We are grateful to the reviewers for their constructive and insightful comments and suggestions. Please find our point-by-point response in the following.

Reviewer 1:

On page 2067, the authors propose that the desert-albedo feedback, or so-called Charney feedback, does not play any important role in the feedback in contrast to AEJ dynamics. They argue that a decrease in surface albedo with increasing vegetation cover should enhance surface temperatures, while a surface cooling is needed for enhancing meridional temperature gradients and thus strengthening the AEJ and Sahel precipitation. However, a decrease in surface albedo due to vegetation encroachment does not lead to a surface warming, but to a surface cooling; the gain in net-surface radiation enhances the latent heat flux, not the sensible heat flux. This is clearly shown in Claussen (Climate Dynamics, 1997) for present-day, artificial Saharan greening and in Claussen and Gayler (Global Ecol. Biogeogr., 1997) for mid-Holocene greening. Actually, further analysis with a similar model system (ECHAM5 – BIOME 1 instead of ECHAM3 – BIOME 1) reveals a strengthening of the AEJ in mid-Holocene climate (unfortunately, this study is a diploma thesis by Anne Dallmeyer at Universität Hamburg, 2008, which is available in German only). In conclusion, I am convinced that desert albedo feedback and AEJ dynamics are just two sides of the same coin, but not two different coins. ➔

Basically, we agree with the reviewer. The key quantity in the Charney feedback is moist static energy (MSE) as clearly pointed out in Charney, Stone, and Quirk (Reply, Science, 191, 100–102, 1976). MSE includes the effects of both latent heat and sensible heat. However, as we pointed out in our manuscript, "changing vegetation has no effect on low-level moist static energy in our CCSM3-DGVM simulations". As such, we see no reason to change any conclusions of our manuscript.

P. 2067, line 20: 'soil albedoes' does not seem to be a proper term. Better use 'soil albedo values' or 'values of soil albedo'. ➔ **Corrected**

Fig. 1: I am puzzled by the colour code. Using rainbow colours for displaying a quantity of the same sign but just different amplitude is puzzling. Does a striking green indicate a completely different quality than a striking red or a yellow? I would suggest using different shades of the same colour – perhaps green for vegetation coverage. Rainbow colours for the other figures are fine, because red clearly marks negative, and blue, positive precipitation differences, and vice versa, for temperature differences. ➔**revised**

Reviewer 2:

Specific comments: Page 2058, Lines 1-5: More references can be added. E.g. Renssen et al 2006a, who find a non-linear transition from humid to arid in the west Sa-hara and a more gradual transition elsewhere in the monsoon region. Also, Claussen et al 2013, who discuss the gradual / abrupt shift from the African Humid Period into the dry-Sahara period based on plant diversity ➔ **added**

Page 2059, Line 2-3: why do you choose to use the low-resolution version? (Is a higher resolution computationally too expensive?) ➔

Given the large number of sensitivity experiments that were necessary to disentangle the effects of dynamic vegetation on African climate as well as the global model domain and the rather long integration times required for achieving climate/vegetation equilibria in the fully coupled system, a higher resolution is still quite challenging with (always) limited computational resources. It is important to note, however, that the key elements of North African atmosphere dynamics (monsoon wind, Saharan high, AEJ, TEJ, etc.) are well represented in the low-resolution (T31) version of CCSM3.

Page 2060, Line 22-23: Using the present-day calendar results in some errors in the seasons and the exact dates of e.g. autumnal equinox (assuming you fixed vernal equinox), see references Joussaume and Braconnot 1997, Chen et al 2011. ➔

We agree with the reviewer. We used a fixed calendar based on on a 365 day year with vernal equinox fixed to March 21 (the Day/Month values refer to the present calendar). However, this does not affect the comparison of OAV with OAVf experiments for the same time intervals. Hence our results and conclusions are unaffected by a potential calendar bias. We added a statement in the manuscript.

Page 2062, Line 10: is this the location of the AEJ in your 0k(OAV) experiment or the actual observed AEJ location? Does CCSM3-DGVM correctly model the AEJ location and strength? ➔
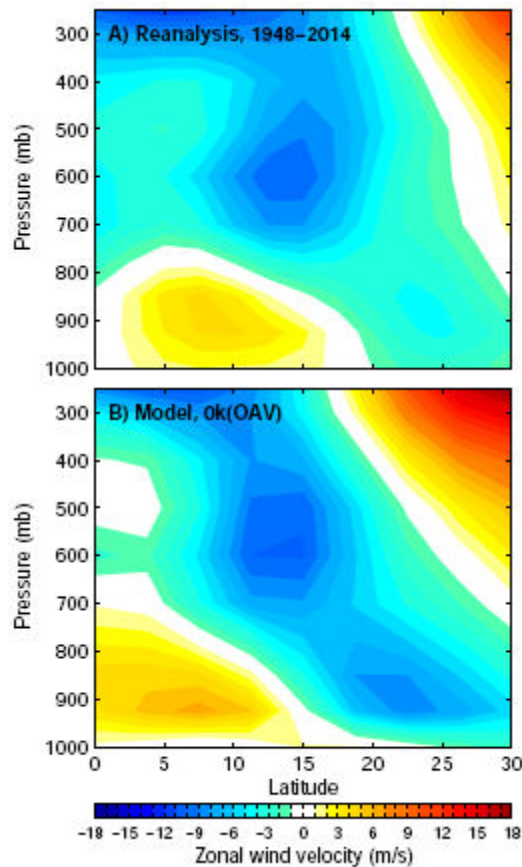

Figure 1. Latitude-height cross sections of July–September mean zonal wind velocity (m/s) over Africa between 20W-30E from the (a) NCEP/NCAR reanalysis and the (b) CCSM3-DGVM 0k(OAV). Positive (negative) values indicate westerly (easterly) flow. This figure shows that the AEJ location and strength in the model is reasonably captured compared to the observed data.

Page 2062, Line 25-26: the reduced surface temperatures in the monsoon region due to increased cloudiness and evaporation is also found by others (such as Braconnot et al 2007, Bosmans et al 2012) ➔ **added**

Page 2063, Lines 1-10: in the text you mention that the increase in ground evaporation in the OAVf experiments is much smaller than the rise in canopy evaporation and transpiration in the OAV experiments. However this does not match with the numbers in table 3. E.g. the 9k-0k difference in OAV evapotranspiration is 0.23+0.29=0.52 mm/day and the 9k-0k ground evaporation difference in OAVf is 0.45. Yes that is a bit smaller but I would not say "much smaller". For 6k-0k OAV evapotranspiration is 0.18+0.23=0.41 while OAVf ground evaporation is 0.34. Hence isn't there still a big change in latent heat flux (through ground evaporation) without vegetation? Maybe the standard error of the evaporation terms can be used to see if the differences in (OAV) canopy evapotranspiration are indeed significantly larger than the (OAVf) ground evaporation differences**.** Or does line 6 mean to say that "increase in ground evaporation (and thus total OAVf evapotranspiration) is much smaller than the total OAV evapotranspiration"? **Yes, the statement should refer to the total OAV evapotranspiration. We changed "canopy" by "total". We apologize for this confusion.**

Page 2063, Line 13 & Figure 5: is m/s the correct unit for moisture transport? (**YES**) Please specify how you computed moisture transport (using specific humidity and winds perhaps?). ➔ **Yes, we used modelled moisture transport UQ and VQ, where U is the zonal wind (m/s), V is the meridional wind (m/s), Q is the specific humidity (kg/kg).**

Page 2065 Lines 6-27: The changes you find in AEJ strength seem indeed to match nicely with surface temperature changes through the thermal wind relation. However you also mentioned earlier that the AEJ is the southern outflow of the Saharan High, so does the Saharan High weaken too? Only if the Saharan High strength doesn't change you can fully relate the AEJ changes to surface temperature changes. However the Saharan High is likely shifted / changed in your early and mid-holocene experiments as you find a northward extension of the monsoon trough

No weakening of the Saharan High was found in the experiments. We therefore relate the mid-tropospheric circulation changes to surface temperature changes via the thermal wind relation.

Page 2066 Lines 5-7: at 700 hPa there is quite an increase in southwesterly moisture transport from ocean to land when dynamic vegetation is included (Figure 5e,f), especially for 6k, probably related to stronger winds (Figure 4e,f). Is 700 hPa what you mean by "low-level"? **No, low-level refers to the boundary (or monsoon) layer. For clarification we added "near surface" to the sentence.**

Page 2066, Lines 27-28: For OAVf experiments you find that the ratio of total precipitation to advected precipitation increases, i.e. for 6kOAVf and 9kOAVf increased precipitation is largely due to local water recycling. This is opposite to previous studies, who find a large contribution of moisture advection in fixed-vegetation experiments (e.g. Marzin and Braconnot 2009, Bosmans et al 2012.

Please do not misinterpret the meaning of the recycling coefficient. An increasing recycling coefficient does not necessarily imply that moisture advection is reduced or

plays no role. Rainfall increases due to both local recycling and moisture advection. There is no contradiction to the mentioned studies.

Page 2067, Line 11-23: How big is the albedo change due to the vegetation changes in your results? Your claim is that albedo decreases (which would allow more radiation absorption and thus warming?) do not weigh up to the increased evapotranspirative cooling? Also, your statement about the CCSM3-DGVM model having very low albedo for saturated soils and therefore diminishing the effect of vegetation on albedo, does this imply that the model underestimates albedo in certain (saturated) regions? ➔

Summer (JJAS) albedo values (We included these numbers in the revised manuscript) over 10°N-25°N and 20°W-30°E:
6k(OAV)= 0.2088
9k(OAV)= 0.2044
0k(OAV)= 0.2183
6k(OAVf)= 0.2083
9k(OAVf)= 0.2057
0k(OAVf)= 0.2166
9k0k(OAV-OAVf)= -0.003
6k0k(OAV-OAVf)= -0.0012

It is true that the model underestimates albedo in certain regions, such as the saturated regions (cf. Levis et al., 2004). However this underestimation is due to too much rainfall in the model rather than the parameters in the land model. We admit that it is unclear how realistic the albedo parameterizations in the model are. In order to cope this uncertainty, further sensitivity studies would be required with different parametrizations for surface albedo (see below).

Page 2067-2068: The independence of (mid-) Holocene model results of initial conditions was also shown by Renssen et al, 2006. Furthermore, the idea of specific sights having either an abrupt or gradual transition was also put forward by Claussen et al, 2013, based on plant diversity. ➔ **added**

Discussion in general: Please specify that results might be model dependent (specifically for DGVM, is your result robust if you were to use different PFTs, more PFTs, a more frequent update of vegetation structure and PFT population densities, a different parametrization for albedo, higher resolution etc.)

We added a paragraph regarding potential model-dependencies. We agree that further sensitivity studies are needed to assess the robustness of our results. Besides model resolution and PFT characteristics in the vegetation model, sensitivity studies should particularly address the effects of different parameterizations for land surface evaporation, transpiration and albedo.

Figures 6b, 7: axes labels are a bit small, please enlarge ➔ **revised**

In conclusion, I would consider the scientific significance of this paper as "good". Its conclusions contribute to the understanding of vegetation feedbacks in orbital forcing of the North African monsoon, but the feedback suggested here (through canopy evaporation and

transpiration) are likely model-dependent. The scientific and presentation quality of this paper are "excellent".

Additional References:
Claussen, M., Bathiany, S., Brovkin, V., & Kleinen, T. (2013). Simulated climatevegetation interaction in semi-arid regions affected by plant diversity. Nature Geoscience, 6(11), 954-958 → **added**

Chen, Guang-Shan, et al. (2011): Calendar effect on phase study in paleoclimate transient simulation with orbital forcing. Climate dynamics 37.9-10: 1949-1960. → **added**

Joussaume, S. and Braconnot, P. (1997): Sensitivity of paleoclimate simulation results to season definitions, J. Geophys. Res., 102, 1943–1956. → **added**

Marzin, C., & Braconnot, P. (2009). Variations of Indian and African monsoons induced by insolation changes at 6 and 9.5 kyr BP. Climate dynamics, 33(2-3), 215-231. → **added**

Renssen, H., Brovkin, V., Fichefet, T., & Goosse, H. (2006a). Simulation of the holocene climate evolution in northern africa: The termination of the african humid period. Quaternary International, 150(1), 95-102. → **added**

Renssen, H., Driesschaert, E., Loutre, M. F., & Fichefet, T. (2006b). On the importance of initial conditions for simulations of the mid-holocene climate. Climate of the Past, 2(2), 91-97. →**added**

Comment to the Editor (Herzschuh) as the conclusions:

The climate-vegetation feedback identified in this study is closely linked to the characteristics of the atmospheric circulation over North Africa (e.g. AEJ). As such, this type of feedback does not apply to other monsoon regions. However, the North African climate-vegetation feedback should work also in other epochs than the Holocene. We added a statement in the manuscript.