Nonlinear Processes in Geophysics Discussions

Nonlin. Processes Geophys. Discuss., author comment AC1
https://doi.org/10.5194/npg-2022-2-AC1, 2022


# Comment on npg-2022-2

Kolja L. Kypke et al.

Author 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-AC1, 2022

The authors thank the reviewers for their time and thoughtful comments on the manuscript.

We agree with the first reviewer that there are many details in the paper. They are included because we feel it important that the model be fully explained and so that the results could be replicated. We attempted to strike an appropriate balance between material in the text itself, and what should be put in the appendices. We feel strongly that the appendix material is not simply an addendum, but is essential to the paper, which concurs with the second reviewer's request that we promote reading of the appendices. The first reviewer expressed a desire that the appendices be more readable, but at the same time, the second reviewer commended the detailed explanation. We will therefore make a few more references within the paper itself, directing the reader to the appendices for further information, and we will edit the appendices to help enhance the clarity of the main points from each subsection therein. In particular we will edit Appendix B so that all of the parameter values will be presented in two tables rather than spread out over five.

The main concerns of the second reviewer were 1) that we were not sufficiently clear about the aspects of the Arctic climate that do not fit our assumptions, particularly the Arctic Ocean, and 2) that our model is not a simplification of a "more complete" model. The first concern is a valid criticism, and we will edit the paper to emphasize the zonal symmetry being assumed and how this symmetry is not present in the Arctic Ocean. We will also emphasize that the model is primarily an atmospheric model by editing the abstract to say "Arctic atmosphere" rather than "Arctic atmosphere-ocean system". However, we do not agree with the reviewer's second assertion. Zonal symmetry of the Earth's atmosphere is a reasonable and well-utilized approximation for a simple annually averaged model of climate. Our current model with a cylindrical atmosphere can be thought of as taking a limit as one restricts the Earth's atmosphere to a vanishingly small region centered at the North Pole. In this limit, the zonally averaged atmosphere becomes a one-dimensional column with downward flow and the PDEs governing the atmospheric fluid flow become ODEs. Alternatively, one can view the model as a meridional and zonal average over a cylinder centred at the North Pole. Although the authors had this view of the model from the beginning, and it is expressed in the sixth paragraph of the introduction, we recognize that the manuscript may not adequately convey this point of view, hence we will edit to emphasize how our model arises from this simplification/limiting process, particularly by adding a paragraph near the beginning of Section 2. Further, once the model is understood as representing a small region around the North Pole, the use of a single scalar to represent ocean heat transport becomes a reasonable approximation, regardless of the fact that the ocean is not zonally symmetric. For calibration of our model we used values of ocean and atmospheric heat transport (F_O and F_A) measured at 70N latitude. Although this clearly does not correspond to a small region around the North Pole, measured values of these heat transports are not readily available further north. Further, and partly to alleviate the concern around what values of F_O and F_A are used, we did a bifurcation analysis varying these two parameters, showing our conclusions are generic regardless of the precise values used (old Figure 5). We also used the solar insolation value, Q, for the portion of the Earth north of 70N latitude. This value came to 185 W/m^2. This value does not change much by restricting to a region closer to the pole; the limiting value is 173.8 W/m^2.

Below we respond to the detailed comments of the second reviewer.

1) In the past, 1D column/box models have been used to describe the globally-averaged climate of the Earth. These provide little detail and generally arise from the gross approximation of the entire atmosphere as a uniform slab or altitude-varying column. Here instead, our 1D model results from the assumption of a zonally symmetric atmosphere, making the polar axis invariant, and the limiting process as one considers a small region centered at the North Pole. Thus we believe our 1D model is relevant there. Furthermore, the Earth's climate is changing most rapidly in the high Arctic, so a polar model can be informative. To our knowledge, a 1D polar model has not been studied before.

2) It is true that the Arctic Ocean is not zonally symmetric, if one is considering the entire Arctic. However, if one is considering a small region around the North Pole, as explained above, this problem is minimized, and a single number can represent ocean heat transport.

3) The stratosphere is not part of the model, as was recognized by the reviewer. Since the air density is very small in the stratosphere there will be minimal absorption of radiation in the stratosphere, however the effect is not zero. As part of our modelling efforts, we did investigate a simplified stratosphere model attached to the present model, however the resulting quantitative changes to the radiation terms were considered not sufficiently large to warrant the additional complication of modelling the stratosphere. (Actually, the manuscript contained a notation in equations (A17) and (A24) that was a hold-over from our stratosphere modelling that did not get edited out; these equations refer to the downward longwave radiation at the troposphere being the constant I_-^{TP} and its non-dimensional version K_-. These will be removed and replaced with zero.)

4) The reviewer requested that we add a schematic figure of the model at its introduction. We will do so at the beginning of Section 2.

5) The reviewer indicates that the model lacks moisture in the main troposphere. This is a mis-reading by the reviewer. The entire atmosphere has moisture content governed by the Clausius-Clapeyron equation and a linear decay of the relative humidity with altitude. These things are discussed in sections 2.1.3 and A.3.1. The absorption of long-wave radiation due to moisture in the air is the third term in the expression for kappa given in Equation (8). Perhaps the reviewer's oversight was due to the fact that just the symbol kappa appears in Equation (11) and onward. We will add text to emphasize the role of water-vapor feedback contained in the factor kappa. This feedback is certainly essential to our model.

6) We thank the reviewer for the commendation; we put considerable effort into explaining the model sufficiently so that it could be replicated.

7) We will refer to Table B2 earlier in the text, and will add equation numbers to the table as requested. In addition, to aid readability, we will combine the values from (old) Tables B4, B6, and B7 into Table B2.

8) We will make adjustments to the figures, including wider lines, panel labels, expanded captions, and legends, as requested.

Other changes to be made to the manuscript:

We will replace the variable notation $M_{max}$ with $M_{tot}$ since it refers to a total amount, not a maximum. Replace several references to $F_{Amax}$ with $F_A^{tot}$, as they should have been. Replace constant $F_{A0}$ with $F_{A1}$ as it is more consistent with the fact that it is the value of $F_A$ at 1. Fix a few other minor typos.