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Biogeosciences Discussions

Interactive comment on "A new European plant-specific emission inventory of biogenic volatile organic compounds for use in atmospheric transport models" *by* M. Karl et al.

Anonymous Referee #1

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This paper presents a new estimate of biogenic VOC emissions in Europe, with a somewhat different approach to that used in the recent NATAIR inventory of Steinbrecher et al. (2008). Given that BVOC emission estimates have so many uncertainties, alternative calculations are in my view welcome, although all such alternatives are limited by a lack of new BVOC emission measurements. The link to MEGAN with a simplified system of equations is also useful

The authors have explicitly aimed to provide transparency in their data-sources and calculations, and on the whole they are to be congratulated on that. I appreciate the extensive tables and their notes, which make it easy to see where emission factors

come from, although I request some alternative outputs below.

I have some rather serious concerns though about the calculations presented in this paper, especially for crops. I will address the major issues first, then return to some detailed comments:

1. It isn't clear how the emissions from crops have been calculated. Although the text explains how the annual crop yields were determined, how were these yields converted to appropriate LAI and biomass (d) values over appropriate growing seasons?

2. Some of the emission factors (EFs) look very high, and are not consistent with the literature cited. Examples:

2a) the EF for durum wheat is given as 1.0 ug/g/h for isoprene, citing Lamb et al., 1993. The Lamb paper gives an EF of 0.041 ug/g/h for all BVOC, with 0.02 ug/g/h suggested for isoprene, a factor 50 different from that suggested here. Also, was all wheat assumed to be durum?

2b) the EFs given for nurseries are huge compared to other agricultural EFs, e.g. 17.76 ug/g/h for isoprene. The Table notes explain that this EF should come from that of mixed forest from GLC, but Table 5 gives an isoprene EF of 10.9 ug/g/h. (Actually, this value also seems high to me, but more later). Further, the biomass density given for the nurseries is higher than that of the presumably mature GLC mixed forest - how can this be explained?

2c) The high emission factor for tomatoes, 30.3 ug/g/h for MT, is ascribed to Arey et al., 1991. This figure looks like a simple mean of the two values given by Arey et al., but the authors have not corrected for the fact that the Arey data were for temperatures of 35 and 38 C. Correcting for this I would get a mean EF of 17.3 ug/g/h, not 30.3 as given.

2d) which raises the question, are all EFs just used direct from the literature, without correction to standard conditions?

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2e) For tomatoes, then many of Europe's plants are grown in green-houses, even in southern Europe. I would guess that this would reduce the emission factors drastically.

2f) Table 7 suggests that agricultural emissions in southern Finland are about a third of forest emissions. Just doing back-of-the envelope calculations I wonder how this can be. Southern Finland is about 50% forest (Lindfors et al., 2000), and much of the rest of the area is wetland or not used for agriculture. Nationally, agriculture is about 7% of the land-area, but this fraction would be higher in the south. Let's assume 25%. With oats, barley and grass as the main agricultural vegetation, these would have BVOC EFs of around 1.7 ug/g/h and maximum densities of around 1000 g/m2.

2g) Forests cover at least twice the area, but have higher EFs of around 4 ug/g/h, and with their biomass density high throughout the warm seasons). Although light levels may be lower than optimum in Finland, I would still have expected a bigger difference between forest and agriculture.

2h) The Simpson et al. (1999) paper estimated forest BVOC emissions from Finland as 341 Gg/yr but crops as just 9 Gg/yr. How can this paper's estimates differ so widely?

2i) This raises again the question as to whether the maximum value of d and LAI were applied over the whole growing season?.

2j) Table 7 suggests agricultural emissions from southern Finland during September to Feb. Southern Finland is rather unkind to crops at these times of year. How did such emissions arise, and just how was the growing season calculated? One potential mistake would be to assume crops all year round and just let temperature decide emissions.

2k) The text given in section 4.3 seems to state clearly that the EFs for tomatoes are likely associated with sampling artefacts, so why were these rates allowed to stand, and be used as the basis for other land-cover EFs?

3. Also connected with crops, I would have found it helpful to see where the emissions

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are coming from, i.e. to see a Table with species-specific contributions.

4. More generally, I would have found some national data helpful, instead of the loosely-defined regions as given in Table 7. I think it would be better to show some detailed data for some of those countries for which detailed alternative reviews are available (e.g. Germany, Finland, UK).

5. This inventory uses LAI from MODIS as part of the calculations for both seasonality (eqn 9). However, MODIS only provides LAI for pixels, not for individual species of trees. Most importantly I don't see any discussion of how LAI of deciduous trees is distinguished from that of coniferous, or how crops and grasslands are distinguished from trees. Does the methodology presented really allow emissions in wintertime from deciduous trees if there are nearby coniferous forests generating non-zero LAI for that MODIS pixel? How is this done? (The MEGAN procedure seems to suffer from the same weakness, and the online data includes only one LAI value per grid-square which presumably should be used for all vegetation. Since MEGAN is so heavily focussed on tropical forests then this LAI problem is arguably less important for that inventory. For a European inventory this problem is rather critical though.

Detailed comments:

6. In general, the use of separate indices for trees, crops and other GLC classes leads to an unnecessary complication in notation.

7. p5006, line 26. The paper states that emission rates of OVOC have an EF of 1.7 ug/g/hr from Guenther et al. However, Guenther suggested emissions of two classes of other VOC, OVOC and ORVOC, both with an emission rate of 1.5 ug(C)/g/hr. Why doesn't the current paper consider ORVOC? Or does it include these in OVOC and half the emission rate?

8. What lies behind the assumption that 1.5 ug(C) is equivalent to 1.7 ug? Many OVOC have quite high O/C ratios and thus a higher emission factor might seem appropriate..

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9. p5006, Are the EMEP CORINAIR (1999) and CORINAIR (1999) references the same? Also, in many cases the Simpson et al. 1999 reference should be used in place of the EMEP/CORINAIR ref, as the latter is grey literature.

10. p4999. It isn't obvious that CO99 is a reference to plant-compositiona stated here. Presumably CO99 is for emission factors?

11. p4999. I am not sure I understand how the GLC emissions were obtained. Am I right in thinking that the mix of species for a given GLC class for non-EU countries is derived from a knowledge of the species distribution in those areas where further details are available, i.e. in the EU? Actually, this table suggests otherwise which is a missed opportunity I think.

12. p4996, line 8. Explain standard conditions here

13. p4996, line 24. What do you mean by non-needed complexity? Are the various extra complications of MEGAN not needed?

14. p4997, line 23. It might be worth noting that e.g. MEGAN uses mass of carbon, not mass of compound.

15. p4998, line 16. It doesn't make much sense to talk about a 10x10km2 inventory when the resolution changes from 5.5 to 9.6km. Was a specific lat/long resolution used, and if so which? Which projection is used?

16. p4998, line 20 - it is very confusing with the letter l as an index, as it is easily mistaken for the number 1 or letter i. If using l, at least use a curly l.

17. p5004, line 20. One cannot claim that a paper from 1995 is presenting "recent" data.

18. p5004,line 24. Say "Many species of conifer are associated" rather than just conifers are usually associated. The latter is in conflict with the fact that many spruce trees have been known for years to be isoprene emitters.

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19. p5005, it was surprising to see so few references for the emission rate of Norway spruce, given its importance in European forests, and the fact that this is one of the few species where recent measurements have been made for European ecosystems (Finland, Germany). The MT EF is based entirely on the old and limited data of Kempf. What about Grabmer also for MT, Janson and de serves (2001), Hakola et al. (2003), Tarvainen et al. (2007), and values used by Stewart et al. (2003) and Steinbrecher et al. (2008)

20. p5006. There seems to be a strong tendency to use the highest emission factors. For example, why prefer Pio et al.,s MT emission of 25ug/g/h over Steinbrecher et al's zero emission? Why prefer Haywood's extreme emission factor for Sitka over earlier measurements? What system is used to decide EFs?

21. p5022, line 7. Didn't MEGAN's European estimate build upon the Simpson et al. (1999) work, and so it should be consistent with that study at least?

22. Table 3. Emission factors are sometimes given to 3 significant figures, even for SQT. Should such accuracy really be suggested for emission where even the first significant digit may be wrong?!

Interactive comment on Biogeosciences Discuss., 5, 4993, 2008.