Response to Reviewer 2's comments

First we would like to thank the reviewer for acknowledging the relevance of our results and for his suggestions. This will certainly help to improve our manuscript considerably.

Recommendation: Major revision.

Anonymous

Let me emphasize that this is an interesting study however the manuscript can be improved-in particular to make its importance clearer to the reader. In the present version, several issues are addressed: (1) continental configuration, (2) vegetation cover and (3) the orbital forcing, but without to extract the major points for consideration. For instance, sections 3.3 and 3.4 present minor findings for the Devonian period, while most significant contributions (sections 3.2 and 3.5) remain not enough explored. This problem being easily solvable, I recommend a major revision.

We agree with the reviewer that our studies' relevance needs to be articulated more clearly. In our opinion this definitely includes Section 3.3 and 3.4: Several papers convincingly argue that the influence of the orbital configuration (Section 3.3) is an important aspect for the Devonian period (De Vleeschouwer *et al.*, 2014, 2017). Furthermore, we find the climate variability pattern described in Section 3.4 of importance, as this strong regional effect might be able to impact marine biogeochemistry and therefore marine ecosystems at high Northern latitudes (Harada, 2016). For the Devonian as a period of several oceanic mass extinctions, we therefore see this as a crucial aspect to discuss.

The influence of changes in vegetation cover (Section 3.2) will be explored in greater depth in the revised version (see also our response to the first reviewer's comments). In particular, we will address the following aspects:

- A graphical presentation of the vegetation distributions used in the model experiments will be added.
- We will better explain the assumptions made for the vegetation cover of the different timeslices and add sensitivity experiments to better assess the uncertainty of the parameter choices made.
- The global distribution of non-vascular plants in the Early Devonian will be taken into account.
- Values of the most important vegetation parameters (albedo, evapotranspiration, roughness length) and their variation will be listed.

Finally, we will revise Section 3.5 according to the suggestions of both reviewers.

In addition to recommendations listed by the first reviewer I identified several areas requiring clarification.

Major comments:

(1) The revised manuscript should provide a table showing exactly how vegetation types are parameterized. Surface albedo, roughness, and evapotranspiration coefficient values used for representing bare soil, shrub and tree have to be presented. It would be helpful to have a brief description of what evaporation/roughness is (in the model) because latent and sensible heat fluxes are both affected by these parameters. If relevant, the phenology should be discussed as well.

We appreciate this suggestion and will provide the information in the revised version of the paper.

(2) The vegetation cover is never presented! Maps of vegetation used as boundary conditions for Middle and Late Devonian would be very helpful, especially for comparing with the figure 4. Moreover, as landplants are very sensitive to temperature-moisture regimes, it would be interesting to check if assumptions used to constrain the spreading of plants (shrub and tree) remain in good agreement with models outputs.

As mentioned above, we will include maps of the vegetation distribution in the revised version. Additionally, we will have a look at the temperature-moisture regimes and how this fits our vegetation distributions.

(3) Personally, I'm skeptical about the interest of the section 3.4. The main reason is that the climatic effect remains very weak, so almost impossible to link with temperature estimates based on δ^{18}O, and potentially dependent on pCO_2 levels. I suggest to remove this part, or significantly reduce its length.

As already outlined shortly above and motivated in detail in the reply to Reviewer 1, we are convinced of the importance of Section 3.4. The strong regional effect is dynamically interesting in its own right, and the relevance of the Arctic's biogeochemistry for marine life (Harada, 2016) makes the described mechanism important in the context of the oceanic mass extinctions during the Devonian.

(4) On lines 19-21 p 20. Authors argue that their results are in disagreement with Le Hir et al. 2011 findings. That is not entirely correct. Le Hir et al. 2011 suggested that the progressive change of the continental albedo has induced a warming (+4°C), but they have also noticed that this warming was not observed in their simulations due to the parallel reduction of the pCO_2. Over the Devonian, the cooling was estimated to -1.9°C in response to the decreasing effectiveness of the greenhouse effect (carbon dioxide level decreases from 6296 to 2125 ppmv). To my knowledge, both studies only differ by

their climate sensitivity ($\Delta T/\Delta pCO_2$) to land cover change.

We agree that the phrasing in this paragraph is misleading. The reviewer is correct in pointing out that Le Hir *et al.* (2011) report a temperature decrease of 1.9°C from their Early Devonian no-land-plants simulation to their Late Devonian simulation with Late Devonian land plant cover (called LP3 in Le Hir *et al.* 2011). This is a scenario comparable to our Early and Late Devonian best-guess simulations; using the same albedo values as in Le Hir *et al.* (2011) we find a 2°C temperature decrease, in very good agreement with their result.

However, while Le Hir *et al.* (2011) emphasise that the cooling due to falling CO_2 levels is mostly compensated by land-plant evolution (see, for example, their abstract where they report "unchanged temperatures"), we wanted to stress the fact that both studies find a cooling trend which is in contrast to the global warming seen in proxy data for the Late Devonian (Joachimski *et al.*, 2009; van Geldern *et al.*, 2006). In the revised version, we will clarify this issue.

(5) A brief paragraph summarizing limitations of the model/study will be helpful for readers not familiar with models. For instance authors should take more cautions with their conclusions concerning the weak influence of the continental configuration - this result being mainly due to the absence of the climate-carbon feedback.

Although we tried to mention the limitations of our model and our study in the relevant paragraphs, we agree that adding a paragraph which summarises these limitations is a good idea.

In addition to the above points, there are a number of minor errors that ought to be fixed:

line 8 p9: For illustrating the impact of paleogeography, continental temperatures appear more relevant.

We will add a discussion on the variations of continental temperatures with changing continental configuration.

the figure 4 is unreadable in its present state. How to compare Shrub-bare soil and Tree-shrub results ? please add panels showing Tree-Bare soil results. To make a more robust analysis, a plot of the snowline over continents should be included in surface albedo panels.

The intention of presenting the differences of the coastal-shrub minus the bare-soil experiments and the tree minus coastal-shrub experiments was to trace the evolution of land plants chronologically from the Early to the Middle Devonian and from the Middle to the Late Devonian in order to understand the associated changes in surface air temperature. If space permits, we will show and discuss the difference between the tree and the bare-soil cases as well. Finally, adding the snowline in the surface albedo panels is a good idea in principle, but complicated in a difference plot due to the fact that the snowline is different for the various vegetation distributions. Instead, we propose to add snow and sea-ice cover to the surface-air temperature maps shown in Figure 9.

line 10 p10: if you want to make that statement, a basic computation of the greenhouse effect may be helpful. (a simple formulation is available in Pierrehumbert 2005. (Climate dynamics of a hard snowball Earth, J. Geophys. Res., 110, D01111, doi:10.1029/2004JD005162.)

Many thanks for this suggestion. We will explore this possibility, but also the option of supporting our statement more directly using model diagnostics.

line 14 p11: continental temperatures seem to be more relevant.

We will add continental temperatures, but also give surface air temperatures to ensure the comparability of our sensitivity simulations with each other.

on lines 1-4 p 14, authors conclude that the discrepency ... we find that meridional ocean heat transport largely compensates for seasonal and regional differences in insolation caused by changes in orbital parameters. This result contrast with De Vleeschouwer et al. (2014) and constitutes an interesting finding of this study, so I suggest to include a specific discussion to convince the reader about the importance of the meridional heat transport (a figure will be very instructive).

Triggered by the short comment by David De Vleeschouwer, we have now investigated this aspect and the comparison with their results more thoroughly. We are grateful that they supplied us with some of their model output data sets. Therefore, we can now add a paragraph discussing the causes of this discrepancy in more detail.

line 28 p18 ...increased precipitation ... an increase in latent flux. The phrasing in this sentence is awkward. I am not sure that it is reasonable to mention this process to explain a warming at the surface.

We agree. In the revised version we will focus in the description of the regional patterns on the shift of the intertropical convergence zone and its effect on temperature. We will discuss the influence of the interaction of the continents' distribution, the orography and the seasonality on this shift.

References

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