

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

**Anonymous Referee #2**

Received and published: 13 November 2012

While the paper is interesting and attempts to tackle important issues regarding deforestation pressures and its effects at different spatial scales, the paper does have a number of significant flaws and requires major revisions. At this time I recommend that the paper be rejected.

I have a number of concerns regarding how the results are placed into context of other literature, a lack of an estimate of uncertainty for the results presented, an apparent lack of consideration of changes in other land covers in explaining the drivers of effects seen and insufficient consideration of the limitations of the model used. Points below are not in any order of severity.

1) The discussion does not fully explain the mechanisms that drive the changes seen in the model. For example, increases in soil carbon is mentioned several times as

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a consequence of deforestation. The paper suggests that a partial explanation for increased soil carbon sequestration is cooler temperatures leading to lower rates of respiration. This is also slightly contradictory as soil temperatures are in some locations predicted to increase with deforestation. However, more significantly changes of the inputs into the system do not appear to be fully considered. NPP should be considered too, given that a large change in NPP will have occurred as a result of the deforestation.

2) The increase in soil carbon is often mentioned while the total system carbon stocks are less discussed, which seems odd as in nearly all cases this seems to lead to a net reduction in carbon stored in the land overtime. Increased soil carbon only offsets total land carbon loss in the high latitudes, where the soils are organic rich soils, I suspect your soil model will not have a realistic organic representation. Page 21, line 25-29 may have been better spent highlighting how this is contradictory to field studies of deforestation and also to highlight this important limitation of many models.

3) Only a few brief mentions of field studies into forest conversion to agriculture are made (e.g. Diochon et al 2009 and Poeplau et al 2011) were at high latitudes you dismiss their results as not being comparable to the model as they operate at different spatial scales, but do use the papers to support results at mid and low latitudes. It would better to explain the model observation conflict by considering the ecological or management differences of what you model as a cropland versus a real cropland e.g. harvest or ploughing.

4) Furthermore to insufficient consideration of the model limitations and consideration of ecological and management differences. No consideration is made for implications of modelling croplands as grass. While this was the standard approach for a long time, an increasing number of publications that have added crop development models to LSMs have highlighted significant differences in energy partitioning (e.g. Van den Hoof et al., 2011 or Levis et al., 2012), albedo, NEE, seasonality and associated feedbacks such as precipitation. A key difference between grasses and crops is the harvest of above ground biomass which will significantly restrict carbon inputs into the soil via

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litter. I realise it is probably not possible at this time for you to change the soil model within the LSM, but full consideration of model strengths, weaknesses and limitation is needed in general.

5) Cloud feedbacks on radiative transmission through the atmosphere. Without this I struggle to believe any of the specific predictions, you end up highlighting that the system presents differing and complex interactions as a result of deforestation at different spatial distributions. However I don't believe that you can draw any specific ecological or environmental conclusions.

6) The paper, on more than one occasion, attributes changes at the local and global temperature changes as determined by the initial deforestation (e.g. p20 120-24). The fraction of the initial values 15-75% tend to converge by 2100, while their temperature differences persist throughout the simulation. Given that the forested components have, in cover terms at least, converged it would be worth considering what has happened with the other land cover types. I am under the impression that TRIFFID is still allowing competition to go on, so has there been a significant shift in grassland or shrubs, in either absolute amounts or spatial distribution. If there are no significant differences between the different simulations, at least point this out.

7) Further, there is no estimate of uncertainty on any of the plots. Given that the differences between simulations is often very small e.g. Fig 4, I would expect that at least some of the simulations are not really difference from each other. This needs to be addressed to determine which results actually need to be interpreted and which are variability of the model.

8) I'm confused as to why the paper sets itself up as investigating agriculture as an important driver of deforestation, the expansion of agricultural land, only to then seem to mostly forget about this aspect. In particular my earlier point regarding the issues surrounding modelling cropland as grasses.

9) Page 23, Soil temperature increase is attributed to a decrease in sensible heat

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flux as a result of an decrease in roughness length affecting surface boundary layer conductance of heat, despite an increase in latent heat and albedo. Latent heat is also affected by conductance. You need to explain or suggest a possible explanation how changes in energy partitioning as led to a reduction in sensible heat in favour of an increase in latent. What has happened to the vapour pressure deficit, impacting the atmospheric demand for water vapour? How much water is available in the soil, is there a reduction in soil resistance? Particularly as you often say that the system is drying.

This is not intended as a comprehensive list of comments regarding the paper, but does cover significant issues. The paper does deal with an important issue and presents some interesting results. However it does require major revisions and I recommend it be rejected at this time.

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

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