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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Effects of two different biogenic emission models on modelled ozone and aerosol concentrations in Europe" *by* Jianhui Jiang et al.

Anonymous Referee #3

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Review Summary

Jiang et al. simulated ozone and aerosol concentrations in Europe using two different biogenic emission models (PSI and MEGAN) to probe uncertainties in regional air quality models. They compared model results with ozone observations from the European air quality database, AirBase, and aerosol observations from eight different measurement locations with an Aerodyne AMS or ACSM. Results were generally consistent with previously published papers demonstrating that MEGAN tends to over-estimate isoprene and under-estimate monoterpene emissions. They also found that the simulated ozone mixing ratios between the model runs varied less than the isoprene emis-

sions. This is also consistent with previous studies showing much of Europe's ozone production is NOx-limited rather than VOC-limited. Finally, their model comparison suggests higher monoterpene emissions lead to better comparison between simulated and observed organic aerosol. The authors acknowledge this could be due to compensating factors (e.g. they could be "right for the wrong reasons"). Overall, the scientific approach is reasonable and the scientific questions are appropriate for the scope of the journal. However, it is unclear what information this paper is adding to the scientific community that has not already been published in previous papers. There are also a number of gaps in the methods section that lack clarity. I recommend publication after the manuscript is revised to address the following comments.

General Comments The authors should better clarify how this particular paper is filling in gaps that have not already been addressed in previous publications. All results sections generally state the results are consistent with work that has already been published, and so it is very unclear what the conclusions from this paper are adding to the growing body of scientific knowledge. The manuscript could better highlight how this work is filling in unique gaps in understanding.

Specific Comments METHODS: SECTION 2.2.1 EMISSION RATES Authors state they estimate reference emission rates of isoprene and monoterpenes based on Lamb et al., 1993 but then go on to say Norway spruce isoprene emissions were estimated to be 10% of alpha-pinene. It is unclear why Norway spruce was handled differently and why it is singled out to be described separately from the other plant species. Please clarify.

The PSI model emission rates are species-specific except for "pasture" and "crop" (Table 1). How much variability would you expect between different types of "pasture vegetation" and "crop vegetation" based on the literature? What proportion of the total area covered in the model is characterized as "pasture" and "crop"? Is it a significant portion of the land that could drastically impact results or is it minor?

Sesquiterpene emissions: authors state that sesquiterpene emissions were assumed to be 5% (by weight) of monoterpene emissions based on field measurements from various studies, but then cite a single paper that is actually a modelling paper and not a review or synthesis of measurements. Please cite the original literature from which this "5% (by weight)" reference is derived.

METHODS: SECTION 2.2.2, RESPONSE FUNCTIONS If sesquiterpenes are being treated as pooled emissions as stated in the previous section, then they will be treated similar to monoterpenes. However, the authors do not discuss what β value they used for sesquiterpenes. In Guenther et al., (2012) the empirically-derived temperature correction coefficient, β, for sesquiterpenes was 0.17. Was that the value used in this study as well? Please clarify.

Again, the authors single out Norway spruce emissions being handled a bit differently than other plant species. In this case, the Norway spruce monoterpene emissions have some light-dependent fraction estimated based on a study in 1993. Why is Norway spruce being singled out for more detailed emission estimation? Is it the dominant species in the modeling domain? This should be clarified. Also, more details should be included about how the light-dependent emissions were estimated instead of just referring to the 1993 paper with no summary of what information was taken from that paper and used in this study. Finally, does this section then imply that all other monoterpene emissions were light-independent? Can this be stated more clearly and justified? If all monoterpene emissions are being treated as light-independent (except for some unstated fraction of Norway spruce monoterpene emissions), then this should be justified because it is well known that a substantial fraction of monoterpene emissions are light-dependent; for example, in MEGANv2.1 the light-dependent fraction of monoterpene emissions ranges from 40-80%! (see Guenther et al., 2012, Table 4).

METHODS SECTION 2.2.3 INPUTS OF DRIVING VARIABLES

Unclear how GlobCover 2006 data is being used to derive species-level distributions.

How did the authors go from fractions of "needleleaf, broadleaf and mixed forests" to plant species distribution using the profiles from Simpson et al., 1999? There is missing information here that links the two.

How much of variation between PSI and MEGAN emissions was driven by differences in normalizing emissions to leaf surface area (MEGAN) versus leaf biomass (PSI)? Are there potential biases that could vary between plant types for comparing total canopy-scale flux that arise from how surface area versus biomass are scaled up? How did the authors ensure they were making meaningful comparisons between the models with the emissions normalized differently? Figure 2 was clear: authors graphed the emission rate per model grid cell. It was less clear how this comparison was done in Figure 3 where the graph simply shows the emission rate. Was this also per model grid? Per entire modeling domain? This should be stated more clearly.

Figure 2: right axis label is cut off on third row.

Figure 4: Figure caption should be re-worded. Currently states, "Mean bias of surface O3 mixing ratios in the afternoon 12:00-18:00 UTC) for each bin of observed ones in July 2011." I suggest revising to more clearly describe what is meant by "observed ones".

RESULTS O3: results are consistent with previous literature demonstrating that O3 production in most of Europe is in a NOx-limited regime as opposed to a VOC-limited regime and thus the isoprene differences between the two models do not translate into large differences in ozone. Not a novel result. Can the authors comment on how this study is different from previous ones that have published the same result?

OA comparison: the study does not have a single AMS/ACSM location in the Northern Europe region. Surely there are measurements at Hyytiälä, or some other boreal forest site in northern Europe. Can you justify why no measurement sites were included for northern Europe?

SECTION 3.2.3: INORGANIC AEROSOLS Authors do not set up a rationale in the introduction for investigating the impact of changing biogenic emissions on inorganic aerosols. Why would differences in biogenic emissions substantially alter inorganic aerosol? Without this rationale in the introduction, this section does not fit with the rest of the paper.

DISCUSSION Authors end the paper by saying, “In future studies, BVOC emission models with more regional specific adaptation in vegetation types and emission factors are urgently needed to reduce the uncertainties in BVOC emission estimates in order to improve air quality modelling.” Why is this the recommendation rather than simply improving the emission factors for the plant functional types in MEGAN? It is not reasonable to model the emissions from every single plant species on the planet. I don’t agree that the results from this study emphasize the need for more plant specificity because even this paper only used a sub-set of 10 specific plant species (with an additional two general classes for “pasture” and “crop”). It seems to me that the major finding from this paper, consistent with published papers before it, is that the MEGAN emission factors could be updated and improved.

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