

Interactive comment on “A continued role of Short-Lived Climate Forcers under the Shared Socioeconomic Pathways” by Marianne T. Lund et al.

Anonymous Referee #2

Received and published: 14 April 2020

The manuscript emphasizes the importance of SLCF agents, especially for the short-term impacts of climate scenarios, with some emphasis on methane. It is concluded that SLCFs continue to play a role in many regions. While it is important to reiterate this message, it is not so obvious what new findings are being presented. On several occasions, the results reinforce what is known, which does not justify publication.

The results for methane depend on methodological assumptions that are not transparent (e.g., emission categories) nor are they discussed in sufficient detail in the presentation of results. I found the discussion about the changing role of BC interesting, which could be highlighted more. I also recommend emphasizing regional differences more

Printer-friendly version

Discussion paper



strongly. The finding that SLCFs are particularly relevant for low- and medium-income countries is relevant. In general, it would be good to deepen such analyses and bring new aspects forward more clearly.

There are some rather bold simplifications in the treatment of aerosols; e.g., it is not clear how the radiative properties of partially absorbing aerosols (with BC) are accounted for. They sensitively determine the radiative cooling efficiency. NO_x is mentioned on several occasions, but its role is unclear. How is nitrate been included? It is semi-volatile and responds to changes in sulfate and ammonium. Has that been accounted for? This is particularly relevant for the comparison of scenarios.

A relatively large temperature signal is expected from the indirect effects of aerosols on clouds, being highly non-linear especially at low pollution levels. I find the scaling by a factor of 2.1 to the impact of sulfate questionable. I recommend investigating (and showing) how sensitive the results are toward this assumption. There could be large regional differences,

I.173 mentions a lack of information. Can't you get this from the chemistry-transport model?

I.175: The description of the -15% for BC after I.175 is unclear (e.g., the rapid adjustment). Can you explain?

I.190: "lower than in the literature". By how much? By 0.885/1.06? Is the effect linear?

I.200: I am doubtful about the linearization of the temperature response by multiplying the emissions with the AGTPs. There are models available to compute this properly. This is particularly relevant for aerosols and ozone (the latter not being discussed at all), and to a lesser extent for methane, which has significant indirect effects, e.g., though ozone. Has this been accounted for?

I.210 Mentions ozone (also I.148), but it does not appear in the rest of the manuscript. It does not show in figures 2 and 3. Why has it not been included?

[Printer-friendly version](#)[Discussion paper](#)

I.241: There is much debate about CH₄ emissions from the fossil fuel sector. What has been assumed in the calculations, and how does it compare with recent estimates? Methane is emphasized in the conclusions, but the attribution of emissions to sectors is not transparent. It would be interesting to deepen the discussion about the role of methane. Currently, the results are being reported but not really analyzed.

I.261: This is an interesting result that could be explained and emphasized more strongly.

I.364-366: This is interesting and could be explained and emphasized more strongly.

I.443-445: This is interesting and could be explained and emphasized more strongly.

I.468-470: This is interesting and could be explained and emphasized more strongly.

Interactive comment on Earth Syst. Dynam. Discuss., <https://doi.org/10.5194/esd-2020-9>, 2020.

Printer-friendly version

Discussion paper

