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Biogeosciences Discussions

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

Anonymous Referee #1

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Major comments

The subject matter of this paper is important. The Arctic environment is changing rapidly. Because of BVOC impacts on air chemistry, it's important to have models that can successfully predict the response of BVOC emissions. This paper makes an important contribution by employing a model with a dynamic vegetation component. As they warm, Arctic ecosystems are expected to see a shift towards woody plants, and this should change the capacity of the ecosystems to emit BVOCs.

The paper has strengths, but also needs substantial improvements before publication. The basic modelling approach is sound, and it's helpful that the authors include the investigators that actually made the measurements. The paper demonstrates a good understanding of many of the ecosystem processes that should be captured by the

model. Overall, I thought the discussion section was strong. Among weaknesses, the comparison of the model to the observations need to be improved. First, much of the discussion is qualitative. The model is said to fit the observations well in many instances, but there is no quantitative analyses: no goodness of fit metrics, and no statistics. Need to formally compare model to observations with statistics. More specifically, using the max and the daily average as a basis for comparison doesn't make much sense. What is the point of a daily average, especially since the meaning of the daily average changes with the long diurnal cycles in the Arctic? Why not just use the times of day that cover the range of the observations? Also, the figures could be improved by consolidation. The same data are presented in multiple figures in two different instances. The figures would also be easier to interpret if instead of presenting the max/daily average, just one metric was used for comparison to the observations. Also, there is very little acknowledgement of potential for experimental error in observations (one mention at the very end). Given the technical challenges with experiments in the Arctic, the potential for measurement error should be addressed.

The employed model is touted as being a mechanistic model, but then an empirical method is used for its calibration to the dataset. This is not itself a problem per se, but the paper states that mechanistic models are better than empirical models. If so, why is such an empirical calibration necessary? Also, a serious deficiency with the model is that it does not account for the effect of previous weather conditions (example, 24 hours and 10 days) on the capacity to emit BVOCs. This effect is potentially very important in the Arctic.

Finally, the list below of minor comments and technical corrections is extensive.

Minor comments and technical corrections

Title: The title suggestions that the article will focus generally on modelling subarctic plants, but instead the article is about one specific effort using one specific model formulation. While of course some of the manuscript is more general, it is also uses

data from just one field site.

Page 1, line 14 – page 2, line 4: The abstract could be clearer. There are some specific recommendations below, but more generally the abstract should be condensed and just the highlights presented.

Page 1, line 14: Title says “subarctic” while abstract goes back and forth between arctic and subarctic. Make sure each use is intentional. Further in the manuscript, (sub)arctic is used. Again, make sure this is all consistent.

Page 1, line 23: “higher levels of warming” instead of “higher levels’ warming”.

Page 1, lines 24-26: The sentence should be written. Do you mean the “measured” BVOC WR, not modeled? If you do mean modeled, what was the standard “were better captured”? Also, “compared” instead of “comparing”.

Page 1, line 26: This sentence is relays an interesting result, but there is not enough context to warrant inclusion in the abstract. Please remove it.

Page 1, lines 30-31: This sentence can be removed, since it’s a circular argument. The high WR led to the high adjustment T curve.

Page 2, line 3: remove “extrapolation”.

Page 2, lines 3-4: How do points (2) and (3) differ? Isn’t “PTF’s responses to warming” a subset of “representation of vegetation dynamics in the past and future”?

Page 2, line 7: “plant” instead of “plants” or include an apostrophe.

Page 2, lines 10-13: First, need to include that BVOCs don’t solely react with OH. In particular, ozone is another important reaction partner for some BVOCs. Second, in a low-NOx environment, BVOC emissions can lead to a reduction in tropospheric ozone concentrations.

Page 3, line 3: “from” instead of “along”. Also, why is G3P the “chief precursor” if

pyruvate is also required?

Page 3, line 6: “part of monoterpene productions” should be clarified.

Page 3, line 8: remove the inner set of parentheses.

Page 3, lines 13-15: This is a contentious statement, and there is no reference. A more nuanced statement is necessary, and could reference Monson, R.K., Grote, R., Niinemets, U., Schnitzler, J.P., 2012. Modeling the isoprene emission rate from leaves. New Phytologist 195, 541-559.

Page 3, line 16: “referred to here” instead of “referred here”

Page 3, line 17: remove space before comma

Page 3, lines 18-20: Should reference Potosnak et al 2013 here. While dwarf willow’s T response was OK compared to G93, the light response was more linear than expected.

Page 3, line 25: Should also include low transpiration rates. Because of permafrost, transpiration rates can be low, which also leads to the high ground temperatures.

Page 3, line 30: Give what LPJ-GUESS stands for.

Page 4, lines 2-4: The objectives could be clarified. To me, (1) “capture the observed BVOC T sensitivity” is the same as part of (2) “To address short-term and long-term impacts of warming on ecosystem BVOC emissions.” Be more specific about your study goals, or further differentiate the difference between 1 and 2.

Page 4, line 8: use straight single quote for minutes symbol.

Page 5, line 1: You have already defined PFTs above, so don’t redefine.

Page 5, lines 2-4: Is this statement true for Arctic-specific PFTs? Please indicate this.

Page 5, line 5: What does “large-scale” mean here? I consider the base Farquhar equations to be leaf-level. Do you mean canopy-scale?

Page 5, lines 4-12: I assume transpiration & stomatal conductance are also modelled to get pi? Maybe you'll talk about this further down, but it would be important for understanding discrepancies between air and leaf temperature.

Page 5, line 18: How is Ci different from pi defined on line 11. Just concentration vs. partial pressure? What does "without water stress" really mean? This is probably tied to my comment above.

Page 5, line 26: "optimum from terpenoid synthesis" should be "optimum for terpenoid synthesis"

Page 6, lines 1-2: Give a reference for the co2 response in the model, as you've done for the other responses.

Page 6, lines 12-15: Again, this gets back to my comments above about transpiration and conductance. It would make more sense to move this discussion to the general description of the model, before discussing biogenics. Also, more detail on this part is necessary. What are the details here? This can be done by references to the literature, if it has been described by LPJ-GUESS before. What is the coupling between estimating leaf temp, internal co2, transpiration and stomatal conductance? Or is a more empirical algorithm used?

Page 7, line 4: Fix grammar: either "appearing" or change sentence structure.

Page 7, line 4-5: I agree there is insufficient data, but mosses may make a large contribution to BVOC emissions in some Arctic ecosystems. So, it's fine to incorporate them into a larger PFT, but are you capturing their emissions? That is, do the emission factors for this PFT reflex the mosses?

Page 7, line 17: first, not firstly.

Page 7, line 20: "other" instead of "rest"

Page 7, lines 18-21: Given the lack of data for the Arctic, it's justifiable to use two years

data for calibration and two years for validation. But, the sensitivity of this procedure should be assessed by flopping the years: how different are the results if the second two years are used for calibration, and the first two used for validation?

Page 8, lines 9-11: The goodness of fit here is a bit deceptive. The fit is entirely driven by the relatively few points that are above 23 deg C. Since everything below that is relatively close to zero, there is little new information added. For example, blocks 5 and 6 only have one observation each above 23 deg C, so the individual fits are very good. I don't see the added value in the doing the individual fits for each block. It seems all that info comes out of the overall fit. Finally, you should understand the justification for using 20 instead of 30. Yes, this makes sense conceptually and certainly for measurements, but realize that mathematically, using your formulation, there is no difference between using 20 and 30, because of the laws of exponents. That is, you'll get the same r2 for the fits with each. This isn't true with more complicated formulations of the T response; for example, the T response in isoprene emission for G93.

Page 8, lines 16-23: This is confusing. Your goal is to compare your measurements to the model. So, yes, using daily averages isn't appropriate. But why discuss them in the first place? I think you'll use them for another purpose, but that's not clear. Why do you use max T & PAR? Wouldn't an average around the measurement time make more sense? And again, your last sentence here is obvious. Particularly in the Arctic, with low sun angles for much of the day, this isn't a strong statement.

Page 8, lines 27-28: Again, examine Equation 3. You'll see that changing from 30 to 20 only introduces a constant.

Page 9, line 4: In Fig. S1, the figure legend should indicate what the dashed vertical line denotes at the value of 0.4 in both panels. The text explains this, but the figure caption should too.

Page 9, line 10: Do you expect to see a one-to-one correspondence between the point intercept info and the LAI values? This surely doesn't hold as LAI gets closer to 1 (and

exceeds it), but you should share your expectation here. Do you assume that there is no overlap with cover, and therefore there should be a one-to-one relationship? If so, state that.

Page 9, lines 18-22: You discussed the LAI response to warming, but not the GPP/NEP/ER response. Why?

Page 9, line 29 – page 10, line 1: This analysis isn't adding much to your argument. Of course you see this, because your model is driven by PAR and T. You don't need to cover this resultâĂŤit follows directly from your model formation (Equations 1-3). Second, I don't understand the relevance of relating mean daily ISO/MT production to the noontime values. What do you learn from this?

Page 10, lines 3-4: For "the observed average rates (blue squares) were well captured by the modelled noon emissions" you need to present some statistics to back up this statement. You should do an xy plot of this data and see what the fit looks like. Even if you don't present the plot as a figure, you should report the statistics of the fit.

Page 10, line 10: As mentioned below, the same data is presented in Figs 4 and 5a. And now you've made the same statement about fit as above. This should be consolidated, and again there needs to be a statistical analysis of the goodness of fit.

Page 10, lines 13-18: Yes, the temperature drives these emissions, but this is a bit complicated because of the chamber observations. There are two issues: one, the model's ability to predict leaf T; second, the increase in air T because of the chamber used to measure BVOCs. Only the first is important for extrapolating your results.

Page 10, lines 25-27: Again, need statistics to back up these contentions.

Page 11, line 30: After not using any statistics comparing the model to observations, why would you use a statistic in this case, when you are comparing the model to itself?

Page 13, lines 23-25: This is an interesting contention. But, the emissions for the storage pool are generally regarded as being due to the physical process of evaporation

of the MTs. Why would this change for Arctic plants?

Page 13, lines 28-29: Yes, and that is why restricting your modelling to the times of day when measurements occurred would help.

Page 14, lines 2-3: Do you mean the bryophyte decrease due to drying is an artifact of the experimental warming and shouldn't be captured by the model? Please elaborate.

Page 14, lines 5-7: Yes, because you used the observed data to fit your model. Remind readers of that point.

Page 14, lines 11-13: Could also mention the drying that was noted above for species responses (bryophytes).

Page 14, lines 18-19: Need some more analysis here. Yes, the two responses are very important. But, you should re-emphasize that your dynamic vegetation model isn't doing a great job of getting the vegetation changes correct. Therefore, the results in Fig 8 are illustrative of the impact, but the details are not certain.

Page 14, lines 25-30: I agree with most of this logic, but since this particular study is looking at whole system measurements, there is potentially an interaction between the true T response of the plants and the issue of canopy temperature described earlier. You should at least discuss the possibility that some of this T response is not at the enzymatic level, as suggested here, but is due to a non-linear increase in leaf T with increasing air T due to canopy warming. Perhaps some of the references cited are leaf-level measurements which could clarify this point?

Page 15, lines 1-3: Yes, this is important, but it also brings in the issue of drought stress. Drought stress can occur frequently in some Arctic ecosystems due to relatively shallow soils above the permafrost. To understand canopy heating, it will be necessary to understand canopy water dynamics.

Page 15, lines 15-16: Great this is stated clearly in the conclusion, but this point should also be made in the discussion.

Figures 2, 4, 5 and 6: For Figs 2, 5 and 6, use you DD/MM on the time axis, but day of year for Fig 4. Be consistent, and I prefer day of year.

Figures 5, 6: The top panels (a) of each figure are the same data presented in Figure 4. These results shouldn't be presented twice.

Figure 7: There is also a lot overlap with Figures 5b and 6b: two of the three sets of data have already been shown. In addition, why is there a break in the y-axis, when mostly the same data have been presented in Figures 5b and 6b without a break?

Figure 9: Why include the higher-T scenarios? I understand they are (unfortunately) realistic due to the IPCC estimates of climate change. But, you don't discuss them much, and there are obviously some weird things happening with the vegetation change (for example, lower +8 compared to +2 for MTs in 2012). Since the vegetation changes predicted by the model are suspect, the results of the +4 and +8 runs are highly speculative.

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