Reply to Reviewer #1

Firstly, we would like to thank the reviewer for his/her time and effort in providing us with a highly detailed and informative 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 #1.

The authors present a theoretical study in which they explore the impact of the expansion of the Sahara on the cooling of the Arctic at the end of the African Humid Period. They use a model to perform a number of sensitivity experiments to isolate the effects. The set-up of experiments is suitable for this exercise. However, I have some doubts whether the model used is really suited to address the problem in question.

As the authors explain themselves, a major weakness of their model is the assumption of constant, present-day pattern of cloudiness. The authors try to give an estimate of the consequences of this assumption on their results by modifying cloudiness over the Sahara only. They argue in terms of changes in albedo and near-surface temperature in this region. But that is only a small part of the problem. What about changes in near-surface heat fluxes, moist static energy, precipitation and the atmospheric energy-cycle in general? Does the model allow for changes in precipitation without any change in cloud cover? This would imply decoupling of the atmospheric radiation budget and the atmospheric energy cycle. Hence, I doubt that the experiments presented allow for any sensible conclusions regarding changes in the global energy cycle – unless the author can convincingly demonstrate that moist thermodynamics is not important in this case.

Regarding style, the paper reads a bit technical. Data and other model results are considered rather uncritically. For example, Jolly et al., 1998 did not claim that the Sahara was covered by grass and shrubs. They suggested that vegetation migrated perhaps as far north as 23 oN. In the same direction, comparison of the simulated extent of Saharan greening with results of another coarse-resolution model (p 1659 l 20 – 25) to conclude that the model over- or underestimated Saharan greening is not very reasonable.

Minor comments:

The term "desertification" is used in relation to anthropogenic overexploitation of marginal land. Using "aridification" or just "expansion of the Sahara" would be less confusing.

REPLY: This study is designed to provide a first order impact assessment of long-term radiative forcing on the Sahara during the Holocene, which will hopefully lead to further studies with more complex modelling set ups. As reviewer #1 comments, the set up of the experiments is suitable for this exercise, in that it allows us to isolate these effects. Of course, in an ideal world we would have preferred to have used a model with a more complex atmosphere, because of the limitations that the current atmosphere model we use has. However, it must be remembered that this is a sensitivity study, focused at providing a first order assessment of long-term radiative forcing on the Sahara during the Holocene. To achieve this goal, it is required to perform a large suite of equilibrium experiments, with a total duration of 83000 years of simulation. It is clear that this would have been impossible with a more comprehensive model like a state of the art general circulation model (GCM) and we argue that our global, 3-dimensional Earth model of Intermediate Complexity (EMIC) model LOVECLIM is an appropriate tool to perform this study.

We agree with reviewer #1 that the calculation of precipitation and the link with could cover could be explained more clearly. The underlying physics is discussed in Goosse *et al*. (2010) which is a comprehensive explanation of the model design. But to clarify and simplify this further for reviewer #1 we have provided an explanation of the issues raised by reviewer #1:

The atmospheric component of LOVECLIM is ECBilt, a quasi-geostrophic dynamical atmospheric model (Haarsma et al., 1996; Opsteegh et al., 1998). The precipitation within the model is decoupled from cloud cover, in that the occurrence of precipitation does not directly depend on the presence of clouds. Instead, precipitation in ECBilt depends on the total precipitable water content between the surface and 500 hPa, which is a prognostic variable in the model. As explained in Goosse *et al*. (2010), this variable is transported horizontally using a fraction (60%) of the sum of geostrophic and ageostrophic winds at 800 hPa to account for the fact that humidity is generally higher close to the surface where wind speeds are lower. It is assumed that the atmosphere above 500 hPa is completely dry and all water that is transported above 500hPa is converted to precipitable water and thus precipitates. Precipitation also occurs in layers below 500hPa if the total precipitable water passes a threshold. This threshold is defined by 0.83 times the vertically integrated saturated specific humidity, assuming constant relative humidity within the layer.

As mentioned, the cloud cover is fixed according to a modern climatology. The rationale behind this is that at the relatively low resolution of ECBilt, a reasonable calculation of cloud physics cannot be expected, and that prescribed cloud cover provides a more realistic representation of clouds. In the radiative scheme of ECBilt, the cloud cover affects the planetary albedo and the downward and upward longwave fluxes (see Schaeffer *et al*., 1998; Goosse *et al*., 2010). The radiative scheme and the thermodynamic exchanges between the atmospheric layers and the surface are computed in the model's physical space, consisting of three atmospheric layers, with temperature obtained at the surface and 650hPa and 350hPa. Near surface heat fluxes of sensible and latent heat are computed from estimates of temperature, humidity and wind speed at 10m and from the characteristics of the surface using standard bulk formulae.

Due to the resolution of the model, we acknowledge that the full complexity of the atmosphere is not resolved, as would be in a GCM. But as discussed in considerable detail above, the aim of the experiments are to provide a first order impact assessment of long-term radiative forcing on the Sahara during the Holocene. Given the usage of LOVECLIM in over 100 previous studies, and the volumes of documentation that exists regarding the model, its performance at a variety of spatial scales, we are confident in the performance of the model in giving a first order approximation of the atmosphere and its numerous processes.

The reviewer also notes that the style of the paper is a bit technical. This is related to the large number of sensitivity experiments that we have performed for this study. In our view, it is important to explain the design of all these experiments, and we see no possibilities to make the paper less technical at this point. We do however welcome any suggestions.

A further issue concerns the point that proxy data and other results were not considered critically. We have several reasons for this. Firstly, to our knowledge there exists no appropriate study to directly compare our results to. All other studies have looked at African

Humid Period (AHP) from the perspective of how the climate affects the vegetation, whereas we approached this from the perspective of how did the vegetation changes affect the climate. In terms of proxy data, there exists numerous studies with LOVECLIM that discuss the simulated transient Holocene climate versus proxy data at various latitudes, such as Renssen *et al*. (2005ab, 2006b; 2009, 2010) and Blaschek & Renssen (2013). We refer to these papers for a discussion of the transient Holocene climate response in the model and a comparison to proxy-based reconstructions. As mentioned, the present paper is a sensitivity study that is based on idealized equilibrium experiments that are less suited to compare to proxy-based reconstructions. In relation to the comment by reviewer #1 that "Jolly did not claim that the Sahara was covered by grass and shrubs. They suggested that vegetation migrated perhaps as far north as 23°N". Indeed Jolly *et al*. (1998) suggested that the area that now defines the Sahara was extremely reduced, with nearly all sites of their study (except southern Egypt) were either steppe, woods/scrubs or mixed forest (Jolly et al., 1998). Similar wording to our manuscript was also used by Claussen *et al*. (1999), so we disagree with reviewer #1, and their assessment of our original claim. However, to clarify this minor point in the introduction we shall rephrase this part of the manuscript.

Another point raised by reviewer #1 is that "comparison of the simulated extent of Saharan greening with results of another coarse-resolution model (p 1659 l 20 – 25) to conclude that the model over- or underestimated Saharan greening is not very reasonable". We would disagree with this assessment by reviewer #1. The 'coarse resolution' model we compared our results to was the CLIMBER2 model applied by Claussen *et al*. (1999). These authors where the first to provide a transient simulation of the Holocene climate in Africa. This is a bench-mark paper that serves as a key reference for subsequent modelling studies. As such, we argue that it is very useful to compare our results to the work of Claussen *et al*. (1999). In addition, Claussen *et al*. (1999) compared their results to the proxy reconstructions of Jolly *et al*. (1998). In the work from Jolly *et al*., (1998) the percentage vegetation range form their site is given from 90 to 10% over the course of the Holocene, with a dramatic, rapid change occurring during the mid-Holocene. The work of Claussen *et al*. (1999) was successfully able to replicate those results. Therefore, we argue that is not highly unreasonable to compare our work with a modelling study that is able to successfully replicate a proxy reconstruction study that is widely accepted to be an important contribution to the field.

But overall, we agree that we need to include more comparison with other proxy-based studies, and shall include reference to other studies, particularly where LOVECLIM has been used in model-proxy based studies (Renssen *et al*., 2005ab; Renssen *et al*., 2006ab; Renssen et al., 2009; Blaschek and Renssen 2013). However, given the paucity of low latitude proxy reconstruction data, as we originally mentioned in our manuscript (Pg 1661 Ln: 6-9), it is to our knowledge not possible to verify <u>temperature</u> results against proxy reconstructions for low latitudes. If reviewer #1 knows of such papers, we would be grateful to receive the references.

The final minor point raised by reviewer #1 is that the term desertification should be replaced by 'aridification' or 'expansion of the Sahara', to avoid confusion. In our view, however, desertification is not a confusing term, as it has been widely used in the literature on "The Termination of the African Humid Period". Therefore, we prefer to continue using desertification.

Additional references

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