**Reviewer A**: (attached)

***(RevA-01)This paper does not separate clearly in the introduction between the natural interactions between the land surface, atmosphere and oceans and how human activities (outside of increasing GHG) will affect this. It is the second topic which is the implied focus of the paper based on its title and preoccupies the rest of the text but it needs to be more clearly related to the natural interactions. The impact of LCLUC on the atmosphere is quickly discarded by the authors without giving any reason, I suspect it is because of the contradictory results at the global scale. But new knowledge on land-surface atmosphere interactions (Taylor et al. 2005-2011) clearly indicates that it occurs at scales much smaller than those addressed by ESMs and that thus it is very likely that they have missed up to now the existing sensitivity. Furthermore the authors point to the need to go to higher resolutions and do not envision that this will also lead to new feedbacks between the surface and the atmosphere which were well below the radar of our global models.***

Accepted and revised as indicated.

***(RevA-02) In the LCLUC ecosystem section the EFTs are presented as the new tool to solve all our problems. But the authors totally fail to recognize that more and more we will have to deal in our observations and modelling with managed ecosystems which have different geochemical interactions with the environment. It is well recognized today that a natural and a managed forest do not give the same carbon balance. What needs to be done in order to address this for instance? The EFTs are just as helpless as PFTs for managed ecosystems.***

We totally agree with the reviewer about the necessity of dealing with managed ecosystems. In fact, this is the main reason why we decided to use EFTs in our research. The identification of EFTs is based on periodic satellite observations of the carbon gains dynamics, the most integrative descriptor of ecosystem functioning (Virginia *et al.*, 2001). EFTs capture the current status of actual ecosystems. For instance, EFTs are able to capture differences between natural ecosystems (e.g. native oak forest) and managed ecosystems (e.g. tree plantations) when they differ in their carbon gain dynamics. Yet another example is Volante et al. (2012) who showed how the intrusion of cattle rising and croplands on natural dry forest and shrublands of NW Argentina significantly changed satellite-derived ecosystem functional attributes related to productivity and seasonality and, subsequently, the EFTs composition (Paruelo *et al.*, 2011).

***(RevA-03) In the section on societal needs the basic assumption is that we only use the renewable water which is part of the water cycle we are used to model in LSMs and ESMs. This is not true anymore as we are using geological water which is not at all accounted for in our models. This section does not address at all the need for the WCRP community to link-up with hydro-geological community. With the GRACE satellite a first step in this direction was done as the instrument measures both reservoirs.***

Accepted and a paragraph added.

***Below you will find some more specific comments on the paper.***

* (RevA-04) Page 2 : Irrigation was first introduced into global models by de Rosnay et al in 2003. Lately it has been used to show that irrigation modifies the onset of the monsoon : Guimberteau et al. 2011.
  + Accepted. These two references were included in the text.
* (RevA-05) Page 2 : GLASS is not spelled out and no reference is given to it. Please cite the paper by van den Hurk et al 2011 in BAMS.
  + Accepted.
* (RevA-06) Page 3 : LCLUC is used without being defined before.
  + Accepted and revised.
* (RevA-07) Page 4 : The content of the paragraph on land/surface interactions is only valid in the mid latitudes. New knowledge on land surface interactions gained in the tropics sheds a new light on this topic and needs to be discussed here (see the wealth of papers which came out on this topic from the AMMA project … which was sponsored by WCRP by the way !). The GLACE (part of GLASS !) experiments and their results need to be mentioned as well as they have helped us identify key areas and uncertainties.
  + This section has been rewritten with a stronger emphasis on atmospheric responses to land use and land cover change. Reference has been made to the impact of large scale irrigation studies in India for instance. The previous version had too much mention of soil-moisture atmosphere feedbacks through the PBL and their role in predictability (the GLACE experiments for instance and the many results from AMMA). This paper should not be concerned with those feedbacks, just the longer term feedbacks from LULCC.
* (RevA-08) Page 5 : I cannot see the equations and thus cannot comment on their value for the argument brought forward.
  + Sorry. We should have sent in PDF file, as well.
* (RevA-09) Page 6 : LUCID acronym is not spelled out.
  + Accepted and revised.
* (RevA-10) In this section there is no mention of the detection and attribution studies which have been carried out on rainfall and river discharge. That started with Milly et al. some years ago and they have all hit the complexity of the relationships between rainfall, evaporation and discharge, which is most influenced by human activities.
  + Detection and attribution of long term trends in hydrological quantities are out of the scope of current article.
* (RevA-11) Page 7 : Is Lawrence and Chase the most relevant reference for LSMs using PFTs to describe plant functioning ? Some review of LSMs used in inter-comparison studies would be more relevant to show the prevalence of this concept.
  + We removed this part of the discussion. We realize that our original writing gave the impression of a comparison of EFTs and PFTs, but that was not the intent. Our interest is to compare the EFTs approach against a fixed land-cover map approach, rather than against a PFTs approach.
* (RevA-12) Page 7 : The point is made that PFTs is not a useful concept as the functioning of the ecosystem cannot evolve under changing environmental conditions. But LSM only fix with the PFTs the link between transpiration, carbon assimilation and allocation. All other parameters (soil moisture stress, albedo, roughness and other surface properties) do evolve. Furthermore the ecosystems in each grid point are composed of a mix of PFTs which can evolve over time. So I do not see how the issues raised by the authors cannot be covered by the current PFT concept.
  + LSMs that incorporate Dynamic Global Vegetation Models based on PFTs do consider the evolution of ecosystem functioning with changing environmental conditions. However, LSMs that make use of fixed land-cover maps do not deal with such evolution.
* (RevA-13) It is unclear how the EFT concept can address the issues with PFTs or even how it could be implemented in LSMs. It might be a suitable tool for remote sensing but that does not make it ideal for models.
  + We agree that in this article we do not explain how they can be implemented in LSMs, but we have tested the concept with positive results. Our simulations have shown reductions of 15-20% of the precipitation biases during the warm season for subtropical South America. We have an article that has been submitted and two others are in preparation.
* (RevA-14) In this entire section nowhere are the issues of human managed ecosystems addressed. This is a major problem for modeling as humans (through plant selection, fertilization and pest control) modify the functioning of PFTs. This is spatially and temporally varying and defeats the principle that all plants of a given PFT have the same interactions with the environment.
  + See response to concern RevA-02 above.
* (RevA-15) Page 9 : water issues also become global because climate change and its impact on the water cycle are global.
  + Accepted and revised.
* (RevA-16) Page 10 : I do not understand how industry and domestic water usages are related to emission or air pollutant and much less how that will affect rainfall. It might be envisaged in some very local context but the generality of the statement cannot hold.
  + Accepted and revised.
* (RevA-17) Page 10 : This section does not address at all the issues of the adaptation of water management to changing rainfall/supply conditions. The WCRP community will need to interact with economists. A first step was done in the FP6 project CIRCE (Nassopoulos et al. 2011).
  + Suggested reference is about the cost benefit analysis and robust decision making on dam construction, and the authors feel the topic is out of scope for current article.
* (RevA-18) This section also misses the impact of the various water usages on the atmosphere (see the WATCH study by Taylor on the Mali wetlands) and the coastal processes. This will require regional ESM which can address these interactions at the appropriate resolution (See CORDEX and in particular CORDEX-MED coupled to HyMeX … another set of WCRP supported projects !).
  + Considered and reference to CORDEX was made in the discussion on the monitoring and modeling in higher resolution in section 4.
* (RevA-19) Page 10 : The introduction to section 5 should start with more basic concepts such as blue, green and grey water which are missing from current schemes before dealing with quantitative approaches to the biogeochemical properties of the water. Furthermore we have not even yet began to address the requests of oceanographers and economists to provide water temperatures for the resources we simulate in our models.
  + The authors consider blue, green, and grey water should be handled in the “Water” paper by Peter Gleick. We still revised the introduction to section 5. Temperature and salinity information from land to ocean is touched in section 1.
* (RevA-20) Since Oki 1999 more studies have addressed the issue of missing observations in our attempt to reproduce past surface conditions. The WATCH special issues have papers addressing this point.
  + Accepted and revised.
* (RevA-21) Page 11 : “Some land surface processes ... relative minor impacts on the climate feedback ...”. This statement is only based on the capacity/sensitivity of our current models. There are a number of studies out there pointing to the fact that this will not hold anymore once the resolutions have increased and small scale land surface contrasts can be represented. So if the sentence reflects the current thinking of the community it must be pointed out that it could be very wrong and that we could be in for some big surprises (The section needs to point to the wide range of surface/atmosphere interaction studies performed with mesoscale models).
  + Accepted and revised.
* (RevA-22) Page 11 : “Need for downscaling models” what does this mean. Is is equivalent to high resolution models or a more esoteric approach?
  + This sentence was removed.
* (RevA-23) The points listed do not all build on arguments of the discussion in the previous pages. There is nothing on interactions with other communities (outside of WCRP), interactions within disciplines covered by WCRP. What is the role of LCLUC and land surface modelling for climate services?
  + This text has been rewritten to better reflect the previous discussions in the paper.
* (RevA-24) Conclusion : land use has also had a big impact on the water cycle changes of the 20th century : Just look at the discharge of the Nile, Ebro, Mississippi, …
  + Accepted and revised.
* (RevA-25) “detection and attribution studies” appear in the conclusion out of nowhere. The ground is not laid in the paper for this topic.
  + Accepted and revised.
* (RevA-26) “The choices we make in LCLUC (why suddenly LULCC ???) will likely (is this adjective needed?) influence future climate through the carbon balance “, “and the water cycle” needs to be added.
  + Accepted and revised.

**Reviewer B:**

***(RevB-01) Overall, the introduction seems quite vague. It contains a suite of ill-referenced and very generic statements. It is not “pointy” – it does not create a strong sense that the issues being presented are important and worthy of a broader consideration.***

Accepted and the sentence was rewritten.

*Introduction:*

***(RevB-02)******The sentence beginning “Land surface provides lower boundary condition to the atmosphere interactively: it receives precipitation, downward short wave and long wave radiations, and returns fluxes, such as upward short wave and long wave radiation, momentum, sensible heat, latent heat, and carbon dioxide, to the atmosphere (Oki, 1999)” is a horribly constructed sentence.***

Accepted and the sentence was rewritten.

***(RevB-03)******The sentence: “Any kind of climatic variations and changes will have significant impacts on human activities over land with societal impacts” is simply false. A tiny variation would not have a significant impact. The point here is the language is very loose – it commonly lacks the precision required.***

Accepted and the sentence was rewritten.

***(RevB-04)******The sentence “On the other hand, human activities interfere with the climate system from local to global scales through changes in land use and land cover, and through the interventions on the water cycle components, for example by irrigation and storage in artificial reservoirs (Haddeland et al., 2006; Hanasaki et al., 2006, 2010).” I agree with this sentence, but so what ? There is no point to this sentence, it does not link to any theme. When you cite GLASS you should probably refer to the new paper in BAMS by Bart van den Hurk.***

Accepted and the sentence was rewritten.

***(RevB-05)******The abbreviation LCLUC is odd – most use ALCC or LULCC.***

Accepted and the sentence was rewritten.

***(RevB-06) The text around “However, this study did not include the feedbacks from the land to the weather is a fundamental limit - so why include this in a WCRP position paper. WCRP is about connectivity in the climate system. A review of the role of LULCC can omit studies that lack feedbacks given there are many that include this.***

This text has now been removed. The reviewer is right that the paper needs to focus on interactions with the climate system.

***(RevB-07)******The text around “A group of scientists brought together their modeled Land-Use change experiments in an attempt to understand the overall climate-impact of the wide-scale deforestation that has occurred over the last century (see Pitman et al, 2009). The idea was to quantify if the current regional weather has been influenced by the anthropogenically altered landscape” is wrong. LUCID was all about regional to global climate. The word “weather” implies something that LUCID never ever considered.***

This text has been revised and now refers to the regional climate, not ‘weather’.

***(RevB-08)******The text “This has important implications for the physical response to land use change and its impact on the regional meteorology” might be true but can you spell these “important implications” out ? What are they, how do we know that the implications are true?***

Additional text has been added to point out the potential impact of land use changes on heat waves.

***(RevB-09)******The text: Still, many articles have discussed the mechanisms by which a change in land cover can affect the overlying planetary boundary layer (PBL), its thermodynamic properties and circulation, and consequently the precipitation processes and regional climate (e.g., Pielke and Avissar 1990; Stohlgren et al. 1998; Kanae et al., 2001; Pielke et al. 2007; Lee and Berbery 2011). You really need to be careful here. This is a WCRP report – and almost all of these are regional scale modeling using regional climate mdoels on short timescales – commonly a few days. Or they are synthesis papers. I suggest you read Pielke et al., 2011 in WIRES to update this but really you need to judge which of these findings are relevant to WCRP. I’d suggest it is reasonably easy to dismiss most of these. Pielke and Avissar is on a weather scale and one model. Kanae is one model and a suspect experimental design. Pielke et al. 2007 is a synthesis paper, not an original contribution. A “review” is not just about saying what people have done, it is assessing whether what they have done withstands scrutiny and is right in hindsight.***

We introduced Pielke et al., 2011 in WIRES. The authors believe that the impacts and mechanisms how LULC chances have an influence on PBL and consequently precipitation is not yet thoroughly understood and nothing is definitive. Therefore we referred several papers in parentheses for further references.

***(RevB-10)******The text: “The changes in the PBL characteristics also have an impact on the evaporative demand through changes in cloud cover (Ek and Holtslag, 2004)” - sure … on short time scales and small spatial scales. Is this relevant to WCRP ?***

(Response to B-10) This text has been removed as not being relevant to the WCRP. The timescales are too short, as pointed out by the reviewer. Instead, longer time scale feedbacks have been added such as irrigation.

***(RevB-11)******Text: “For instance, Cai et al (2009) have demonstrated the role that the land-atmosphere feedbacks have had on the recent Australian drought: their model results imply that feedbacks in the system … true, but I thought this was observationally based not modeling based.***

A model was used in this study. We are happy for this text to remain.

***(RevB-12) Text: The proposed and modeled links between land cover change and feedbacks and riverflow should be tested further using the outputs from the LUCID project“ is not possible because LUCID did not save enough data – one would need to re-run these experiments I think.***

We have removed this explicit reference to the LUCID models being used to look at river flows. Instead, there is a recommendation for the impact of land cover change on river flows to be examined in the final section.

***(RevB-13)******Text “The results are important with regards to drought prediction and the possible mitigation strategies that might be employed in future” is fine but I think you need to elaborate on this to make it a valid statement. Why are they important? How do they relate to drought and how might they help mitigation?***

The text has been expanded to point out that land cover change and feedbacks has a role in drought mitigation.

***(RevB-14)******Text: “Hence, extensive impacts on ecosystems, both from natural origin (e.g., climate extremes) and human made (e.g., land use changes), may alter one or several pathways of the ecosystem–climate feedbacks, which may end up affecting the regional and global climate” is weak. “May” twice in the same sentence? This is a position paper – I think you should be more explicit about why you say “may” and what needs to be done to change “may” into something more definitive.***

We rewrote this section to make it more explicit and address the reviewer’s concerns.

***(RevB-15)******Text: “Current global land surface modeling has not yet integrated most of the latest achievements in process understanding and regional- or local-scale modeling studies”. You need to be far more clear than this – what has/has not been included and of that NOT included what is known to matter ? Not much I would suggest.***

Text has been altered to include more explicit examples, such as groundwater and nutrients and sediments.

***(RevB-16)******Text: “Precise information on the land use/land cover is essential to have better estimates on material cycles, and coupling of the LULC change model with biogeochemical land surface model would be necessary for convincing future projections considering both climate and societal changes” Maybe – this is interesting but very superficial. If this is true and a wise future strategy it needs to be explained in far more detail I think.***

As a summary of previous sections how LULC influences water and nutrient cycles, the authors consider the revised sentence is reasonable.

***(RevB-17)******The paragraph: “Hydro-meteorological monitoring networks need to be maintained and further expanded to enable the analysis of hydro-climatic trends at the local level and the improvement in the accuracy of predictions, forecasts, and early warnings. As clearly illustrated in Figure 6 (Oki et al., 1999), global hydrological simulations are relatively poor in the areas with little in-situ observations. Basic observational networks on the ground are critically indispensable for proper monitoring and modeling of global hydrology; however, it is also required to utilize remotely sensed information in order to fill the gaps of in-situ observations. Reliable observational data are essentially necessary not only as the forcing data for global hydrological modeling, but also for the validation of model estimates. River discharge and soil moisture data are critically important for global hydrological studies. However, contributions from the operational agencies in the world are not yet well established and need to be enhanced.” Well, of course I agree. But WHY ? Where is the evidence that these observations feed back into modeling or prediction ? Are existing data available ? Are they used systematically in model development ? If so, why are the models weak. If not, why are the data not use … what can remote sensing give us. Are the models able to use these data? If not, why not?***

All the model developments and validations are utilizing observations, and numerical weather forecasts fully utilize satellite observations. The authors think it is annoying to include additional text describing these points here.

***(RevB-18) Text: “However, it should be the time to develop land surface models primarily for responding to the demands of understanding the land surface processes and possible future changes for supporting various decision-making in society” Again, I agree, but what does this entail ? How does this relate to other WCRP strategies? Does it conflict with the basic wish to improve global models?***

Accepted and the sentence is substantially revised.

***(RevB-19)******Text: “From this point of view, the integrated land surface models, which consider anthropogenic interventions explicitly … should be developed and implemented in order to provide more realistic impact assessment and support the design of practical adaptation measures” Can they be ? Do they do the time step fluxes of energy, water etc – do they replace existing models and add value ?***

Yes they do, but they will not replace existing models but add modules representing some processes which is not considered yet.

***(RevB-20)******The section on Research needs is simply inadequate. Most of these are not new – they have been stated many times before. There is no strategy here, no insight.***

***The statements lack context, detail or authority. deNoblet, Pielke, Pitman et al have all said similar things and even Kabat makes these points.***

The text has been revised and simpler statements based on the conclusions of the WCRP meeting have been stated. The statements are based on the community meeting and it would be unfair to make statements that go further than what the meeting suggested. Therefore, I think the statements need to remain as a faithful record of that meeting.

***(RevB-21)******Similarly, the text around “WCRP gaps in LCLUC research” is also inadequate. It is almost like thought bubbles – single sentences that the authors want to be true but have not demonstrated in any clear sense. In effect, these “gaps” seem terribly superficial. They may be right but there is no defense, no explanation*.**

Accepted and revised.

***(RevB-22)******Finally, the concluding remarks read like a series of random thoughts that are not logically connected***

Accepted and revised.

**Reviewer C:** (Paul Dirmeyer, attached)

*The paper motivates and presents key recommendations for future WCRP research with specific regard to the land surface, specifically land use and land cover changes and their impacts on climate. There are inconsistencies among the sections. I presume the earlier sections are supposed to summarize the state-of-the science at this time, leading into the discussion of gaps in Sec 5.*

Noted and reflected in the revisions.

***(RevC-01)******Sec 2 does this well for surface water, but is incomplete on the atmospheric feedbacks, discussing mostly the surface fluxes and empirical relationships (see below).***

The text that was based on the surface water has now been removed and replaced with a simple review of the way that the land-atmosphere feedbacks affect the role of land use and land cover change on regional hydrology and hydrometeorology. Extra references have been added to support the statements.

***(RevC-02)******Sec 3, on the other hand, merely discusses the definition of PFTs vs EFTs – I feel like only a thin slice of the field of land use change in ecology has been discussed.***

We have rewritten this section to incorporate examples of the effects of changes in land use and land management.

***Below are general comments for each section. I am also returning a marked‐up version of the electronic document with specific comments and suggested edits embedded. Unfortunately the equations did not translate.***

***(RevC-03)******Sec 2 seems to suggest the hydrologic feedback pathways are basically well understood if not well modeled. I’m not sure that is the case, especially once one gets into the surface layer / boundary‐layer / clouds & convection branch. Also, I think the final paragraph falls rather flat in terms of motivating/suggesting the next steps. Either no recommendations should be given here (saved for the last section) or more should be given.***

The final paragraph has now been removed and a better overview of recommendations has been added to the final section. The section referred to has been rewritten to discuss the explicit role of LULCC and feedbacks on the regional climate/hydrology rather than all the feedback pathways such as clouds etc.

***(RevC-03b)******Sec 3 is mainly concerned with the definition of vegetation within models, championing the EFT approach. However, I find it leaves many questions.***

We have rewritten many parts of this section in the attempt to clarify why EFTs are discussed.

***(RevC-04)******First, the last sentence of the section suggests these can vary from year to year, which implies for any climate forecasting / projection effort, these EFTs need to be predictable and predicted. This is not addressed*.**

(see next response)

***(RevC-05)******Second, there is an implication in the description of EFTs that they amount to a sort of statistical model, or at least statistically defined ecological model. That leads me to recollect the work of Gab Abramowitz who showed that a simple neural net can outperform land surface models at the point scale over homogeneous terrain, where the PFT approach should do best – what about such pure statistical approaches in this less tractable case?***

(Response to C-04 and C-05) We are not sure we understand this comment. The identification of EFTs is based on periodic satellite observations of the carbon gains dynamics, the most integrative descriptor of ecosystem functioning (Virginia *et al.*, 2001). EFTs capture the current status of actual ecosystems, therefore although statistical tools are partially used in the methodology, the input and output are physically based.

***(RevC-06)******Third, the abrupt end of the section leaves a bit of a gap regarding where to go next – I should be left with a better notion of where we stand.***

As we stated earlier, we have rewritten many parts of this section in the attempt to clarify why EFTs are discussed.

***(RevC-07)******Together, Secs 2 and 3 should present a launching point for future research recommendations****.*

The final section now has a better combined overview of the future research needs.

***(RevC-08)******Sec 2 does a better job of this than Sec 3, but much is not presented regarding the current state of the art (e.g., see Intl. J. Climatol., Vol 30, Issue 13). I’m not sure to what level a review should be presented in this paper, but currently the level of documentation of the current state is very irregular.***

The text has been altered so that a full review is not implied (this was not asked for). More a steer in the ideas that could form a strategy for WCRP, what is missing and why is it important. The issue of the interaction of LULCC and atmospheric feedbacks on the long-term regional climate state and the impact on freshwater and heat is key. This is now what we are aiming for, rather than a review of all the literature.

***(RevC-09)******Sec 4 is very general, consisting of “motherhood” statements. Compared to the other sections, it lacks statistics or mechanisms to underpin the recommendations.***

Comments were accepted and tried to revise the section.

***(RevC-10) At least one or two specific examples would help. Also, it seems the first few paragraphs could have more references would help. A‐level papers in top‐flight journals (Nature / Science) plus IGBP/IHDP/DIVERSITAS reports could be cited.***

Accepted and revised.

***(RevC-11)******Sec 5: The bullets listed as recommendations from the WCRP meeting in Denver map unevenly to the rest of the text.***

The text has been revised to improve the readability of this section. In addition, the recommendations now reflect the text in the bulk of the paper.

***(RevC-12)******For example, downscaling was not mentioned at all, yet it has a bullet. There needs to be better correspondence between the preceding text (to motivate these bullets) and the recommendations themselves.***

The text has been revised and this has been removed.

***(RevC-13)******Sec 6: The concluding remarks are where the general motherhood comments should be made. They would pair well with the statements in the paragraph written here.***

The text has been revised.

**Reviewer D:**

I recommend publication once the authors address the comments below.   
  
Major comments:   
1. Since this is something of an overview/position paper, I reviewed it with an eye toward seeing whether it touched on my own ideas about land use / land cover change (LULCC) research. It does touch on some of them, though not directly. I personally feel that the following two issues should be emphasized more:   
(Response to the overall comments) The comments are noted and reflected in the revisions.

***(RevD-01) a) The paper covers the use of dynamic vegetation models to capture land cover changes in a changing climate. The idea is that as climate changes (e.g., warms), vegetation responds: leaf area index changes for a given species, or the species distribution itself is modified. These changes can then feed back on climate. That’s fine, but it seems to me that these effects would be overshadowed by direct human-imposed land use change – deforestation, for example, or the conversion of shrubland to cropland or pasture. While such anthropogenic changes are discussed, \*predicted\* land use change is not. Predicting variations in surface conversions requires working with non-Earth scientists, particularly (I guess) sociologists adept at predicting human dynamics in the coming century.***

We agree. We have rewritten the section on ecosystems and the section “WCRP gaps in LULCC research” to present this issue and the need for interdisciplinary research.

***(RevD-02) b) The paper uses a Budyko framework to indicate that some types of vegetation produce higher evaporation rates than others. In the paper, this is equated somehow to “feedback”, which may be overstating things – I didn’t really follow the logic there. In any case, the critical issue is whether or not current land surface models (not necessarily dynamic vegetation models, since predicted LULCC can perhaps be prescribed through sociological prediction scenarios) capture this vegetation-type-dependent evaporation. There must be plenty of model simulation data available right now to make a stronger first-order statement regarding land model behavior in this regard.***

The role of feedbacks is part (implied) in the Budyko framework. The other parts (rainfall type, land cover) are outlined in the text. I agree that the models should be tested whether they capture this behavior as well as finding large-scale observations to corroborate it. The paper by Roberick and Farquarhar goes a long way to checking out the models. I believe more work could be carried out to understand the role of feedbacks and how they might change in this climate-hydrology relationship.

***(RevD-03) 2. The emergence of dynamic vegetation models is, of course, relevant to LULCC research. While the paper does describe some of the benefits of modeling dynamic vegetation, it does not mention a critical problem with it: climate models are inherently biased, and climate biases will lead to biased vegetation distributions. Furthermore, in principle, the extra degrees of freedom associated with the inclusion of a dynamic vegetation model can lead a climate even further down a wrong path; whenever two models are coupled together, previously unexplored “coupled modes” must be analyzed, and any detrimental effects of the coupling must be mitigated. I felt that the paper did not present a balanced picture of the benefits and challenges of dynamical vegetation modeling.***

The issue of dynamic vegetation modeling was not explicitly addressed in the meeting and we did not consider it to be a major part of the motivation of this paper, which was more focused on anthropogenic changes to the land cover. A discussion of the challenges facing dynamic vegetation modeling is not seen as a priority for this paper.

***(RevD-04)*** *3.* ***Parts of section 5 (“Current Gaps and Future Challenges”) seem disjointed.***

Please see revised version

***(RevD-05)******Some of the research needs aren’t supported by earlier text (e.g., those regarding soil properties and downscaling)***

The text has been altered so that it better reflects the earlier text. For instance the reference top soils and scaling have been removed and the conclusions about the impact of land cover on river flows have been expanded.

***(RevD-06)******or aren’t specifically related to LULCC (e.g., the importance of improved hydrometeorological monitoring).***

Hydrometeorological monitoring is crucial for better understanding and prediction of land processes in climate system, and the authors are happy to see the description remains in the text.

***(RevD-07) (For downscaling, the importance of looking at fine resolution is discussed in section 4, but not with any supporting arguments; basically, the text just says that it’s important.)***

The sentence on the downscaling is removed from the text, and recent research directions on higher resolution modeling are introduced in section 4.

***(RevD-08) Section 5, particularly the bulleted lists, seems a bit more “thrown together” than the earlier sections, and I get the feeling that this section should perhaps be the most important one. Some effort should be made here to clarify the issues – particularly those directly related to LULCC. The abstract and conclusions section would benefit from similar tightening.***

The text has been altered so that clear recommendations are being made that reflect the meeting in WCRP and the text within this paper.

***Minor comments:***

(RevD-09) The grammar in places needs work. (e.g., “researches” in the title of section 4 – one of many examples.)

We have made some changes to the grammar.

(RevD-10) p. 2, line 12: Clarify meaning of “geological formation”, given that topography and soil properties are already mentioned.

We meant properties in deep geological layers, and changed into “geological condition”, implicitly excluding topography.

(RevD-11) p. 2, line 14: The longer time scales are **\*in addition\*** to the short time scales of relevance.

Accepted and revised.

(RevD-12) p. 2, 4th paragraph: Define GHP, GLASS, and GSWP2.

Accepted and revised.

(RevD-13) Figure 2: This figure is unhelpful. It’s not very clear what it’s trying to show.

We could remove this figure, however, we felt it helped explain the role of land cover on the heating of the atmosphere and how it changes in a drought.

(RevD-14) p. 4, 2nd paragraph. This is confusing. What is “it” in “it is always greater” – the percentage drop in rainfall or the percentage drop in runoff?

The text has been altered to make this logic more clear.

(RevD-15) p. 4, last sentence of 2nd-to-last paragraph. I think you mean that the runoff **\*percentage\*** change is always greater than the precipitation **\*percentage\*** change. Otherwise the statement is incorrect.

The text has been altered so that the percentage change is more explicit.

(RevD-16) p. 5, line 3-4 (“forests have higher feedback strength than crops”). Please define what is meant here by “feedback strength”. It’s not obvious. Note that a higher evaporation sensitivity does not **\*necessarily\*** imply a higher sensitivity of climate to the land surface.

The feedback strength is not evaporation sensitivity. It is the role the atmosphere plays in limiting evaporation though changes in the PBL structure. The text has been altered to make this clearer.

(RevD-17) p. 5, 3rd paragraph (“… a more linear relationship…”). The discussion here seems to lose sight of the fact that the relationship between precipitation change and runoff change is probably affected much more by the climate of the region (according to Budyko, i.e., where you are on the x-axis in Figure 3) than by the vegetation type itself. Some clarification is warranted.

The text has been altered to make it clear that the role of vegetation and feedback strengths and so on is only secondary.

(RevD-18) p. 6, lines 17-19. This sentence (“Interestingly, for a model…”) seems wrong to me. Such a model would behave very differently in terms of land-atmosphere interactions in drought and wet periods (which would place the system, for example, in different parts of the x-axis of Figure 3). The vegetation species and their LAIs might be the same, but the effects of water stress on the surface fluxes would be drastically different.

We have rephrased this paragraph to clarify the points raised by the reviewer.

(RevD-19) p. 6, lines 19-22. This sentence (“Dynamical vegetation models…”) is a bit of an overstatement. The inclusion of a dynamic vegetation model does not lead to an automatic improvement in the ability to examine land-atmosphere interactions. (See comment 2 above.)

Agree. We have rephrased the text.

(RevD-20) p. 7, 2nd-to-last paragraph. NDVI estimates from space are far from perfect, with problems of signal saturation, etc. Should caveats be given regarding the figure shown?

We have experience accounting for the uncertainties in the NDVI estimates (Alcaraz-Segura *et al.*, 2010a; Alcaraz-Segura *et al.*, 2010b). We are planning to account for such uncertainties in the EFTs estimation from different datasets and vegetation indices. The EFTs will be provided with a consistency assessment.

(RevD-21) Section 4. The presence of this section seems a little odd in this paper; the text covers much more than the LULCC topic. LULCC is presented here as one facet of a multidimensional problem, and most of text covers the multidimensional problem. (For example, what does “sharing hydrological information relating to the transboundary river basins” have to do with LULCC?) I guess the text is okay, though a stronger emphasis on LULCC would presumably be more appropriate.

Accepted and revised.

(RevD-22) 3rd-to-last sentence in paper (“ Crop growth and development effect…”). This sentence comes in two parts. I don’t know what the first part means. I think the second part is understandable if a “that” is removed.

The text has been altered to make this more clear.