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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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Thanks for the reviewer's constructive comments and suggestions for improving this manuscript. All comments have been answered. Below, we first give general answers to the reviewer's comments and the detailed comments will be addressed separately point by point.

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,

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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.

Response: (1) We agree with the reviewer that the comparison of modelled and observed variables should include some statistics and will add Willmott's index of agreement (describing models' prediction with pairwise-matched observations) as well as mean bias error (describing mean deviations between modelled and observed values) for each comparison.

(2) The reason why we present both daytime average and daily maximum values to compare with the observed is that the BVOC sampling on each field plot was conducted at a certain time point of a day for 30 min, while the modelled processes are at daily scale. Considering the strong diurnal cycle of BVOC emissions (for an arctic example, please see Lindwall et al. (2015)), neither daily average nor daily maximum were accurate enough to directly compare with the observations. We reason that by presenting both rates, we can see the range of the modelled daily emissions.

(3) There is some data repetition in Figures 4-6, and separating them in different figures was aiming to explain different perspectives. In the revised manuscript, we will move Figure 4 to the supplementary to reduce data repetition. There will be no data repetition in Figure 5 and 6.

(4) We agree that the discussion about potential uncertainties from the measurements should be addressed in depth, mainly covering point intercept-based coverage and side effects from OTC-chambers.

(5) Regarding to the empirical method used in calibrating T response, we agree with the reviewer that there is empirical element in the parameter estimation, which, however, reflects some processes understanding, e.g. underlying enzyme activation. Even for mechanistic models in bio/geosciences fields, it sometimes cannot avoid of using empirical relationships determined from observations where multiple processes may anticipate. In our case, the calibrated T response (empirically) is used for influencing fraction of photosynthetic electron transport contributing to isoprene and monoterpene production, which is internally linked to other processes and potentially reflect more

dynamics than other empirical models.

(6) The current model did not consider the past weather conditions, rather, it emphasizes the enzymatic acclimation to short-term climate. As we do not have observed data to support which period of past weather should be taken into consideration, adding emission acclimation to the past weather will bring additional uncertainties to the modelled fluxes and complicate the current comparison. Ekberg et al. (2009) has fitted their observational data to obtain a relationship between past weather conditions (48 h) and isoprene emissions (not for monoterpene) from wetland sedges. We would like to further address this issue in the future model development with assistance of available climate data on measurement sites (past 24-96 h temperature as well as leaf level BVOC sampling data).

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.

Response: We can well understand the reviewer's concern about including only one study site and will therefore add a sub-title to specify it. The title will be changed to "Challenges in modelling isoprene and monoterpene emission dynamics of subarctic plants: A case study from a tundra heath".

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.

Response: Thanks for the comments. The abstract will be adjusted to condense the length and to make points clearer.

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.

Response: We will carefully consider each mention of “subarctic” and “arctic” through the whole manuscript. We will check and correct to use the most precise term in each place. The observational data originate at the Subarctic, but many ecosystem processes and components function similarly both in the Subarctic and the Arctic, and many of the issues handled are similar in both regions.

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

Response: Accepted.

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”.

Responses: Yes, it should be “measured”. The suggested changes will be implemented.

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

Response: The reason behind is the underestimated leaf T is one of the main factors influencing short-term BVOC fluxes. The sentence sounds a bit misleading, but we will clarify 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.

Response: The adjustment of T curve was only based on the emission rates from the control plots under naturally varying weather conditions within the growing season, and no emission rates from the warming plots were used. So, it is not high WR, which led to the high T curve. The improved ability of capturing the observed WR by the adjusted

T curve indicates a better representation for arctic plants.

Page 2, line 3: remove “extrapolation”.

Response: Changed.

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”?

Response: we agree that these two points sound similar. Here, in point (2) “PFT’s responses to warming”, we mainly mean plant’s physiological adaption to warmer climate, while in point (3) “representation of vegetation dynamics in the past and future”, we mainly refer to long-term vegetation development, e.g., composition changes, disturbances, and expansion, etc. We will clarify the sentence.

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

Response: Accepted.

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.

Response: The text will be clarified. Thanks for the points.

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

Response: We have changed the words. The description of G3P as the chief precursor was not accurate and will be corrected.

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

Response: This is not accurate. It will be changed to “monoterpene productions”.

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

Response: Corrected.

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.

Response: We will add the description about process-based models can represent BVOC synthesis activities in chloroplasts and vary between species and leaf long-term growing environment. The suggested reference will be added.

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

Response: Accepted.

Page 3, line 17: remove space before comma

Response: Corrected.

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.

Response: We will add this description. Thanks.

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.

Response: Based on the observations, there is no permafrost at the studies plots. The issue is relevant for other arctic regions.

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

Reponses: the full name will be added.

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.

Responses: Thanks for point out this part. The first aim will be clarified by changing it to "capture the observed T responses of BVOC emissions for a subarctic ecosystem", to clarify that we mainly tackle questions about emission responses to temperature. The second mainly aim to compare short-term and long-term warming effects on the whole ecosystem. We will clarify both aims.

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

Response: corrected.

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

Response: corrected.

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

Response: Two of the cited references were conducted studies in high latitudes. So yes, this statement is true for arctic PFTs. As the reviewer suggested, the sentences will be clarified.

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?

Response: The "large-scale" here mainly refers to the spatial scale. LPJ-GUESS uses the canopy-level photosynthesis calculation based on Haxeltine and Prentice (1996), where a set of canopy-level equations were developed from the Farquhar leave-level equations. We will clarify our description in the main text.

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.

Response: Yes, in the model, the stomatal conductance influences transpiration as

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well as intercellular CO_2 concentration. We will clarify this in the descriptions.

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

Response: Thanks for pointing out. It should be p_i and p_i is influenced by stomatal opening, so we will correct both.

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

Response: corrected.

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

Response: corrected.

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?

Response: Using leaf T instead of air T for photosynthesis was developed in this study, not in other LPJ-GUESS studies. The reason for putting the description of leaf T development after BVOC process description is that the development of leaf T algorithm mainly considers the strong sensitivity of BVOC to leaf T. We agree with the reviewer that the description of leaf T as well as its linkage to the transpiration and stomatal conductance should be extended. We will also stress the leaf T rather than air T was used for photosynthesis in this study.

Page 7, line 4: Fix grammar: either “appearing” or change sentence structure.ÂÍ

Response: changed.

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?

Response: The emission factors for the CLM have considered the observed moss emission rates. At the ecosystem level, we cannot distinguish how much emission was from the moss relative to the other species.

Page 7, line 17: first, not firstly.

Response: corrected.

Page 7, line 20: “other” instead of “rest”

Response: corrected.

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?

Response: We did sensitivity testing using data from other two years (2010 and 2012) to calibrate, but this resulted only in slight effects on the best values for the checked LAI, GPP and ER, and the trend was consistent with that in Fig. S1. Further, it did not affect the selection of 0.04 μmol CO2 μmol photons -1.

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 realizes 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.

Response: The reason of adding block fitting is to illustrate the general fitting to the whole dataset also worked for each block (except for block 1) providing stronger evidence for the general trend. From the exponential equations along, we agree with the reviewer that there is no difference between using 20 or 30 degrees as reference temperature. However, the reference T in the model is not only used as BVOC T response equation, but also used in estimating photosynthesis electron flow rates at this reference T to convert the input emission capacity to fraction (see P7, line 35- P8, line1-2). The photosynthesis responses to the reference T of 20 and 30 degrees are not exponential. More explanation about the difference causing by different reference T will be added.

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.

Response: We agree that the issue indeed was a bit confusing because of our wording. We will clarify that not all measurements were only between 10 am- 2 pm, but also with a few sampling between 9 am – 5 pm, and this is the reason why we still keep the daily average in the results. Since we don't use hourly inputs, it is not possible to average emissions for the time period of days with the measurements. We used theoretical maximum T as the input for extracting daily maximum emission rates. 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. Lindwall et al. (2015) has shown strong diurnal cycle of BVOC emissions in the Arctic. This reference will be added to support our statement in the last sentence.

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

Response: As explained in the previous responses, the reference temperature is not only used for the exponential equation (Eq. 3), but also used in LPJ-GUESS to link the photosynthesis rates at 20 degree.

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.

Response: Corrected.

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.

Response: From the model side, LAI is the most relevant variable which can be used to compare with the point intercept measured plant coverage. The point intercept-based method does count numbers of plants that pin hits, not only the top canopy layer. So it should not have problem in comparing with LAI when LAI get larger or closer to 1. In this context, we did not assume no overlap with cover.

Page 9, lines 18-22: You discussed the LAI response to warming, but not the

GPP/NEP/ER response. Why?

Response: We did compare GPP response to warming as well and it showed that an underestimated vegetation CO2 fixation to warming in most cases, which can also been seen in the evaluating LAI response to warming (the absolute difference of total LAI between C and W plots). So we only included the LAI response to warming in the manuscript. Although the plant CO2 fluxes are also linked to BVOC emission, we consider the warming responses of LAI more relevant as they are aggregated effects of both photosynthesis responses and vegetation composition changes (directly linked to the changes in emitted compounds and relative magnitudes).

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 result. It 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?

Response: Thanks for your points. We will move figure 4 in the supplementary instead. We think if reader is interested to see the seasonality of BVOC emissions in this region as a general picture about temporal dynamics of emissions, they can still reach it. As we explained in the earlier responses, due to the strong diurnal variations of BVOC emissions during a day, sampling time is crucial for determining the emission magnitudes. Although many samplings were conducted during the period of 10 am - 2 pm, there were also samplings conducted beyond this period. Through presenting both daily average and maximum values, we can conduct an approximate evaluation of how the model performs by checking if the measured rates were in the modelled range.

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.

Response: Thanks for pointing this out. We will add Willmott's index of agreement (A) as well as mean bias error (B) to describe the model's performance.

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.

Response: We will move Figure 4 and relevant descriptions into the Supplementary. So in this case, we will not have much data overlap in the figures. We will add statistics to the remaining figures.

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.

Response: very good point. In this context, we can only discuss what we have considered in terms of leaf temperature estimation. However, the side effects of measurement chambers on leaf temperature were considered to be minor. As tested by De Boeck et al. (2012), the main side effects from chambers on leaf temperature is related to reduced wind speed. In our case, the measuring chamber has a fan to mix air during sampling time, in which we do not expect large impacts on the observed leaf temperature, but could have some impacts on chamber air temperature. However, for the warming treatment where the OTCs were installed to passively increase surrounding temperature, the OTC-resulted reduction of wind speed may elevate leaf T more than the expected on air temperature warming and could be considered as one reason for why the modelled WR was generally lower than the observed (Please see Section 4.2, first paragraph).

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

Response: We will add Willmott's index of agreement (A) as well as mean bias error

(B).

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?

Response: When we evaluated the modelled emission rates at daily scale, we focused on presenting the absolute differences from the observed. But as the reviewer suggested, we will add statistics for the model-data comparison. For the modelled annual emissions, we don't have the observed data and to illustrate warming effects on the emissions, the Mann-Whitney test was applied.

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?

Response: Parameterization of Eq. 4 is based on global scale study by Schurgers et al. (2009). It may be the case that for arctic plants, there is larger storage pools for MTs or different leaf anatomy which could influence release from storage pools. We are lacking of knowledge to quantify these effects on MTs emission, but we will clarify our discussion here.

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

Response: Please see the previous response regarding to daily processes in the model.

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.

Response: The observations found an increase of bryophyte coverage, but our model predicted a decrease of the coverage. The decreasing trend in response to warming is consistent with the study by Elmendorf et al. (2012) where they summarized 61 tundra warming experiments. Elmendorf et al. (2012) elaborated that drying of soil moisture is one of the reasons of declining of bryophyte coverage, which was captured by our model.

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

Response: As mentioned in the earlier reply, we only used the emission rate and T at control plots to get the response curve, but compared the modelled WR with the observed, which has shown the improvement.

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

Response: changed.

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.

Response: Thanks for the good points. We agree with the reviewer that we only illustrate a potential impact, but that there are still uncertainties in capturing the vegetation dynamics in detail.

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?

Response: The decoupling of leaf T from air T at 2 m height may partly contribute the observed strong warming responses. It may also relate to arctic plant species.

Discussion about the strong decoupling of leaf T from air T as well as its linkage to potential T response will be added.

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.

Response: Thanks for the reviewer for pointing this out. In LPJ-GUESS, leaf energy balance has been considered and the evapotranspiration is a function of soil water content. For this particular site, there is no permafrost and the soil is relatively moist. As the reviewer correctly pointed out, drought stress for some other arctic regions are possible, which may lead to further increase in canopy surface temperature.

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

Response: changed.

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.

Response: We will change to DD/MM for Fig. 4

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.

Response: We will only keep Figure 5 in the main text.

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?

Response: I guess you mean Figure 6b and 7b. The reason for adding a break in y axis in Fig. 7 is the modelled WR from the simulation with the original T response is

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really low, which is only presented here. In the revised version, we will use scatter plot to compare the differences between two T responses curves.

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.

Response: The purpose of having Fig. 8 (Not figure 9) in the manuscript is to illustrate factors influencing BVOC emissions at long-term scale, highlighting that vegetation changes can affect the response at decadal timescales, and also illustrating that this can lead to a change in the response over time, or a non-linear response to the level of warming. We agree that these estimates are uncertain, also given the fact that the model does not capture all observed vegetation trends. We will expand the description of the results and discuss the underlying response.

De Boeck, H. J., De Groote, T., and Nijs, I.: Leaf temperatures in glasshouses and open-top chambers, New Phytologist, 194, 1155-1164, 2012.

Ekberg, A., Arneth, A., Hakola, H., Hayward, S., and Holst, T.: Isoprene emission from wetland sedges, Biogeosciences, 6, 601-613, 2009.

Elmendorf, S. C., Henry, G. H. R., Hollister, R. D., Björk, R. G., Bjorkman, A. D., Callaghan, T. V., Collier, L. S., Cooper, E. J., Cornelissen, J. H. C., Day, T. A., Fosaa, A. M., Gould, W. A., Grétarsdóttir, J., Harte, J., Hermanutz, L., Hik, D. S., Hofgaard, A., Jarrad, F., Jónsdóttir, I. S., Keuper, F., Klanderud, K., Klein, J. A., Koh, S., Kudo, G., Lang, S. I., Loewen, V., May, J. L., Mercado, J., Michelsen, A., Molau, U., Myers-Smith, I. H., Oberbauer, S. F., Pieper, S., Post, E., Rixen, C., Robinson, C. H., Schmidt, N. M., Shaver, G. R., Stenström, A., Tolvanen, A., Totland, Ø., Troxler, T., Wahren, C.-H., Webber, P. J., Welker, J. M., and Wookey, P. A.: Global assessment of experimental climate warming on tundra vegetation: heterogeneity over space and time, Ecology Letters, 15, 164-175, 2012.

Haxeltine, A. and Prentice, I. C.: BIOME3: An equilibrium terrestrial biosphere model based on ecophysiological constraints, resource availability, and competition among plant functional types, Global Biogeochemical Cycles, 10, 693-709, 1996.

Lindwall, F., Faubert, P., and Rinnan, R.: Diel Variation of Biogenic Volatile Organic Compound Emissions- A field Study in the Sub, Low and High Arctic on the Effect of Temperature and Light, PLoS ONE, 10, e0123610, 2015.

Schurgers, G., Arneth, A., Holzinger, R., and Goldstein, A. H.: Process-based modelling of biogenic monoterpene emissions combining production and release from storage, Atmos. Chem. Phys., 9, 3409-3423, 2009.

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