Apparently, both reviewers struggled with this manuscript, because the format was confusing. This manuscript is not a classic format of a review, although at one point we erroneously mentioned “review” in the text. It is written in a format of a short perspective/opinion paper, resulting from a workshop where all authors discussed this issue. Therefore, the manuscript does not provide new sound scientific conclusions based on new results, and reflects the opinion of the authors. It provides a rationale towards a hypothesis to be further investigated, and a plea for incorporating local ecosystem feedbacks in global and regional climate models, based on recent scientific literature. Prior to submission, we asked one of the Chief Editors whether such format would be all right for this journal, and the response was positive, after a brief check between Chief Editors.

So, we have the impression that the format and style of our manuscript has put the referees on the wrong foot, and that this led to some misunderstanding, and we sincerely apologize for that. Despite our own shortcoming on this matter, we also feel that this stems partly from the fact that *Biogeosciences* has no special formats or policies for perspective/opinion papers (but see above where we mentioned the positive response after our preliminary enquiry). If our paper would become acceptable for *Biogeosciences*, we agree with referee #2 that it should be identified in the journal as perspective/opinion paper. Our revision should be framed in this light, and we hope that, together with other modifications and improvements, including caveats and more careful formulations, this takes away the main problems that the referees (especially #1) had with this manuscript. We agree with many points put forward by the referees concerning more balanced statements, uncertainties and careful formulation. We think our revision removed the misunderstanding and improved the paper and we want to thank the referees for their critical reviews. We hope our paper will be published in *Biogeosciences*, because this is the audience that we seek to address.

Here below we reiterate the referee’s comments in italics, and we respond to the comments and how we deal with them in our revision.

Anonymous Referee #1

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.*

Our intention with this manuscript should now become clear from text modifications in the abstract and introduction of this perspective/opinion paper and from our remarks above. We think our revised version of the manuscript makes a more balanced and strengthened case, aiming at putting the issue more prominently on the research agenda. Our main purpose is to highlight possible important feedbacks from local-scale processes to the climate system, while it is true that conclusive evidence for such resulting impacts still needs to be gathered. Indeed our paper is advocating for more research, which could provide such evidence, or else invalidate this hypothesis.

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.

The paper of Clausen (Claussen, 1997) is indeed based on a simplified approach: coupling an AGCM with a diagnostic biome model. But it was the first paper showing the complete Charney cycle including (albeit asynchronous) biogeophysical interaction in North Africa. Moreover, this paper led to several others which corroborated the idea of a strong feedback leading to multiple equilibrium states. The occurrence of multiple states and, hence, the potential for 'tipping', is indeed model dependent. There are several models that show it, others don't – but that's interesting science. Perhaps the problem lies in the word *local* (here used by the referee) ecosystem feedback. In fact, in the introduction where we refer to this literature, we do not talk about *local* feedback, as assumed by the referee, but continental to regional-scale feedback. In the paper of Clausen (Claussen, 1997) the entire Sahara was involved. And changing entire continents and large regions do yield significant impacts in GCM's. We provided an additional reference for tipping elements in the Earth's climate system (Dakos et al., 2008), implying empirical evidence for the idea that past abrupt shifts in the climate were associated with tipping points, strengthening our case.

Also, in the introduction we now stress two general examples with five specific references of how the energy balance, hydrological and nutrient cycles could be connected from local to large scales through atmospheric processes. We come back to these examples more specifically later in the manuscript. We think that we clarified our intention with these common assertions that the referee refers to. That point being made, we do not provide any new measured evidence or scientific conclusions; our intention is to put the issue on the research agenda by building up the argument, and rephrased it as such.

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.

We now removed 'global' and added 'especially on regional scales'. Also, we now stress that we focus on regions where we expect local ecosystem feedbacks to be important. Our statement does not imply that IPCC AR4 is 'wrong'. However, the IPCC assessment can be made better, otherwise, why does IPCC continuously update the report? We acknowledge in the abstract that current global and regional climate models used for IPCC assessments are the best we have.

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.

We removed 'and most importantly' when referring to local ecosystem feedbacks, and please see also our changes outlined above; we think we now claim a role for the land that is defensible. Also, this is a perspective/opinion paper, and it is not our aim to provide new scientific evidence, but to put this issue on the research agenda. We think that whether ocean clouds and cryosphere or the land is more important depends on the process which one looks at; it might be true for temperature, but might not be true for C balances (Friedlingstein et al., 2006;Le Quere et al., 2009); still this remains a scientific issue, and could be region dependent, and the text was slightly modified to reflect this. However, an extensive comparison is beyond the scope of the current paper.

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.

By the impact on the land surface processes on the atmospheric circulation on continental to regional scale, we have in mind effects of all possible changes during 1870-2000 as in the LUCID study (Pitman et al., 2009). This paper provides a summary of effects of historical land cover changes on energy and moisture fluxes, but it does not report on changes in circulation patterns. The study also concluded that, despite of the best efforts to make the comparison coherent, it was still inconsistent regarding the implementation of land cover changes into the land surface models, despite agreed maps of agricultural land. The representation of crop phenology, the parameterization of albedo, and the representation of evapotranspiration for different land cover types were inconsistent between models as well. Besides, the ocean and sea ice in their simulations was not interactive, which limits the effects of land cover changes on climate, including circulation patterns. So, it would be premature to say that the LUCID study challenges all previous studies that found profound impact of land cover changes on atmospheric circulation (Chase et al., 2000). The LUCID study stresses a need to go towards more coherent and, in our opinion, more comprehensive representation of land surface processes in climate models. We now modified and expanded the text, and included references on potential impacts on atmospheric circulation (Lare and Nicholson, 1994;Chase et al., 2000;Fischer et al., 2007;Haarsma et al., 2009;Jackson et al., 2009). We also added Pitman et al. (Pitman et al., 2009) to give a balanced statement on different climate models.

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.

We modified the text at the end of the introduction, and added 'regional' to 'climate system' in section 4, so the paper throughout stresses now a regional and no global response. We checked the manuscript throughout for this. Concerning soil moisture-and vegetation-climate feedbacks; they are supposed to be relevant on seasonal to centennial time scales (Koster et al., 2004;Seneviratne et al., 2006). We now added this in the text in section 2. Also we added some careful statements on time scales in section 5.

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

We agree and we now added 'Despite uncertainties linked with inter-model discrepancies'….

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).

The study of Seneviratne et al. (Seneviratne et al., 2006) illustrates shifts in soil moisture regimes and resulting land-atmosphere interactions in IPCC AR4 simulations. These shifts occur on decadal and sub-continental scales, and do significantly affect regional climate predictions for Europe. We added the time and spatial scale in the manuscript. The question of whether the discussed local ecosystem feedbacks could lead to shifts of similar scale is open, but cannot be conclusively answered with the negative given the present state of research. In section 5 we now indeed better highlight the need to answer this question, to avoid potential 'surprises'.
The papers we refer to in the section on the Amazon predict bistability (two stable states) and shift from forest to savanna (Oyama and Nobre, 2003;Salazar et al., 2007). From more recent literature, similar alternative regimes can be inferred (DeLonge et al., 2008;Lelieveld et al., 2008). We prefer to keep this text unchanged, for focus and clarity.

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.

Please refer to our response to the next point of the referee.

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.*

We think that there is no basic disagreement with the referee here. We do not intend to make a statement that local feedback or any other process is dominant as compared to others. What we want to provide here is a rational towards a hypothesis. Please frame this in the light of a perspective/opinion paper. The idea is that positive feedbacks could be organized synergistically (hence the term 'propagate') in a way that local ecosystem feedbacks could be the trigger for a regime shift, just like the straw that breaks the camel's back. Please also refer to the abstract where we state: '… we reveal the hypothesis that, if the balance of feedbacks is positive at all scales…' etc. We added now in section 4 the following statement for clarification: 'So, we suggest that the feedbacks discussed in this section might be synergistically organized in some regions such that they all point in the same direction, i.e. the balance of feedbacks might be positive at all scales.' We hope we now make an acceptable statement. If a couple of papers show an effect, then this is not strong evidence. But it is good enough to generate a hypothesis that deserves further attention. See also our last response concerning section 4.

*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.*

We agree and now rephrased this sentence: 'The examples suggest missing cross-scale links…'.

*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.*

The papers we refer to here are analyses of conceptual models with simple atmospheric processes whereby the feedbacks were not validated against data. Therefore, these papers are hypothesis generating, and we basically agree with the referee. We now replaced this (indeed sweeping) statement with a more careful wording: 'We argue that… might impede…'; so we deleted 'no doubt' and 'major'. We hope this is now an acceptable statement, and that it has been made clear (in the manuscript as well as in this response) that it should be read in the light of the perspective/opinion format of this manuscript.

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.*

Our aim with this paper is contrary to undermining land surface research, instead we seek to stimulate it, in order to improve our understanding, and yield climate predictions that we are more confident about. We do not argue that the IPCC assessments or current models are 'wrong'; in fact they are the best we had at the time of AR4. However, there is a continuous upgrading of the models and of predictions, which, without changing the broad picture, become more and more refined, also at the regional scale. Here, we simply argue that our understanding is limited, and we seek for improved predictions by providing a perspective for incorporating cross-scale links between feedbacks at multiple scales. We accordingly revised our final statement, which is now expressed in a more restrained fashion: 'By following this approach we will receive further understanding of the relevant feedback processes and of their possible relevance in the context of climate projections'.

T. Chase (Referee)

*I had trouble reviewing this manuscript. I interpreted it as more of an EOS-type editorial that a straight scientific piece and mentioned that in my pre-screening review. If such a thing is acceptable in this journal and is identified as such then it should be accepted albeit with a few more caveats and less excessive language. There are no new scientific results presented and I don't think the work stands up as a traditional review paper as there is clearly a strong point of view, no balance of opposing pieces of evidence and no new conclusions based upon a different examination of previous evidence which would I would expect from a traditional review. Stylistically, there is a tendency for the authors to build a case with a lot of speculative language and then reach an overwhelmingly strong conclusion. For example in section 4, The conclusion: "Research so far leaves *no doubt*" is buttressed by several sentences proceeding it in which "may" and "possibly" are used. This kind of thing needs to be cleaned up. Of course there are doubts which are made clear in the previous sentences. In fact doubt is the underlying idea of this field. For example, in the recent Pitman et al. LUCID paper it was clearly demonstrated how much in doubt even the basics in this field are. The surface fluxes simulated in that paper were all over the place (see Figure 2 in Pitman et al.) indicating there is not even basic agreement among the various models as to the fundamental transfer of energy from the land-surface to the atmosphere in magnitude or sign. Everything happening after that, of course, is suspect. I look at this paper as a plea to improve land surface modeling, something that is sorely needed and I agree mostly with the overall conclusions of the piece. If such a thing is appropriate here and it is identified a such I recommend publication though personally I think the land surface community needs to go back and get the basics right before adding on all these complexities.*

We have modified the text including caveats and more careful wording. These are outlined by responding to referee #1 who had very similar remarks. We now also refer to the LUCID paper (Pitman et al., 2009). Please refer to our remark about the format of this paper starting on top of page 1 of this response. Indeed, our paper is a perspective/opinion paper, and we agree that it should be identified as such.

Chase, T. N., Pielke, R. A., Kittel, T. G. F., Nemani, R. R., and Running, S. W.: Simulated impacts of historical land cover changes on global climate in northern winter, Climate Dynamics, 16, 93-105, 2000.
Claussen, M.: Modeling bio-geophysical feedback in the African and Indian monsoon region, Climate Dynamics, 13, 247-257, 1997.
Dakos, V., Scheffer, M., van Nes, E. H., Brovkin, V., Petoukhov, V., and Held, H.: Slowing down as an early warning signal for abrupt climate change, Proceedings of the National Academy of Sciences of the United States of America, 105, 14308-14312, 2008.
DeLonge, M., D'Odorico, P., and Lawrence, D.: Feedbacks between phosphorus deposition and canopy cover: The emergence of multiple stable states in tropical dry forests, Global Change Biology, 14, 154-160, 2008.
Fischer, E. M., Seneviratne, S. I., Vidale, P. L., Luthi, D., and Schar, C.: Soil moisture - Atmosphere interactions during the 2003 European summer heat wave, Journal of Climate, 20, 5081-5099, 2007.
Friedlingstein, P., Cox, P., Betts, R., Bopp, L., Von Bloh, W., Brovkin, V., Cadule, P., Doney, S., Eby, M., Fung, I., Bala, G., John, J., Jones, C., Joos, F., Kato, T., Kawamiya, M., Knorr, W., Lindsay, K., Matthews, H. D., Raddatz, T., Rayner, P., Reick, C., Roeckner, E., Schnitzler, K. G., Schnur, R., Strassmann, K., Weaver, A. J., Yoshikawa, C., and Zeng, N.: Climate-carbon cycle feedback analysis: Results from the (CMIP)-M-4 model intercomparison, Journal of Climate, 19, 3337-3353, 2006.
Haarsma, R. J., Selten, F., Hurk, B. V., Hazeleger, W., and Wang, X. L.: Drier Mediterranean soils due to greenhouse warming bring easterly winds over summertime central Europe, Geophysical Research Letters, 36, L04705 2009.
Jackson, B., Nicholson, S. E., and Klotter, D.: Mesoscale Convective Systems over Western Equatorial Africa and Their Relationship to Large-Scale Circulation, Monthly Weather Review, 137, 1272-1294, 10.1175/2008mwr2525.1, 2009.
Koster, R. D., Dirmeyer, P. A., Guo, Z. C., Bonan, G., Chan, E., Cox, P., Gordon, C. T., Kanae, S., Kowalczyk, E., Lawrence, D., Liu, P., Lu, C. H., Malyshev, S., McAvaney, B., Mitchell, K., Mocko, D., Oki, T., Oleson, K., Pitman, A., Sud, Y. C., Taylor, C. M., Verseghy, D., Vasic, R., Xue, Y. K., and Yamada, T.: Regions of strong coupling between soil moisture and precipitation, Science, 305, 1138-1140, 2004.
Lare, A. R., and Nicholson, S. E.: Contrasting Conditions of Surface-Water Balance in Wet Years and Dry Years as a Possible Land-Surface Atmosphere Feedback Mechanism in the West-African Sahel, Journal of Climate, 7, 653-668, 1994.
Le Quere, C., Raupach, M. R., Canadell, J. G., Marland, G., Bopp, L., Ciais, P., Conway, T. J., Doney, S. C., Feely, R. A., Foster, P., Friedlingstein, P., Gurney, K., Houghton, R. A., House, J. I., Huntingford, C., Levy, P. E., Lomas, M. R., Majkut, J., Metzl, N., Ometto, J. P., Peters, G. P., Prentice, I. C., Randerson, J. T., Running, S. W., Sarmiento, J. L., Schuster, U., Sitch, S., Takahashi, T., Viovy, N., van der Werf, G. R., and Woodward, F. I.: Trends in the sources and sinks of carbon dioxide, Nature Geoscience, 2, 831-836, 10.1038/ngeo689, 2009.
Lelieveld, J., Butler, T. M., Crowley, J. N., Dillon, T. J., Fischer, H., Ganzeveld, L., Harder, H., Lawrence, M. G., Martinez, M., Taraborrelli, D., and Williams, J.: Atmospheric oxidation capacity sustained by a tropical forest, Nature, 452, 737-740, 2008.
Oyama, M. D., and Nobre, C. A.: A new climate-vegetation equilibrium state for tropical South America, Geophysical Research Letters, 30, 2199 2003.

Pitman, A. J., de Noblet-Ducoudre, N., Cruz, F. T., Davin, E. L., Bonan, G. B., Brovkin, V., Claussen, M., Delire, C., Ganzeveld, L., Gayler, V., van den Hurk, B., Lawrence, P. J., van der Molen, M. K., Muller, C., Reick, C. H., Seneviratne, S. I., Strengers, B. J., and Voldoire, A.: Uncertainties in climate responses to past land cover change: First results from the LUCID intercomparison study, Geophysical Research Letters, 36, L14814
10.1029/2009gl039076, 2009.
Salazar, L. F., Nobre, C. A., and Oyama, M. D.: Climate change consequences on the biome distribution in tropical South America, Geophysical Research Letters, 34, L09708, 2007.
Seneviratne, S. I., Luthi, D., Litschi, M., and Schar, C.: Land-atmosphere coupling and climate change in Europe, Nature, 443, 205-209, 2006.