Atmos. Chem. Phys. Discuss., 14, C195–C197, 2014
www.atmos-chem-phys-discuss.net/14/C195/2014/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "The climate impact of ship NO_x emissions: an improved estimate accounting for plume chemistry" *by* C. D. Holmes et al.

Anonymous Referee #1

Received and published: 23 February 2014

The authors presented and discussed a new representation to describe in a parametric form the dispersion of a plume emitted from a ship in the atmosphere. In chemistry-climate models, these processes needs to be treated with a sub-grid scheme. They claimed that the more accurate description including eight meteorological and chemical factors (end section 2.1) improves the calculation of the chemical species concentrations and the subsequent radiative forcing (RF). In my opinion, this research is interesting and timely since it attempts to show the role of small spatial and temporal scales in large scale processes. However, the research fells short in showing clearly and in an elaborate way the benefits of the new parameterization. Moreover the evalu-

ation against observations is very short questioning the improvements in using the new parameterization. In consequence, the paper should largely improve to convince the reader that the new parameterization is necessary to be included in chemistry-climate models. Below, I include my main remarks.

1- As mentioned, the authors introduce a new parameterization, but the reader is left alone in the formulation and the sensitivity analysis.

a) Could they describe the equations/functions that forms the base of the new parameterizations and their dependences? Perhaps an Appendix is necessary to be included.

b) How sensitivity is the parameterization to the 8 factors mentioned? In my opinion, this is a key part of the research. The authors need to show which variables are relevant. Are the 8 factors equally important? Under which meteorological and chemical situations?

2- Clouds play a key role (stratocumulus, shallow and deep convection) in marine boundary layers and can regulate differently the dispersion and transformation of chemical species (Verzijbergh et al., 2009, Atmos. Chem. Phys. 9, 1289-1302). How are the dynamic and radiation effects of clouds included? In my opinion, a better description of clouds could be more beneficial that the new parameterization of dispersion. The authors need to discuss these aspects in their article.

3- Closely connected to the previous point, and due to the lack of description of the parameterization. I am a bit surprise that an important sub-grid effect, the segregation of species, is not discussed neither included (Sykes et al., 1992, Atmospheric Environment 26A, 2565-2574; Galmarini et al., 1995, Atmospheric Environment 29, 87-95)? As far as I know, the limitation and inefficient mixing by turbulence can retard the chemical transformations in the first hours after emission. Could they explain if this process is included? If not, could they omit it?

4- I miss throughout the paper a systematic validation (including uncertainties) with

respect observations. I think it is fundamental to include this information to confirm the improvement of the new parameterization.

5- The last sentences of the conclusions are a bit confusing. The authors mentioned that there are uncertainties in the background atmosphere related to the emission and model formulation. What sort of uncertainties? Would it be better to explain these uncertainties (I guess related to clouds, non-uniform emissions,...) and place them if they are more important that the processes represented by the new parameterization?

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 3427, 2014.