Atmos. Chem. Phys. Discuss., 12, C3271–C3278, 2012
www.atmos-chem-phys-discuss.net/12/C3271/2012/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Evaluation of two isoprene emission models for use in a long-range air pollution model" *by* A. Zare et al.

Anonymous Referee #2

Received and published: 5 June 2012

GENERAL COMMENTS:

The authors present an original method of evaluating and comparing two isoprene emission inventories; that of GEIA and MEGAN. They make use of background ozone from the European EMEP observation network and compare it to simulated ozone from the DEHM chemical transport model. The authors show that overall the day-to-day variability of background ozone is better captured with MEGAN.

It is not clear in the Introduction why the authors choose to compare the GEIA and MEGAN emission inventories, when Arneth et al. (2008) found that they have similar global isoprene emissions. Are there large regional discrepancies? The GEIA inventory, which precedes MEGAN, has been replaced by MEGAN in most global chemical transport models. Are research groups still using the GEIA inventory to estimate iso-

prene emissions? Both inventories use empirical algorithms of the same heritage. It would be easier to justify a study that compares MEGAN and the process-based LPJ-GUESS inventory (Arneth et al., 2007) and the influence of these two inventories on ozone levels in remote, rural locations in Europe. I suggest that the authors provide a justification for their comparison and use appropriate references to back their approach.

The authors do not demonstrate familiarity with relevant literature in the Introduction. The authors do not quote the source of relevant findings, but instead reference papers that quote findings in their introduction. For example, Guenther et al. (2000) is quoted to substantiate that emissions of natural NMVOCs exceed emissions from anthropogenic NMVOCs. This information is quoted in the introduction of Guenther et al (2000). More appropriate references include Guenther et al (1995), where total natural NMVOC emissions are estimated, and Oliver et al. (1996), a global anthropogenic emissions inventory.

The authors reference studies that have used emission inventories in their analysis, but without any information about the relevance of those studies to their comparison and evaluation. For example, the authors state that "A number of studies have also been conducted to evaluate results of the integrated BVOC models . . . with satellite, aircraft or ground-based observations . . ." on pg. 9251 line 5-7, but do not provide information about the major findings of these studies or at least the information from these studies that is relevant to their own.

The justification that the authors provide for evaluating the isoprene emission inventories in terms of ozone observations instead of isoprene concentration measurements is somewhat contradictory. They state on pg. 9248, line 5-6 that "Isoprene has a short lifetime, and hence it is very diïňČcult to evaluate its emission estimates against measurements" and reiterate on pg. 9264 line 6 that "Due to the scarcity of observed isoprene data and the short lifetime of isoprene . . . an indirect evaluation was made based on measurements of ozone concentrations in Europe". The authors use ozone data that was measured at the same sites in which isoprene concentrations were made

ACPD

12, C3271–C3278, 2012

(summarized in Table 1), implying that if isoprene data is scarce, so too is ozone data. Although the lifetime of isoprene is short, isoprene emission inventories could also be evaluated by comparing measured isoprene concentrations with simulated isoprene concentrations in DEHM using the two different emission inventories, as enhanced levels of isoprene is coincident with isoprene emissions. This reduces uncertainties in transport (as isoprene is short-lived) and isoprene oxidation chemistry (as loss of isoprene due to oxidation occurs in the first oxidation step, whereas ozone formation is complicated by local concentrations of hydrocarbons and NOx) in the model. My concern here is that, in order to compare simulated and measured ozone, transport and NOx concentrations in the model should be validated, or a validation paper using DEHM should be referenced in text.

The authors should make more of an effort to determine what makes the largest contribution to discrepancies between the two emission inventories. The authors conclude the results section by stating that "The better performance using MEGAN can be attributed to a better parameterization of environmental activity factors or more accurate emission factors in MEGAN." Considering that those are the only inputs for estimating isoprene emissions, this is an obvious statement that leaves the reader dissatisfied with the analysis. I recommend that the authors determine the largest contributor to the discrepancy (i.e. emission factors due to different land type maps used for example; or the scaling factors – that is the temperature or radiation or leaf age etc. used).

Overall the manuscript gives the impression of being rushed. There are grammatical errors that should have been picked up, such as the use of "nitrate proxy" instead of "nitrogen oxide" on pg. 9259, line 18. The figures include captions and colorbars that are only legible if the manuscript is viewed at 200-300 times its printable size.

I suggest publication of the work in ACP, but only after major revisions to the manuscript.

SPECIFIC COMMENTS:

ACPD

12, C3271–C3278, 2012

Why mention other isoprene emission inventories in the Introduction (BEIS series, se-BVOC and BEM, for example)? The paper focuses on evaluation of MEGAN and GEIA. It is not clear what inclusion of these emission inventories adds to the Introduction or analysis that follows.

The statement "...but not evaluated with observations." on pg. 9251, line 18-19 is not correct. Curci et al. (2010) compared satellite-derived isoprene emissions with MEGAN in Europe.

Be careful about using decisive statements such as that on page 9251, line 23. Arneth et al. (2008) carry out a detailed comparison of numerous isoprene emission inventories. Does your analysis offer the advantage of a hemispheric analysis (and here the authors should define what they mean by hemispheric, i.e. the model domain), with particular focus on the sensitivity of O3 in remote regions to changes in isoprene emission?

How well does the model reproduce NOx conditions over the model domain and/or the EMEP measurement sites? This would be an important point in the discussion for isolating isoprene emissions as the major uncertainty in the model.

On pg. 9254, line 12-13 the authors choose to not use the soil moisture parameterization in MEGAN, despite inclusion of water-limited regions such as the Sahel. They comment on the role soil moisture plays in increasing the discrepancy over these water-limited regions (pg. 9257, lines 7-9). Please include a justification for exclusion of the soil moisture parameterization.

On pg. 9255, line 15 the authors provide the annual isoprene emissions for the study area covering the northern hemisphere and much of the tropics. The emissions from MEGAN are close to the upper limit of the global estimates reported from previous studies (pg. 9255, line 16). What contribution does the excluded region in the southern hemisphere make to global emissions in the DEHM model? Wouldn't this lead to emission estimates that are higher than 770 Tg isoprene y-1?

ACPD

12, C3271–C3278, 2012

Are the authors implying that on pg. 9257, line 19-20 MEGAN base emissions of shrubs are higher than those in GEIA, thus contributing to discrepancies in the African savannah and the subtropics? This sentence should be made clearer.

Pg. 9258, paragraph 2: A description or figure of the sites listed in Tables 1 and 2 relevant to isoprene emissions would be useful. What vegetation types dominate these sites? What is the average leaf area index (annual or summertime) for these sites? Or are these sites described in a paper that the authors can reference in this section?

On pg. 9258 line 28 the authors state that "...MEGAN results have a very small difference from observed mean values". However, in Table 1 the discrepancy is much larger for Peyrusse Vielle (France). The authors don't discuss this exceptional site. What contributes to the large discrepancy for this site?

Pg. 9262, line 12-14: MEGAN and GEIA are evaluated in Figure 12 in terms of their correlation with measurements. However, Figure 12 shows that the strength of MEGAN is its ability to capture day-to-day variability of daily average O3 and daily maximum O3. This should be included in the discussion of Figure 12.

MINOR COMMENTS:

Pg. 9249, line 12: "... inhabitant of agricultural products, ..."does not make sense. Rewrite sentence or replace clause with "... crops, ...".

Pg. 9251, line 7: The list of references presented here should include Curci et al. (2010), which compares satellite-derived isoprene emissions with MEGAN in Europe.

Pg. 9251, line 31: Replace "... showed ..." with "...presented...".

Pg. 9252, line 9: Provide latitude and longitude boundaries of the model domain.

Pg. 9253, line 13-14: This sentence does not make sense. Remove or alter.

Pg. 9253, line 27: Define PCEEA (i.e. provide the full name and a brief description).

ACPD

12, C3271–C3278, 2012

Pg. 9254, line 29: The authors state that they focus on 2006 to coincide with the analysis year of Guenther et al. (2006). However, the analysis year in Guenther et al. (2006) is 2003. The publication year is 2006. Please fix this inconsistency.

Pg. 9257, line 26: Please specify the upper limit (in km or hPa) of the "lowest model layer".

Pg. 9258, line 25: Place the temporal variability information "3h" and "twice a week" earlier in the paragraph when the authors distinguish between Rigi, Switzerland and the other sites, so that it is clear what the authors mean by "time variant" on pg. 9258, line 15.

Pg. 9261, line 18: MEGAN is only marginally better than GEIA in the average statistics that are reported. This should be stated in text when describing Figure 11.

Pg. 9262, line 4-6: Please justify why these two sites were selected for further analysis in preference to any other background sites.

FIGURES:

Figures 1 and 4: The unit "KT/grid/year" is not defined in text and not consistent with units used in other isoprene emission studies. Please change this to "atoms C cm-2 s-1" or "mg m-2 day-1".

Figure 2: Similar to the above comment "KT/day" is not comparable to other studies. Please change to be either "atoms C s-1" or "mg day-1".

Figures 1, 3, and 5: Please change the color scale of difference maps to that shown in Figure 9 for better visual clarity.

Figures 1, 3-7, 9-10, 12: Please increase the relative size of colorscales so that they are legible.

Figure 7: Please provide the altitude of the lowest model layer of DEHM in the title of this figure.

ACPD

12, C3271–C3278, 2012

Figure 9: “Noisoprene” is not one word. Please fix.

Figure 9: Bottom two left panel labels are ambiguous. Are the authors showing the percentage contribution of isoprene to ozone in DEHM after running DEHM with and without biogenic isoprene emissions? The labels imply that the bottom two left panels are difference plots. Please fix the labels and provide a clearer title for this figure.

Figure 11: (1) Start the x- and y-axes at ∼25-30 ppbv so that the data occupies more than 20% of the plot. (2) Is it necessary to plot labeled data points when it isn't possible to distinguish labels? Just include symbols in this figure.

REFERENCES Arneth, A., Monson, R. K., Schurgers, G., Niinemets, Ü. and Palmer, P. I.: Why are estimates of global terrestrial isoprene emissions so similar (and why is this not so for monoterpenes)?, Atmos. Chem. Phys., 8, 4605-4620, 2008.

Arneth, A., Miller, P. A., Scholze, M., Kicklet, T., Schurgers, G., Smith, B., Prentice, I. C.: CO2 inhibition of global terrestrial isoprene emissions: Potential implications for atmospheric chemistry, Geophys. Res. Lett., 34, L18813, doi:10.1029/2007GL030615, 2007.

Curci, G., Palmer, P. I., Kurosu, T. P., Chance, K. and Visconti, G.: Estimating European volatile organic compound emissions using satellite observations of formaldehyde from the Ozone Monitoring Instrument, Atmos. Chem. Phys., 10, 11501-11517, 2010.

Guenther, A., Hewitt, C. N., Erickson, D., Fall, R., Geron, C., Graedel, T., Harley, P., Klinger, L. F., Lerdau, M., McKay, W., Pierce, T., Scholes, B., Steinbrecher, R., Tallamraju, R., Taylor, J. and Zimmerman, P.: A global model of natural volatile organic compound emissions, J. Geophys. Res., 100, 8873–8892, doi:10.1029/94JD02950, 1995.

Guenther, A., Geron, C., Pierce, T., Lamb, B., Hartley, P., Fall, R.: Natural emission of non-methane volatile organic compounds, carbon monoxide, and oxides of nitrogen from North America, Atmos. Env., 34, 2205-2230, 2000.

ACPD

12, C3271–C3278, 2012

Guenther, A., Karl, T., Harley, P., Wiedinmyer, C., Palmer, P. I. and Geron, C.: Estimates of global terrestrial isoprene emissions using MEGAN (Model of Emissions of Gases and Aerosols from Nature), Atmos. Chem. Phys., 6, 3181-3210, 2006.

Olivier, J. G. J., Bouwman, A. F., van der Maas, C. W. M., Berdowski, J. J. M., Veldt, C., Bloos, J. P. J., Visschedijk, A. J. H., Zandveld, P. Y. J. and Haverlag, J. L.: Description of EDGAR Version 2.0: A set of global emission inventories of greenhouse gases and ozone-depleting substances for all anthropogenic and most natural sources on a per country basis and on 1° x 1° grid, Bilthoven, The Netherlands., 1996.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 9247, 2012.