**Biogeosciences
Discussions**

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.

Anonymous Referee #2

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The manuscript presents a comprehensive inventory of biogenic COV emissions over Europe. The main interests of this work upon the other previous or recent European biogenic inventories are (i) to consider natural AND anthropogenic (agricultural) emissions, (ii) to consider detailed land use and biomass data for agriculture emissions, (iii) to use and compare different biogenic algorithms (G97 and MEGAN) and (iv) to present results for new referential years of calculation (2004 and 2005). The manuscript is well written and properly documented. The given information used to carried out the calculations are generally clearly presented and of interests. However, all the choices made are not always clearly enough justified/discussed. Moreover 2 of the 4 geographic zones are, to my point of view, not appropriately selected (see general comments).

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Some figures could be improved (see detailed comments).

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

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

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.

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. 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.

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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.

Section 3.5 : this comparison section needs some improvements; I found this section is focussing 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. 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!). Be careful, on page 27, line 1: the NATAIR estimate is 33% higher than the one obtained in this

work, not 50%

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).

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

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).

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

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

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

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

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

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

Figure 6 would deserve some colours. An additional emitters type figure could be added.

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