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

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This paper presents a summary of the works that were conducted within the frame of the European ECLIPSE project. First the overall strategy of the project is declined and then, the results obtained at every step of the project are exposed and discussed. The choice of a multi-angle approach (SRF, GTP, model scenarios) brings strength to the results and allows providing important and new advances for the climate change research community. The paper is well written and organized and the quite large number of results is presented in a clear and concise way. Furthermore, the discussion about the consistency of the 2 paths of research is appreciable and allows the identification of future needs for climate research studies. As this article is a presentation paper, the methods and results of the works that exposed in the accompanying articles will not be commented, and I will rather focus on the way there are highlighted and discussed.

My comments and questions are the following: 1) The comparisons between the model outputs and the observations show an insufficient degree of restitution of the gas & particle concentrations in different regions of the Earth atmosphere. However, this lack of representativeness of the models is only mentioned, but it is not considered in the discussion in the rest of the paper. Ex : Does the amplitude of BC underestimation in the Arctic or the overestimation of SO2 in the continental atmosphere strongly affect the final result about the impact of BC and SO2 emission reductions on the final temperature increase? The compensation between the impact of OA and BC may also be altered by a wrong representation of the BC to OA ratios in the models. Furthermore, it is not mentioned how/if the improvement of OA reactivity and formation (or BC lifetime changes) in the ECLIPSE models quantitatively changed the model predictions compared with observations. It is not fully satisfactory to consider that the models well restitute the NOx concentrations when they both under- and over- estimate observations. The issue of NOx is a problem of proximity and nothing is said about the regionalization of over- and under- estimation. And the evaluation of the efficiency of a scenario (on an air quality basis) relies on the exposure of citizens to high NOx concentrations. Finally, the reader there may wonder if the differences between the results of the models in the reference run are linked with the differences in the predicted impacts of emission control on climate change. Then, the predictions of a given model that under- or over- estimates the concentrations of a given SLCP could be interpreted in the light of its comparison to measurements.

2) The same questions arise concerning the ability of the models to restitute past behaviors. Figure 6 indeed shows that models reproduce the past changes in the warming trend during the 1990-2005 period, but the absolute amplitude of the trend is not well captured by the models. Despite this, these discrepancies are not considered as a limitation for the interpretation of model predictions in the rest of the discussion.

3) Concerning the models, insufficient information is given about their differences and similarities, which is a crucial point when running an ensemble. In particular, when

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looking at the diversity of responses to BC forcing, the way they consider the direct and semidirect effects of BC should be detailed. Secondly, the constitution of a model ensemble is also questioning, due to the low number of models running on a same compound for some experiments. The differences and similarities in the model structures thus strongly impact the amplitude of the answers. Considering this, the mean model response as well and the range of their responses to emission perturbations have to be interpreted with caution.

4) About the constitution of the MIT scenario: the mitigation basket is obtained through a selection of emission control measures on the basis of their potential for reducing the global warming. Such a procedure asks several questions: On which basis are the options combined? Are the set of measures consistent in terms of operational set-up? Isn't there a possibility that the combination of several measures is not politically or financially realistic? Finally, is the basket realistic for Air Quality ? Indeed, as the selection is based on GTP20, it is possibly not the most expectable basket for air quality. There may be other actions (considered as more efficient) that will have to be considered in the future years to reduce the exposure of urban citizens to air pollution, and it would have been interesting to consider their potential for limiting the global warming rather than considering only SLCP actions on the basis of their GTP20. Of course, rethinking the MIT scenario is not the purpose of this presentation paper, but the discussion about the fact that "the co-benefits of the non-CH4 SLCP mitigation measures are quite limited" is strongly affected by the constitution of the mitigation basket, and this may be highlighted. This is important because the evaluation of the Air Quality Impacts of Short-Lived Pollutants is one focus of the paper, as mentioned in the title.

More technically: The gain in air quality brought by the MIT scenario compared with the CLE scenario is shown for ozone and PM2.5, but the absolute improvement in the concentrations, as modeled in the CLE scenario, is not shown (it is just rapidly mentioned in the text). It makes difficult the appreciation of the gain of the mitigation scenario.

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