Recommendation: Reject.

Anonymous

Paper summary:

Brugger et al. use the ocean-atmosphere model of intermediate complexity Climber-3α to quantify the sensitivity of the Devonian climate system to changes in the configuration of the continents, in vegetation cover and in orbital parameters. They also analyze patterns of quasi-periodic variations in global mean surface temperature arising from the waxing and waning of sea ice over the North Pole. In a 3rd step, they combine changes in the above-mentioned boundary conditions, plus pCO_2 and solar constant, to conduct 'best-guess' simulations, the purpose of which is to best capture the climatic conditions during the Early, Middle and Late Devonian (415, 380 and 360 Ma respectively). They focus their analysis on changes in surface air temperatures (SAT).

The authors demonstrate a weak impact of the paleogeographical evolution on globally-averaged SAT, although strong seasonal changes arise at the regional scale in response to the changing land-sea mask. Changes in vegetation cover, from bare land to coastal shrub and then trees, impact simulated mean annual SAT by no more than 0.2 °C. Over continents, the warming induced by the decrease in albedo is overwhelmed by the increase in evaporation and associated cooling effect. The main signal emerging from the sensitivity tests to the orbital configuration is the increase in global temperature with increasing obliquity and increasing eccentricity, whereas precession has a very minor impact. Regarding the analysis of the periodic flips between two climatic states over the North Pole, the authors suggest a mechanism linking sea-ice extent, ocean salinity and convective processes. The resulting climatic mechanism is associated with changes in global SAT on the order of 0.2 °C. Best-guess model runs feature a cooling trend throughout the Devonian, with mean annual globally-averaged SAT decreasing from 22.2 °C in the Early Devonian to 21.0 °C in the Middle Devonian and 19.3 °C in the Late Devonian. The authors explain that this trend is mainly due to the imposed drop in pCO_2 throughout the period (from 2000 ppmv to 1000 ppmv), while the impact of changes in vegetation cover can be neglected. These results contrast with a previous study that suggested that the climatic impact of decreasing pCO_2 throughout the Devonian may have been compensated by the concomitant decrease in albedo induced by the spread of land plants (Le Hir et al., 2011). Simulated tropical sea-surface temperatures are systematically lower than suggested by proxy data. Brugger et al. therefore suggest that the ocean $\delta^{18}O$ composition may have been lower in the Devonian, thus biasing temperature reconstructions.

Major comments:

In this contribution, the authors conduct numerous sensitivity tests. They cover several topics previously investigated by other research groups using other climate models, namely the sensitivity to the vegetation cover and to the orbital configuration (Le Hir et al., 2011; De Vleeschouwer et al., 2014). As stated by the authors in their introduction, this is important to determine the response of the Devonian climate system to different kinds of perturbations, in order to better understand the mechanisms potentially lying behind the coupled changes in the ocean-atmosphere system and in the biosphere at that time. Such systematic experiments should also permit to determine to what extent the results obtained in previous studies are model-dependent. The manuscript is concise and well-written and the experimental setup, in spite of the numerous experiments and sensitivity tests, is well described so that the study is easy to follow. This is appreciated.

However, the paper does not convey any clear, robust message. The authors cover a wide spectrum of scientific questions and provide numerous experiments, but this generally goes with a lack of robust analysis of the model output. For this reason and other weaknesses exposed in detail hereafter, I do not support publication of the manuscript in Climate of the Past. Below, I explain my motivations in rejecting this manuscript and I try to propose constructive comments.

1. On the choice of the numerical model. I do not like recommending using up-to-date, high-resolution models. Climate models of previous generations are useful because they generally provide results that are very close at first order to the ones obtained using more robust models while permitting numerous sensitivity tests due to their lower computational cost. This is obviously what motivated Brugger et al. to use Climber-3α. However, I am wondering if this ocean GCM coupled with a very coarse-resolution statistical-dynamical atmosphere model is really adapted to the kind of experiments the authors are conducting. Indeed, most of the signal analyzed by the authors is provided by the atmospheric component, be it in response to changing vegetation cover or orbital configuration. In that context, I would suggest adopting a relatively robust atmospheric model. I do not necessarily want to say that the authors should repeat their experiments with another model, but please, at least, clearly state the name of the model rather than hiding this behind a reference to Montoya et al. (2005) and include a discussion of the limitations. I also suggest clearly quantifying the bias associated with the interpolation onto the 22.5°x7.5° grid, see below.

2. On the weakness of the signal identified in the Arctic. The manuscript is generally well organized: the authors first investigate the impact of different forcing factors on simulated Devonian climate and subsequently combine these parameters to conduct their best-guess model runs. Section 3.4 'Flips between two climatic states in the Arctic' does not fit within this framework. It impedes the reading flow and, more critically, the amplitude of the SAT signal identified by the authors – ca. 0.2°C – is so weak that it would have no significant implications on the deep-time sedimentary record. I suggest deleting this part of the manuscript, which would at the same time allow the authors to expand on the other topics and thus to make their manuscript more robust in many aspects, see other major comments.

3. On the lack of sensitivity tests to the pCO_2. The rationale adopted by Brugger et al. more or less consists in decomposing the signal obtained in their best-guess model runs through individual sensitivity tests to the different parameters in play (continental configuration, vegetation cover…). In this context, it seems that a sensitivity test to the pCO_2 would be helpful. In particular, the authors state on page 18, lines 17-19 that "The global cooling over the Devonian seen in the set of best-guess simulations […] is dominantly driven by the strong decrease in CO_2 concentrations". This statement is not supported by the results provided in the current version of the manuscript. One could further imagine a series of best-guess experiments with pCO_2 held constant in order to clearly show that this constitutes the main driving factor of simulated climate cooling. The same could be done for the solar constant, the impact of which is not independently quantified. Also, the impact of changes in the orbital parameters is tested in Section 3.3 but it is not subsequently discussed. For instance, one could imagine showing the SST spread associated with different orbital configurations in Fig. 10.

4. On the need for a more robust analysis of the impact of the paleogeographical changes. In Section 3.1, the authors explain that changes in the land-sea mask induce strong seasonal SAT changes at the regional scale. What about the changes in land surface area and associated surface albedo? Please provide a table including key numbers such as land surface area for each timeslice. This is particularly important because results of the interpolation of the paleomaps shown in Fig. 2 onto the coarse grid of the atmospheric model are uncertain. Please check that the surface area on this grid is in first-order agreement with the original reconstructions provided by C. Scotese and plotted in Fig. 2.

5. On the need for a more robust analysis of the vegetation cover impact. The authors show that, in their simulations, the spread of Devonian land plants induces no critical changes in the mean annual, globally-averaged SAT. These results are interesting because they contrast with previous work (Le Hir et al., 2011), which suggested that the decrease in albedo may have been sufficient to counter-balance the drop in CO_2. I think that this may constitute the most interesting part of the manuscript. However, several points need to be discussed/tested before drawing any robust conclusions:

- Unknown vegetation cover. Maps of imposed vegetation cover are not provided and the surface area occupied by these plants in the different experiments (i.e., coastal shrub and trees) is not specified. In other words, the boundary conditions are unknown. The authors describe how they built their maps in Section 2.2.2, but this remains obscure. For instance, they explain that they "assume 80 % grass-like vegetation cover in areas lower than 500 m in altitude and closer than 500 km to the coast". Well, what does it give on the 22.5°x7.5° grid? What's the magnitude of the bias (in terms of surface area) imposed by the interpolation onto this grid?

- Lack of justification for the definition of the plant spatial cover. Also, the authors do not justify their choices regarding the imposed land plant cover: why are trees absent in continental interiors? For instance, Le Hir et al. (2011) used a vegetation model and trees are simulated over the South Pole. Similarly, why is vegetation absent beyond 1500 m in altitude? This discussion is of first-order importance, since the choices made here drive an essential part of the model response to changing vegetation cover throughout the period.

- Lack of justification for the definition of the plant expansion scenario. Closely related to my previous point, I see no robust justification for the scenario imposed by the authors in their best-guess model runs. Le Hir et al. (2011) conducted an interesting review of the literature to propose a scenario of Devonian land plant evolution in their models. They identified several steps in the colonization of the land surface by Devonian plants and selected appropriate plant functional types accordingly for each time slice. The same kind of approach is required to ensure robust modeling results. I am particularly surprised that the authors impose a bare land in their Early Devonian best-guess model configuration: it is recognized that non-vascular plants appeared as soon as the Mid Ordovician (Rubinstein et al., New Phytologist 2010) and that vascular plants were already present by the Late Ordovician (Steemans et al., Science 2009), with possibly strong implications on the Earth System at that time (Lenton et al., NatGeo 2012). Modeling studies further suggest that non-vascular plant cover may have been relatively widespread during the Ordovician (Porada et al., NatCom 2016; Lenton et al., PNAS 2016). I encourage the authors to explore the literature to build their experimental design more robustly.

- Poorly documented plant properties. The properties of the vegetation cover are poorly documented. What are the values of evapotranspiration and roughness associated with each plant functional type in use? Changes in evapotranspiration are a primary driver of the simulated SAT changes. Values have to be discussed.

- Lack of justification for the plant properties. Also, do the plant properties correctly represent Devonian plants? The authors should discuss this and use their numerical model to investigate the range of possible values, in order to determine if their results are robust. This is a crucial point.

Minor comments:

1. Title of the manuscript: I suggest replacing ‘insolation’ with ‘orbital configuration’ to make it clear that this does not refer to the changes in solar constant, which are also accounted for in the best-guess model runs.

2. Page 4, lines 20-21: Changes in solar constant are not systematically investigated.

3. Please avoid throwaway sentences, e.g. on page 9, lines 12-13: “Furthermore, shifts in the distribution of land and ocean areas as well as changes in ocean circulation likely contribute to the temperature difference”.

4. Throughout the manuscript: the authors refer to their idealized orbit (e.g., page 7, line 6) to name their ’23.5° obliquity, null eccentricity’ orbital configuration. The authors may refer to the “median orbit” used by De Vleeschouwer et al. (2014). Please also quickly justify this choice for the best-guess runs (minimal seasonality?).

5. Section 2.2.4 and Fig. 10: I do not get the age interval associated with each continental reconstruction. Why are the authors writing “410+-5Ma”? How is it defined? See also Fig. 9.

6. Page 7, line 22: “vegetation is spread globally”. Is that true? After Section 2.2.2, it seems that areas located farther than 1500 km to the coast and regions higher than 1500 m in altitude are not covered.

7. Page 20, lines 22-25: Given the large margins of uncertainty associated with the model boundary conditions and atmospheric model (see major comments), I am not sure that it is reasonable to draw this kind of conclusions.

8. Page 22, lines 5-8: “Best-guess simulations […] show a general climatic cooling trend in accordance with reconstructions showing decreasing levels of atmospheric CO_2 concentrations during the Devonian”. This is circular reasoning. The drop in CO_2 is a boundary condition of the model. On the contrary, the simulations do not capture the temperature trend reported based on proxy data throughout the Devonian (Fig. 10).

9. Table 1: Why are the authors not conducting their sensitivity test to vegetation cover on the 380 Ma configuration, like they did for the orbital parameters? Is there any reason for this?

10. Figure 1: It may be helpful to include stage names.

11. Figure 3: Continental outlines may be helpful. Also, the figure caption should clearly state how the difference is computed. I guess it is 415Ma–360Ma.

12. Figure 4: The figure is not easy to read. Would it be better using discrete color map levels and contours? Also, I suspect the increase in albedo in the NH shown in Fig. 4e (through sea-ice development obviously) to play an important role in the cooling shown in Fig. 4g. This may deserve a few words when discussing the patterns shown in Fig. 4g. Last point, the authors refer to the snow cover. Please show this on the maps.

13. Figure 9: Please state that the thick black line represents the coastline at the ocean resolution.

14. Figure 10: As it is, this figure is not very instructive. A way forward would be to investigate the SST changes simulated at the precise locations where the proxy data have been recovered. The simulated spread could be even larger when considering seasonal variations rather than (I guess, this is not stated) annual mean values.