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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.

M. Karl et al.

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We would like to thank anonymous referee #1 for careful reading of our paper and helpful comments. His/her suggestions helped to improve the inventory and all comments have been considered in the revised manuscript. We emphasize that the main goal of the paper is more to present method development / improvement in a transparent way and it is less on absolute numbers for emission factors and emissions - we will stress this also in the Abstract. The comments of referee #1 are addressed in the following:

General 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?

Annual crop yields have only be used to calculate biomass correction factor for agriculture crops. LAI has been used to scale emissions throughout the year, but the length growing season has not been considered. For the updated inventory, a new growing season concept is introduced to account for the crop emission changes with growing season. This procedure is described in detail in the subsection 2.4.3 ("Seasonal variability of emissions") of the revised manuscript.

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

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 μg/g/h at 30°C and PAR=1000 μmol/m^2/s.

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

For non-irrigated corn crops (including both durum and common wheat and other cereals), emission factors in the NatAir inventory for isoprene, MT pool, MT synt, OVOC, and SQT are 0.5, 0.5, 0.5, 2.0, and 0.1 μg/g/h, respectively. These were adopted in the revised manuscript.

2b) the EFs given for nurseries are huge compared to other agricultural EFs, e.g. 17.76 μg/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

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the nurseries is higher than that of the presumably mature GLC mixed forest - how can this be explained?

As for most agriculture crops, the nurseries emission factors in the updated inventory for isoprene, MT pool, MT synt, OVOC, and SQT are 0.5, 0.5, 0.5, 2.0, and 0.1 μg/g/h, respectively.
Biomass density of nurseries corresponds to that of Mixed Forest (CLC) as we assume that in nurseries both deciduous and coniferous trees are cultivated (it is not higher!). GLC Mixed Forest for Northern Europe and Africa has a lower foliar biomass density because they are composed of tree species with on average lower biomass densities.

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.

In the updated inventory, emission factors were adopted from the NatAir inventory (Steinbrecher et al., 2009) and are normalized to standard T and PPFD, i.e. 30 °C and PAR=1000 μmol/m^2/s.

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

See 2 c).

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.

Much lower MT emission factors as indicated in the NatAir inventory are taken in the revised manuscript. Furthermore, tomatoes and other greenhouse grown crops are treated as non-seasonal crops in the new growing season concept. The effect of green-house cultivation on plant emission to air is not considered in the inventory as the

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information on the use of green-houses is not contained the applied databases.

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.

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, and brings it in better agreement with the expected distribution between the different land uses. In the revised inventory, agriculture emissions are only 1% of the total BVOC emissions in Finland. In southern Finland, BVOC emissions from agriculture are 40 times lower than the forest emissions.

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.

See 2 f).

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?

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, emissions in Finland are 8 Gg/yr agriculture crops and 404 Gg/yr from forest, and thus much closer to the estimate given by Simpson et al. (1999). However, other land uses like wetlands and shrub vegetation are predicted to play a bigger role

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in Finland; in the revised inventory, emissions from other land uses are estimated to be 166 Gg/yr compared to 5 Gg/yr estimated by Simpson et al. (1999). In section 3.4 of the revised manuscript, which is now titled "Country-specific emission patterns", the results of our inventory for Finland are compared to the estimate of Simpson et al. (1999) and the recent emission inventory for boreal forests in Finland by Tarvainen et al. (2007). Table 7 in the revised manuscript presents the detailed comparison with the emission estimates from Simpson et al. for the main land use types.

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

In the revised manuscript, with a new growing season method, LAI is scaled to the growth development of each phenology type (crop growing season, deciduous, evergreen). 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.

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.

See 2i).

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

This has been done in order to provide maximum consistency and transparency of the inventory. A sensitivity tests on agriculture emission factors is provided in section 4.3 to illustrate the need for a better constraint of these EF in a plant-specific inventory.

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The aim of the presented inventory is to include as much plant-specific information as is available. 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.

In the updated inventory, emission factors for tomatoes and related vegetation groups were replaced by values from the NatAir inventory (Steinbrecher et al., 2009) and are normalized to standard T and PPFD, i.e. 30 °C and PAR=1000 μmol/m^2/s.

3. Also connected with crops, I would have found it helpful to see where the emissions are coming from, i.e. to see a Table with species-specific contributions.

In the current stage of inventories this request would require to store and interpret a large amount of output. In the revised inventory, agriculture land use is treated with uniform EF for (almost) all crops, and therefore large differences between the different crop groups are not expected. 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.

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).

In section 3.4 ("Country-based emission patterns") in the revised manuscript, our emission estimates for four countries, i.e. Spain, Finland, Great Britain and Germany, are discussed and compared to available national inventories from literature. Table 7 in the revised manuscript presents the detailed comparison with the emission estimates from Simpson et al. (1999) for the main land use types in these countries.

5. This inventory uses LAI from MODIS as part of the calculations for both seasonality

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(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 focused on tropical forests then this LAI problem is arguably less important for that inventory. For a European inventory this problem is rather critical though.

MODIS LAI data with 1 km horizontal resolution was retrieved for the years 2004/2005 from Boston University:
ftp://primavera.bu.edu/pub/datasets/MODIS/MOD15_BU/C4.1/LAI/data/monthly/1km/

Both the land use information and the LAI data is aggregated on a pixel of approximately 10x10 km (exact: 0.089x0.089 degrees), thus both data is on the same horizontal resolution and both data sets are originally given on 1x1 km resolution. In the revised inventory, the growing season and phenology for each tree species is considered and it is distinguished between deciduous and evergreen trees. The total LAI in a grid cell is a linear combination of the vegetation type LAI.

The total LAI in a grid cell ($\mathrm{LAI}_{tot,i}$) is the sum of the LAI contributions of each vegetation type v (seven agriculture types, deciduous and evergreen), $\mathrm{LAI}_{v,i}$. Total LAI by definition equals the MODIS LAI ($\mathrm{LAI}_{modis,i}$) in that cell:

$$\mathrm{LAI}_{tot,i} = \mathrm{LAI}_{modis,i} = \sum_v \mathrm{LAI}_{v,i}$$

LAI values for each vegetation phenology type LAI_v in a grid cell are calculated from the MODIS LAI of the cell considering the respective phenology correction p_v and area fraction a_v.

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Thus for an evergreen coniferous tree, LAI_{eve} is:

$$LAI_{eve} = \frac{a_{eve} \cdot p_{eve} \cdot LAI_{modis}}{a_{crop} \cdot p_{crop} + a_{dec} \cdot p_{dec} + a_{eve} \cdot p_{eve}}$$

This approach ensures, that even for LAI_{dec} = 0 in wintertime, emissions from coniferous trees can be non-zero in the same grid cell. For the case of a complete snow coverage during one month, with LAI_{tot} = 0, it is ensured that emissions from all vegetation types are zero.

The seasonality treatment is described in section 2.4.3 of the revised manuscript.

Detailed comments

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

The notation with the indices for tree species, crop species and general land use classes is kept. There are principal differences in how standard emission factors and foliar biomass densities are derived for the respective species and classes are derived and how the biomass correction is done. It proofs to be a useful concept to divide between plant-specific information and general land use information.

In the revised manuscript, the index t is used for tree species, index c for crop species, index g for general land use classes, and index b for the BVOC class. These indices are changed throughout the manuscript for area fraction a, foliar biomass density d, biomass correction factor f, and standard emission potentials ϵ.

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 doesn8217;t the current paper consider ORVOC? Or does it include these in OVOC

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and half the emission rate?

We originally intended a separation into emissions of non-reactive (OVOC) and reactive (ORVOC) other VOC. However this classification may sometimes be misleading; for a global non-methane carbon mass budget both groups are relevant. We therefore decided to follow the concept of NatAir: Other VOC or Oxy VOC comprise all oxygenated compounds with C1-C12, like alcohols, ketones, aldehydes, etc., which are volatile.

The updated manuscript considers OVOC to be oxygenated VOC and mainly composed of short-chained VOC. The oxygenated VOC (OVOC) class includes both longer lived VOC (like acetone) and reactive VOC; sesquiterpenes are treated as separate class. It is stated in section 2.3 of the revised manuscript:

"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 μg g_{DW}^{-1} h^{-1} is applied."

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.

The updated inventory considers OVOC to be oxygenated VOC and mainly composed of short-chained VOC. A default value of 2.0 μg g_{DW}^{-1} h^{-1} is applied for most plant species and land use classes.

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.

All CORINAIR (1999) have been replaced by EMEP CORINAIR (1999) references in

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the updated manuscript. The EMEP CORINAIR (1999) serves as source for a number of foliar biomass density values for trees in Table 3, as this guideline provides more details than the Simpson et al. (1999) reference.

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

The CO99 and G95 references will be removed from Table 5 in the updated manuscript.

11. p4999. I am not sure I understand how the GLC emissions were obtained. Am Iright 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.

In general, the plant composition of each GLC vegetation class was extracted from the GLC/CLC2000 documentation or adopted from the corresponding CLC vegetation class. For main ecosystems species distribution was derived from data within the EU, for example for broadleaved deciduous forest in Northern Europe, for which the known plant species distribution from boreal forest was adopted. This is indicated in the caption of Table 5 in the updated manuscript.

12. p4996, line 8. Explain standard conditions here

In the revised manuscript, the definition of standard conditions is given at the beginning of section 2 ("Inventory Description"): "Data on emission potentials at standard conditions for tree and crops species and for the landcover classes was adopted from the work of Steinbrecher et al. (2009), the NatAir (Improving and Applying Methods for the Calculation of Natural and Biogenic Emissions and Assessment of Impacts on Air Quality) project inventory of VOC emission from natural and semi-natural vegetation in Pan-Europe. Throughout this work, emission potentials are given for standard conditions, i.e. 30°C leaf temperature and 1000 μmol m^{-2}s^{-1} photosynthetic photon flux

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density (PPFD)."

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

Soil moisture and leaf age as additional variables to determine BVOC emissions largely increase the complexity of computational emission models. A recent study of Boissard et al. (ACP, 2008) based on 25 emitter species and with varying environmental conditions (10°S to 60°N) shows that isoprene emissions are mainly sensitive (76% of the overall variability) to instantaneous T and PPFD and to the cumulated 3 weeks air temperature. The dependence on past temperature and light levels is considered in the MEGAN model applied in this work.

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

The following note is added in the revised manuscript:
"We note that the MEGAN isoprene emission inventory by Guenther et al. (2006) originally is provided in mass carbon and is converted to mass of isoprene for the comparison with the estimates of this work (section 3.5)."

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?

A lat/long resolution of 0.089 $\times$ 0.089 was used and this is stated in the updated manuscript. The description is corrected in section 2.1 of the updated manuscript:
"All land use data was aggregated and then projected on a regular latitude/longitude grid. The data is available in WGS84 projection with a horizontal resolution of 0.089 $\times$ 0.089 degrees for continental Pan-Europe. Since longitudes are widest at the equator and converge towards the poles, the grid cell area in the new latitude/longitude grid corresponds to about 9.6 km by 9.6 km at the southern border (20°N) and to about

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9.6 km by 3.2 km at the northern border (70°N) of the domain."

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.

In the revised manuscript, the index t is used for tree species, index c for crop species, index g for general land use classes, and index b for the BVOC class. These indices are changed throughout the manuscript for area fraction a, foliar biomass density d, biomass correction factor f, and standard emission potentials ϵ.

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

The sentence is revised in the updated manuscript:
"A number of studies revealed that monoterpene (MT) emissions from many plant species exhibit a strong dependence on light and were negligible during the night (Staudt and Seufert, 1995; Staudt et al., 1997; Moukhtar et al., 2005; Dindorf et al., 2006)."

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.

Due to the revision of section 2.3 this statement was completely removed from the paper.

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).

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In the updated inventory, standard emission potentials for all tree species were adopted from the NatAir inventory (Steinbrecher et al., 2009).

20. p5006. There seems to be a strong tendency to use the highest emission factors. For example, why prefer Pio et al. [2005]'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?

See 19.

21. p5022, line 7. Didn't MEGAN8217;s European estimate build upon the Simpson et al. (1999) work, and so it should be consistent with that study at least?

There is indeed close agreement between the MEGAN inventory estimate for Europe and that from the European inventory of Simpson et al. (1999). The mentioned sentence was revised as follows: "It can be concluded that annual European isoprene emissions calculated with the new global isoprene emission inventory of the MEGAN model (Guenther et al., 2006) are in exact agreement with the European inventory of Simpson et al. (1999). In general, the MEGAN inventory agrees well with recent European bottom-up estimates (Table 8), while the former GEIA inventory overestimates isoprene emissions in Europe by more than factor 2 compared to European bottom-up inventories."

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?!

Standard emission potentials in Table 3-5 are given with one significant digit in the revised manuscript.

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