**"Obliquity forcing of low-latitude climate"** by **J. H. C. Bosmans et al.**
**Author response to anonymous referee #1**

This paper analyses climate simulation results from a complex Earth system model to understand the influence of obliquity on low-latitude (thus tropical) climate. In detail, it is a more extensive (global) analysis of simulation results which are already described elsewhere, where previously the focus was more local (Africa, Mediterranean).
I found the described idea interesting. Thus, in principle this paper should be published in Climate of the Past. However, I believe some more details need to be shown and discussed before the idea is really convincing and supported by the paper. Thus, I have concerns about three major issues, (a) on the originality of the proposed idea, (b) on what the experiment is really telling us and (c) on a missing wider discussion.

**(a) Originality of the proposed idea:**
Right now the paper cites as original source of the Summer Inter Tropical Insolation Gradient (SITIG) hypothesis a paper by Lourens and Reichart (1996). It turns out that this is one chapter out of the PhD thesis of G.J. Reichart, which never made it in the peer-reviewed literature. While L Lourens is coauthor of the present paper here, G.J Reichart is not. Furthermore, from the given citation it was not possible to figure out who the overall author of the PhD thesis was. I found this proceeding questionable. Since this chapter by Lourens and Reichart (1996) was never published in peer-reviewed journals (for reasons we do not know), it needs to be considered unpublished. It is similar to a paper submitted to Climate of the Past Discussion, that is rejected: The paper is available online in submitted, but not in peer-reviewed form. I urgently suggest, this is not a reference, that should be used here. If any it might be mentioned in the discussion that some ideas on SITIG were proposed in the whole PhD thesis of GJ Reichart (then citing Reichart 1996, PhD thesis, not a single chapter). So one needs to acknowledge that the origin for any kind of low-latitude index on climate was the monsoon index proposed first by Rossignol-Strick (1983) (not cited by the authors, the authors cite another, later paper of this author). It was later changed to something similar as proposed here by Leuschner and Sirocko 2003. It might be worth investigating, which of the ideas is better, e.g. by revising Fig 6 and also including the other 2 indices here.

Author response:
Indeed the reference to Lourens and Reichart (1996) refers to a chapter on low latitude forcing of glacial cycles, which is part of the PhD thesis of G.J. Reichart. This thesis is available through the National Academic Research and Collaborations Information System:
http://www.narcis.nl/publication/RecordID/oai%3Adspace.library.uu.nl%3A1874%2F274419/genre/doctoralthesis/uquery/Reichart/id/2/Language/NL. We will make the citation clearer.
We will adapt part of our introduction (lines 23 p223 - 18 p224) and include a reference to Rossignol-Strick 1983 (indeed the first paper to mention the monsoon index), as well as remove the reference to the Reichart PhD thesis. The latter will be mentioned in the discussion. Furthermore we will refer to the paper of Mantsis et al 2014 (The response of large-scale circulation to obliquity-induced changes in meridional heating gradients, Journal of Climate). They discuss the effect of inter- and intra-hemispheric insolation gradients on the physics governing meridional overturning circulation such as the Hadley cell and ITCZ. They explain the main "SITIG" idea in their figure 11 and also show a stronger winter hemisphere Hadley cell during high obliquity. Our study adds more detail on moisture transport and precipitation as well as a link to the interpretation of proxy records.

The idea to incorporate the monsoon index of Rossignol-Strick (RS83) and that of Leuschner and Sirocko (LS03) into figure 6 (now figure 1) will be taken up, as it should provide a base on which to discuss and compare all the mechanisms proposed in these studies. The monsoon index of LS03 is very similar to SITIG, with a slight lag as the previous is defined using insolation on August 1st, while SITIG is based on June 21st. The power spectra of both show a peak both at precession and obliquity but the obliquity peak is much larger in the relative sense than it is in the monsoon index of RS83. The latter already has a stronger obliquity peak than insolation at a single latitude (23N, 65N) but by looking at the insolation difference between the tropics (SITIG, LS03) instead of the difference

between the equator and a tropic (RS83) the influence of obliquity is stronger. Therefore SITIG (or LS03) shows a better match to for instance the sapropel record. In order to illustrate this, we've included sapropels (from core RC9-181, used by both Lourens & Reichart '96 and Rossignol '85) in the figure. Comparison to this record shows that insolation differences such as SITIG or ISMI (Indian Summer Monsoon Index, LS03) are a better match than 23N insolation or M (RS83).
Furthermore, it should be noted that the monsoon index M of RS83 is based on caloric half-year insolation, while SITIG is based on 21june (peak) insolation. Including insolation over a longer period such as RS83 introduces more obliquity variance - recreating M using 21june insolation has a smaller obliquity signal. This also indicates that SITIG is a better candidate to explain the orbital cyclicity in proxy records than M. Furthermore, the cross-equatorial moisture transport we see in our model results match better with the SITIG hypothesis than with M, as the latter only considers the gradient between the equator and the (northern) tropic. The importance of moisture transport in for instance the North African monsoon has been shown in Bosmans et al 2015 (Climate Dynamics). These points will be taken up in the discussion.

**(b) What is the experiment really telling us:**
Right now it is argued that with eccentricity switch off, and analysing the effects of changes in Earths tilt from minimium to maximum with an Earth system model without land ice it is analysed what the response of the low latitudes climate to these respective insolation changes is. I think this approach needs more evidence and information for the following reasons: The effect of land ice sheets on climate is (at least) twofold: First, higher surface albedo changes the radiative balance, second the height of the ice sheet changes in the atmospheric circulation and thus climate. The later was recently shown to be potentially the reason for abrupt climate change (Zhang et al., 2014), and this surely played no role here (no ice sheets in the model). The first however, still needs to be shown. Thus, the authors should analyse the difference in surface albedo in high latitudes. Maybe winter snow is not melting anymore in summer (probably not building an ice sheet, because of the short simulation time, and because the model might not be able to do so), thus having a similar effect on the radiative balance than an ice sheet. So in a first step, at least the albedo changes need to be described. If it turns out that albedo changes were still high (which I would suggest they are) it might be necessary to think about an additional simulation experiment, in which high latitude surface albedo is fixed in order to stick to the bold statements that high-latitude changes are not important for tropical climate change on obliquity time scales. It is up to the authors to decide here how to deal with sea ice.

Author's response:
Indeed we acknowledge that there is a strong effect of (land and sea) ice on climate. Here we investigate the effect of obliquity without changes in land ice, as multiple proxy studies have suggested that obliquity affects (tropical) climate at times without ice ages or with a lag that is too short to be explained by glacial cycles (see for instance lines 4-22, p223). Possible effects of obliquity including land ice changes are discussed in the Discussion section (mainly 4.3). In this part we will further clarify that the effect of obliquity is not only simply a change in the insolation gradient, but that this change may be intensified by albedo changes. Figure 2 (below) gives the albedo changes between the two obliquity experiments. As expected, albedo is reduced in polar areas where sea ice is reduced due to increased (summer) insolation during Tmax. This may indeed affect the meridional temperature gradient, which we will mention in the Discussion.
Of course ideally more experiments can be done with fixing albedo (or even insolation) at higher latitudes, but this is currently not feasible with the model.
Also, it is not our aim to state that high-latitude changes are not important for tropical climate changes on obliquity time scales, our aim is to show that such climate changes can exist without high-latitude changes. In Section 4.3 we acknowledge that further research, for instance including ice sheets, is necessary to get a grip on the overall impact of obliquity on (meridional) temperature gradients and climate.

Furthermore, to be able to assess the impact of the chosen simulation scenarios a figure showing incoming radiation (insolation) as function of latitude and season (month) for both scenarios Tmax and Tmin and the difference of both is needed. All analysis (figures) are restricted to the latitudinal band between 50_N and 50_S. Thus, it is impossible to judge, if changes in the high latitudes occur and potentially have an effect on tropical climate. I therefore suggest to change the figures to show results from 90_N to 90_S.

Author's response:
We will include a figure of the insolation changes per latitude and month. We previously left this out as this figure is given in other papers already, but for completeness we will include it in the re-submitted version.
We chose to show figures of 50N-50S as our focus is on the tropics. Showing global results would also distract from our main point and make the figures overall smaller.

**(c) Missing wider discussion:**
So far, it is proposed that the SITIG hypothesis is for the tropical regions an alternative to the Milankovitch hypothesis, which proposed that summer insolation changes at 65_N is responsible for climate change on orbital time scales. They furthermore mention that a similar idea to that of SITIG was proposed by Leuschner and Sirocko (2003) and some other ideas about monsoon strength. However, what I believe is missing here is how this idea is related to / or different from that of Laepple and Lohmann (2009), in which the local seasonal cycle in insolation and temperature are related. Laepple and Lohmann (2009) nicely show, that local insolation might be more important to local climate change on orbital time scales than the concept of explaining everything with incoming light in summer at 65_ N. This idea seemed at least to be applicable to Antarctica (Laepple et al., 2011), but for the tropics the analysis are more diverse and less significant in Laepple and Lohmann (2009). So some comparisons and discussions are necessary here. There are other hypothesis on the obliquity forcing mainly on the idea how and why high-latitude ice sheets change, (e.g. integrated summer insolation by Huybers and Tziperman (2008)), so they might not be important here.

Author's response:
The study of Laepple and Lohmann (2009) applies the relationship between present-day temperature and insolation to the past, which gives overall a good indication of temperature changes in the past. Their conclusion that local insolation might be more important to local climate than 65N insolation is similar to ours, but our study focusses on more than temperature and local insolation. We focus on changes in for instance precipitation or moisture transport through the interhemispheric insolation gradient that can help explain obliquity signals in for instance the sapropels.
Furthermore, applying the relationship between present-day temperature and insolation to the Milankovitch cycles would result in a very weak obliquity signal in temperature over the tropics, as insolation over the tropics has very little obliquity variance (see Figure 6 of submitted paper, Figure 1 below). It can therefore not explain the obliquity patterns in low-latitude proxy records.
The effect of obliquity on (local) polar climate and ice sheets is beyond the scope of our study - however there is a brief discussion of the effect of obliquity-induced changes in the intra-hemispheric insolation gradients on poleward heat and moisture transport in Section 4.3. Therefore we do not include the references to Leapple et al 2011 or Huybers and Tziperman 2008 (the latter discusses precession).

Minors:
• First cited figure is figure 6, so this should become figure 1.
This is indeed changed; figure 6 (now with more insolation indeces) becomes Figure 1.
• I found the acronym Tmin and Tmax for scenarios with low and high obliquity rather confusing. Tyically in climate science T is the temperature. So I would suggest to use a different variable for obliquity. However, I also realized that in other papers of the same authors analysing the same simulations these acronyms were already introduced, so I have little hope here.
The reviewer is correct that T is often for temperature, but since we already used T for tilt in previous studies (also those of Tuenter, 2003) we keep this acronym for comparability.
• Citation Bosmans et al 2014 in Climate Dynamics was published in 2015.
Indeed this is updated (it was accepted in 2014 but the actual article number etc was only given recently)

• Citation Leuschner and Siroko 2003 has an incomplete journal name.
This will be corrected in the reference list of the revised paper.
• Please give a link to PhD thesis of GJ Reichart 1996 (available online at Utrecht University) and correct the citation Lourens and Reichart 1996 to Reichart 1996 (PhD thesis), if still cited in the discussion.
The link is
http://www.narcis.nl/publication/RecordID/oai%3Adspace.library.uu.nl%3A1874%2F274419/genre/doctoralthesis/uquery/Reichart/id/2/Language/NL. We will update the citation.
• Fig 6: Please use the same scaling for both left and right y-axis (both give insolation or insolation change in W m**−2**) in subfigure 6a. Now left shows 140, right 100 W m**−2**, exaggerating the influence of SITIG. Please show curves in different colors in both subfigures. Include results for the other two hypothesis (monsoon index, index calculated by Leuschner and Sirocko 2003).
We initially chose to plot different y-axis to compare the insolation patterns and amplitudes more easily (magnitudes vary so 1 y-axis would make the curves less easy to compare). However we have now changed the figure to include more indeces, each with its own y-axis.

References
Huybers, P., and E. Tziperman (2008), Integrated summer insolation forcing and 40,000 year glacial cycles: the perspective from an icesheet/energy-balance model, Paleoceanography, 23, PA1208, doi: 10.1029/2007PA001463, doi:10.1029/2007PA001463.
Laepple, T., and G. Lohmann (2009), Seasonal cycle as template for climate variability on astronomical timescales, Paleoceanography, 24, PA4201, doi:doi:10.1029/2008PA001674.
Laepple, T., M. Werner, and G. Lohmann (2011), Synchronicity of Antarctic temperatures and local solar insolation on orbital time scales, Nature, 471, 91–94, doi:10.1038/nature09825.
Rossignol-Strick, M. (1983), AFRICAN MONSOONS, AN IMMEDIATE CLIMATE RESPONSE TO ORBITAL INSOLATION, NATURE, 304(5921), 46–49, doi:{10.1038/304046a0}.
Zhang, X., G. Lohmann, G. Knorr, and C. Purcell (2014), Abrupt glacial climate shifts controlled by ice sheet changes, Nature, 512(7514), 290–294, doi:10.1038/nature13592.

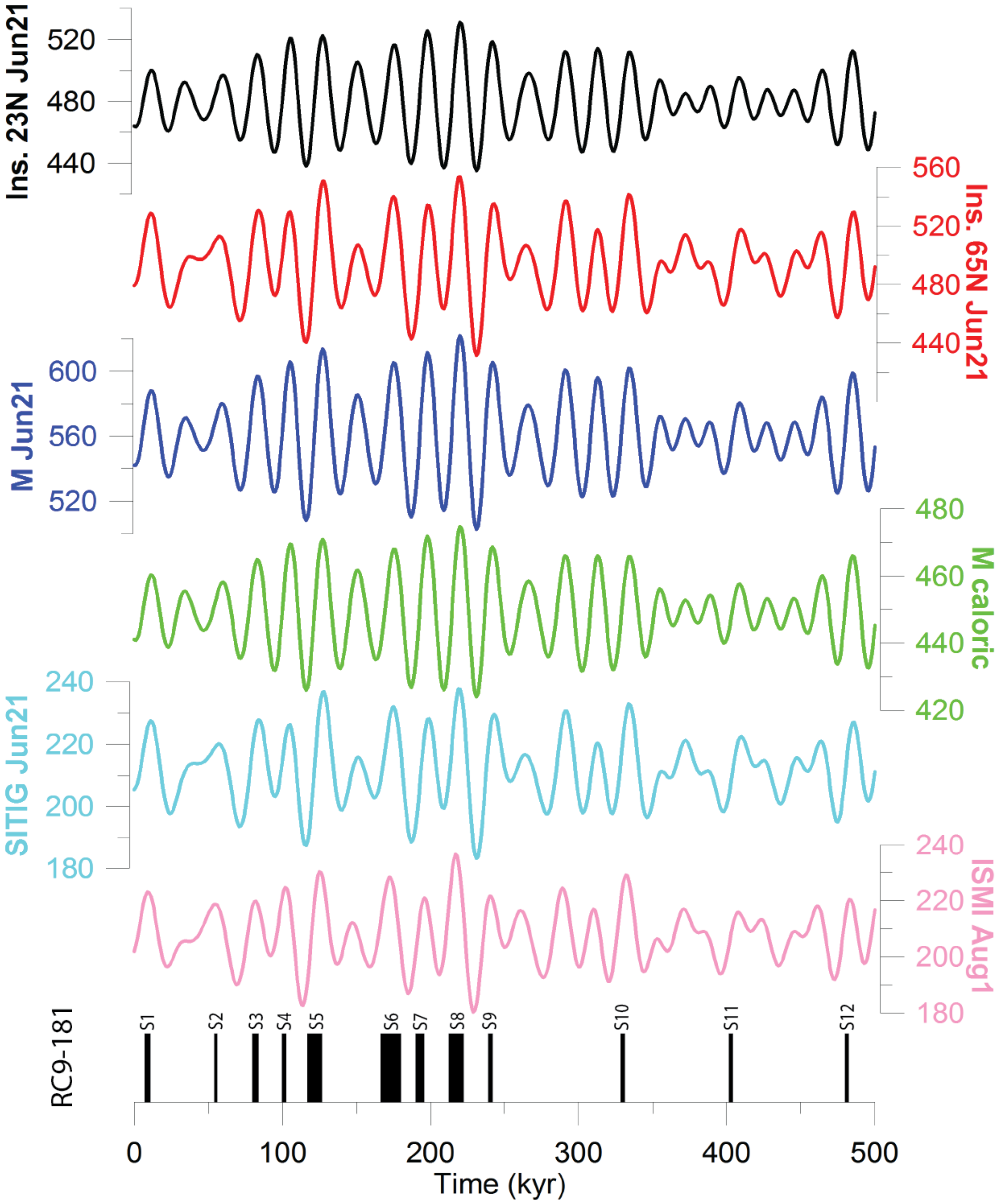


Figure 1a. Insolation over the past 500 kyr. From top to bottom: insolation at 23N on June 21st (black), insolation at 65N on June 21st (red), monsoon index M (Rossignol-Strick 1983) on June 21st (blue), monsoon index M for the caloric summer half year (green), SITIG on June 21st (light blue) and ISMI (Leuschner and Sirocko, 2003) on August 1st (pink). The lowest part shows sapropels in marine core RC9-181 (Vergnaud-Grazzini, 1977, Rossignol-Strick 1985).

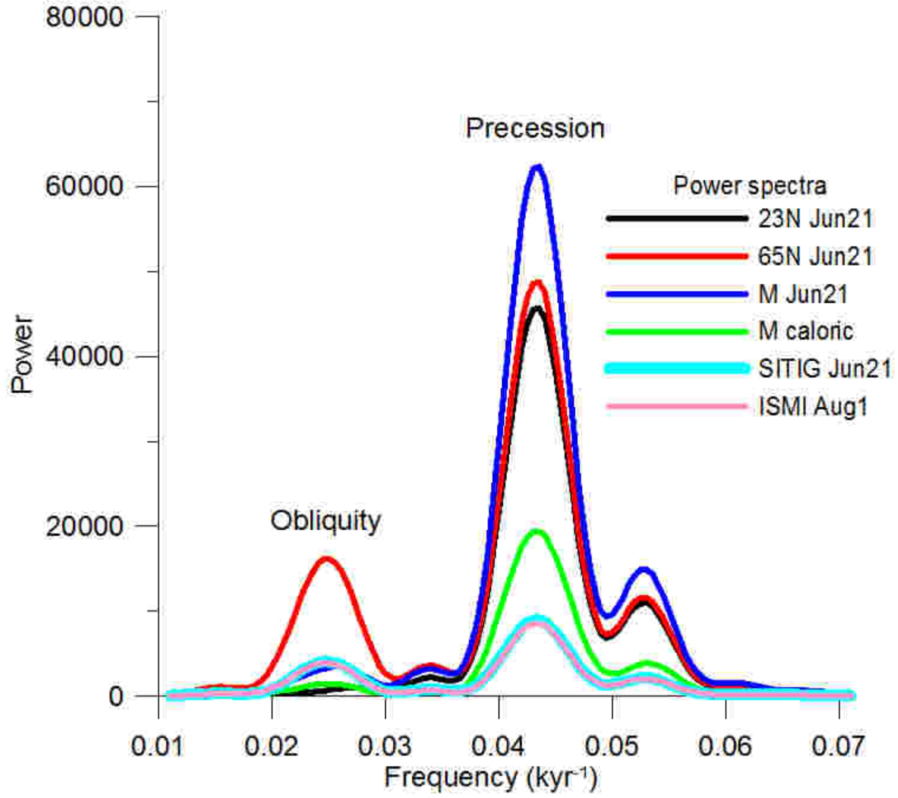


Figure 1b. Power spectra of the insolation curves shown in Figure 1a. The overall amplitude of SITIG and LS03 is smaller as they represent an insolation difference, but they have the largest (relative) obliquity peak.

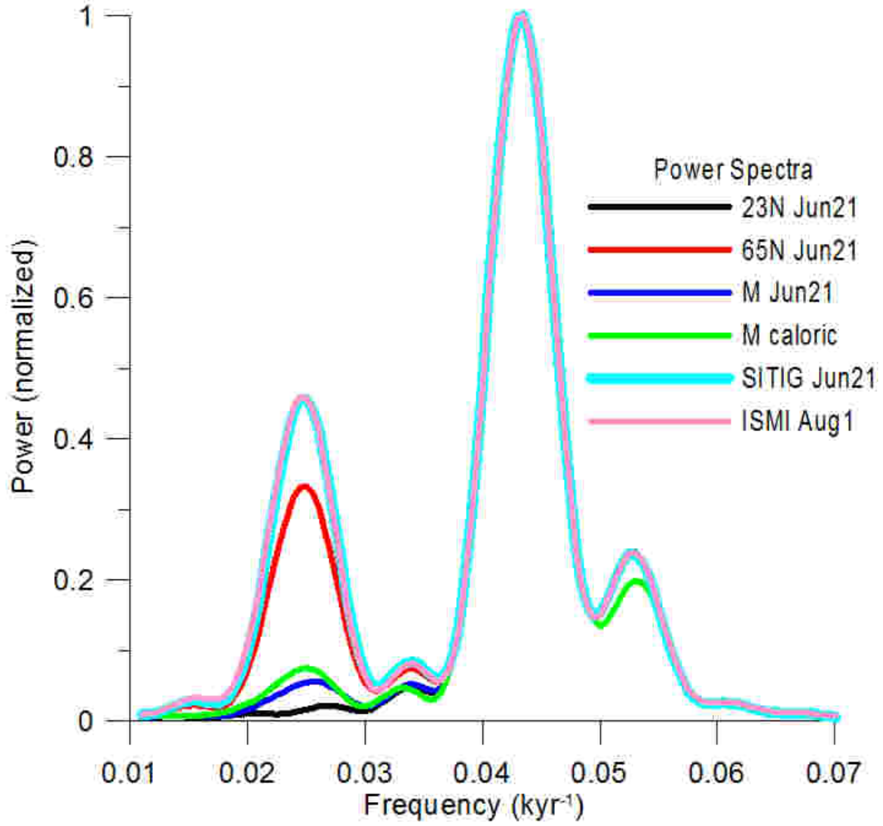


Figure 1c, same as 1b but normalized (each curve is normalized by its peak power at ~0.045/kyr)

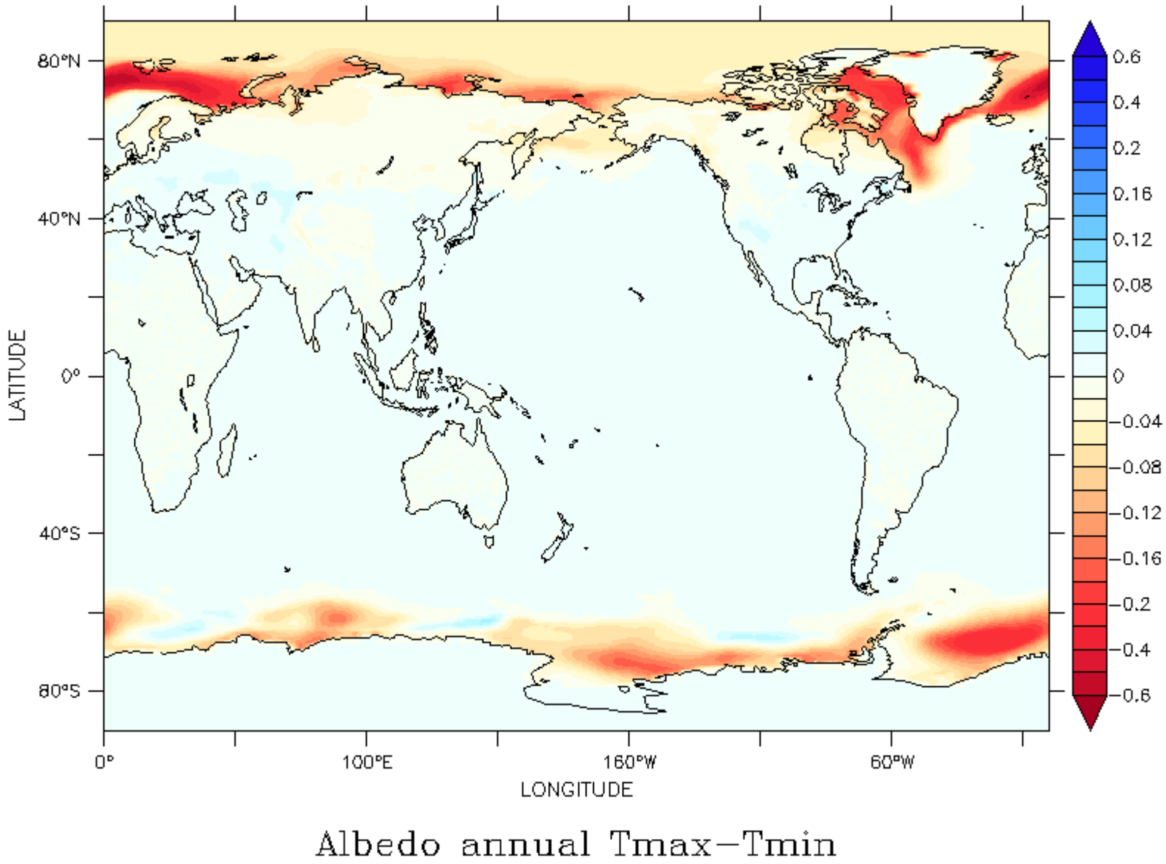


Figure 2: Difference in annual mean albedo between Tmax and Tmin. Overall, albedo is lower in polar regions where sea ice is reduced due to higher (summer) insolation during Tmax.