**Atmospheric
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Discussions

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Interactive comment on
"Photosynthesis-dependent isoprene emission from leaf to planet in a global carbon–chemistry–climate model" *by* N. Unger et al.

R. Grote

ruediger.grote@kit.edu

This manuscript deals with a very important issue – namely to provide an alternative to the current emission modeling that is linked to photosynthesis. (One of) the expectations behind this is that the differences in emission responses that we see in various types of vegetation (or even species) such as differences in maximum emission intensity, temperature optimum, or the shape of responses to radiation or CO2, could be explained by differences in photosynthesis (resp. electron transport). This gives the

model not only more flexibility in terms of environmental impacts but is also very important for spatial and temporal upscaling. Canopy integration is made easier because photosynthesis related gradients may be directly related to gradients in emission – so if the model reproduces gradient in light, vcmax and nutrient content, this will be reflected in emission patterns too. Also, seasonal emission patterns may – at least partly – be related to seasonal developments of photosynthesis (such as a reduction of vcmax in winter). Therefore, I congratulate the authors for being the first to approach this issue in a global climate/ air chemistry modeling approach.

However, given the expectations mentioned above, it is not quite clear to me how this linkage is implemented – what seems to be important since it is already applied it on a global scale. Let me explain this in more detail in the following paragraphs:

1. Coupling the Farquhar model to the Niinemets model is based on the calculation of the electron transport rate of photosynthesis. This very much depends on the implementation of the photosynthesis model. For example gamma_star (in Farquhar et al. 1980) is calculated from the Michaelis-Menten parameters kc and and ko which vary in dependence on 4 parameters (ko25, kc25, and their activation energies). Alpha_qe depends directly and indirectly on 4 additional parameters (curvature parameter, vcmax, jmax, and activation energy of jmax, see for example Caemmerer et al. 2009). The result is quite sensitive to all of these 8 parameters and there is a range of values available for each of them. However, only vcmax is given here. How are the other parameters defined? Has the sensitivity of the model to these parameters been tested?

2. If vcmax depends on nitrogen (P17727, L15) – is nitrogen dynamically described? If not – why is it mentioned at all? In fact, the literature seems to judge it as quite important to consider vcmax (nitrogen, sla) differences across the canopy for upscaling (usually linked to nitrogen gradients, see e.g. Niinemets 1997, Niinemets et la. 2004, 2010) – particularly in a model that accounts for up to 16 canopy layers. Some of the pitfalls that come with canopy scaling have been outlined in Keenan et al. 2011. Wouldn't it be reasonable to demonstrate at least the suitability of these simplifications

before using them?

3. Drought impact is quite large and in a similar magnitude for GPP and isoprene. This is somewhat surprising because the majority of measurements find photosynthesis-related emissions less sensitive to drought stress than GPP (see overview in Niinemets 2010, referenced as 2010a in the manuscript). It is also one of most uncertain impacts in the MEGAN model, which is parameterized from only one publication and heavily depends on wilting point parameterization (according to Guenther et al. 2012). It would therefore of particular interest to get more insight into the indirect drought impact via stomata conductance. If I understand it correctly, a decreased conductance decreases the internal co2 concentration (ci), electron transport limited photosynthesis and thus emission. The degree of reduction thus depends on implementation and parameterization. Given the different suggestion to do this (see different formulation e.g. in Farquhar et al. 1980, von Caemmerer and Farquhar 1981, Harley et al. 1992, and von Caemmerer et al. 2009), I feel that some more description and sensitivity analyses is needed to show that the implemented emission model responds reasonable. Additionally, - since the decreased ci should directly increase the emission by the k-term (as stated in P17729, L9) – It would be interesting to see of what magnitude the two counteracting effects actually are and if the relation between both depends on the degree of drought stress?

4. It is a bit irritating that vegetation types such as the 'deciduous forests' are put into one PFT, given the huge differences in species-specific emission potentials and the considerable importance of differentiating vegetation classes (e.g. Schurgers et al. 2011). In the MEGAN this has been to some degree accounted for by providing regional specific emission factors. This is not the case here, isn't it? Thus, I wonder 1) if the dominant forest within a PFT actually behaves similar regarding emission independent from its location (resp. species composition?), and 2) if the average parameterization of photosynthesis is able to reproduce the average emission response of the PFT. An indication that this might need to be considered is the finding that the

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given representative vcmax values for forest PFTs are a lot smaller (30 for deciduous) than those assumed for high isoprene emitters (e.g. all deciduous species listed in the isoprene emission data set of Pacifico et al. 2009 have vcmax values between 50 and 100 umol m-2 s-1). Can you comment on this?

The latter point, of course, is also the reason why the comparison with a specific site doesn't work. Shouldn't the model be run with two parameterizations – one PFT specific and one site specific - to show that the model is at least able to provide reasonable estimates if correctly parameterized?

The questions above are only referring to the direct linkage between photosynthesis and emission models. There might be some more questions regarding for example the simulation of seasonality of emission. It is very well known that the onset of emission lacks behind that of photosynthesis which is generally attributed to the need to build up enzyme capacity/ activity of emission products (see e.g. Monson et al. 2012). But this might be addressed in another discussion.

Best regards,

Ruediger Grote

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