Reply to Reviewer #3

Firstly, we would like to thank the reviewer for his/her time and effort in providing us with a highly detailed and constructive review of our paper, "The Impact of Sahara desertification on Arctic cooling during the Holocene". Please find below our responses to the points and comments raised by reviewer #3.

This paper presents a collection of sensitivity experiments using the Earth System Model of Intermediate Complexity (EMIC) LOVECLIM1.2. This model allows for a large number of sensitivity studies that would otherwise not be possible for this type of study. The main goal of the paper was to identify the variability in cooling in the Arctic associated with simulated desertification that occurred in the Sahara during the transition from the Holocene to modern. The expanding literature continues to recognize the importance of the role of land surface changes on Northern Hemisphere climate. Paleoevironmental teleconnection modeling studies of this type form an important basis for understanding transient climate change, in connection with proxy data validation, and therefore are well suited to discussion in Climate of the Past.

Comments:

In the Experimental Design section the discussion of how the LIS and GIS meltwater fluxes are not included (P1657L8) should be discussed just after the OGGIS simulation is described or page 1656L14. So a reorganization of section 2.2 is necessary.

REPLY: We agree with reviewer #3 that a reorganisation of section 2.2, with regards to the explanation of the how the LIS and GIS meltwater fluxes are not included, is required. In the revised manuscript we shall do this, as well as explaining in further detail our motivation for not including them as this has been asked for by other reviewers.

In the results section on from P1658L17 to P1659L11 there is some discussion of the possible atmospheric and ocean mechanisms involved in connecting changes in the Arctic with changes in the Sahara. I feel the ideas in this section need to be addressed in further detail with more thought put into where the model may accurately describe the dynamics and thermodynamics involved in the discussed mechanisms. Additionally, deficiencies in the model (not just clouds in the later sections) should also be addressed.

REPLY: Again we agree with reviewer #3 that the manuscript will benefit from further discussion on other deficiencies within the model. This point has been raised by all reviewers, so we shall address this issue in detail within the revised version of the manuscript. In particular, the decoupling of cloud cover and precipitation, and how precipitation is generated within the model.

There is a brief mention of the Azores high and Icelandic low that suggests some thought has gone into the connection with the variability of the North Atlantic Oscillation (NAO). Furthermore, there may be (long timescale) changes in this mode and other modes of climate variability such as the East Atlantic Mode (e.g. Barnston and Livezey 1987). This raises the question of the fidelity of the LOVECLIM model in reproducing the major modes of climate variability as compared with observation. There is no mention of this in the paper, and the description of LOVECLIM in Goosse et al 2010 does not seem to address the models ability in simulating these main models of variability (e.g. ENSO, NAO, AMO, PDO etc). Some discussion in the literature (Mairesse et al. 20130 suggest a more negative NAO during the 9-6 ka period in this model with a weaker meridional gradient and weaker westerlies. This seems to contradict the changes in winds observed between 9k0kEQ_OG-9k9kEQ_OG as the simulations transition towards modern. Although there seems to be no robust changes in the PMIP2 set of model with respect to this issue (e.g. Gladstone et al. 2005) these issues should be expanded up here.

In addition to changes in seasonality from orbital changes, changes in albedo from a darker more vegetated Sahara (9k-6k) to a contrasting dark ocean and bright desert (0k) are expected to impact the zonal circulation in the region and the downwelling of dry air over land from 9K to 0K. This should also be discussed more in context to the comments above. For changes in the meridional patterns the author can strengthen their discussions on changes in the Hadley circulation and shifts in the intertropical convergence zone (ITCZ). There is a body of literature emerging on the teleconnections between the position of the ITCZ and changes in the tropics and change in landcover in other areas (e.g. Liu et al 2014).

REPLY: Reviewer #3 is correct to raise the issue the potential effect of atmospheric modes on the teleconnection we described within our paper. However, we feel that given that we are looking at a first-order response of the long-term effect of radiative forcing on the termination of the African humid period, the variability of these modes is not likely to have such a long term effect. It must be remembered that for us to explore this issue required the simulation of an equivalent of 83000 years, which would only be possible in a global, 3-dimensional model in an EMIC like LOVECLIM, as these models are specifically designed and implemented to study long term climatic responses. As a next step we would hope the issue raised in our paper is explored further by groups using more complex models. In addition, as the reviewer correctly highlights, our general understanding of the variability of the various atmospheric modes is quite poor, and as the Gladstone *et al*. (2005) paper demonstrates, a collection of 9 models involved within the PMIP2 series of experiments fail to clearly demonstrate a clear indicator of how the NAO varied since the mid-Holocene. Therefore, any detailed discussion on this and the linking of our results to a specific variability of the NAO would be viewed as highly speculative and non-constructive to the main point of our paper, that of a first order impact assessment of radiative forcing due to the termination of the African Humid period.

The change in atmospheric heat transport is briefly discussed (P1659L5-11). Is the heat transport in Fig 4 mainly from latent heat transport from transient eddies (V'Q') or the total transport including sensible heat transport (VQ+VT) (This is not clear)? It is pointed out that the majority of the transport is from eddy latent heat transport, but on the other hand the Bjerknes compensation is mentioned where the relative balance operates on the total global atmospheric transport between land and ocean.

REPLY: This is the total heat transport VQ + VT, this will be clearly expressed in the revised manuscript

Equally critical to these sensitivity modeling studies is validation with proxy based climate data. The section from P1659L19 to P1660L3 does not adequately address the literature on the how the model performs in the transition from the Holocene optimum to present day.

REPLY: Indeed this is a good point that reviewer #3 has raised and we shall include additional discussion which relates the modelling studies to proxy studies, and how LOVECLIM performs in general. There are numerous papers written where the performance of LOVECLIM has been compared to proxy records, and so these will be discussed within the context of this study. These papers include: Renssen *et al*. (2005ab); Renssen *et al*. (2006ab); Renssen *et al*. (2009); Blaschek and Renssen (2013).

There is a long section (compared to the rest of the paper) on pages 1660-1661 that addresses the issue on the prescription of modern clouds in the model and the impact of this on the sensitivity of the model. The section begins with a discussion that initially concludes that the surface of a cloudy and vegetated region can absorb the same amount of incoming solar radiation; I suppose to motivate the next set of sensitivity experiments. Considering the fact that the surface net longwave, sensible and latent heat balance will be completely different between vegetated and desert, the previous discussion seems a bit pointless. The authors might consider revising their motivation for this next set of sensitivity experiments. Even using the cloud cover based on the modelled Amazon distribution seems like a highly speculative endeavour (e.g. many IPCC models show difficulty in obtaining the same modern precipitation regimes over the Amazon). There are only a couple of cloud simulations in the large set of sensitivity experiments, so these simulations could be reconsidered.

REPLY: A major weakness of our first order set of experiments is the fact that LOVECLIM relies on a fixed cloud cover dataset, which is based on a modern climatology. Therefore, cloud cover over the Sahara would always be representative of a desert environment, even during early to mid-Holocene simulations. Therefore, to test the impact of this on our results, it was important that not only did we address this issue within our paper, but that we also designed a series of sensitivity experiments to test how this effects our results and the hypothesis of the teleconnection between the low and high northern latitudes.

A major premise of the series of equilibrium experiments is that the Sahara at 6 and 9k was warmer than present day. This in itself is a contestable result because it has been observed that the present day Sahara is warmer than vegetated regions of a similar latitude. Therefore, it is assumed that at 6 and 9k, a vegetated Sahara would be cooler than present day. However, it is not possible to confirm or refute this due to a lack of proxy data for the early and mid Holocene for the Sahara region. However, if in our sensitivity experiments, that include the addition of 'vegetated' cloud cover over the Sahara at 6 and 9k, the Sahara is cooler than present day conditions, then the teleconnection between the Sahara and the Arctic we describe, would no longer hold. Therefore we needed to explain this.

In LOVECLIM, the total upward and downward fluxes are calculated as a function of the modern data ISCCP D2 dataset. Whilst this is a reasonable first order approximation for the Sahara at modern day, it is not so during periods when the Sahara was vegetated, i.e. 6 and 9ka. This is because having a cloud cover that is essentially associated to a desert environment, as opposed to a vegetated environment, there is less clouds than there would be in reality. Hence, the simulations at 6 and 9k would ignore changes in cloud albedo. These changes in cloud albedo would approximately balance impacts from changes in surface albedo, which we do include within our simulations. As this calculation approximately balances (which is why we included this in our original manuscript), the temperature differences seen between 6k100gEQ and 6k100gEQ_clouds, and 9k9kEQ_OG and 9k9kEQ_OG_clouds, would depend on differences between the latent and sensible heat fluxes, with less moisture available, lower latent heat flux, high sensible heat flux and warmer surface temps for the 6k100gEQ and 9k9kEQ_OG simulations respectively. However, what we see is that the temperature differences between 6k100gEQ and 6k100gEQ_clouds, and 9k9kEQ_OG and 9k9kEQ_OG_clouds are within the bounds of the error and can thus be considered negligible. Therefore, based on these tests, we can say that the major premise of our original experiments, that the Sahara was warmer at 6 and 9ka, holds within our sensitivity experiments and thus the teleconnection described between the Arctic and Sahara is robust.

We agree with reviewer #3 that the original description of the surface heat fluxes in our manuscript need to be put in a greater context as described above, and we shall address this in our revised manuscript.

With regards to using the Amazon cloud cover, as mentioned previously, in LOVECLIM precipitation does not depend directly on cloud cover, so we are not simulating an Amazon precipitation regime over the Sahara.

Finally, with the added explanation of the motivation for the sensitivity experiments, we feel it is not necessary to include further experiments with clouds.

The ocean model components of EMICs are often the most sophisticated and resolved component of the coupled system, yet there is little discussion of the changes in ocean circulation through the Holocene to modern transition especially in the OGGIS simulations (e.g. the connection between changes in Sahara albedo, SSTs, deep water formation an sea ice distribution are probably quite important if there are any significant changes).

REPLY: We agree with reviewer #3 that the ocean model of LOVECLIM is the most sophisticated and resolved component of the coupled system. The suggestion that we include discussion of the changes in various oceanic components through the transition from the mid-Holocene to the present day is not really possible in our experimental set up. In our opinion, this topic is outside the focus of this study, and moreover, the transient response of the Holocene ocean circulation in the our model has been the subject of several recent papers (Renssen et al. 2006a, Renssen et al. 2010, Blaschek & Renssen, 2013). So for a detailed discussion we would prefer to refer the reader to the latter papers. However, we shall include reference to these papers in the revised manuscript.

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