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Nonlinear Processes in Geophysics
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Interactive comment on "Anthropocene Climate Bifurcation" *by* Kolja Leon Kypke et al.

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Review of Anthropocene climate bifurcation, by Kypke et al.

This article provides an improvement on the classical energy balance model (EBM) provided by the prognostic equations (1) and (2). These prognostic equations are the same as those nowadays found in textbooks, and based on the pioneering works of Budyko and Sellers. It is applied at a regional scale, hence a F_o term accounts for the net supply of heat by ocean transport. Compared to the textbook formulations, the diagnostic equations are more elaborate. These are equation (6) to (10), and include specific parameterisations for sensible and latent heat transport, with details given in the lead author master's thesis. The albedo feedback given by equation (8) follows the fairly standard hyperbolic tangent formulation, and it is here presented as a consequence of the dynamics of sea ice.

Expectedly, the model presents bifurcations caused by the albedo dependence on temperature, as already studied by Budyko and Sellers, North, Ghil, and others. The claim of the authors is that the diagnostic equations have been given more attention in the present contribution, and are more accurate than in previous works, such that their model can be used for actual predictions of climate in the 21st century and beyond (most plots extend to 2300).

Specifically, the authors present previous EBMs as "lacking in the geophysical details and mathematical rigour required to make useful predictions". They go on: "This paper presents an EBM built upon basic laws of geophysics" and "it provides new mathematical evidence signifying that catastrophic climate change in polar regions is inevitable in the coming decades and centuries if current anthropogenic forcing continues unabated". This conclusion follows from a bifurcation analysis of the model, and the identification of co-dimension 2 (cusp) bifurcations in the parameter space spanned by the CO2 concentration and the net heat flux penetrating the domain studied.

Overall, I support the publication of this contribution. However, I believe that the positioning of this study with respect to the state-of-the-art is arguable. On the one hand, the claim that earlier studies have been "lacking mathematical rigour" seems somewhat excessive (the recent review of Ghil and Lucarini, 2019, arXiv 1910.00583, is a nice entry point, which shows how much mathematics has already gone in previous works). I would also not say that they did not provide "useful" predictions. On the other hand, they present general circulation models (GCM) as "too stable" to provide reliable warning on the sudden catastrophic events, citing Valdes (2011). Indeed, low-dimensional models tend to have a more clear-cut bifurcation structure than high-dimensional models such as GCMs, but there are also good reasons for the smoother character of certain transitions in the GCMs (spatial patterns, partial capture of scaling laws, effects of turbulence). In particular, a number of simulations with so-called earth models of intermediate complexity have been run well into the future and they do not necessarily present such "catastrophic" transitions; it would be legitimate to ask why they didn't so, while they certainly include more "geophysical detail" than the current study. The interest of an EBM bifurcation analysis is not so much to provide an accurate prediction that would supersede the current state-of-the-art. It rather lies in sensitivity analysis and examination of the conditions that would generate a bifurcation (see, in particular, my comments below regarding line 109).

I would also like to make a few comments about the semantics.

There is an important distinction to be made between the existence of a bifurcation, and the potentially abrupt character of a transition (l. 50). The bifurcation, at least the way it is presented here, tells us about the topology of the attractor, which is a measure of the invariant manifold. The "abruptness" relates to the dynamics of the transient changes that occur when the system moves from one fixed point to the other. The existence of a bifurcation does not imply abruptness. Think of the melting of large ice sheets (incidentally, lacking in the present model). One way to address this ambiguity is to consider the dynamics of non-autonomous systems, as done for example with "mathematical rigour" by Ashwin et al. (2012, doi: 10.1098/rsta.2011.0306).

Furthermore, I would advise caution with the arguments that using "basic laws of geophysics" generates more accuracy (ll. 30-35). No climate model is computed "ab initio". Any model requires parameterisations which always include an empirical component, and the very simple EBM presented here is certainly no exception. Furthermore, the dynamics of climate are also related to biology and therefore involves knowledge and arguments that go beyond geophysics.

With these reservations expressed, I would like to reiterate that I am overall supportive to the publication of this paper, and we now proceed with the line-by-line comments.

1. l. 109 - The difference between α_c and α_w is quite large, and it seems pretty clear that the bifurcation depends on the amplitude of this difference, and of the slope of $\alpha(T)$ curve (as can be seen with a Lamerey diagram, in the way done by, e.g.,

Brovkin et al., 10.1029/1998JD200006). In the real world, the spatial distribution and seasonal cycles of snow and ice are likely to effectively smooth the albedo dependency expressed by equation (8).

2. l. 174 - β_1 and β_2: aren't these ζ_1 and ζ_2 ?

3. l. 176 - Start with a section 3.1.

4. l. 210 - Given that the definition of albedo function is so important for the existence (and significance) of bifurcations, it is necessary to be very explicit about the latitudes covered.

5. Figure 6 - Again, the arrow sketched on figure b should not be interpreted as if the transition was instantaneous. More generally, what are drawn here are steady states, while actual trajectories depart from the steady state.

6. Section 3.2.2 - Whether the heat flux will increase with global warming is arguable. For example, if Greenland melts, the thermohaline circulation may reduce in intensity, and so would the supply of heat to the high latitudes. If sea ice melts, the thermal contrast between the Atlantic and Arctic will also change, and the consequences on ocean and atmospheric transport are not necessarily trivial.

7. l. 304 - "the EBM predicts that CO2 mitigation strategies, if introduced soon enough, may avert the drastic consequences of this bifurcation." We have to be careful about writing such sentences. This paper is not about mitigation strategies. We are not discussing what would be the effect of policies on CO2 emissions and CO2 concentrations. So the EBM predicts nothing about mitigation strategies. It only predicts that conservative RCP scenarios avoid the bifurcation.

8. l. 319 - It might be good to write somewhere explicitly that land ice masses are considered as constant in this study (I might have missed this, though)

9. l. 382 : the IPCC is “reporting” (results of published work), not “predicting”.

10. l. 398 - It is correct that for 4.55° C is a reasonable number, though on the high range, but the last part of the argument seems a little bit overstretched. Some GCMs have a low climate sensitivity, too, and yet they rely on what the authors call “geophysics”.

11. l. 400 : “based on geophysics rather than statistical data” : I would write “based on physical rather than statistical modelling” (though, to be fair, several assumptions included in the physical model are based on statistical modelling or regressions, but we understand what is being said here).

12. l. 409 - “the analysis of this paper presents a mathematical proof that a bifurcation can occur in an EBM” : this is correct but again I would do justice to other authors who already presented bifurcation analysis in EBMs.

13. l.423 - The author nicely present their plans for the next years with the project to develop a 3D model. What is the inteded added value compared to existing initiatives like PLASIM ?

14. Code availability: Especially given the stance on open source in the conclusion paragraph, I would strongly encourage the authors to provide the code necessary to reproduce the main results, either in the form of a version-controlled repository (e.g.: gitlab), or an doi-ed archive (e.g. zenodo).

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