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Interactive comment on "Local ecosystem feedbacks and critical transitions in the climate" *by* M. Rietkerk et al.

Anonymous Referee #1

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Review of Rietkerk et al. "Local ecosystem feedbacks and critical transitions in the climate"

This is a paper that I have deep sympathy with. I fundamentally agree with its conclusions that we need a hierarchy of models, coupled or nested to explore the role of land-atmospheric feedbacks. I think the authors are right – but I do not think the authors actually provide a coherent or consistent argument that they are right. Simply, I do not think they have made their case beyond a suite of pieces of evidence where in other papers (commonly authors of this paper) people have asserted that local scale processes must be important.

My job as a reviewer is to try to pull this paper apart. Are the conclusions robust (for example). To play devil's advocate in an attempt to be rigorous in my review I am going to assert that there is no evidence that withstands scrutiny that local scale processes affect climate and then see if any arguments the authors make prove by assertion wrong.

I reiterate – I *agree* with the authors that this is an important area of research but I also assert that the authors have produced a paper that the believers will believe in and will not convince anyone else of their case. It is therefore a wasted opportunity and this version of the manuscript should be rejected.

My comments follow the structure of the paper.

Introduction

Claussen, 1997 is based on a highly simplified modeling approach. Lenton, 2008 is largely a synthesis and provides little evidence for tipping points in the land system that cannot be argued to be model dependent. I do not think these paper form a strong defense for the statements in the introduction.

I would welcome strong evidence that local ecosystem feedbacks actually affect the energy balance, hydrological balance and nutrient cycles to a scale that might act as a signal against the noise of natural variability in a climate model. Most of the time, the energy balance and water balance in the climate models do not trigger a feedback of measurable scale in a climate model. There is a lot of evidence that how the land is parameterized matters up to the complexity of something like a 3rd generation land surface scheme. Beyond this there is little additional benefit. A 3rd generation scheme gets the signal to first order, it is the first order forcing that drives the atmospheric response, and second or third order forcing is within the noise of the atmospheric variability.

Of course, you may not agree, but the assertion that forms lines 25-27 on page 10123 needs rigorous defense as at present you do not provide evidence to support this common assertion.

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Interactive Comment

I do not think it plausible that local ecosystem feedbacks is “an essential step” to better understand and predict global climate change. Are you suggesting the 4th Assessment Report of the IPCC is wrong ? I would accept – contentedly – that “ local ecosystem feedbacks are an essential step to better understand and predict the local consequences of global climate change”. I might accept that “ local ecosystem feedbacks are an essential step to better understand and predict the regional consequences of global climate change in some regions”. But suggesting that global climate sensitivity or the impact of A1FI is dependent on local-scale ecosystem feedbacks is indefensible. It is – sadly for a terrestrial modeler – the oceans clouds and the cryosphere that are the major driver of global climate sensitivity not local ecosystem services. I know there is a link between the land and clouds and even a link to the cryosphere but these are secondary. So prove me wrong – prove the land is the key driver or change the text to claim a role for the land that is defensible.

Section 2

I fully agree with lines 8-10 on page 10124 – this does state the issue properly. Line 13 suggests impacts on circulation patterns. I think LUCID is the only study to do a proper intercomparison and they did not find impacts across 7 models on the circulation. It is pretty clear that the perturbations found in earlier studies are likely noise that older statistical tests failed to screen. Given LUCID is the most recent statement, show me why LUCID is wrong and single model studies are right or modify the text.

I agree that terrestrial processes are an important amplifier on palaeo timescales. I take no issue with most of Section 2 therefore. But these are commonly multi-century time scales (I know not all are, but we are looking for things that drive a global response according to your text). These palaeo studies do not support your hypothesis on timescales of < 100 years and therefore do not really make a case for significance to climate simulations of this century.

I think you begin to strengthen your case lines 21-30 on page 10125 but you really

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should look at how uncertain the Koster et al results were. They provide an interesting research strategy but do not provide conclusive evidence of anything.

Page 10126 line 3 – discussion of global warming causing shifts. On what time scale and on what spatial scale ? It has to be quite fast (say decadal) and large scale to likely affect climate.

I thought the evidence from observations regarding he Amazon was that it was likely ultra-stable in reality (I can make it die in my climate model of course)

Section 3

I take no issue at all that these processes are not important at local scales. To affect the larger scales, or to affect how a large-scale climate change would affect a region, the feedbacks discussed in this section would have to be organized. They would all have to point in the same direction, over a landscape and be a large feedback relative to the existing energy, water and carbon balance. Affecting the energy balance a little, or affecting the carbon balance a little (and by "little" I mean relative to the magnitude of the existing fluxes) would not likely impact the climate at the larger scale where water and heat was largely driven via ocean-atmosphere coupling.

Section 4

I do not see that you have presented strong evidence that "local feedbacks could lead to critical transitions between alterative regimes". I would like you to – not just for this paper but because I would love the ammunition for my own research funding requests. I would *like this to be true* but you cannot state it without providing rigorous evidence. I might believe that process X is the dominant driver of phenomenon Y. But I have to provide strong evidence of this, not merely the idea.

The Dekker e al (2007) paper is a nice piece of work. It used an approach that pretty well forced there to be a rainfall response. I take no issue with this paper – but I do take issue in its use in Section 4 to argue that a rainfall response is likely.

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Interactive Comment

I now want to draw explicit attention to my basic problem with this paper. I choose this example because its the best one – but it is not the only one. Lines 25-27 on page 10127 through to line 12 on page 10128.

You state [and I have used * to emphasize specific words]:

The exploration of positive feedbacks on continental-regional and local scales, *suggests* that local feedbacks *could* lead to critical transitions between alternative regimes at larger scales. Interestingly, literature reveals that those feedbacks markedly influence each other and *may be* intimately linked (Scheffer et al., 2005; Janssen et al., 2008; Dekker et al., 2007). For example, Dekker et al. (2007) show how local vegetation-hydrology feedback *could* impact regional-continental evapotranspiration precipitation feedbacks, increasing precipitation (Dekker et al., 2007). As a consequence, and strikingly, their model predicts the Sahel-Sahara boundary to be situated hundreds of kilometres more northward as compared to models not accounting for this link. The local vegetation-hydrology feedback *may then* affect the large-scale albedo moisture circulation feedback, boosting hysteresis in the climate system (Janssen et al., 2008). Thus, local positive feedback *could* propagate to regional-continental scale through cross-scale links (Fig. 1), *possibly* leading to critical transitions in the large scale climate.

There are many “may be’s” “could” and “might” in that paragraph and it ends – sensationally with: “These are clear examples of missing cross-scale links in global and regional climate models.”

Seriously – this is not a reasonable pitch in a scientific paper. And for the record, Janssen et al. (2008) – another nice paper – says nothing about the sorts of climate systems (that is large scale climate, as stated in the following words) that you are talking about here.

But it gets worse. The next paragraph states:

Research so far leaves *no doubt* that the omission of cross-scale links between local ecosystem feedbacks and large-scale land-atmosphere feedbacks in global and regional climate models implies a *major* impediment for our ability to understand critical transitions between regimes in the large-scale climate.

Really ? "No doubt". How is that consistent with the literature that shows non-land based processes are the main drivers, the evidence that the models seem to work well (see Chapter 8, AR4) and all the "could" and "maybe" you wrote into the associated paragraph. You are – simply – believing something is important and arguing for it. You are not presenting a balanced case.

Section 5

There are a suite of issues with this paragraph. However, since you have not presented a case for the *need* I have not provided a commentary on the solution.

In summary, while I may agree with some of the content of this paper I simply cannot recommend publication. This is exactly the sort of paper that has undermined land surface research becoming core to global and regional climate research. It is why AR4 lacked detail on some aspects like land cover – because we *asserted* our science was vital to a community with high standards of proof, rigor etc. rather than demonstrating that it was important.

Interactive comment on Biogeosciences Discuss., 6, 10121, 2009.