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Biogeosciences Discussions

Interactive comment on "Spatial scale dependency of the modelled climatic response to deforestation" *by* P. Longobardi et al.

Anonymous Referee #1

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This is an interesting paper that ultimately has merit and may well be published. However, I have a range of concerns around the reliability of the results that has led me to recommend that this version of the manuscript should be rejected. The following are in no particular order.

1. There is a lot of much newer literature relevent to this paper that is not cited. De Noblet et al. (2012) in J. Climate, Boisier et al. (2012) in JGR both point to major discrepencies in how land cover change is simulated. The current paper is a single climate model with some very worrying attributes and at no time do the authors place the results from this paper in the broader context of what they all mean. This contrasts with de Noblet and wiht Boisier who struggle to explain what we do and do not know in the context of model dependencies. I *know* you cannot do this in a paper based on

one model, but you can talk about who reliable your results are in the context of how uncertain simulating teh impacts of land cover change is.

2. Further in that light, I do not trust a model response when clouds are fixed. I do not think you can examine remote changes in this context. I note changes in rainfall and evaporation but clouds are presecribed. I know you note this severaltimes and I know you cannot change it, but htat does not make it ok. Specifically, I do not know how to judge how reliable your results are and therefore I do not know what to make of them. Given you are perturbing the surface energy balance and the tranpiration BUT NOT changing incoming solar associated with any cloud feedback your results do not include the dominant feedback that couples the land feedbacks with the atmopshere. I therefore find it hard to trust the results.

3. There is a considerable amount of evidence that TRIFFID is over sensitive to perturbations.

Further, the was TRIFFID models the respise of soil carbon to temperature and moisture is - at best - anomalous. I would be extremely nervous about the reliability of your results linked to soil carbon. You could use different soil respiration methods or you could carefully present a discussion of how reliable you think these results might be.

4. Page 14641, lines 10-13 - this statement ignores natural reforestation. line 17 - a remarkable assumption - no increase in productivity lines 20-30 is highly selective. What about Findell's papers which do land cover cahnge well and find no remote responses. Or Pitman et al. (2009) using 5 models and no remote response. Are the statistical methods reliable in the papers you cite [no - probably not] line 28 - you cannot talk about CO2 fertiliation in a model that does not also do nitrogen.

5. Page 14642 - should reference Bonan's science paper on forests - 2010 I think - line 15 - statement linking higher CO2 wiht lower LHF makes no sense - line 23 - several authors have now done proper deforestation patterns - see recent literature ... Feddema for e.g. VERY few doing this properly do banding.

6. Page 14646 - I understand your text around use of EMICs but this DOES NOT make the results reliable. I just do not accept your statement that computational and time constraints make this legitimate. You could have done this with a coupled GCM with a mixed layer ocean model. The fact it was convenient to use UVIC is understandable, but this does not necessarily make the results publishable in a top journal.

7. Page 14646 - Section 4.1. This is all artifacts of how TRIFFID models the system. I do not use TRIFFID so I really do not care about this model per se. I *do* care when you use methods to tease out interesting processes that inform me about the system since these are likely model independent. So, Section 4.1 is importnat to you but not to the mode general reader I think.

8. There are a suite of statements that are phrased such that they make little sense. I will not have caught all of these. However:

Page 14648. line 21-23 makes no sense - evaporation is not driven by soil temperature. Soil temperature responses to how net radiation changes, and how it is partitioned btw sensible and latent heat.

Page 14659 - line 3-5 does not make sense. You need to carefully read Boisier et al to see how these things link together from a surface energy balance perspective. Page 14661 - line 9-11 is confused.

Page 14661 line 22 ... Cd changes the total turbulent flux - it need not decrease SH. I do not believe your increase in NPP in deforested landscapes. I believe - of course - your model shows this but it does not make sense. Rather than try to explain it, perhaps look into how its parameterized and see if you can tease out where the problems are. I note you comment that land cover change needs to be implemented properly [I agree]. Page 14665 line 11-13 is clear about this. And yet you instantaneously change land cover. This is somewhat contradictory.

9. Many of your results would not be statistically significant - aand you do not test for

significance. You need to use a Findell like approach with autocorelation accounted for. I suspect a lot of your results will turn out to be model variabliity. That which is not forms the basis for a more indepth discussion.

Without proper statistical analysys, you are interpreting some results that are noise - and I would reject this paper on that basis alone.

10. Figure 6 cannot be kg/m2/s ... or at least the time series cannot be. There is something odd here too. The map which is in kg/m2/s has max values of 1E-5 or 0.8 mm/d. Your time series go to double this magnitude. Might be a quyestion of timing but I was not sure.

11. In all figures, mask non significant data and data very close to 0.0. Your use of a continuous scale makes this hard to read.

Overall, I do think there is merit contained in this paper and I suspect you will utimately get it published. However, for me, there is a lot of work you need to do beforehand and in my view the resulting paper will be quite different to the existing draft and constitute a new and different paper. I have therefore recommended rejection but I would also encourage you to do the work to resubmit this in due course.

Interactive comment on Biogeosciences Discuss., 9, 14639, 2012.