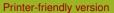Biogeosciences

Discussions

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Interactive comment on "What are the challenges for modelling isoprene and monoterpene emission dynamics of subarctic plants?" *by* Jing Tang et al.

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Received and published: 23 June 2016

This is a nicely written manuscript which addresses an important question in BVOC estimation – namely the representation of cold environments in global estimates and the uncertainties of modelling in this respect. It is also well timed since a lot of new information has recently been published about this topic and the implementation of this knowledge into a model is overdue. However, I feel like I have to urge the authors to be more careful in what they regard as 'good agreement' between measurement and simulation or at which point they conclude that the model's suitability has been 'demonstrated'. Overall, I see a lot of model deficiencies and uncertainties in this study which should probably be the prime focus of the investigation. In this respect, I would welcome figures or statistics that show the actual relation between measurements and simulations rather than column- or point diagrams. Apart from this, I think that the

model description part needs some elaboration.

Response: Thanks for the reviewer's suggestion. Briefly here, we will address the model's agreement with observations using a Willmott's index of agreement as well as mean bias error. Apart from BVOC related processes, a description of general photosynthesis processes will be added to Section 2.2.

Specific comments:

P1, L22: 'Short time scales' not only need to be defined, mentioning them here is also irritating. In fact, the question about simulations and observations referring to different time periods is troubling me throughout the manuscript.

Response: The term refers to a period of hours to a few days versus long-term scale of months to years. The clarification will be added in the main text. Since the simulated results from the model were at daily scale and the measured fluxes could be at any time point of a day, presenting the modelled daily average and maximum values aimed to bridge the differences in the time periods.

P1, L24: The model 'was able' to reproduce carbon fluxes for the majority of the vegetation period but showed considerable weakness in representing the seasonality, probably due to mismatch of phenological phases. This should be recognized.

Response: The modelled CO_2 fluxes do show some uncertainties in representing fluxes at the beginning of growing reason, which is discussed (see P13, L6-8) and related to phenological phases (the start of growing season). We agreed with the reviewer and will add the time period when the model did captured the observation. Also, we will change the term "was able to" to "showed reasonable agreement to" .

P1, L26: The difference of effective temperature in model and observation is certainly one reason for a mismatch in emission simulations which has been correctly acknowledged here. However, giving this as the only reason for a possible deviation is misleading at this point.

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Response: Thanks for point out. The sentence will be clarified by stressing that leaf T is one potential main cause, but not the only reason for mismatches between model and observations.

P2, L17ff: Major uncertainties are also other driving factors for emissions that are usually not considered in models, namely air chemistry, soil water availability, UV light and biological stress impacts. Also the representation of seasonality (which is composed of phenology and enzymatic activity changes) is a point worth mentioning here. The authors are mentioning most of these points at a later stage but I feel that it needs mentioning here.

Response: Thanks for the great point. More details will be added in the introduction, paragraph 2.

P3, L5: I think that in the Pacifico and Unger papers, the Niinemets approach is used. So this is to some degree a repetition here.

Response: Agreed, we have reduced the references to unique implementations.

P3, L10: seasonality and/or past weather conditions? In fact this is the same problem. You might differentiate into effects of phenology and enzymatic activity shifts though.

Response: We will change "seasonality" to "vegetation phenology" to differentiate relatively short-term acclimation (past weather condition) with vegetation phenological phases.

P4, L15: From the later remarks I take it that the BVOC emissions were not taken round the clock so the time or time period during the day when the measurements were made should be mentioned.

Response: A detailed description about measuring time will be added into the Section 2.3.3.

P5, L7ff: I am a bit irritated here. The Haxeltine and Prentice photosynthesis approach

is for seasonal or annual photosynthesis estimation, assuming a kind of optimal adjustment to average environmental conditions. Nevertheless, the model seems to work on daily timesteps here. The description given about the model itself looks very much like the Collatz approach – so what is taken from Haxeltine here? Regarding the description, many abbreviations are introduced here that seem not to be used later on –please check.

Response: Thanks for pointing this out. We agree that the description (mainly references) of the photosynthesis processes was unclear. Though Haxeltine and Prentice model use monthly data as input, but it still have daily time step photosynthesis processes, which is what LPJ-GUESS is based on. The original simplified Farquhar model used in Haxeltine and Prentice is developed by Collatz et al. (1991) approach which works at sub-daily scale. The model upscaling of leaf-level calculation to canopy scale is based on the Haxeltine's approach. The abbreviations which are not used later on will be deleted.

P5, L14ff: Since emissions depend on temperature in a highly non-linear fashion, I think it is generally acknowledged that calculating them with daily average values is necessarily not capturing the dynamics. Regarding the Niinemets model, for example Unger et al. used a 15 minutes time steps. From the description it sounds like LPJ feeds daily photosynthesis results into daily emissions. Can you elaborate on the problem? Also, I think that the reference temperature used in equation 3 and/or the parameter in the response function needs to be adjusted because the model is not using them as an immediate response value anymore but as parameter for daily average emission. (30 degrees as an average value throughout the day would probably exhaust the emission apparatus so that the response curve would not be valid anyway.)

Response: Thanks for the good points. The simulations in this manuscript used daily climate inputs and therefore the model works on daily scale, resulting in daily emissions. To overcome (the largest part of) the problem rightly raised by the reviewer, we compute a daytime mean (rather than daily mean) temperature to simulate BVOC emis-

sions (details in (Arneth et al., 2007)). This will be stressed in the revised manuscript. Still, the reviewer is correct that an average daytime temperature may still yield an underestimation of the emissions with the convex shape of the temperature response, certainly if the temperature variations during daytime are large. We will add discussion on this problem. To make our outputs comparable to a few time points measurements during a day, we came up this idea of presenting both daytime average and also daily maximum emission rate.

About the fitted curve with reference temperature of 20 degree, we are now aware of potential uncertainties caused by different time resolutions. In an ideal case, if we have more frequent BVOC samplings in a day as well as in the main growing season, we could average daytime T and emission rates before do the curve fitting. However, the current dataset is too few to support us to implement this parameter adjusting. The reviewer is correct and we will address this issue in the discussion at well. Thanks!

P5, L15: Instead of using I for isoprene as well as monoterpenes shouldn't you use Ei and Em or similar? This can further be modified for storage (e.g. Ems) in equation 4. Response: The equations will be modified based on the suggestions.

P5, L22: Here, the influence is named 'phenology' while later the same function refers to 'seasonality' (L30). Since these are two different things – is this a lumped index? Specific or specifically parameterized for PFTs? Empirical or dependent on weather or climate?

Response: The use of "phenology" here is indeed not correct, $f(\sigma)$ represents the seasonality of the emissions caused by variations in enzyme activity. The effect of phenology (represented in the model as the abundance of leaves) is captured separately by affecting the amount of absorbed radiation. We will correct the sentence.

P5, L27ff: see also comment from L14ff. It seems that the reduction of reference temperature is rather a necessity from applying the model on a daily time step than a particular feature of arctic plants.

C5

Response: Applying the reference temperature of 20 °C is of relevance for arctic plants since in most cases, the daytime T is close to or below 20 °C. We used the measured hourly BVOC fluxes with temperature in July to get the fitted temperature curve (see Fig. 1). The fitted response has been directly used in the model.

P5, L29: it is stated that the reference temperature is changed. This is to 20 oC as elaborated on later, correct?

Response: yes, we used the reference temperature of 20 °C. This will be clarified in the text.

P6, L2: fCO2 according to? Since it seems that variable CO2 air concentrations are used, it would be helpful to know to which degree CO2 might be responsible for differences between the years (probably small, but anyhow).

Response: Yes, the changes are small indeed, as f(CO2) varies with the inverse of the CO2 concentration. This gives a reduction of ∼3% between 2006 and 2012.

P6, L14: If the energy balance calculation was modified specifically for this study and is not published elsewhere, this modification should be explained.

Response: The development we had in this manuscript was essentially based on the work by Sedlar and Hock (2009) and therefore we did not include more details than just citing the original paper. But we will add more details about what are the main effects of adjusting the longwave radiation calculation.

P8, L15: I don't get how this can give you LAI values. Could you elaborate a bit? Looking at figure 3 there seems to be a difference between Lai and what is measured but the measurements are nevertheless used for evaluation. So how are the two related?

Response: The point intercept-based measurement gives a description of plant coverage (Finzel et al., 2012). During the growing season, the chances that the pins hit on leaves are generally higher and therefore we link these measured data with LAI which describes leaf coverage per ground area. It is not one-to-one relationship to compare

C6

(influenced by sampling inclining angles, sampling time, hits on stems etc., see discussion in section 4.1), but we think the modelled LAI is the closest variable we can compare with the measurement.

P8, L21: I agree that model results in daily resolution might not be comparable to measurements done at noon. This seems to be a general problem as mentioned above. I also agree that you can calculate noon temperature from average temperature to get a representative value of noon emission – but why don't you do the same with PAR? Instead of using the average value which is definitely wrong you can estimate maximum PAR from average PAR (e.g. Berninger F (1994) Simulated irradiance and temperature estimates as a possible source of bias in the simulation of photosynthesis. Agric. Forest Meteorol. 71:19-32)? Have you estimated the sensitivity of this error on the results?

Response: Thanks for the great point. Generally, it is not a very difficult problem to compute the maximum PAR, but we cannot really compute an instantaneous photosynthesis flux at noon (or any other time) with Haxeltine and Prentice approach, because it describes daily photosynthesis. It also becomes difficult to estimate potential sensitivity from different PAR values.

P9, L3: Check wording. I think it should be the modelled co2 fluxes that are sensitive to a change of parameter. This should also be indicated in some kind of measure, i.e. the degree to which the parameter was varied.

Response: The wording will be altered to clarify the text. The range for the parameter $\alpha c3$ was based on a previous study by Pappas et al. (2013) and the changes of modelled CO_2 fluxes as well as LAI, responding to the parameter $\alpha c3$ were illustrated in Figure S1. From Fig. S1, we can clearly see how the modelled CO_2 and LAI varied with the parameter $\alpha c3$. Before running sensitivity testing of $\alpha c3$, we have selected several parameters to do sensitivity testing and then estimate Sobol sensitivity index to quantify the explained ability of each parameter to the modelled CO_2 fluxes as well as

LAI.

P9, L9ff: In fact, the deviations are considerable. Not only GPP and thus emission is considerably overestimated in both years early seasons – which should be quantified and considered in annual estimates – but LAI is totally wrong in all PFTs except LSE+EPDS and CLM under current climate where the overestimation is a mere 10-15 percent. In L15/16 it is stated that these are the most important PFTs but in the next sentence the other PFTs are described to have a 'large coverage'. Are there any numbers that I have missed that give an objective picture about the abundances?

Response: Since the CO_2 fluxes are not continuously measured, quantification of the overestimated CO_2 fluxes of early season in annual estimates is unfortunately not possible. Considering that the modelled LAI and the point intercept-based may be not one-to-one relationship, the relative abundance of different PFTs coverage were evaluated. The measured coverage can be influenced by hits on non-leaf parts, pin size, subjective judgement of species and sampling inclining angles (see Discussion 4.1). We agree with the reviewer that the wording was at times confusing, e.g. the words "dominated" and "large coverage" and we will correct it.

P10, L5: Monoterpene emissions seem to be met particularly because measurements occurred mostly on days with low emissions (according to figure 4). This is a problem because the high simulated emissions practically lack evaluation that should be addressed. I can certainly imagine other ways of representation or statistical analysis that can be used to elaborate on the point.

Response: Thanks for pointing this out. We will add discussion on the potential lacking evaluation of high monoterpene emission rates. We will also add the statistics for the comparisons and Figure 7 has changed to scatter plot to illustrate the modelled and the observed WR.

P10, L10: Similarly, I have large difficulties agreeing that figure 5 supports the statement that isoprene emissions were mostly captured by the model.

C8

Response: This sentence actually pointed out that model is doing fairly good job on describing day-to-day variations of isoprene emission, though still have some discrepancies in capturing absolute magnitudes for some days. We will change our wording here and add statistic to support our description.

P11, L26ff: The simulated annual emissions include the largely wrong response of LAI as well as the wrong response in early season emission, right? Can the error somehow be estimated? I have the feeling that these calculations might be too far off to be considered here.

Response: As mentioned in an earlier response, the closed-chamber based co2 fluxes were not continuous measurements. The concluded overestimated CO2 fluxes during the early seasons were based on very few measured data points. To further consider their influence on the annual estimate is difficult without continuous data support. The simulated annual estimate is uncertain considering the mismatch in LAI and early season CO2 fluxes, and we will clearly point out the uncertainty in the revised manuscript. However, presenting annual emissions in this manuscript is to look at longer timescales despite the discrepancies found in the evaluations.

P12, L14ff: The discussion seems to be overall comprehensive. Still, as for example in the first line, I think the authors are overenthusiastic about their results. This also applies for the conclusions.

Response: we will adjust the wording.

P14, L23ff: The comparison with common parameterization should not only be concentrated on the arctic environment but also on the problem with the time resolution (see above).

Response: The time resolution could be a possible cause. As mentioned in an earlier reply, the model has used daytime temperature, instead of daily temperature, which could reduce potential differences caused by two time scales. We will add discussion

about potential influences of time resolution on emission T response in Section 4.2.

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