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Nonlinear Processes in Geophysics
Discussions
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Interactive comment on "Oscillations in a simple climate–vegetation model" *by* J. Rombouts and M. Ghil

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REFEREE: 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

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.

AUTHORS: We thank this referee for the remarkably positive evaluation of our paper and for the specific comments below. These are now addressed forthwith.

REFEREE: 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*0.3*0.25 = 12 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).

AUTHORS: Similar concerns were expressed in the early days of energy balance models (EBMs; see Ghil, 2001, and references therein), where even larger and more unrealistic albedo differences between low- and high-temperature surfaces were used in simple, albeit infinite-dimensional models. Still, the EBMs' suggestion of multiple equilibria being possible in the climate system on long time scales has led to a rich literature on bifurcations — more recently and excitingly called "tipping points" — and their potential role in both past and future climate evolution (Lenton et al., 2008).

These concerns overlap with some of those of **Referee 1** and have led us to add a subsection to the paper's final section.

The paper's final section is now entitled **"4 Concluding remarks"** and it has the following three subsections: **4.1 Summary**, **4.2 Discussion of the results**, and **4.3 Broader**

context. The latter had to be kept reasonably brief, given the overall satisfaction of both referees with the writing and especially the conciseness of the paper. **Section 4.3** therefore mainly refers to reviews and other papers that cover the topics at greater length; It is attached to this reply as a supplement.

REFEREE: 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.

AUTHORS: This paper is only trying to make a case for the possibility of vegetation playing a more important role than contemplated heretofore and does not claim in the least to have definitively proven that this is so. A similar argument about local vs. global effects has been made with respect to the oceans' thermohaline circulation. Recall that the Stommel (1961) paper — much quoted recently in the context of multiple equilibria and symmetry breaking in the meridional overturning of the Atlantic or even global ocean — was originally written to explain seasonal changes in the overturning of "large semienclosed seas (e.g. Mediterranean and Red Seas)"; see, for instance, Dijkstra Ghil (2005).

Again, we outline these considerations in **Section 4.3** in supplement , along with the corresponding additional references. References already included in the original version of the paper and cited in the replies here are not listed in full.

REFEREE: 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:

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.

AUTHORS: Earlier observational work of Joseph Otterman in semi-arid areas had, of course, a seminal role in motivating Jule Charney's modeling work on the biogeophysical feedback. Thank you for bringing this high-latitude work of his to our attention. Both Otterman references have been added, as shown below.

This vegetation-albedo feedback appears to be important in semi-arid regions (Otterman, 1974), where it interacts with the hydrological cycle. J. G. Charney and colleagues (Charney, 1975; Charney et al., 1975) were the first to include in a model this biogeophysical feedback, as he called it; many others have followed since (Claussen et al., 1999; Zeng et al., 1999; Zeng and Neelin, 2000). The vegetation-albedo feedback also matters in certain high-latitude regions (Otterman et al., 1984), where boreal forests mask snow in winter, causing an effective warming of the surface (Brovkin et al., 2003; Bonan, 2008)."

[Additions or modifications to the text of the original version are in red.]

REFEREE: 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.

AUTHORS: It is true that, in more complex models, it is often difficult to clearly identify actual fixed points or limit cycles, as opposed to much fuzzier versions thereof. The paragraph in question has been modified accordingly.

Svirezhev and von Bloh (1996, 1997) introduced another set of simple, spatially zero-

dimensional (0-D) models for vegetation-climate interactions. These highly simplified models include an ODE for temperature evolution, absent from Daisyworld, but look at only one type of vegetation, whereas Daisyworld has two. In their two-ODE model, Svirezhev and von Bloh (1996) find multiple steady states. Such bistability seems to occur across a hierarchy of climate–vegetation models, from the simplest (Dekker et al., 2007; Janssen et al., 2008; Aleina et al., 2013) to more complex (Brovkin et al., 1998; Claussen, 1998; Irizarry-Ortiz et al., 2003; Renssen et al., 2003) ones, although it is, of course, harder to ascertain in the latter. In models across the hierarchy, vegetation and temperature are often coupled with precipitation, which provides an additional feedback mechanism."

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

AUTHORS: Corrected, thank you.

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

AUTHORS: Corrected, thank you.

Please also note the supplement to this comment:
http://www.nonlin-processes-geophys-discuss.net/2/C95/2015/npgd-2-C95-2015-supplement.pdf

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