Reply to Reviewer #2

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 #2.

This study investigates the potential impact of Saharan desertification – or expansion – on Arctic temperatures during the last 9 kyr. The authors apply the LOVECLIM model to isolate the climatic effects of changes in Saharan vegetation during the Holocene based on transient and equilibrium climate simulations. The authors conclude that the expansion of the Saharan desert, and associated changes in surface albedo, contributed significantly to decreasing temperatures in the Arctic during the Holocene. According to the model simulations, as much as 42% of the Holocene Arctic cooling can be attributed to the expansion/desertification of Sahara.

This interesting study highlights the importance of land-atmosphere teleconnections for understanding climate change, and the topic is highly appropriate for Climate of the Past. As such, this contribution may serve as an ideal starting point for a discussion of this important aspect of Holocene climate change. In general, however, I feel the authors are somewhat uncritical with respect to potential shortcomings of the model approach and results. The paper would benefit from a more thorough discussion of these aspects as well as a brief discussion of how the model results compare to actual reconstructions of Arctic climate change during the Holocene, an aspect that is entirely lacking.

Contribution from Saharan expansion to Arctic cooling

The impact of desertification in Sahara on Arctic cooling is quantified through a set of equilibrium experiments. However, the LIS and GIS melt-water fluxes were not included in the OGSIS equilibrium simulations, but including them "would result in constant freshening of the ocean" - preventing the oceans from reaching a quasi-equilibrium state. The authors state that "neglecting the melt fluxes likely resulted in a marginally warm early Holocene climate" (L5-13, P. 1657). However, this constant freshening of the ocean is likely to have slowed down the AMOC significantly, which would have contributed to Arctic cooling throughout parts of the Holocene. In general, the numbers quantifying the impact of Saharan desertification must be very uncertain considering the processes omitted in the equilibrium simulations. The paper would benefit considerably from a brief discussion of these aspects, which would place the impact of the Saharan desertification in a more realistic context.

REPLY: The focus of this paper is on the climatic impact of Sahara desertification due to radiative forcing, and comparing this to the effects of other long-term radiative forcings such as changes in orbital parameters and greenhouse gas levels, and changes in surface albedo due to ice sheets and vegetation. When designing our experiments for this study it was decided the most appropriate way was to perform a series of carefully designed equilibrium experiments that would allow us to capture a first order impact of radiative forcing of the Sahara upon Holocene climate. Therefore, by definition all the simulations we performed needed to be in equilibrium. This implied that when performing the 9k equilibrium simulations it was not possible to include LIS and GIS meltwater freshwater forcings for the duration of the equilibrium simulations, as then the simulations would have never reached an equilibrium state. Even if we did include them, it would make no sense to include the varying meltwater fluxes of LIS and GIS, because then we would have to include varying GHG and orbital forcings as well, to keep the experiments consistent. However, clearly this would leave us with a series of transient experiments and not equilibrium experiments. In addition, a previous comparison of transient experiments with and without LIS melt fluxes that were performed with an earlier version of our model, have shown that the impact of LIS melt was modest (0.5°C cooler in the Arctic between 9 and 8 kyr BP, Renssen et al. 2009). Likewise, other experiments have suggested that the impact of GIS melt was only locally of importance (Blaschek and Renssen, 2013). Therefore, to make our sensitivity analysis as straight forward as possible, we decided to omit this potential source of freshwater forcing to the high northern latitudes. To clarify this in our manuscript we will reiterate this point further to make it clear We are confident that our experiments are suitable to assess the first-order impact of radiative forcings on the Holocene climate. As a next step, more comprehensive models could be used to perform additional experiments that include the impact of meltwater fluxes.

To place an upper limit on the potential impact of desertification in Sahara, the authors "simulate extreme early (9 ka and late (0 ka) Holocene environments". These simulations, which were carried out with the OG model that disregards the ice sheets and melt-water fluxes, are based on extreme vegetation changes that exceed those estimated from pollen and macrofossil data (Joly et al., 1998). Therefore, to state that "the modeling results indicate that up to 42% of the cooling in the Arctic over the period 9ka – 0ka was a direct result of the desertification in Sahara" (L5, p 1654) appears a little misleading – and highly uncertain - considering the limitations of the approach. Here, the authors seem to uncritically accept the model results without any discussion of the context and assumptions involved.

(NOTE: It is a bit unclear how the number "42%" is computed – the authors refer to a temperature decrease of 4.0 C (Fig. 2d), but it doesn't look like the average temp difference north of 66.5N is 4.0 C. Guess it refers to the difference between the first and last row in table 2, but it is unclear to me how the 1.7C contribution due to 100% desertification is obtained?).

REPLY: We should stress here that our experiments with 0% or 100% vegetation cover in the Sahara were set up as idealized sensitivity experiments that were used to analyse the extreme responses. So, the extreme vegetation ranges of 100% to 0%, as opposed to the reconstruction by Jolly et al. (1998), of 90% to 10% were chosen for ease of computation. If we had decided to choose 90% as an upper limit then we would be left with several questions:

-How do we allocate this 90% vegetation at 9k? Do we do it per grid cell? or over the entire Sahara domain? If over the entire domain, which regions do we leave arid?

It should also be noted that there is no proxy data available for the spatial extent of vegetation at 9k. So an attempt to prescribe the 90% vegetation cover suggested by Jolly et al. (1998) would have resulted in considerable uncertainty. Therefore, we opted to go for 100% to 0% for these reasons.

However, we do agree with reviewer #2 that we need we have shown too much confidence in our upper limit and we shall clarify this in our revised manuscript, whilst clearly stating the assumptions associated with this upper limit. In addition we shall place more emphasis on the upper limit being less than 42%.

To clarify the calculation of the 42%:

The figure for the contribution of the Sahara to Arctic cooling over the Holocene in an extreme environment is calculated using the following set of equations. The following equations shall be added to the appendix.

9k100gEQ_OG – 0k100dEQ_OG = Δ° C due to ALL forcings

9k100gEQ_OG – 9k100dEQ_OG = Δ° C due to Sahara VEGETATION forcings

9k100gEQ_OG – 0k100gEQ_OG = Δ° C due to ORB & GHG forcings

(-10.5 - -14.5) = 4.0°C due to ALL forcings

(-10.5 - -12.1) = 1.6°C due to Sahara VEGETATION forcings

(-10.5 - -12.5) = 2°C due to ORB & GHG forcings

=> 1.6°C is 40% of 4°C

However, the values given in the manuscript are given to 1 sf. whereas the original calculation of 42% used data that extended to 2 sf. In that the calculation was 1.64/3.95 which gives a value of 41.5%. In the revised manuscript we shall use the data to 1 sf for consistency.

Also, I don't understand why the Arctic appears colder in Fig. 2a compared to Fig. 2d, which was computed with extreme differences in vegetation?

REPLY: Reviewer #2 makes a very good observation in that figure 2a) depicts a change in Arctic temperature of -2.9°C, where as 2d) is supposed to show a -4°C change. One can see that this is not the case and after checking, the incorrect figure has been included in the manuscript. In the revised edition the correct figure will be included. This does not affect our conclusions.

Another limitation in the model approach concerns the prescription of clouds in the LOVECLIM model – as explained by the authors in the paper. To address this problem, the authors carry out sensitivity tests with a cloud cover over Sahara at 9 ka that resemble the modern cloud cover prescribed for the Amazon region in the LOVECLIM model. This is a reasonable first-order test that takes into account changes in the radiation balance, but it seems to ignore related changes in precipitation and the hydrological cycle in general. It is unclear if the hydrological cycle is allowed to vary accordingly or if it is decoupled from the cloud cover. These aspects are important to assess the validity of the model results and should be discussed in more detail.

REPLY: As explained in detail in our response to reviewer #1 (please find details there), in LOVECLIM the precipitation does not depend directly on cloud cover. Precipitation is calculated based on the total precipitable water content between the surface and 500 hPa, which is a prognostic variable in the model (Goosse et al., 2010). The model includes a full hydrological cycle, including a bucket model for soil moisture and runoff routing to the oceans. As the relationship between cloud cover and precipitation is not clear, we shall clarify this in the revised manuscript

(The discussion of how the prescription of a modern Amazon-like cloud cover at 9 ka influences Arctic cooling is quite confusing (L 10-20, P. 1661) – it is somewhat unclear whether the discussed changes refer to changes between 9ka and 0 ka, or changes relative to those obtained with a modern Saharan cloud cover throughout the Holocene).

REPLY: We agree with the reviewer #2 that the discussion about how we calculated the cloud cover from the Amazon is somewhat confusing, and we shall clarify this in the revised manuscript.

Comparisons to climate reconstructions

Given the limitations of the model approach, it is really hard to assess the degree to which the model correctly captures the dominant processes as well as the actual climatic changes that took place during the Holocene. It would have been informative to include comparisons to proxy-based reconstructions of changes in Arctic climate during the Holocene – as well as changes in Saharan climate, but I acknowledge that such reconstructions are sparse. Several studies discuss Holocene changes in Arctic climate (e.g. Wanner et al., QSR, 2008; Vinther et al., JGR, 2005; Kaufman et al., QSR, 2004) and this study would benefit from placing the model results into such a context. For instance, do the proxy-based reconstructions show any notable change when the vegetation changes supposedly accelerated, or was the change too gradual?

REPLY: Again we agree with reviewer #2 that the manuscript would benefit with a review of the model's performance when compared with proxy-based reconstructions. Several Holocene modelling studies using LOVECLIM have been undertaken that compare model results to proxy reconstructions. For instance, Renssen et al. (2005ab)

have compared the model's response to orbital and greenhouse gases with proxy-based reconstructions of temperature in the Arctic and Antarctic. Renssen et al. (2009, 2010) included in addition the impact of the remnant Laurentide Icesheet, enabling them to explain the spatial and temporal complexity of the Holocene Thermal Maximum in the mid- and high northern latitudes. As well as including greater reference to these papers will be included in the revised manuscript,

A more in-depth assessment of the model limitations and a discussion of how the model results link up with reconstructed changes in Arctic climate would increase the relevance and impact of this study. However, the study represents an excellent starting point for a discussion of land-atmosphere teleconnections as well as the factors contributing to Arctic cooling during the Holocene.

<u>Additional references</u>

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