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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 #3

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Karl et al. present an interesting new biogenic emission modeling excercise for Europe, in which several complementary land use data bases are used to bring the level of detail as high as possible. Emission factors of plant species and crops are compiled from various old and new studies, and an attempt is made to account for the variability of the growth conditions in different parts of Europe through an innovative bioclimatic correction factor.

The paper is carefully composed and makes pleasurable reading. Much effort has been put in describing the approaches taken in the modeling as thoroughly as possible. The authors have also carried out several sensitivity runs to test their choices of parame-

terisation, and their discussion on these aspects is appreciated. One interesting result of this work is the finding that the new isoprene emission approach MEGAN, hailed as the next staple of the techniques used in biogenic emission modeling, only brings minor changes in the emitted isoprene amounts in Europe, except for the southermost end of the model domain.

General comments

While the authors widely discuss various other uncertainty aspects that could affect their results, they do not include a discussion of their chosen emission algorithms. Of course, the algorithms they use are the commonly accepted ones and at present time there are no obvious alternatives. However, in this work emissions are calculated around the year, also in areas with snow cover and below zero temperatures during several months, such as the Nordic countries and parts of western Russia. I would like to see some mention about the applicability of the algorithms - developed and tested for much warmer climates - in the northernmost regions of the modeling domain with their harsh winter conditions.

Closely connected with the previous issue is the fact that a large part of the northernmost fifth of the modeling domain is indeed covered with snow for long periods during winter, with the deciduous trees bare. How is this taken into account in the emission calculations? In Table 7, the authors give emission estimates for southern Finland, in which there are emissions from agriculture also during fall and spring and even in winter, even though the growing season at those latitudes is relatively short and harvesting takes place in September-October at the latest. I would assume that at least in the winter months there should be zero crop foliar biomass present and capable of emitting VOCs in Finland?

I agree with the Anonymous Referee #1 in that the authors should carefully rescreen their emission factor data base to ensure that they all refer to the same normalization temperature and PPFD. In addition to the paper cited by the Referee, some other

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contrary examples are Janson, 1993 (reference temperature 20°C) and Komenda and Koppmann, 2002 (25°C) as well as Lamb et al. 1993 who use normalization to PAR = 400 uE (i.e. umol) m-2 s-1 in their work. In this context, by the way, it is not obvious how the authors have arrived at their MT emission factor for Pinus sylvestris, which is stated to be the average of the first two of the above refences, since K&K give ranges of values for both young and mature trees while J only gives one number and there is no easy to detect combination that would yield the value 2.25. However, I tend to consider these possible inconsistencies in the emission factor compilation only a minor flaw instead of a serious problem as long as the chosen factors are meticulously enumerated, allowing further developers to adjust them where they see fit. Any emission factors existing in current literature are still inherently affected by large uncertainties - a situation which hopefully corrects itself with time as more comprehensive emission measurements become available.

Specific comments

Please, correct the spelling in the reference Bonn and Moortgat, 2003. The last name of the second author is NOT spelled with a second r before the second t (i.e. Moortgart) which, for some reason, seems to be a deep-rooted misconception in most of the papers I have seen their work referred to.

On the second row of the caption of Table 3 ORVOC is listed as one of the VOC classes. I believe this should read OVOC. And while we are at it, the authors should include some definition for their use of the term OVOC somewhere in the text, perhaps in Chapter 2.3.

On the second row of the caption of Table 4 ORVOC should also be replaced with OVOC.

In the footer of Table 5, row three, Pinus silvestris should read Pinus sylvestris.

Caption of Table 7: the term "three month averages" is not clear. Do the numbers in

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the table represent the average emission during the whole season (i.e. mg m-2 per three months) or are they average monthly emissions (i.e. mg m-2 per month) during each season? Or something completely different?

The quality of graphics in Figures 2-6 is very poor. The text and figures in the map panels are almost illegible especially on screen but also in the printer-friendly version. While it may not be possible to make improvements in the maps which are probably created in some supercomputer environment, it is certainly possible with modern graphics software to make at least Figures 5 and 6 less fuzzy. As they are now, the different gray shades do not separate well, especially in Figure 6 it is very difficult to see where one shade ends and another starts. I would suggest using outlines in the pies and the bars, which would allow the use of black and white and only a few gray shades instead of several. It also bothers me that the two panels in Figure 6 are unequal in size even though they both represent the same time period. They also have different text size which gives them a somewhat less professional look.

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