use these values because they are not available from any other model (and specifically not from the ECLIPSE models)."

It would strengthen the reader's confidence in the use of these single-model values to point out that they appear to be fairly robust in comparison with the response to historical aerosol forcing in several other models (Shindell, Evaluation of the absolute regional temperature potential, Atmos. Chem. Phys., 12, 7955–7960, 2012).

P15183, L11-13: This text discusses the NMVOC-related solvent measures, but I didn't see previously any discussion of metrics for NMVOCs (though it did say metrics were calculated for 'others'). It's not obvious how one would calculate those given the large number of species. Could the authors please explain how these measures fit within the ECLIPSE methodology given that measures are selected based on their GTP? Were there in fact NMVOC GTPs, and if so, for which species?

Figure 12: It seems the uncertainty on the multi-model means is something like the average of the uncertainty of each individual model (or perhaps propagating those indi- vidual ranges mathematically) but it doesn't include the difference between the models. If so, this doesn't seem a sensible approach to me.

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