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Biogeosciences Discussions

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Interactive Comment

# *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.

**M. Karl et al.**

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We would like to thank anonymous referee #3 for his/her comments on our paper. We have considered all suggestions during the revision of the inventory and the manuscript. In the following we address the general and specific comments.

**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.*

Boissard et al. (ACP, 2008) demonstrated that isoprene emissions in Europe from 10°S to 60°N) are largely explained the dependence on instantaneous Tair and PPFD and the 3-week cumulated Tair.

A discussion of this is added to section 4.5 of the revised manuscript:
"The commonly observed temperature and light dependence of BVOC emissions seems to hold also at high latitudes in boreal and subarctic regions. It has been shown that the light and temperature dependent parameterization of BVOC emissions is applicable to northern boreal environmental conditions (Rinne et al., 2000; Tarvainen et al., 2005; Hakola et al., 2006; Tarvainen et al., 2007). Tiiva et al. (2008) demonstrated the increase of isoprene emission due to temperature increase in subarctic heath land (*Betula pubescens*). The effect was found to be a direct response to increased air temperature, which indicates a rapid physiological effect of current temperature on isoprene production as outlined in Lerdau and Gray (2003). The mean isoprene emissions from subarctic heath land were comparable to the emissions earlier found from a subarctic peatland ecosystem (Tiiva et al., 2007). The emissions from the subarctic peatland even increased 10-fold during an exceptionally warming period, again giving evidence for the validity of the proposed temperature- and light-dependence (Tiiva et al., 2007)."

*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?*

See reply to Referee #1, question 5.



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*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?*

In the revised inventory which implements emission factors from Steinbrecher et al. (2009) and a new growing season concept for agriculture crops, deciduous, and evergreen plants corrects the emission calculation. The emission activity of crops is now restricted to the length of the growing season in the different bioclimatic regions of Europe. The new approach ensures a fully consistent treatment of the growing season. In fact it is found that crop emissions are zero during winter (DJF) in Finland. Table 7 in the revised manuscript presents both the seasonal variation of emissions and the comparison with the country-based emission estimates from Simpson et al. (1999) for the main land use types.

*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 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 $\mu$E (i.e. $\mu$mol) m-2 s-1 in their work.*

In the updated manuscript, emission factors from the NatAir inventory (Steinbrecher et al., 2009) are adopted for all vegetation types. All emission factors are now given as normalized values, i.e. standard emission potentials in $\mu$g/g/h at 30 deg. C and PAR=1000 $\mu$mol/m$^2$/s.

*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*





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*of the above references, since KK 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.*

In the updated inventory, emission factors of Pinus sylvestris and all other trees were replaced by values from the NatAir inventory. Current uncertainties of standard emission factors are discussed in the manuscript, section 4.2 and 4.3.
Though plant-specific land use information is available with relatively high accuracy, a lack of plant-specific emission factors and biomass densities currently limits the setup of a highly accurate plant-specific emission inventory. The future development of an improved plant-specific inventory based on more accurate EF - and foliar density - measurements can probably provide much more reliable information on plant-specific basis.

**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.*

Spelling of this reference is changed in the revised manuscript.

*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*



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*replaced with OVOC.*

The definition of OVOC in our work is given in section 2.3 of the revised manuscript: "Available emission data for oxygenated VOC (OVOC) are scarce and only rough estimations on oxygenated VOC emissions exist (e.g. Seco et al., 2007 and references therein) and there are only few studies with chemical speciation of OVOCs. The oxygenated VOC (OVOC) class includes all oxygenated compounds with C1-C12, like alcohols, ketones, aldehydes, etc., which are volatile. A large fraction of oxygenated VOC, often abbreviated as oxyVOC, is composed of short-chained VOC, like methanol, ethanol, formaldehyde, acetaldehyde, acetone, formic acid, and acetic acid. In this work, the standard emission potentials for OVOC were adopted from the NatAir study (Steinbrecher et al., 2009). For most plant species and land use classes a default value of 2.0 $\mu$g $g_{DW}^{-1}$ $h^{-1}$ is applied."

The term ORVOC was replaced by OVOC throughout the revised manuscript, in the text, tables, and figures.

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

This was corrected in the updated manuscript.

*Caption of Table 7: the term "three month averages" is not clear. Do the numbers in 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 caption of Table 7 was changed in the updated manuscript. Provided are now 3-monthly emission sums (= season emission sums) in Gg.

*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.*

The quality of figures 2-6 has been improved for the revised manuscript. Figure 6 is now a colored plot and both panels are equally sized. An Electronic Supplement is provided that contains the figure panels 2 and 3 from the original manuscript.

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



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