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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.

E. Holland (Referee)

eholland@ucar.edu

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This is an ambitious extensive review paper that provides many strong arguments for Earth System models that link biota and the paleo perspectives to chemistry and climate. The undertaking is a worthy one and the authors have summarized a lot of information.

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 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.

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.

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 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. The introduction states that CO2 will not be considered, yet on p. 7733, there is discussion of

CO_2 impacts that is too cursory to discuss a controversial subject.

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.

There are extensive studies of NO and N_2O 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 NO_2 (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.

Technical comments: p. 7722, line 22, atmosphere, "by-passing aerobic layers and the likelihood of oxidation". line 27 constraint is the wrong word choice

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