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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 #2 for the constructive review. We follow the suggestions of this referee and replaced the selected geographic zones by a country-specific comparison with previous works. Her/his comments have been taken into account for the revision of the inventory and the manuscript.

General comments

Although I find very interesting to attempt to take into account the long term effect of climate acclimation through the f factors, it is not very clear how these factors were derived from biomass data (page 8).

The explanation of the bioclimatic correction factor has been made clearer in the revised manuscript (section 2.2). The procedure to derive these factors is described as follows:

"To derive the biomass correction of a vegetation zone of interest, tree foliar biomass data and agricultural biomass data for both the selected vegetation zone and the Continental vegetation zone is needed. Tree and forest foliar biomass data was derived from litterfall and foliar net primary productivity (NPP) data and crop and agriculture biomass data was derived from national crop yield information. The zonal bioclimatic factor for a tree species is defined as the ratio of the average foliar biomass of this tree species in the selected vegetation zone to its average foliar biomass in the Continental zone. The zonal bioclimatic factor for a crop species is defined as the ratio of the harvested yield of this crop in the selected vegetation zone to its crop yield in the Continental zone. The extraction of the necessary tree foliar biomass data and crop yield data from available databases is described in the following."

Same remark for the factor 2 mentioned page 9, line 9.

The following remark is added in section 2.2 of the revised manuscript:
"NPP data for the foliar compartment from Luyssaert et al. (2007) were converted from carbon to dry biomass using a conversion factor of 2. We note that the conversion factor from dry biomass to carbon may vary depending on the actual C/N ratio (Holland et al., 2005) but due to incomplete datasets a factor of 2 was generally used for the conversion."

Page 10, lines 18-20: the OW97, OW00, OW01 and STR97 references are representative of Mediterranean conditions not of temperate and humid part of Europe as stated.

Table 3 and 4 provides the reference biomass densities for all trees and crops. For main tree and crop species and classes these were corrected with the bioclimatic factor for different climatic regions. The text in the revised manuscript (section 2.2) is changed

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accordingly: "Most values in Table 3 and 4 are representative for growing conditions in the temperate and humid part of Europe, which is in our work referred to as the Continental zone. To determine D_i, foliar biomass densities of main tree and all crop species and classes were corrected with the bioclimatic factor for different climatic regions."

As previously suggested by other Referee, the emission factors used in this work need some proper normalisation (to temperature in particular) and further comments on how their selection was made from all the available values existing in the literature. I don't clearly understand why the tomato EF is biased and if so, why it was deliberately used in this work.

We refer to our reply to Referee #1, question 2c) and 2k).

Why a simplified version of MEGAN was used (Page 16, line 14-18)?

There are several reasons for using a less complex version of the MEGAN emission model for isoprene emissions:
1) to reduce computational demand and complexity;
2) to be comparable with previous G97 algorithm that describes only temperature and light-dependence of emissions;
3) because the additional factors like soil humidity are only scarcely validated in Europe;
4) because a 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.

In section 2.4 of the revised manuscript we added:
"The main reason for choosing a simplified setup of the MEGAN model is to be comparable with previous algorithm of Guenther 1997 that describes only temperature and light-dependence of emissions. The use of temperature and light dependent isoprene

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emission algorithms is supported by a recent study of Boissard et al. (2008) who demonstrated that, based on 25 emitter species and using varying environmental conditions (10°S to 60°N), isoprene emissions are mainly sensitive (76% of the overall variability) to instantaneous temperature and photosynthetic photon flux density and to the cumulated 3 weeks air temperature."

Are the presented emission values averaged over 2004 and 2005? If yes, why not giving both values to present some additional annual variability? Why 2003 was not selected in this work as another referential year? It would have given an interesting upper value of the BVOC emissions during this exceptionally warm year.

Two comparable warm years 2004 and 2005 were chosen to obtain annual average emission. Both are warm years but not as warm as the extreme year 2003. In the revised manuscript, section 3.1, values for both years are given separately:
"The average annual total BVOC emission obtained from the two years 2004 and 2005 for the Pan-European domain is 12 Tg. Calculated annual emissions from both years are very similar: with 11.6 Tg in 2004 and 12.3 Tg in 2005."

Year 2003 had an extremely hot summer where draught and soil moisture deficit can be expected to impact BVOC emissions from the Mediterranean and in France. Both years 2004 and 2005 can be seen as representative for the warming that Europe experienced in the last two decades. Extreme draught events in year 2003 prevent the application of the simplified emission algorithms that were used in this work.

Section 3.2 : Table 6 results could be presented in a pie chart like Figure 4; at least the sum of the emissions per vegetation type (5.3, 4.8 and 1.7 Tg) should be added in table 6 since they are discussed in the text.

The total of the four regions was added to Table 6.
A new figure is added showing the seasonality of BVOC emission per land use type (forest, agriculture and other land use), see also the last point of "Specific comments".

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Figure 4b: I am very surprised by the hot spot emissions of MT in the Western France - which is actually located in Normandy and is different from the selected Atlantic zone - attributed to tomatoes and other vegetables which are not very common in this area! A very similar spot can be seen Figure 4b and 4d over Cairo - this point is not discussed in the text - which is not a place known for its tomato production. I suggest considering for another artefact in the land use data/emission model.

Hotspots have clearly been caused by a combination of high MT emission factors from agriculture and high area fractions of the respective crop. In the revised inventory, which implements both uniform agriculture emission factors and growing season of crops, no MT hotspots occur in France. The Nile Delta remains a place of high MT emission and the following is added in section 3.1 of the revised manuscript:

"The Nile Delta is a region with relatively high sesquiterpene (35 mg/m^2), OVOC (500 mg/m^2) and newly synthetisized monoterpene (900 mg/m^2) emissions in July 2005. This region is covered by 100% with the GLC vegetation class of irrigated cropland. The Nile Delta is thus one example how inaccurate, not crop-specific, land use information can affect emission rates calculated in this inventory."

Section 3.4 It would be helpful if the 4 regional types of vegetation were presented and described earlier than in this section (possibly p10 in section 2.1?). Above all, the selection of the European zones representative of Atlantic and Mediterranean environments is somehow awkward and critical. Indeed the first one (-1°W 46°N, +1°E, 48°N) corresponds mainly to the Loire and Yonne valleys, which do not represent the best typical Atlantic environments (in France, Brittany would be much better), and the second one (-7°W, 38°N, -5°W, 40°N) corresponds mainly to the Spanish Extremadura area which is not the best represent for a Mediterranean environment (it is also under Atlantic influence). I would suggest making more relevant selections.

The comparison of BVOC emissions based on European regions is replaced by a coun-

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try based emission comparison in the revised manuscript. Section 3.4 ("Country-based emission patterns") is completely revised in the updated manuscript and describes the seasonal variation of emissions from four European countries: Spain, Finland, Great Britain and Germany. The mentioned European regions have been completely discarded since their selection was not representative for the major vegetation zones and the regions did not allow for a comparison with existing national inventories and reviews.

Section 3.5: this comparison section needs some improvements; I found this section is focusing a bit too much on comparing this work with the NATAIR comparison. Other works are presented too, but, for instance, the Andryukov and Timofeev (1989) and Lathière et al (2006) assessments are not discussed at all. Also, it is interesting to note that the Lübkert and Schöpp (1989) work gives a very similar estimate for forest annual emissions (7.5 compared to 6 Tg) using - I guess - a much simpler model; the relevance/necessity of heavy and sophisticated calculations as the one done in this work could be discussed.

In the revised manuscript the following is added in section 3.5: "Forest emissions obtained in our work are very similar to the early estimate of Lübkert and Schöpp (1989). These authors used a very simple model for calculating forest emission based on accurate altitude-resolved forest cover data and spatially and temporally interpolated temperature data. They use only one emission factor for each deciduous and coniferous tree which depend only on temperature and are different for day and night. The good agreement with our work indicates that the use of accurate forest data is probably more important for the correct prediction of forest emissions than the application of a great amount of detail to describe the short-term variation of emissions."

The other two mentioned studies by Andryukov and Timofeev (1989) and Lathière et al (2006) are less relevant for the discussion. The work Andryukov and Timofeev (1989) consistently overestimates forest emission by a factor of 3.5 compared to Lübkert and Schöpp (1989) and their method to calculate emissions is not described. Lathière et

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al (2006) present a global BVOC inventory to study the impact of climate change on BVOC emissions. Due to the use of average emission factors for large vegetation groups of the world (plant functional types) it is not suitable for detailed comparison on the European level.

Also, the comparison should mention that the annual non forest BVOC emissions are very similar for this work (15-6=9 Tg) and for Steinbrecher et al. (2008) (20-12 = 8 Tg) although this latter study used less detailed data for agricultural emissions. When comparisons are made with the Steinbrecher et al. (2008) it would worth mentioning that this study was made for inter-annual variability and thus to present its whole annual BVOC emissions range (e.g. 15 to 25 Tg for total BVOC); the annual emissions calculated in this study are then not so different from this NATAIR inventory. All the inventories compared were not made for the same referential year. I wonder how some term like the mean temperature could, partly, explained the differences obtained in these inventories. For instance, the comparisons between the 4 ISOP estimates are fairly critical since two of them were carried out for 2004 2005, the other two for 2002 2003 which were much warmer (especially 2003!).

The inter-annual variability of emissions from the NatAir study is considered in section 3.5 of the revised manuscript:

"The NatAir study calculates European emissions based on the years 1997, 2000, 2001 and 2003 to address inter-annual variability of emissions. The years 1997 and 2002 were fairly cold years while 2001 and in particular 2003 were relatively warm years. Their estimate of annual total BVOC emissions amounts to 20 Tg for the average of the four years, with a variation from 15-25 Tg for the particular years. The lowest NatAir estimate of 15 Tg is about 2-3 Tg higher than our estimate and the earlier estimate given by Simpson et al. (1999). From the average total NatAir amount of 20 Tg BVOC emissions, 12 Tg are estimated to originate from forests and 8 Tg from other land uses, including agriculture. Non-forest emissions obtained in our work sum up to 5

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Tg and are lower than the average NatAir non-forest emissions. Forest emissions of our estimate are 45% lower than the NatAir estimate. Due to the use of the identical forest tree species information (forest cover database and standard emission factors), the emissions from forests from our work should be comparable. A possible reason for this may be the higher temperatures in the selected years of the NatAir study."

Different average temperature in the applied reference years may be a further reason for discrepancies. In the revised manuscript it is stated:
"Likely reasons for the lower isoprene emissions in our inventory include the use of different meteorology data, lower temperatures and light abundance in the selected years, application of a biomass correction factor for main tree species. The impact of the bioclimatic correction factor on our European emission estimate will be discussed in section 4.4."

Be careful, on page 27, line 1: the NATAIR estimate is 33% higher than the one obtained in this work, not 50

In our revised inventory, annual forest BVOC emissions are 6.5 Tg, which is 45

Section 4.1, page 29, lines 8-9 : OK but these 'biomass inconsistencies' should result into emissions inconsistencies too? However, I can only see some problems for sesquiterpene emissions (figure 4d).

In the revised manuscript, the effect of biomass density inconsistencies due to the use of general land use classes become visible in the European emission rates for July 2005 as a clear border between the south of Finland and Russia for isoprene, OVOC, sesquiterpenes (Figure 2a+c,d in the updated manuscript) and for pool and newly synthetisized monoterpenes (Figure A3 in the new Electronic Supplement) emissions.

Section 4.2: standard EF for a same species can fluctuate over much more than a factor of 2.

In section 4.2 of the revised manuscript, the following sentence was added:

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"Monoterpene emission rates among different Scots pine trees of the same age and origin, growing under similar conditions, even varied by more than one order of magnitude (Komenda and Koppmann, 2002)."

Specific comments

Page 7, lines 9-13: it would be easier to use 'dt' and 'dc' for tree and crop densities (same suggestion for the bioclimatic correction factors f).

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.

Page 9, line 21: I would suggest a ',' after records.

This is changed in the revised manuscript.

Page 10, line 10: this sentence looks awkward to me. Do you mean that 'European countries were described or represented by certain vegetation zones'?

European country-based crop yields were allocated to the vegetation zone to which main parts of the respective country belong geographically (see explanations in Table 2).

Page 10, line 20: wouldn't it be 'temperate' rather than 'temperature'?

This is corrected in the revised manuscript.

Page 27, line 10: the Steinbrecher et al., 2007 is not in the reference list.

The reference was intended to be Steinbrecher et al. (2009) for the NatAir inventory. The respective sentence was removed in the revised manuscript, since the NatAir OVOC standard emission factor has been applied to calculate OVOC emissions in the revised manuscript.

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Page 33, lines 9-12: so why this algorithm was not used?

The algorithm to describe the seasonality of emissions from deciduous and evergreen trees from Staudt et al. (1997, 2000) has been applied in the revised manuscript to obtain the seasonal variation of emissions from deciduous and evergreen vegetation. The respective paragraph was modified in the revised manuscript:
"Based on long term laboratory experiments with Pinus pinea trees, Staudt et al. (2000) developed an algorithm to adjust for seasonality that was proven to fit very well with observations in the field, where monoterpene emission factors of this species varied between 2.8 and 15 μg/g/h during the course of the year (Staudt et al., 1997). The algorithm of Staudt et al. (2000) has been applied in this work to determine the (long-term) seasonal variation of emissions from deciduous and evergreen vegetation."

The Steinbrecher et al., 2008 reference in the References list should be corrected (the title, authors list of the published paper have changed).

The reference has been updated in the revised manuscript.

Figure 6 would deserve some colours.

All seasonal emission plots - Figures 4-6 of the revised manuscript - are coloured.

An additional emitters type figure could be added.

A new Figure was added - Figure 6 of the revised manuscript - shows the seasonal variation the contribution from main land use types (agriculture, forest, other land use) to the total European BVOC emissions.

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