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Biogeosciences Discussions

Interactive comment on "From biota to chemistry and climate: towards a comprehensive description of trace gas exchange between the biosphere and atmosphere" *by* A. Arneth et al.

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We would like to express our thanks to the encouraging comments received from Beth Holland, anonymous referee no. 1 and Georg Wohlfahrt, as editor. We will consider all their comments in the revised version of the manuscript.

Comments from Georg Wohlfahrt, Editor Similar to reviewer #2, I though also see problems the way the paper is structured, but feel that this can be changed fairly easily by introducing more meaningful sub-chapter headers, by chang ing the order of the sub-chapters and by providing some rationale for the organisation of the paper that allows the reader to better follow. p. 7721, l. 19: here it would be nice to provide some

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rationale of the organisation of the remaining manuscript so that the reader is prepared for what follows

–> We have modified the last paragraph of the introduction to include a 'roadmap', clearly defining the organization of the manuscript

p. 7721, l. 20: this header seems a poor description of what is presented below –>The header is changed to "Importance of non-CO2 trace gases exchange at the land surface for atmospheric chemistry and climate"

p. 7729, l. 14: logically I'd rather like to see chapter 2.3 after 2.1 –>Will be done.

p. 7731, l. 19: "Global trace gas exchange modelling" ?; –>Yes, better title that way!

p. 7731-7752: in this chapter you are dealing with certain key processes (fire, land use), ecosystems (crops, wetlands) and species (NOx, CH4, BVOC, O3, H2) - is there a way to make this selection look better organised ? maybe a distinction between key ecosystems, processes and species can help to guide the organisation of this chapter

–>Yes. Based on this suggestion and that of reviewer Beth Holland we have now re-structured section 3 under the following section headings: 3 Global trace gas exchange modeling 3.1 DGVM model structure 3.2 Inclusion of new land cover types and processes in DGVMs 3.2.1 Land use, land cover change and related non-CO2 trace gas emissions 3.2.2 Wetlands and methane emissions 3.2.3 Nitrogen exchange in natural and agricultural ecosystems 3.2.4 Fire and fire-related emissions 3.2.5 Hydrogen 3.3 Advances in modeling plant physiology 3.3.1 BVOC 3.3.2 Ozone effects on vegetation

Comments from Beth Holland, referee no. 2 The ambitious undertaking means that there are some areas that could use clarification. Expanding DGVMs to include more detailed leaf level physiology and biogeochemistry provides an important opportunity to link a number of key processes. The justification for writing a review of this scope is not clear, nor is the use of DGVMs as the organizing framework for the links to climate and chemistry. For example, there is no mention of land surface models that

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play a central role in linking the terrestrial biosphere to the climate system. It would help the reader if there were a stronger conceptual framework on how to group some of the processes. We have revised the introduction to specifically mention the importance of land surface models in climate science, and emphasize the impetus within the DGVM community to further develop models of non-CO2 trace gas exchange at the land surface. The first sentence of the second paragraph (page 2) no longer mentions land-atmosphere feedbacks in the context of DGVMs, as this may have given the false impression that DGVMs can be considered an alternative to land surface models within climate models. I struggle with some of the organizational aspects of this paper. As it is currently organized the paper is more of a list of things that need to be tackled and coupled and there is significant redundancy that I found distracting. The current organization around layers of the atmosphere was convenient in some cases, but led to significant redundancy. For example some of the processes depend on the fundamental plant physiology, and carbon and water exchange including, BVOC emissions, methane transport, ozone impacts and dry deposition. Others depend on better representation of the nitrogen cycle including NH3 emissions, NO exchange, and N2O emissions.

–>Thanks for these suggestions, these will be taken into account, also in response to comments made by the editor.

Specific areas: Given the emphasis on aerosols, I find the discussion of NH3 quite weak. NH3 is the atmosphere's most abundant base and is quite important for neutralizing and forming many aerosols, yet it was given only a cursory treatment. NO and N2O emissions depend fundamentally on N availability (Parton et al and Li et al.) Yet, a great deal of emphasis is on why N deposition is not a driver in forest soils. . . There is a contradiction there. N2 is the final end product of the redox chain. It is likely that the majority of N going through denitrification ends up as N2. Nitrification and denitrification are notoriously difficult to measure.

–>The text will be clarified to add emphasis on NH3, and revise/add detail on forest N

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emissions

The section on biomass burning and soil NOx emissions is quite weak. Other good citations include: Neff J.C., M. Keller, E.A. Holland, A. Weitz & E. Veldkamp (1995) Fluxes of nitric oxide from soils following the clearing and burning of a secondary tropical rain forest. Journal of Geophysical Research, Atmospheres, 100(D12) 25,913-25,922. Weitz and Veldkamp published good long term measurements following fire. –>In response we have included reference to the suggested citations and revised the text accordingly

The introduction states that CO2 will not be considered, yet on p. 7733, there is discussion of CO2 impacts that is too cursory to discuss a controversial subject. –>Good point, we have deleted these sentences referring to discussion of CO2 impacts

The C:N ratios on page 7739 are misleading. A full discussion of C:N ratios is provided in Townsend et al. Ecological Applications and in Parton et al., the original Century modelling papers. A better citation is Paul and Clark, Soil Microbiology and Biochemistry, 1996 version, a key text in the field. –>We do not find that the text on C:N ratios is misleading and kept it unchanged.

There are extensive studies of NO and N2O fluxes from semi-arid regions. See R.E. Martin et al, and A.R. Mosier for a variety of papers. One of the breakthroughs in modelling soil fluxes of NO has been the use of satellite measurements of NO2 (GOME and Schiamachy) to constrain the global estimates. Jeagle and Randall Martin have published extensively in this area. These measurements have highlighted the importance of semi-arid areas. –>Thanks for these suggestions, we have modified the text in section '3.2.3 Nitrogen exchange in natural and agricultural ecosystems', accordingly.

Technical comments: p. 7722, line 22, atmosphere, "by-passing aerobic layers and the likelihood of oxidation". –>Added

line 27 constraint is the wrong word choice –>Changed to "affected"

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Anonymous reviewer no. 1 The manuscript could profit from a better discussion on ozone/VOc/NOx links, where a lot of new knowledge was published in the last year. In particular in tropical areas, where measured OH values are one order of magnitude off from model simulations. –>In response we have altered the manuscript, and added in section 2.3 'Processes in the troposphere', references and text to a number of recent studies (e.g., Lelieveld et al. (2008), Wu et al. (2009), Voulgarakis et al. (2009))

Interactive comment on Biogeosciences Discuss., 6, 7717, 2009.