

Response to comments by anonymous referee #2 on “A continued role of Short-Lived Climate Forcers under the Shared Socioeconomic Pathways” by Lund et al.

We thank the referee for the detailed and thorough review, which has contributed to substantial improvements to our manuscript. Several steps have been taken to address the referee comments and concerns. Responses to individual comments are given below.

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 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.

The primary objective of this study is to provide a quantification the near- and long-term impact of individual species with a greater level of geographical and sectoral breakdown than previously existing in a unified framework, and to deliver a transparent and readily applicable data set of emission metric values for further use both in the scientific community and beyond to study the effectiveness and implications of emission changes following mitigation and policies implemented in at level of individual emission sources. We also provide the first (to our knowledge) breakdown of the SSP-RCP scenarios with this level of detail, highlighting regional evolutions that warrant further attention and work. Furthermore, following comments by referee #1 we now make a substantial methodological advancement by include the carbon-climate feedback. We have tried to make these points clearer throughout the manuscript. We have also rewritten section 3.1 to improve the flow and make the separate discussions about regions and sectors clearer, and made modifications to highlight the regional heterogeneity more clearly where possible.

In response to comments by both referees, the Methods section has been expanded to include more details about the underlying assumptions, and to guide readers outside the emission metric community. This includes e.g., specifications about AGTP for individual components and how they are treated within this concept, the choice of impulse response functions, references to the aerosol parameterizations and properties underlying the simulations of atmospheric concentrations and kernels, and emission inventories.

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.

We agree that this is a simplification, and this is also discussed in the manuscript (we have modified slightly to make it even clearer). However, information about the dependence of radiative efficiency of indirect aerosol effects on emission location is to our knowledge not readily available (spatial distributions of indirect RF are of course available but would not provide the type of information we need these are typically run using all emissions as input while aerosols can travel across distances and influence clouds beyond their source region). Moreover, because we scale the regional direct radiative efficiencies, a spatial dependence is in part accounted for in the resulting AGTP for a given region, under the assumption (and that is of course not well known) that there is a similar relative influence of geographical differences in local meteorology and dynamics on both direct and indirect aerosol effect. Aerosol indirect effect are uncertain and model dependent, which poses a general challenge for climate studies across modeling tools with different level of complexity – from ESMs to emulators. The overall uncertainty in RF may well be larger than any regional difference in the efficiency. We note that we do included an analysis of the spread in our results arising from uncertainties in forcing.

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

Generally, offline chemistry transport models do not include aerosol-cloud interactions. An estimate of the indirect aerosol forcing can be derived with subsequent radiative transfer calculations (for the first indirect effect only) but is not available to us in the form of a radiative kernel which is the approach used here. A first order estimate of the radiative forcing due to aerosol-cloud interactions has been calculated for the total global emissions by Lund et al. (2019), but similar calculations to investigate the sensitivity of the forcing to emission location (i.e., RF per unit regional emission) has not been performed and does not, to our knowledge, exist in e.g., the bulk of HTAP2 literature.

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

Can you explain?

To clarify, we have modified this paragraph, which now reads:

We also account for the semi-direct effect of BC (i.e., the rapid adjustments of the atmosphere to the local heating), which has been found to partly offset the positive direct radiative forcing (Samset & Myhre, 2015). Here we use the multi-model data of the ratio between semi-direct and direct BC RF from Stjern et al. (2017) and calculate an average adjustment factor to account for the influence of rapid adjustments of -15%. This is then applied to the AGTP of BC for all regions, except South Africa where Stjern et al. (2017) found a small positive forcing from rapid adjustments.

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

The difference depends also on the time scales of climate response IRF, and so the difference between AGTPs using different IRFs will have a temporal dependence as well. Following this comment and a comment by referee #1 we have performed a set of sensitivity simulations for the pulse based metrics using different combinations of IRF for the climate response and CO₂ to show the order of magnitude impact of our methodological choice. A separate discussion with two new figures has been added to the supplementary material.

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., through ozone. Has this been accounted for?

We agree that there are non-linearities in the system that are not properly represented by the AGTP approach. We also agree that there are models (i.e. coupled chemistry-climate models) that can handle this better. The problem is that these models are not suited for running experiments to quantify impacts of specific (and thus small) emissions from specific sources (by region, sector and

compound). And even the coupled models may not fully include the non-linear chemistry due to the coarse resolution of current climate models. So, the approach by the community is to build simpler models (e.g. FAIR, Smith et al., 2018).

There are two major steps in the cause-effect chain going from emissions to temperature change. First the relation between emissions and the effective radiative forcing, and then the relation between ERF and temperature change. For the relation emission ==> ERF we have performed an additional sensitivity test that where we include the non-linear effect of methane forcing efficiency, i.e., decreasing with increasing background levels of methane (see also response to comment by referee #1). For aerosols and ozone precursors we do account for the part of the non-linear effects of emissions taking place in different regions with differences in the physical climate (e.g., temperature, radiation and precipitation) by using simulations from the HTAP experiment to calculate the em ==> conc relation for 13 global regions and then a 4-D radiative kernel to get to the global ERF. This means that our AGTPs have different values for e.g. SO₂ emissions in Europe vs. South Asia because the oxidation, transport processes and removal by precipitation is different. The part of the non-linear effect caused by the changing background levels of the pollutants in the different emissions scenarios (e.g., saturation effects in ozone chemistry or cloud responses to increasing aerosols in a higher background pollution case) is less well quantified and is not included in our analysis.

For the relation ERF ==> global temperature change we use a standard two-term impulse-response function relating global mean ERF to global mean temperature change. This has been, and still is the standard approach, in simplified climate models (and the rationale for using the GWP-metric). In coupled climate models there are indications that feedbacks (and thus climate sensitivity) are state-dependent, i.e. that the sensitivity increases as the Earth warms. However, at this point, this is still not fully understood and is not well quantified at intermediate warming levels as it diagnosed from 4xCO₂ experiments of CMIP6.

Smith, C. J., Forster, P. M., Allen, M., Leach, N., Millar, R. J., Passerello, G. A., and Regayre, L. A.: FAIR v1.3: a simple emissions-based impulse response and carbon cycle model, *Geosci. Model Dev.*, 11, 2273–2297, <https://doi.org/10.5194/gmd-11-2273-2018>, 2018.

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?

As per the established emission metrics framework, temperature responses are reported in terms of the emitted species, not the subsequent forcing mechanism. The ozone precursors include the impact of ozone and methane. In addition, we include nitrate aerosols, which is only recently becoming more common. In response to this and comments above, we have added a sentence in the methods after the AGTP equation to better clarify this point to readers outside the metrics community, referring the reader to the careful documentation existing in the previous literature: “Emissions of SLCFs can have both direct and indirect radiative effects. For BC, OC and SO₂ we account for the direct, semi-direct and indirect RF as described below. AGTPs for NO_x, CO and VOC includes the forcing due to tropospheric ozone production and (for NO_x) nitrate aerosol formation, as well as the longer-term effect on methane lifetime and methane-induced ozone loss. The AGTP for methane includes the direct forcing, as well as the effect of OH-induced changes in its lifetime and adjustments to account for indirect effects on tropospheric ozone and stratospheric water vapor. See Aamaas et al. (2013) for details and AGTP equations for individual species.”

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.

We thank the reviewer for raising this point. We use the historical, present-day and future emissions from the CEDS and SSP-RCPs inventories developed for CMIP6, and methane emissions follow the assumptions made there. From comments by both referees, we realize that the Methods discussion did not describe this very clearly and have expanded it. We also add a list of the sectors considered and their definition. While a comprehensive assessment of the influence that drive methane emissions is beyond the scope of this study, we have on several occasions added more details, following more specific comments by referee #1. The following new paragraphs have been included in the Methods section:

“Historical emissions are from the CEDS database, while future emissions follow the SSP-RCP scenarios. Gridded and harmonized emissions are available via ESFG from the Integrated Assessment Modeling Community (IAMC) for nine SSP-RCP combinations that form the core of the Coupled Model Intercomparison Project Phase 6 (CMIP6) experiments (Gidden et al., 2019): SSP1-1.9, SSP1-2.6, SSP2-4.5, SSP3-7.0, SSP3-7.0 lowNTCF, SSP4-3.4, SSP4-6.0, SSP5-3.4, and SSP5-8.5. The gridded SSP-RCP data product, including the methodology for country and sector level emission mapping, is documented by Feng et al. (2020). Regional and sectoral emission scenarios are extracted using the geographical definitions and spatial mask from HTAP2 (Janssens-Maenhout et al., 2015).

We consider the energy (ENE), agriculture (AGR), waste (WST), residential (RES), industry plus solvents (IND), transport (TRA) and shipping (SHP) sectors, as they are defined in the harmonized CEDS-SSP emission inventory (Feng et al., 2020; Hoesly et al., 2018). Due to the large spread in historical estimates and lack of emissions consistent with CEDS, we do not include emissions due to land-use/land cover. Additionally, agricultural waste burning is excluded as these are more difficult to mitigate and estimates of future CO₂ emissions are not available.”

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

We have expanded and added:

“These balancing characteristics do not imply that SLCF emission reductions measures should not be implemented, but that the net benefits on global temperature may be lower than expected if mitigation measures that simultaneously affect both cooling and warming SLFCs are implemented, in turn also placing added focus on the need to reduce CO₂ in order to mitigation warming in both the near- and long-term. Such detailed characteristics at the emission source level are needed for the design of effective mitigation strategies.”

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

We have added:

“While previous decades have seen a southeastward shift in air pollution emissions, from high income regions at northern latitudes to East and South Asia, these findings suggest that a second shift may be underway, towards low- and middle-income countries in the developing world. Further studies are needed to improve the knowledge about the resulting climate and environmental consequences, as well as how to strengthen the mitigation options, in these regions.”

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

We have expanded the explanation and the section now reads:

“Secondly, as described in Sect.2, we use an AGTP for BC that is 15% lower than in previous studies using the same methodology. This is done to account for the rapid adjustments associated with BC short-wave absorption (Stjern et al., 2017), which has been found to reduce the effective RF in a range of global climate models via changes in stability and cloud formation (Smith et al., 2018). For

our study, this factor applies to BC emissions from all sources and hence results in a reduced the net warming impact.”

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

While we agree that the recent CMIP6 results on ECS is interesting, we feel that a detailed discussion would distract from the core of the present study. We have added the reference to Zelinka et al. (2020) where the reasons for the difference in ECS estimates are discussed.