**Atmospheric
Chemistry
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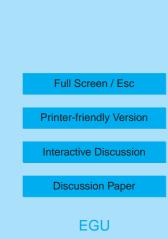Discussions**

Interactive
Comment

Interactive comment on "Process-based estimates of terrestrial ecosystem isoprene emissions" *by* A. Arneth et al.

A. Arneth et al.

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We would like to express our thanks for the encouraging appraisal of our work and the supportive summary comments from the reviewer. The three comments that were made address important aspects of the paper, and we will seek to clarify these points in the revised version as follows:

(1) Use of the Niinemets et al model: One objective of briefly reviewing the leaf isoprene models was to bring the existing modelling approaches to the attention of the readers of ACPD; since the leaf models all have a very strong plant physiology focus we anticipated that not all readers of ACPD would be entirely familiar with them. The chief goal (cf., our response to referee #1), however, was to asses the models' applicability in terms of their response over a wide range of environmental conditions, as well as in terms of their 'ease of use' (e.g., requirements for parameterisation). The results from

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our leaf-level model experiments clearly draw attention to the difficulties of using the two non-steady state leaf models in a DGVM framework (e.g., Figure 1-3); and when compared to the Martin et al approach we concluded that the Niinemets et al. routine fulfilled our requirements best, since the latter requires only one chief parameter to be pre-set. As stated in response to reviewer #1 we will revise the introductory paragraph to section 4.1 accordingly, clarifying this rationale for using the Niinemets et al model. The new Figure 4 (in response to referee #1) will provide additional clarification.

(2) Model results at Harvard are indeed somewhat perplexing since the general model performance appears satisfying for the site. LAI is somewhat overestimated, but agreement for GPP was within about 10%, and due to the link of isoprene emission to photosynthesis, I scales to a large degree with GPP; cf., p8034, lines 7-10. This latter point will be emphasized in the revised version of the manuscript. But it should be kept in mind that there is no site-specific parameterisation used to tune the carbon and vegetation dynamics of the model (apart from using local climate input); and even when considering the (by contrast to isoprene) much better understood exchange of CO_2 or water vapour: when DGVM model output is compared with flux data the agreement can range between very good at some flux sites to fairly poor on others. Unfortunately, it is not always possible to pin-point the exact reason for the model-data discrepancy, possible culprits include lack of representation of site history, model deficiencies particularly at dry sites, incomplete description of carbon-nutrient interactions, etc. (e.g., Sitch et al. GCB, 9/2003; Morales et al., GCB, 11/2005). By and large, agreement of model output with flux data within 20 or 30% is considered as quite acceptable.

We believe it unlikely that the discrepancy points to the fact that the model works better, in general, for ecosystems where emitters are dominant, since at Harvard rather a sizeable part (36% to our knowledge) of the basal area is from the main emitter (Q. rubra) and since LPJ-GUESS reproduced a canopy dominated by its equivalent, intermediate shade-tolerant PFT (Figure 6). One -but not the sole!- big uncertainty is indeed the basal emission estimates, and we will reflect on this aspect more detail in

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the revised manuscript (also in response to reviewer #1). Finally, there is of course also uncertainty associated with the data (although it is truly not our intention to repeat here the mantra of modellers: 'the data is wrong!'), which was obtained by a flux-gradient method. We suspect that -similarly to what was seen for NEE or water vapour fluxes- data from many more isoprene flux sites would be needed to evaluate in more detail the reasons for model-data mismatch, and these may become available increasingly so as sensors & software for flux measurements are being developed. It is encouraging, however, that for the five sites used here there was no clear bias visible (i.e., no consistent over- or underestimation of modelled isoprene production) when compared to data.

(3) The reviewer raises an important aspect to include in the discussion. There is indeed a need for more quantitative information from controlled experiments to test whether the relatively simple, semi-mechanistic CO_2-response implemented in our model holds over a wide range of environmental conditions. We will include a statement to this effect in section 7 of the revised manuscript.

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 8011, 2006.

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