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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Assessment of high to low frequency variations of isoprene emission rates using a neural network approach" *by* C. Boissard et al.

Anonymous Referee #2

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General comments:

A better understanding and quantification of seasonal variations of biogenic emissions in general, and of isoprene emissions in particular is very much needed f6r assessing the role of BVOCs in atmospheric chemistry and has utility with the readership of ACP. The artificial neural network approach (ANN) promises more flexibility to check for complex interactions and interdependences and has already shown its ability to represent the complexity of ecological and biological phenomena (see for example the papers of Papale & Valentini (2003) or Kuhn etal (2005). As a learning technique it is driven by real data and requires a large number and good quality of data for training and for

validation in order to come to reasonable generalisations.

With its attempt to use ANN for extrapolating BVOC emissions to the large scale, the paper developes a novel approach and merits publication. Starting from a review of literature studies describing seasonal variation of BVOC emissions in general, the paper developes the method and algorithm reasonably well and describes the database used for running the assessment. However, the methodology is not always reproducable and contains several unresolved flaws. The description of type and origin of data and of the criteria for data selection is lacking the transparecy that is needed for checking the consistancy of the statistical basis of the method. The structure and formate of annexes, tables and figures appears somewhat casually taken from the more detailed thesis of F. Chervier; there are numerous instances of poor grammar, imprecise wording, inconsistent use of appreviations, confusing paragraph and sentence structures. I recommend accepting the paper for publication in ACP after a strong re-writing and re-structuring following the comments below, with focus just on isoprene emissions.

Specific comments

Key references to be considered:

Papale D., Valentini R. (2003) A new assessment of European forests carbon exchanges by eddy fluxes and artifical neural network spatialisation. Global Change Biology 9: 525-535

E. Simon, U. Kuhn, S. Rottenberger, F. X. Meixner & J. Kesselmeier (2005) Coupling isoprene and monoterpene emissions from Amazonian tree species with physiological and environmental parameters using a neural network approach. Plant, Cell and Environment 28, 287-301

Formal aspects:

1) The general structure of the paper is not consistent. Following the title, the introduction should focus exclusively on seasonal variability of isoprene and should include the

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review also to be focussed on isoprene.

2) Several graphs and tables are not readable. Table 1 needs to be enlarged by 400% to get readable; in a printed version it would require something like 3-4 pages. 20 out of the 45 studies presented in Tab.1 do not provide any emission data and can be skipped.

3) Deciduous vs. conifer is not a proper division in Table 1; there are deciduous conifers like Larix species. Should be broadleaved-conifer and/or deciduous-evergreen

4) I propose for below reasons to consider only isoprene emissions in a revised paper; this would allow to present in Table 2 only those studies with data available and used, to include in Tab. 2 also the standardised emission rates as in Tab. 1. and to indicate how the individual data of emission rates and of T and light measurements were extracted from the original literature.

5) There is no reason to separate appendices from figures and tables.

6) Several studies in Tab.4 are not dealing with the species indicated in the table: Q. coccifera vs. Q. ilex

7) App. A Symbols in legend and figure do not always match like in case of Ulex

8) Ch. 4.2. deals with database ISO-DB development in the title but with the algorithm development in its content. Such type of inconsistencies and lack of care makes the paper very complicated to read and to understand

9) In general, the text is very hard to read: see for example chapters like 4.3. with some 50 lines without any para or the incredible amount of acronyms/apprevations often not explained

ISO-DB description/ data selection:

10) Data quality and selection criteria is critical for neural network approach. It is not clear how the data in Tab2 were extracted from Tab1 – the majority of Tab 2

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measurements are not seasonal studies but are just a number of data taken during individual/few days, as shown in appendix A: the emission variation of a species like Eucalyptus spans 4 orders of magnitude similar to all species during the course of the year.

11) Where are the data coming from? Most papers do not provide original data of isoprene emissions and related T and L measurements, but some average and normalised value of basal ER and/or some graph of diurnal courses. Did authors extract these data from raw data after contacting the authors? Which control parameters / driver data data were taken from original literature, which from NOAA global datasets?

12) It would be useful or even essential to be able to judge the reproducibility of results, to show the original data used for the calculations in a database available on the web, both of emission rates and of meteorological drivers. The web address given in Tab.2 for database access provides only a slideshow presentation at AGU 2006

13) Appendix B describing the input regressors is imprecise and not clear at all: – Day length D used for instantaneous - not clear, D is always 24 hours; was the meaning: hours of daylight? Did they use only measurements during daylight hours? The values shown in App A for individual species and days span over 3-4 orders of magnitude; obviously there are nighttime values included which are close to zero and detection limit - - Solar flux is given in umol for L0 and in Wm2 for L; is it PPFD in the first and global radiation in the 2nd case? Must be said, would make quite a difference and cannot be compared.

- "Bold letters are regressors eventually considered in ISO-LF"; such type of imprecise wording is not allowed in the methodology of a scientific paper.

- The 34 input regressors are not independent but are based on the same data for different periods. In addition, L0 appears to be a physical unit different from L1, L7, L14, L21.

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Conceptual issues

14) The titel and terminology "high to low frequency variation" is needlessly general, is not related to the biology of the emission process, and therefore somewhat misleading. Why not instantaneous (diurnal) and seasonal variation? this would be clear to everybody.

15) Instantaneous variability is driven by temperature and light and is very well predicted by the Guenther95 algorithm. Therefore, the paper should deal with the variability of the basal emission potential as modulated during the course of a season.

16) Controls and pathways of emissions of reactive monoterpenes, oxyenated compunds, sesquiterpenes are totally different from those identified for isoprenen emissions. For example, terpenoid emissions from storage organs are constrained by the morphology of the barrier – this is totally different from emissions like isoprene under instantaneous metabolic and physiological controls - they cannot be treated with one approach.

17) Instead, emissions of monoterpenes under light and temperature control (the Q. ilex emission type) has been proven in several studies to be well described by the G95 isoprene algorithm. It is most surprising that this relatively simple case with availability of many excellent seasonal data does not perform well with the ANN approach.

18) ANN is a data driven apporach, data quality is most critical. Considering the data related shortcomings and lack of transparency, it is impossible to judge the validity of the statistical method. In this context, the poor performace in case of Q.ilex type of emissions is alarming.

19) The sensitivity analysis goes in the right direction , but related Fig. 5 is not readable. Speculation about soil nitrogen impact on BVOC emission in temperate winter is not convincing: ST1u – soil temperature 0-10cm is fully reflecting air temperature T0 and this with L0 is the primary driver of emissions. Anyhow, I see only one

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temperate species Ulex europeus reported in App. A with winter data close to zero.

20) A key reference promising further details of neural training and validation (Dutot et al. 2007, p. 12432 line 7) is not given in the reference list

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Interactive comment on Atmos. Chem. Phys. Discuss., 7, 12417, 2007.