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Climate of the Past Discussions

Interactive comment on "Detecting vegetation-precipitation feedbacks in mid-Holocene North Africa from two climate models" *by* Y. Wang et al.

Y. Wang et al.

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Interactive comment on "Detecting vegetation-precipitation feedbacks in mid-Holocene North Africa from two climate models" by Y. Wang et al. Anonymous Referee #1

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The manuscript by Wang et al. is a short, well-focused, and interesting contribution to analysis of feedbacks between climate and vegetation in North Africa during the mid-Holocene. They compared results from simulations with two models, FOAM and CCSM-2. The authors found, contrary to many previous modeling studies, a negative feedback between vegetation and precipitation at the annual time scale. The reason for the negative feedback is a parameterization of the land surface processes that leads to a higher evaporation from bare soils than from vegetated surface. This is understandable at the monthly time scale because vegetation removal delays water release from the deep layer, but it is rather puzzling at the annual time scale. Besides, albedo of soil without vegetation canopy is assumed to be lower than albedo of bare ground, which results in switching off Charney's radiative feedback. A plausibility of these assumptions should be discussed in details before the manuscript can be accepted for publication.

Response: We think that there is some sort of misunderstanding here. At monthly timescale, vegetation removal does delay water release from the deep layer. However it enhances the water release from the top layer as more energy is reached the surface as in our mid-Holocene experiments. It is still not clear that, at monthly timescale, what the sign of vegetation-precipitation feedback is as indicated in our paper (Fig. 3). However, at annual timescale, when bare ground has the same reflectivity as grassland, the competition of water release from the surface between evaporation and transpiration provides the source of negative interaction between vegetation and precipitation. We think that this "puzzling annual timescale" is controlled by the gross primary productivity, biogeochemistry, and the net primary productivity in vegetation module. Regarding to surface albedo, unfortunately, our model output does not contain surface albedo information. However, the referred literatures (COHMAP, 1988 and BIOME 6000) in our paper have indicated that the mid-Holocene soil is wetter and darker than those of pre-industrial and present-day. Small and Kurc (2001) found that observed surface albedo was lower under wet conditions in semiarid areas (This reference is added to my revised paper.). The dominated vegetation in mid-Holocene North Africa is grassland, which has a prescribed albedo of about 0.16. This value is intercomparable with the background albedo of a wet/dark soil of about 0.14 (see Notaro et al. 2007 for details). The Charney's theory is based on a large difference of surface albedo between vegetated surface and bare ground. It is not valid in our mid-Holocene case where the effect of surface albedo difference is negligible as compared to the hydrological factor between vegetated surface and bare ground.

Major comments

1. Many studies suggest that runoff in semidesert is enhanced with vegetation removal. This should lead to a water loss from the soil-atmosphere system. Is this accounted in the models?

Response: The runoff is accounted in our climate models. Local runoff calculated when the soil water of a layer exceeds water holding capacity is used as input into the river routing scheme for freshwater discharge into the oceans. The zonal distribution of average runoff predicted by the LPJ DGVM also compares favorably to a 1o x 1o gridded dataset of observed runoff (see Fig. 8 in Stich et al., 2003). We do not see a dramatically enhancement of runoff with vegetation removal at annual scales.

2. Soil is a product of vegetation development. One can assume that vegetation removal will lead to removal of soil layer as well and this should affect the surface albedo. As far as I understand, this effect was neglected in the simulations, and this might have led to overestimation of the negative feedback.

Response: Yes, we agree with the reviewer on this important aspect. However, in our climate model simulation, we prescribed our soil category so that it is not changing with time. We cannot see any significant soil development within the time-slice of our model integration. Hence, we think the fixed soil type will not significantly change our results. However, in the future, when soil module has been developed and implemented into our climate models, we would like to further investigate the consequence of a dynamic soil development on the negative feedback between annual precipitation and vegetation cover.

3. I have recently reviewed a paper by Notaro et al. 'Statistical and Dynamical Assessment of a Simulated Negative Vegetation Feedback on North African Precipitation During the Mid Holocene' which methodology and conclusions are similar to this paper. A novelty of Wang et al. paper in comparison with the paper by Notaro et al. should be stressed here.

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Response: The paper by Notaro et al. (2007), in which I am the second author, is a separate research paper as compared with this paper. Notice that Notaro et al. (2007) has a slightly different title now. The new title of Notaro et al. (2007) is "Combined statistical and dynamical assessment of simulated vegetation-rainfall interactions in North Africa during the mid-Holocene." In my paper, I focused on the three major findings as outlined in my conclusion section. In particular, the timescale dependence of vegetation-precipitation feedback is a distinguished finding that has not been reported in other modeling and paleoclimate studies. Furthermore, the effect of background climate state on vegetation-precipitation feedback has not been reported in previous vegetation-climate interaction studies. On the other hand, Notaro et al. (2007) focused on the annual timescale interaction between vegetation and precipitation. They employed methods that include statistic approach and initial value ensemble simulations. In this short paper, we really want to initialize the whole vegetation-climate community with our three major findings that we reported here. We expect further investigations such as Notaro et al. (2007) to follow up in the near future.

Minor comments

p. 962, l. 26: I think that LPJ is used in a very different way in two models. What are possible implications of different coupling approaches for the feedback analysis?

Response: Yes, LPJ DGVM is coupled differently in two climate models. In substituting the original FOAM land component with the LPJ-based one, we retain the original FOAM surface/soil diffusive temperature calculation scheme that assumes 4-soil layers, but replaces the simple, single layer (bucket) soil water component with the 2-layer soil water scheme of LPJ (Sitch et al. 2003). However, we do not expect the different coupling procedures will affect our feedback analysis as the cause of the negative feedback comes from the competition between bare ground evaporation and plant transpiration when there is almost no difference in bare ground and vegetation reflectivity as in mid-Holocene North Africa. The coupling of CCSM 2 and LPJ can be found at Levis et al. (2004b, listed in my revised paper).

p. 964, l. 4-5: it would be better to provide spatial resolution in degrees, not number of grid cells.

Response: Yes, we have modified our revised paper to take into account this comment.

p.964, l. 6: 'the simulated pattern agrees with ...'. This is too general statement. Is there any disagreement, especially in North Africa? If so, provide more details here.

Response: Please see paper by Gallimore et al. (2005, Climate Dynamics), which I have referred in my revised paper, for more detailed discussion on the model performance and any disagreement between model simulated vegetation and observations. This short focused paper mainly deals with the vegetation-precipitation feedbacks in two climate models.

p. 967, l. 13-14: FPAR in this context is a fraction of photosynthetically active radiation ABSORBED by a plant canopy. Please specify this.

Response: This terminology has been revised accordingly in our revised paper.

p. 968, l. 1-3. Explain more in details what are relevant difference in soil components between two models.

Response: Accordingly, we have added more description about the soil components in these two climate models in our revised paper. In the CCSM, soil texture varies by grid-cell and with depth (Oleson et al., 2004). The data comes from the IGBP soil dataset (Global Soil Data Task 2000). Soil colors are from Zeng et al., 2002). This explains why vegetation and soils at 6K in the CCSM tend to have little albedo difference and sometimes in a direction opposite of the expected (Levis et al. 2004a). In the FOAM, soil texture is fixed for the integration. In substituting the original FOAM land component with the LPJ-based one, we retain the original FOAM CCM2-based surface/soil diffusive temperature calculation scheme that assumes 4-soil layers, but replaces the simple, single layer (bucket) soil water component (Harrison et al. 2003) with the 2-layer soil water scheme of LPJ (Sitch et al. 2003). The bare ground surface albedo

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only depends on the top layer soil moisture in our climate models. These references were added to our revised paper.

Fig. 2. A term 'Total Vege vs' on the figure labels looks clumsy. You can remove it without any consequence for understanding. Also, figure caption repeats the same peace of text three times. This can be easily avoided.

Response: This phrase has been taken out from our captions in our revised paper.

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