

Interactive comment on “Oscillations in a simple climate–vegetation model” by J. Rombouts and M. Ghil

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J. Rombouts and M. Ghil offer a well-written article presenting a dynamical system model for climate–vegetation interactions. The model has a two-dimensional phase space and the authors provide a detailed analysis of the bifurcation diagram. They identify fixed points as well as the conditions for the emergence of a limit cycle via a Hopf bifurcation. The limit cycle is discussed by reference to other low-order dynamical systems and its relevance in the context of climate variability on long time scales is considered.

The article is nicely self-contained and the introduction is remarkable by its relevance and concision. The mathematical analysis seems faultless and the narrative is clear, so that the conclusions about the model itself appear indisputable. The only point left

C53

to criticism concerns then the implications of the model output to our understanding of climate dynamics, and how these results fit or may fit the modelling hierarchy to yield a consistent theoretical framework.

From this point I view I can see two points, which deserve discussion.

First, we need to think of the representativeness of equation (2). Taking T as a global average of the temperature, it is not entirely clear what the relation $A^*(T)$ ($A^*(T)$ being the nullcline) should be. Figure 4 suggests a transition from bare-world to vegetated world over about 5 degrees. This induces an absolute change in the land albedo of the order of 0.20 at least, resulting in a net shortwave forcing of the order of $163 \times 0.3 \times 0.25 = 12 \text{ W/m}^2$ (163 W/m^2 is about the SW radiation absorbed by Earth's surface). This is one order of magnitude larger than current estimates of the vegetation albedo forcing associated with the difference between the Last Glacial Maximum and the present-day (Crucifix and Hewitt, Climate Dynamics, 2005).

On the other hand, some Earth models of intermediate complexity do indeed show multiple equilibria but they are mainly of local relevance (e.g. in Western Africa); the co-existence of multiple equilibria in the northern latitudes has so far not found any support from such models (see, e.g. Brovkin et al., 2003). Finally, when such models show global oscillations, they are traced back to ocean circulation cycles. This of course would not exclude a role of vegetation as contributing to the precise shape of such oscillations, vegetation could even be one of the elements that determine the dynamical regime of the whole system, but we currently have no hint that vegetation effects would be such a dominant factor as is being suggested here.

So, with these elements in mind, a much stronger case of the applicability of the model results need to be made. I believe this cannot be done globally, but the model equations may have some potential for explaining local phenomena.

The other comments are anecdotal:

C54

p. 147 l. 9 : Otterman et al. 1984, "Effects Of Nontropical Forest Cover On Climate", Journal of Climate Appl. Meteorol, 23, 762–767 would be a classical reference for high-latitude vegetation feedback

p. 149 l. 8 : Renssen et al. 2003 : it is correct that their model show large oscillations around 6,000 years ago that are reminiscent of stochastic oscillations between multiple equilibria. However, at least my reading of that paper is that there is no evidence for multiple stable equilibria in that model.

p. 161 l.2 : missing 'r' in pattern

p. 164 l. 13: substitute ',' for '.' before 'and we have'

Interactive comment on Nonlin. Processes Geophys. Discuss., 2, 145, 2015.