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Biogeosciences Discussions

Interactive comment on "Process-based simulation of seasonality and drought stress in monoterpene emission models" *by* R. Grote et al.

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Final Author Comments

General We are thankful to both reviewers for the trouble they have taken and for their overall positive statement about the manuscript. We will integrate their comments and suggestions in the revised version. In particular, we will clarify the description of the simulation setup and the applied models (i.e. that the SIM algorithm is proposed as a general applicable tool for representation of seasonality in emission models) and put emphasis on the discussion of model performance. We will conclude this discussion by more pronounced statements about knowledge gaps that are disclosed by the model analysis and that could be used to guide further research. Unfortunately, due to these gaps, it is currently not possible to conclude which model 'is best' at representing reality.

Specific Reviewer 1:

- "Would it be possible to use photosynthesis observations to help constraining the loss rate? And how is the iterative derivation done?"

Photosynthesis observations have been applied to evaluate the model of assimilation used here. This has been shown in detail in Grote et al. (2009, Oecologia). This determines the supply of carbohydrates available for further synthesis or transport. However, it is not possible to determine the loss rate from the mitochondria from these measurements without measurement of intermediate concentrations within the cells. The loss rate has thus been approximated by changing the value in steps of 0.0005 umol umol-1 s-1 to obtain concentrations of GDP that are similar to those measured by Nogues et al. (2006) in Q. ilex chloroplasts. This will be clarified in the text of the revised manuscript.

- "The evaluation of the leaf-level modelling of drought stress is done too succinct. . . . I would welcome a figure that provides more insight in the ability of the model to capture the seasonal variability and drought stress effects."

We agree that the information about evaluation of drought stress responses is quite short. However, this has been described in detail in Grote et al. (2009). That study presents the comparison of continuous half-hourly simulation results with measured emission (and photosynthesis) for all measurement dates. In this paper, we rather focus on the relative importance of drought when compared and/or combined with that of seasonal changes in emissions potential. We feel it is not necessary to repeat previously reported results, and unfortunately the available data is insufficient to further the analysis reported in Grote et al. (2009). We suggest amending the manuscript to draw the readers attention to the results published in Grote et al. (2009) for further analysis and discussion on inter-model drought stress responses.

- ". . .The results are used to discuss the sensitivity to diurnal changes, seasonal changes and drought impact. The authors describe the reasons for these differences in sensitivity in the Discussion part, but do not discuss the range that is considered likely for such a sensitivity. Despite the lack of canopy-scale observations for the site, it should be possible to provide a general judgment on the models' behaviour: how strong should the (temperature and) light dependencies be on the different timescales?"

We agree with the reviewer in that a better understanding of real temperature and light dependencies over different timescales is essential to understanding emissions and evaluating emission models. Unfortunately, data availability is very limited (see Pacifico et al., Atmos. Env. In press http://dx.doi.org/10.1016/j.atmosenv.2009.09.002). However, past model studies about biogenic emissions have already reported the relative sensitivity of emission models (Arneth et al., 2007), and show them to be most sensitive at higher temperatures and radiation values (Keenan et al., 2009 ACP). This suggests that model differences will probably be larger at more southern locations. Our results therefore are likely to represent something of a upper range of model differences due to temperature and light with respect to the time scale included in the study. Model behavior over longer time scales is supposed to be similar to that reported here, unless potential climate change is accounted for, in which case model differences have been shown to increase dramatically (Keenan et al., 2009 ACP). Figuring out how strong the temp/light dependencies should be on different timescales should be a strong focus of research efforts in the future. We will expand this explanation in the text of the revised manuscript.

Furthermore, technical corrections will be carried out as indicated.

Reviewer 2:

- "Where was the eddy covariance measurement site?"

The eddy co-variance measurements took place on the same site where the drought exposed Holm oak trees were measured. This site is included in the CarboEuroFlux network (see e.g. http://www.bgc-jena.mpg.de/public/carboeur/sites/puechab.html and http://www.cefe.cnrs.fr/fe/puechabon/projects/p_mind.htm, where the location of the

mesurement plots and the flux tower is shown).

- "How many trees were sampled (i.e. is the deviation in Fig 3 between trees or sampling occasions)?" and "How many samples were taken for the enzyme analyses?"

Three trees per occasion were sampled. The deviation in Figure 3 is across these 3 trees (now mentioned in the figure caption). All measured leaves (3 per occasion) from 2006 were also used for the enzyme analyses.

- "Even though Figure 1 provides an overview of how the model framework is constructed, some clarifications are needed, and actually a table might be better in the comparison of different models and their submodels. It is not clear to me what is the difference between SIM and PSIM, and where is PSIM used?"

We will improve the description of the component models. Indeed, PSIM is an extension of SIM including the seasonality algorithm and description of phenological development but describes also physiological procedures for carbon and nitrogen allocation that are not used in this investigation.

- "Why is FPGA set to 0.375 – experimental evidence or sophistigated guess?"

The relation had been set based on unpublished results from work on pines by Joerg-Peter Schnitzler that has been personally communicated. The value is difficult to find in literature for trees. For crops and weeds it has been reported to be between app. 0.1 and 0.3. However, uncertainty tests show that emission results are not sensitive to this ratio.

- "Why should the concentrations of photosynthetic products and intermediates be declining in time? During drought? Are these steady-state models or are they run in a steady state?"

The concentration of intermediates declines because of use for transformation into higher molecules and transport into other plant compartments. These processes may have numerous environmental dependencies which are not well known and thus not considered here. Instead we make the reasonable assumption that the 'leaching' of assimilates depends on its concentration and is not directly dependent on production rate. Thus the concentration declines when the 'outflow' is larger than the 'inflow', provided by photosynthesis. This is the case during conditions where photosynthesis is severely reduced, i.e. during night or extreme drought. A steady state would be a very special case of this model system which is usually not reached.

- "It is not at all clear to me how the loss rate is derived from data presented in Fig 3. Please clarify."

The loss rate is iteratively determined to approximate a reasonable concentration of intermediates that has been published in the literature. Seealso the response to reviewer 1, first question.

- "line 35: shouldn't this be BIM2?"

No. Here the different seasonality descriptions: the SIM model and the procedure inherent in the MEGAN model are compared.

- "p. 11 line 5-11: This is confusing and unclear. I don't find the data supporting these results."

We would like to thank the reviewer for pointing this out. The interpretation of the 'diurnal effect' is indeed confusing. We will rewrite the interpretation of this section of results in a more clearly understandable fashion in the revised manuscript.

- "Where does data to Fig 2 come from? There is too few information on GPP measurements or simulations earlier. . . . "

As mentioned above, the GPP measurements have been obtained as part of the CarboEurope project (not Euroflux). Eddy covariance fluxes were measured at a half hourly time step at a height of 11m (about 6m above the top of the dominant tree species). The eddy covariance facility included a threedimensional sonic anemometer (Solent R2 during the 1998–1999 periods and R3 since 2000, Gill Instruments Instru-

ments, Lymington, UK) and a closed path infrared gas analyzer (IRGA, model LI 6262, Li-Cor Inc.), both sampling at a rate of 21 Hz. Flux data were processed following Aubinet et al. (2000). This is explained in Allard et al. 2008 but we will add it in the description too.

- ". . .Figure 7 should include arrows or something to point out the exact dates 150 and 260. What happens on day 220 when the emissions suddenly drop down?"

We will include an indication when certain drought stress thresholds are passed which will also be commented in the subscription of Fig. 7. According to the climate data, temperature declines around day 220 from app. 24 to 18 degrees (daily average). This is accompanied with radiation decline but only with small rainfall.

Furthermore, technical corrections will be carried out as indicated.

In behalf of all authors: Ruediger Grote

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