We are very grateful to the Referee, Prof. Dr. Lars Kutzbach for his positive evaluation of our paper, constructive critics and very detailed specific suggestions. Below we try to answer every comment.

General comments

This manuscript presents very interesting data on inter-annual variability of CH_4 and CO_2 fluxes from a high-arctic wet tundra site. Multi-annual datasets of CH_4 from high-arctic tundra are still scarce, and thus the presented data is highly valuable for improving our understanding of the complex network of environmental factors and processes that control CH_4 fluxes from permafrost-affected soils of tundra ecosystems. Such understanding is crucial for appropriately assessing possible future responses and feedbacks of the arctic carbon cycle to climatic changes. The manuscript shows that the temporal CH_4 flux dynamics and the likely driving factors are considerably different in the studied permafrost-affected ecosystem compared to other wetlands that were previously studied and thus demonstrates that our knowledge about the arctic carbon cycle is still rather limited. The authors describe their measurements methods and results in high detail and extensively discuss their findings. Thereby, they share many interesting thoughts about possible biogeochemical and soil-physical mechanisms that could explain the observed temporal dynamics of CH_4 fluxes in the studied (and other) permafrost-affected wetland(s).

We appreciate the Referee for this positive evaluation of our work.

However, I think that the manuscript's analytical scrutiny needs to be improved. I would recommend starting this article with a set of more defined research questions or hypotheses and then arranging the data analysis and discussion so that these questions/hypotheses are addressed step by step (as was also suggested by Nigel Roulet).

The abstract and introduction of the revised manuscript will be changed according to this suggestion and suggestions of other Referees.

Now, the manuscript suffers from a very general statement of the objectives of the presented study (". . .obtain and analyse a multi-annual dataset.") and a somewhat lengthy discussion appearing partly too speculative.

In this manuscript we communicate three main messages (following this and the other Referees comments, the abstract will be changed in the revised manuscript to emphasize them more clearly):

1. We have documented the interannual variability in growing season CH_4 fluxes, that can not be explained by "traditional" environmental factors (although the seasonal variability within each specific year is quite "normal" and can be related to these environmental variables).
2. We have documented late season CH_4 and CO_2 fluxes and argue for the reasons behind their dynamics.
3. We hypothesize that CH_4 emission during late season can affect the flux during the next growing season, and show how this hypothesis can explain our interannual variability.

Message 1. could deserve a publication on its own, however such a manuscript would only then report on negative findings. Message 2. could also deserve a publication on its own, however, we try to avoid seeing the "autumn burst" only as a phenomena *per se*, but rather try to find its place in the

annual and multiannual functioning of ecosystems where it happens. Message 3. is admittedly very speculative so far, but it seem reasonable to us, it and it fit our observations. Adding this message to the manuscript we turn it from negative to be pointing at challenges for the future.

In this process of straightening the data analysis and discussion, the manuscript can be considerably shortened to become more concise. Besides compressing the text, also some figures can be omitted (e.g., Fig. 6 and Fig. 7; maybe Fig. 11).

The diurnal dynamics of CH_4 fluxes might be interesting for a potential reader, however, if the manuscript must be shortened, we can move Figure 6 to a supplement. Figure 7 can also be moved to a supplement, if required.
Figure 11, illustrating the suggested hypothesis is essential. Without the figure the explanations in the text would be very hard to perceive.

On the other hand, I think that the authors should include other published studies on CH_4 fluxes from permafrost-affected landscapes in their discussion (e.g., Wille et al., 2008, Global Change Biology 14(6), 1395–1408; Parmentier et al., 2011, J. Geophys. Res., 116, G03016; Sturtevant et al., 2012, Biogeosciences, 9, 1423–1440). Wille et al. (2008) and Sturtevant et al. (2012) report CH_4 fluxes during autumn refreezing of soils but do not report autumn pulses of CH_4 and CO_2 emissions.

We thank the Referee for the suggestions, all of them will be included in the discussion in the revised manuscript, while still considering space concerns.

A list of further specific comments is given below.

We thank the Referee for the comments.

The English writing has to be significantly improved before re-submission. There are many orthography (especially comma placement), word order and style issues that need to be corrected. Comments on these language issues are provided in the list of technical comments below. However, I cannot give in this review a complete list of all the errors as they are unfortunately too many. I strongly recommend that the manuscript should be given to a native speaker for proof-reading.

Both orthography and punctuation will be double checked in the revised manuscript.

I recommend the manuscript of Mastepanov et al. for publication after major revisions addressing the problems raised in my and the others' reviews.

We appreciate this.

Specific comments:

Page 15854, line 3: "High latitudes" are typically defined in a much broader way. In many studies, everything closer to the pole than 60° is taken as "high latitudes". In this sense, it cannot be said that CH_4 emission studies from high latitudes are scarce since there are many boreal peatland CH_4 studies. Maybe just write "…carried out in the Arctic…"

We agree with unclearness of “high latitudes” definition, so in the current manuscript it was immediately made more concrete - “north of the Arctic Circle”. The suggested “…carried out in the Arctic…” has the same meaning and will be used in the revised manuscript instead.

Page 15855, line 16: “What kind of “atmospheric data”? Please be more specific.

This phrase will be extended in the revised manuscript.

Page 15857, lines 1ff: Was the chamber equipped with a pressure equilibration vent? Hutchinson and Mosier (1981), Soil Sci. Soc. Am. J., 45, 311–316.

Our chambers had no special pressure equilibration vents. We are obviously aware of the pressure concerns, however, our experience has shown that this problem is not essential for our setup (automatic chambers with slowly closing lids). Also, the chambers newer close absolutely air-tight, so some functional analog of pressure vent is present.
In parallel with the current manuscript we are preparing another more technical one (planned for submission as a Technical Note to Biogeosciences) where we describe more details of the measurement setup and calculations of fluxes and discuss the performance of different solutions. The problem of possible artifacts caused by pressure changes will be addressed in that publication.

Page 15857, line 10 to page 15858, line 7: I like this detailed description of data availability and non-availability. It gives a good impression on what this dataset actually looks like. I think that it would be good to additionally provide information on the reasons of equipment failures. This could be valuable information for scientists conducting or planning to conduct similar investigations under harsh Arctic weather conditions.

We thank the Referee for recognizing our dataset description and will add information on the reasons of equipment failures in the revised manuscript.

Page 15858, lines 8-13: Was the linear regression applied to all five minutes of CH_4 and CO_2 concentrations measurements? How was the nonlinearity in the concentration-over-time data dealt with, which is at least for the CO_2 data to be expected if five minutes of data are used. Was nonlinearity in the gas concentration-overtime data a problem?

As mentioned before, flux calculation details will be described and discussed in the technical publication. We have developed a software package, which helps us to do the data handling and flux calculations in a flexible, but unified way; this software is used at many monitoring stations – Zackenberg, Nuuk, Svalbard, Abisko, Fäjemyren. The program operates by time window which is a bit broader than five minutes closing time (with correction for the delay in gas lines) and finds a best linear interval within this time window (higher R^2 of linear interpolation for CH_4 flux). By default settings (can be changed when necessary), the program first examine all possible 60-second intervals (scanning the total time window with the step of 1 record), then 65-second intervals, 70-, 75-, … until 180-second intervals. The one of all these intervals having highest R^2 is selected as a suggested slope. Then the program plots the whole 10-minute dataset, the suggested slope and suggested interval. The operator looks at this plot and either confirms the choice (pressing a button), or corrects it (clicking on more visually adequate beginning and/or end on the data interval to be

used). In this case the program does linear slope calculation for the selected interval. The operator can also mark the dataset as “bad”, and flux calculation is not performed for this measurement.
As CO_2 flux can be more complex (effect of CO_2 limitation, etc) the program by default applies the best fit for CH_4 interval for CO_2 data as well, with correction for delay in gas lines between the instruments. However, the operator sees both CH_4 and CO_2 data with their slope lines, and can chose an interval which is better for both CH_4 and CO_2, or two different intervals, if necessary.
In most cases the slopes suggested by the program are accepted; in most cases suggested slopes are based on 180 seconds of concentration data (three out of five minutes).
In our experience, if chambers perform well, CH_4 concentration change is very linear, except ebullition events. CO_2 dynamics can be non-linear in case of high fixation rates, causing CO_2 limitation. In such cases we use shorter initial slopes for CO_2.
During strong winds chambers become leakier, and sometimes the data is so bad that it should be excluded from calculations. Under very strong winds (storms) the whole system is stopped to prevent the damage of the chambers.
The basic thing here is that every measurement is quality controlled manually.

Page 15858, lines 14-17: Please give some more details on this approach to estimate ebullition fluxes. This is not standard and should be described in more detail. How are ebullition events counted? Do you use a specific algorithm for this?

We have a semi-automatic algorithm to process ebullition effects, count them and estimate the amount of CH_4 they carry out. This will also be described in the technical paper. However, at the Zackenberg site, described in the current manuscript, the ebullition events are very rare. As described in chapter 3.4, in this study we do not consider any ebullition fluxes. That is why no detailed information is provided for the corresponding methods. We mention ebullition in this manuscript only to show that we are aware of this flux component, and accept that it may give 0.1-1% inaccuracy to our flux estimations.

Page 15858, lines 20-22: What do you mean with “visual integration”? Why gaps were not filled?

As one of main aims for the presented study was an impartial search for correlations (or absence of correlations) between CH_4 fluxes and ambient factors, we chose to avoid gapfilling. A standard way of filling gaps in flux data is using some semi-empirical model for fluxes, based on environmental factors (temperature, etc.) and parameterized to fit the existing flux data. However, if we dispute the 'conventional' relationships and how applicable they are to multi-year datasets we obviously can not use them to gapfill the data that we examine.
By “visual integration” we mean that we tried to see the pattern (shape of the curve) of fluxes, which is very clear in most cases. This might also be called some way of “gapfilling”, however, we want to stress that the traditional gapfilling was avoided and only real measurements are shown.

Page 15859, line 27: It is not clear for me what you mean with "< 1°C precision" in this context.

The ground water at the site is not expected to freeze at exactly 0 °C, as distilled water does; the temperature sensors/loggers (Tinytag Plus) had small and different offsets. We estimate this offset by the temperature readings during the zero curtain, forcing these readings to zero (although we know they should not be exactly zero). This offset correction was up to 0.4 °C. Also, the sensors/loggers have the digital resolution of 0.1 to 0.2 °C (the recorded value jumps up and down

between two fixes levels). Due to all these inaccuracies we can not just say “the soil temperature started to fall below zero” – as even during the zero curtain some inaccurate values are a bit below our inaccurate zero. However the real start of falling temperature can be clearly seen in the data, and we can localize the very first data points with falling temperature – for some sensors they are at -0.2…-0.4 °C, for others -0.5…-0.8 °C. But in any case we are able to catch the moment of falling temperature before it falls to -1°C. That is what was meant by "< 1°C precision".

Page 15860, line 1: Please specify if the start or the end of the zero curtain period is the starting point for the freezing period.

The end of zero curtain period was what we meant. This statement will be clarified in the revised manuscript.

Page 15860, lines 2-3: Can you estimate when the time point of freezing of the entire active layer is reached, e.g., from your soil temperature measurements? Are the automated soil temperature measurements continued over the winter?

Unfortunately only the sensors at 5, 10 and 15 cm depths were in place during 2007-2010. They provided year round measurements, but no information of deeper layers freezing was available. New deeper temperature profile was installed in 2010, but each year is different in the freezing rate, so reconstruction of freezing times for 2007-2010 is not possible.

Page 15860, line 4: “I find the term “Post-season” too vague. Which season? Thaw season? Growing season?

In this line we exactly define this term for the further use in the manuscript. We agree post-growing-season sounds better and will us it in the revised manuscript instead.
We assume that the most important thing is to give a straight definition to any uncommon term, and then use it consistently.

Page 15861, lines 13-15: Please clearly distinguish “soil thaw depth” and “active layer depth”. The active layer depth is the depth of the layer that in parts of the year is frozen and in parts of the year unfrozen. The development of soil thaw depth over one thaw season does not mean that the active layer depth increases over the season. The active layer depth can only be determined over at least two years as the mean maximum soil thaw depth of several (minimum 2) years.

These terms will be checked and corrected in the revised manuscript.

Page 15863, line 9: Do you use the term “ecosystem production” (NEP) synonymously with “net ecosystem exchange” (NEE)?

Here we write that “ecosystem production started to decrease” AND “NEE crossed zero…”. We are obviously aware of the difference between these terms. In the revised manuscript we will split this sentence into two separate ones, avoiding “and”.

Page 15864, line 8: It does not become clear what you mean with “real" ebullition event.

Again, this is a very interesting issue but we assume the details of this discussion can not fit into this manuscript. When the concentration in the closed chamber increases linearly at some rate and then start to increase much faster for about 1 minute and then comes back to the initial rate, the only realistic explanation of this is an ebullition event. We are lucky if it happens in the middle of the chamber closure, and we can achieve steady emission rates before and after the bubble release and the concentration change caused by the bubble. If the ebullition effect occurs in the beginning of closure time (the probability of such scenario is higher, as the small vibration of closing chamber can provoke release of a bubble which is close to go) we se just high concentration increase in the beginning of the dataset and lower rate of concentration increase in the following. This case is easy to mix up with a situation when the chamber is leaky and initial rate of concentration change is higher than following.

Page 15865, line 4: Soil does not "melt", it "thaws".

This term will be checked and corrected in the revised manuscript.

Page 15866, lines 15-16: Peat and vegetation have never "negative weight". They may have some buoyancy but this is not equal to negative weight, I would say.

This term will be checked and corrected in the revised manuscript.

Page 15866, line 26: Instead of "permafrost melt" better: "lowering of the permafrost table".

These terms will be checked and corrected in the revised manuscript.

Page 15866, line 28: "thawing" not "melting"

These terms will be checked and corrected in the revised manuscript.

Page 15869, lines 10-11: Here, a comparison with other published CH_4 flux studies from high-latitude ecosystems would be appropriate; e.g., Wille et al., 2008, Global Change Biology 14(6), 1395–1408; Parmentier et al., 2011, J. Geophys. Res., 116, G03016; Sturtevant et al., 2012, Biogeosciences, 9, 1423–1440 (see also general comments).

We thank the Referee for the suggestions, all of them will be included to the discussion in the revised manuscript.

Page 15871, lines 11-23: Again, please compare your results also to CH_4 flux studies from Arctic ecosystems.

We thank the Referee for this suggestion, the comparison will be added to the revised manuscript.

Page 15871, line 24 to page 15872, line 11: In this paragraph, you should also discuss the potential of biased flux measurements during times of low atmospheric turbulence (often in nighttime) as discussed by e.g.; Schneider et al., 2009, J. Geophys. Res. – Biogeosciences, 114, G03005; Juszczak et al., 2012, Polish Journal

of Environmental Studies, 21(3), 643-658 ; Lai, et al., 2012, Biogeosciences, 9, 3305-3322.

We thank the Referee for this suggestion, the discussion of this phenomena will be added the revised manuscript.

Page 15871, lines 20-21: But later you elaborate on the hypothesis that the autumn pulses are due to gas bubble releases. How does this fit together?

(probably page 15872 was meant)
We have never argued that the autumn pulses were caused by ebullition. In our view, this gas came from entrapped gas bubbles, but their release has a very different mechanism. As shown in Figure 9 and described in corresponding text (page 15876 line 27 to page 15877 line 4):
"close-up flux dynamics at freezing time (Fig. 9) shows that every single peak of CH_4 is accompanied by a simultaneous peak of CO_2. The CH_4/CO_2 ratio is almost constant for each peak, confirming the gas having a single origin (one entrapped bubble). Such a bubble does not exhaust instantly, like it happens at ebullition but the exhaust of one bubble takes a few hours. This confirms the idea that the gas is squeezed through very thin channels, probably the remnants of vascular plant tissues."

Page 15871, lines 21-22: What are these physical reasons? This is not clear to me without further explanation.

(probably page 15872 was meant)
The molecular diffusion is determined by concentration gradient and diffusion coefficient for the media. The concentration of CH_4 in water solution has a limit (solubility), so the gradient is also limited. Earlier calculations (Christensen et al., 2003) shown that this limit is about 2% of summertime flux.

Page 15871, line 23: The aerenchymatous transport is not through the vessels of the vascular plants; thus it is no vascular transport.

(probably page 15872 was meant)
The word "vascular" will be omitted in the revised manuscript.

Page 15873, lines 11-12: Why not fitting a model that represents gross photosynthesis and ecosystem respiration in one model equation? From the automatic chamber system, there should be enough data points available to constrain such a model. Examples would be: Lasslop et al., 2010, Global Change Biology, 16(1), 187–208; Lund et al., 2012, J. Geophys. Res., 117, G02001; Parmentier et al., 2011, J. Geophys. Res., 116, G03016.

We assume that adding such a model to the current manuscript is not realistic because, as this Referee also noted in the beginning, the manuscript should be concise. We do not expect any outcome from such model to affect the results and conclusions of the manuscript.

Page 15874, lines 5-8: It does not become clear to me how your study shows that there is no strong "CH_4 flux regulation by methanothrophic activity". Please expand.

(probably page 15876 was meant)
Following the suggestion by Referee 1, this whole paragraph will be omitted in the revised manuscript.

Page 15874, lines 18-19: How can we know if this is "regularity" or stochasticity?

(probably page 15876 was meant)
This is explained later in the manuscript, using the hypothesis of autumn burst affecting following growing season emissions.

Page 15877, line 7: “hypothesis” instead of “theory”

These terms will be checked and corrected in the revised manuscript.

Page 15878, lines 11-21: I am not sure if I totally understand Figure 11. The figure suggests that the columns and the arrows are comparable quantities. But in the text you write that the CH_4 storages indicated by the columns are just scaled according to the maximum peak emission of the corresponding year. I do not see how peak emissions and storages are physically/mathematically transferred in each other. Without an attempt for quantification of the storage sizes, comparison with storage changes due to emission events is questionable.

We apologize for the possible misinterpretation of Figure 11. In this figure we try to draw a hypothetical storage of CH_4 in subsurface pools, however, we do not have any measures of this storage. As we tried to explain in the text (this part will be rewritten to be more clear in the revised manuscript), we make an assumption, that the peak growing season emission reflects the storage in the peat matrix; so we use peak emissions as a proxy for storage. Red bars in Figure 11 are numerically equal to peak CH_4 emission values (shown in Table 1 and Figure 4); however, in Figure 11 they represent storage.
Blue arrows show the total CH_4 discharge (decrease of stored CH_4) during the autumn burst. Strictly speaking, we also do not have these numbers, as we never were able to monitor the late emissions until their end. So instead of total discharge the arrows are sized to known discharge – amount of CH_4 emitted during our monitoring (Table 1, g C m^{-2}).
Very roughly, the longest blue arrow should be about 4-5 g C m^{-2}, so the highest red bar – about 5-6 g C m^{-2} and the lowest – about 1-2 g C m^{-2}. The magnitude seems realistic – for example, Strack and Waddington (2008, JGR 113, G02010) report the total peat profile bubble CH_4 stock of 0.3 - 1.0 mol m^{-2} (3.6 - 12 g C m^{-2}) for a boreal Canadian fen.
In the revised manuscript we will change the style of Figure 11 – it should look more like a sketch (as Figure 12).

Page 15880, line 2: Photosynthesis rates vary strongly over the day with strong diurnal variability of shortwave radiation even under polar day conditions. Please adjust your reasoning here.

We agree with the Referee and will not use this reasoning in the revised manuscript.

Page 15899: Figure 11. This figure needs an expanded caption that better explains the presented ideas. It is also important to indicate that the storages are actually not measured or calculated storages but re-scaled peak emission values.

Figure 11 and its caption will be reworked in the revised manuscript.

Technical comments:

Page 15854, line 3: “carried out” instead of “carried on”.

The abstract will be changed in the revised manuscript. We will keep in mind the suggested wording.

Page 15854: lines 8-9: Awkward sentence; I suggest: “The start of the growing season and the increase in CH_4 fluxes were...

The abstract will be changed in the revised manuscript. We will keep in mind the suggested wording.

Page 15854, line 16: I suggest “cumulative” instead of “accumulated” (to better distinguish between a physical accumulation and a mathematical summation).

The abstract will be changed in the revised manuscript. We will keep in mind the suggested wording.

Page 15854, line 22: Remove comma after “gases”.

The abstract will be changed in the revised manuscript. We will keep in mind this suggestion.

Page 15854, lines 25-26: I suggest re-writing like: “...conventionally known factors controlling methane emissions are...”

The abstract will be changed in the revised manuscript. We will keep in mind the suggested wording.

Page 15855, lines10-11: I suggest “...major questions regarding our ...”

The introduction will be changed in the revised manuscript. We will keep in mind the suggested wording.

Page 15855, line 12: better “strengths” instead of ”strength”

The introduction will be changed in the revised manuscript. We will keep in mind the suggested wording.

Page 15855, line 16: Place a comma after “methane”. There are many occasions like this in the text where you should place a comma after an introductory phrase or clause. Please check the whole text with respect to this issue. I will not list all places here.

The suggested changes will be made throughout the manuscript.

Page 15855, line 17: Place a comma before “and”. There are many occasions like this in the text where you should place a comma before a coordinating conjunction (and, or, but, nor, yet, for, so) that separates two independent clauses. Please check the whole text with respect to this issue. I will not list all places here.

The suggested changes will be made throughout the manuscript.

Page 15855, line 20: Hyphenate “long-term”.

The introduction will be changed in the revised manuscript. We will keep in mind the suggested wording.

Page 15856: Remove “characteristics”.

The suggested changes will be made in the revised manuscript.

Page 15856, line 23: Insert “m”: “0.6 m x 0.6 m” to be mathematically consistent (equals 0.36 m^2).

The suggested changes will be made in the revised manuscript.

Page 15857, line 7: Insert the definite article “The” before “active”. There are many occasions like this in the text where the usage of definite and indefinite articles is not optimal. Please check the whole text with respect to this issue. I will not list all places here.

The suggested changes will be made throughout the manuscript.

Page 15858, line 12: “based on” instead of “based of”. Please check the usage of prepositions throughout the text.

The suggested changes will be made throughout the manuscript.

Page 15859, line 4: “a reference” instead of “the reference...”

The suggested changes will be made in the revised manuscript.

Page 15859, line 8: “differential” instead of “differentional”

The suggested changes will be made in the revised manuscript.

Page 15859, line 11: “widely used” instead of “wide used”

The suggested changes will be made in the revised manuscript.

Page 15859, lines 24-27: I suggest using “zero curtain period” instead of just “zero curtain”.

The suggested term will be used in the revised manuscript.

Page 15860, line 18: “altogether”: better: “for the average of these three months”.

The suggested changes will be made in the revised manuscript.

Page 15860, line 19: Remove “for”.

The suggested changes will be made in the revised manuscript.

Page 15861: I suggest: “...was lower than in any other year...”

The suggested changes will be made in the revised manuscript.

Page 15861, line 25: Remove comma before “and”: These two clauses are not independent.

The suggested changes will be made in the revised manuscript.

Page 15863, line 5: “Afterwards” instead of “After that”.

The suggested changes will be made in the revised manuscript.

Page 15864, line 21: I suggest “ time scale” instead of “proxy”.

We prefer to keep the word “proxy” as this one is later compared with other proxies (which are not time scales).

Page 15865, line 7: Remove comma before “whether”.

The suggested changes will be made in the revised manuscript.

Page 15866, lines 14-18: This sentence is awkward and needs considerable re-writing.

The sentence will be changed in the revised manuscript.

Page 15866, line 19: Remove comma before “than”.

The suggested changes will be made in the revised manuscript.

Page 15866, line29: “permafrost table” instead of “upper permafrost bound”

The suggested changes will be made in the revised manuscript.

Page 15867, line 28: “...shifted with respect to calendar time...”

The suggested changes will be made in the revised manuscript.

Page 15868, line 25: Place a comma after “however”. Check throughout the manuscript.

The suggested changes will be made throughout the manuscript.

Page 15870, line 5: “remainder” instead of “reminder”.

The suggested changes will be made in the revised manuscript.

Page 15870, line 9: Remove “in some sense”, very vague wording...

The suggested changes will be made in the revised manuscript.

Page 15870, lines 10-15: These sentences are difficult to understand. Please try to write these in a way that is easier to understand.

These sentences will be changed in the revised manuscript.

Page 15870, line 24: I suggest “knowledge” instead of “wisdom”.

The suggested changes will be made in the revised manuscript.

Page 15871, line 8: Remove comma before “that”. Check throughout the manuscript.

The suggested changes will be made throughout the manuscript.

Page 15873, line 6: “collars” instead of “corners”

Here we really mean “corners” (or may be “verges” is better word for 3D construction?)

Page 15873, line 7: Remove comma before “although”. Check throughout the manuscript.

The suggested changes will be made throughout the manuscript.

Page 15873, line 13: Remove comma after “season”.

The suggested changes will be made in the revised manuscript.

Page 15874, line 2: Remove comma before “if”. Check throughout the manuscript.

The suggested changes will be made throughout the manuscript.

Page 15874, line 4: Please try to write more specific. “progress differently” is quite vague: How different?

This phrase will be extended in the revised manuscript.

Page 15877, lines 18 and 26: “interchange” does not really fit. I think.

The word “alternation” will be used instead in the revised manuscript.

Page 15879, line 16: This text line seems to be mixed up.

We do not see what is wrong with this line, but we will double-check the revised version.