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Earth System Dynamics Discussions

Interactive comment on "The influence of vegetation on the ITCZ and South Asian Monsoon in HadCM3" *by* M. P. McCarthy et al.

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General comments

We thank the reviewer for their very constructive comments. The original review comments are copied below, and our response is in bold. The methods, results, and overall conclusions are unchanged, but we have undertaken substantive revisions to parts of the manuscript to improve the presentation and address common points raised by all the reviewers. Therefore the responses below will make reference to the appropriate sections in the revised manuscript

General Comments The authors have presented an interesting paper highlighting the importance of the representation of land-surface properties in the northern hemisphere

extratropics to tropical anomalies in general and the simulation of the South Asian monsoon in partic- ular. The manuscript could be made acceptable for publication with a few changes in order to better demonstrate the mechanism involved and to allow the variety of experi- ments used to be more easily followed.

Specific comments

line 24: please elaborate on why the shorter time periods used in other ex- periments revealed that the land cover change did not affect regional and local climate. Do you mean that the experiment samples were not long enough to reveal statistically signif- icant results (signal above noise), or that the experiments were not run out for long enough for the various land-atmosphere feedbacks to actually emerge?

The shorter time period reference is not appropriate for all the references given so has been removed.

Page 94, lines 3-7: is there any suggestion in Osborne et al. (2004) as to why the regional responses to the land surface conditions differ over India and China? Are there different climate mean state conditions over those two regions, for example, that mean the response of the atmosphere to the surface is suppressed in one and enhanced in the other?

Osborne et al suggest that the differences relate to differences in the low level monsoon flows passing over the Arabian sea before hitting India, but in the case of South China pass over a region of significant tropical forest cover. The bulk of the discussion in this manuscript concerns large scale circulation responses so extending the discussion of this paper significantly in the introduction is probably not essential for the sake of brevity

Page 94/95, Model description: it would be good to briefly describe or validate the models simulation of the monsoon in HadCM3 since much of the rest of the paper discusses it. E.g., in Turner AG and Slingo JM (2009; Q J R Meteorol Soc 135:549-

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567) or in Turner AG, Inness PM, Slingo JM (2005; Q J R Meteorol Soc 131:781-804).

Additional references have now been included in the introduction to reflect this

Page 96/97, Experimental description: I feel the experimental description (particularly the naming) used here and in Table 1 could be improved upon, allowing better clarity. Specifically, since many of the paired experiments change the leaf phenology, it would be better to describe them (for example) as WHS and WHSnoleaf, as this would obviate the reader having to refer back to the table. TRIF3 could become TRIF1future also.

as suggested section 2.3 has been rewritten to improve clarity. As correctly noted the paired experiments are only really changing leaf phenology, and that proved not to be a major factor in the results presented. So to simplify the revised manuscript now discusses only TRIF, WHS, IGBP, and TRIFfut

Page 97, line 6: the results in this manuscript seem to rely on a shift in the thermal equator and associated movement of the ITCZ. Given that only northern hemisphere extra-tropical vegetation is perturbed in these experiments, the manuscript should at least speculate on how the results would change if the same datasets were compared in the southern hemisphere. Would there be any cancellation of the influences of the northern and southern extratropical vegetation on the thermal equator and ITCZ?

Actually the TRIF, WHS, and IGBP experiments change vegetation globally, which is hopefully now clearer in the revised section 2.3. But the main surface climate responses are in the Northern Hemisphere. The discussion of the main findings in section 3.1 have been redrafted for clarity in this regard too

Page 97, line 26: the southward shift of the ITCZ could be ascribed to a southward shift of the thermal equator (in this case caused by the preferential cooling of the northern hemisphere extratropics). Such shifts in the ITCZ and thermal equator have been described for HadCM3 by Johns et al. (2003), already cited, in their case being northward in response to anthropogenic greenhouse warming occurring preferentially in the

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northern hemisphere. It would be useful to mention that study here in this context.

mention of this result of Johns et al. has been included in the penultimate paragraph of the introduction

Page 99, line 27: rather than just being "affected" it would be better to describe more precisely what is happening during the onset and decay phases of the monsoon. Are they strengthened/weakened or advanced/delayed for example? This will help explain the W01 index to the reader, whereby there are strong increases around onset and withdrawal times and possible changes to the length of the monsoon season. Page 100, line 6 also contains a generic mention of a dynamical response to the vegetation changes.

see response to next comment

Page 100, line 20: the manuscript may be correct that the changes in snow cover and large-scale temperature perturbations in over Eurasia are weakening the monsoon but it would be worth exploring this mechanism more carefully. Turner Slingo (2011), for example, suggest that Himalaya/Tibetan Plateau warming through winter and spring (corresponding to reduced snow cover and similar to Figure 3 here) would lead to increased monsoon rainfall. The results in Figure 3 suggest reduced monsoon rainfall. It may be then that the monsoon is actually responding to the cold temperature anomalies and increased snow much further north. This discrepancy should be mentioned. I would also suggest employing another index (that of Xavier et al., 2007: Xavier PK, Marzin C, and Goswami BN (2007) An objective definition of the Indian summer monsoon season and a new perspective on the ENSO-monsoon relationship. Q J R Meteorol Soc 133(624):749-764) to help determine in what way the various experiments are perturbing the meridional tropospheric temperature gradient. The reversal of such an index is closely linked to the onset of the monsoon. One would expect to see a weakening of this index, or a delay to the time at which the index reverses, in the experiments presented in Figure 3. Page 101, line 14: the crucial point about the onset

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in particular being perturbed is that the anomalies of snow do not persist beyond late spring and so cannot affect so well the remainder of the monsoon season (as explained in Turner Slingo, 2011).

While the experimental set up employed in this study allow us to quantify the net sensitivity of the simulated monsoon in HadCM3 to these alternative vegetation ancillary datasets that have all been used in different configurations of HadCM3 but never explicitly compared, it does not provide the most appropriate set of experiments to improve on the analysis of specific mechanisms than is available elsewhere in the literature. It is true that the temperature and snow resposnes over the Tibetan Plateau are somewhat more mixed than further north, and the spatial pattern of snow perturbations differ from the experiments of Turner and Slingo, this is noted more explicity in the revised section 3.3. The Xavier et al. index has been incorporated into Figure 5 and replaces the Goswami and Wang-Fan index. Fig. 5 also now reflects daily resolution data rather than the monthly means previously presented, and the associated text in section 3.3 revised to reflect this. The monsoon start date is delayed by 6 days in the TRIF experiment and duration reduced by 8 days in total. As suggested by Michel Crucifix in his comments we also compare the precipitation differences with the spread of results from the perturbed physics ensemble for context.

Page 93, line 19: change "models" to "model".

done

Page 95, line 17: insert a colon after "periods".

done

Page 100, line 25: it would be useful to point to the relevant figure panels when describing the resulting temperature perturbations.

This section has been rewritten

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Figures 1,3: these figures are currently too small and should be enlarged to at least fit the width of the associated figure caption. Even when blown up on the screen, it is difficult to interpret the wind vectors in Figure 3 and these should be improved. In addition, at present there is much overlap of some titles from Figure 3 into the adjacent columns, which looks messy. Both these figures would be easier to interpret if there were separate column and row titles.

The figures have been modified to reduce the number of panels presented, and to use colours rather than linestyles to differentiate the experiments in Figures 2 and 5. The additional information removed such as change in shrub and grass cover (Fig. 1), or seasonal effects (Figs 2-4), are useful but not critical to the results and conclusions and therefore can be provided as supplementary material

Figure 4: since colour has already been used in the manuscript, it would be clearer if the curves in Figure 4 were different colours rather than line styles.

done

Interactive comment on Earth Syst. Dynam. Discuss., 3, 91, 2012.