

Nonlin. Processes Geophys. Discuss., referee comment RC2  
<https://doi.org/10.5194/npg-2022-2-RC2>, 2022  
© Author(s) 2022. This work is distributed under  
the Creative Commons Attribution 4.0 License.

## Comment on npg-2022-2

Marek Stastna (Referee)

---

Referee comment on "Climate bifurcations in a Schwarzschild equation model of the Arctic atmosphere" by Kolja L. Kypke et al., Nonlin. Processes Geophys. Discuss.,  
<https://doi.org/10.5194/npg-2022-2-RC2>, 2022

---

This manuscript presents a single column model of the Arctic atmosphere. The authors nondimensionalize this model and present a bifurcation analysis. They then draw fairly standard conclusions (Arctic change is nonlinear with possibly irreversible changes). The manuscript is well written, and the subject matter fits NPG well. The model is highly over simplified, and I would consider it unlikely the climate modelling community will pay it much attention. The mathematical analysis is fairly standard, and the numerical techniques used, in Matlab, should not trouble the majority of the readership. The model is built up in large part following Pierrehumbert's book, and is presented in a similar spirit. Conclusions are drawn on published climate system scenarios, and this is a very nice touch. I have detailed suggestions below, but all of these are consistent with a manuscript that deserves to be published once appropriate changes are made.

Where I am somewhat troubled is by the "tail wagging the dog" aspect of the entire exercise. The point here is to use standard ODE theory, and modelling assumptions are made to suit this. The authors are reasonably transparent about this in their comments throughout the paper. Where they could be clearer is in acknowledging what does not fit their assumptions; namely the Arctic Ocean. In essence, the fundamental problem I have with the model is that it is not a simplification of a "more complete" model. There is no way for someone to say, "Ok I like these results now I will include a more realistic representation of the zonally asymmetric ocean" (or some other aspect of the climate system that is near and dear to their heart). I don't think this would get me to suggest that the paper should not be published, but I think the authors could put some thought into this.

Detailed comments:

The introduction is one sided on box/column models. These models do have advantages, but some criticisms as well. Both points of view should be outlined.

The authors state that the Arctic atmosphere is, to a good approximation, zonally symmetric. The Arctic Ocean, however, is not. This needs to be clear.

The stratosphere is never mentioned. It is clear it is not part of the model, but it likely merits a short discussion at the very least.

The model is described in words; a diagram is essential.

The model appears to lack any moisture in the main troposphere. This merits discussion.

The strength of the model is the detailed exposition that explains it, and I want to add a positive comment as a note of appreciation for the authors' efforts.

The table of quantities, B2, is very useful. Adding references to it earlier in the main text would be even more useful. If there is room, adding a column for equations the quantity appears in would be helpful.

Overall the figures are very nice, but could be improved.

The figures would be much improved by using `plot(quantity1,quantity2,'linewidth',2)` and in some cases the "grid on" command in Matlab.

Panel labels, as opposed to titles are likely preferable, and if this is the case the captions should be expanded.

Figure 3 could use a legend for the bottom panel.

I would fill in the region between the curves in Figure 4 and increase the font size of the text in the figure.

So much good material is in the Appendices. Can the authors strongly push the readers to actually read the Appendices?