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Interactive Comment

Interactive comment on "Quantifying the contributions of natural emissions to ozone and total fine PM concentrations in the Northern Hemisphere" *by* A. Zare et al.

Anonymous Referee #2

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The manuscript "Quantifying the contributions of natural emissions to ozone and total fine PM concentrations in the Northern Hemisphere" by Zare and co-authors try to improve predictions of ozone and fine particulate matter (especially, the fine fraction, PM2.5) by including several new emission sectors (lightning, soils, wild animals, etc.). In this sense, the contributions of natural emissions of several precursors and trace gases to ozone and PM2.5 formation have been assessed for the year 2006. The DEHM model has been updated with several improvements, which are discussed along the manuscript.

The paper addresses and interesting and sounding topic related to climate impacts on air quality and therefore it is worth publishing in Atmospheric Chemistry and Physics.

Although the paper is well organised and detailed, there are some aspects of the paper that require revision before publication. My main objections for the publication of the paper in its present form are detailed below.

General comments:

Despite I am not a native speaker, I have detected some mistakes in the grammar, that has to be consistently checked throughout the manuscript.

My main concern is related to the validation of the simulations. The authors have performed an interest study on how to improve the annual simulations of air quality over the northern hemisphere; however, to make sure that there is a real improvement in the simulations, an extensive validation against measurements have to be performed. I know the availability of measurements is very scarce for several species (e.g. monoterpenes), but I have strong doubts that considering just 2 stations in Europe and 2 in the United States can provide the validation needed for this type of study.

Moreover, I did not find any validation of trospospheric ozone in the manuscript. I have no doubts about the improvement in the emissions included in the DEHM model, but I have no information about whether these improvements lead directly to an improvement in ozone predictions or not.

Emissions included in DEHM come from different sources, use different methodologies, are gridded at different resolutions. . . How could you assure the consistency between those different databases and methodologies?

In addition, I am not sure if using emissions inventories representative for a climatological period (that is, non-yearly specific emission inventories) can lead to an improvement in the simulations for a specific year of study (year 2006). I know it is computationally demanding, but including a longer period would contribute to remove the yearly-specificity in the results presented.

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Last, for PM2.5 the authors find a reduced bias when implementing the aforementioned developments in DEHM, but I really miss a detailed discussion on how this bias is reduced. So the reader is left with a lack of information on what the motives for improved simulations are. In this sense, conclusions are not very conclusive. I would expect a deeper analysis of the implications of this study for the state of the art and not a summary of what I've read before in the manuscript. The authors should elaborate on this a little bit more.

Minor comments:

The authors have to be very careful when using ppbV, since this is not a concentration unit, but is used for expressing mixing ratios. So I would use micrograms per cubic meter instead of ppbV when referring to concentration of trace gases.

The authors should be consistent with the use of "O3" and "ozone" throughout the text.

The figures and tables in the manuscript are also relevant, but the quality of some figures should be improved before final publication (as stated below).

Figure 2 is really hard to read; have the authors considered using a double Y-axis (or a logarithmic Y-axis) so the readers can appreciate the behaviour of those monoterpenes with low annual fluxes?

The colour scale in Figure 4 should be changed, since the readers cannot appreciate any colour differences between 0.1 and 0.7 (concentrations of SOA).

I cannot find any reference on the text to Figure 15; so if the authors want to keep this Figure, as it defines the different domains analysed, I would cite in the methodological part, together with the description of the modelling/analysis features.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 16775, 2013.