<u>Consolidated response to reviewers #1 and #2</u>

Firstly, we would like to thank the referees very much for their time and constructive comments, which will certainly help to improve the manuscript. We are happy to read that both reviewers consider our work to be 'a highly valuable contribution' and that the effort of collecting a long term data series is acknowledged: 'the data are extremely informative' (reviewer 1) and 'data derived from such a long-term monitoring program are very rare but remain necessary to understand the sedimentation processes in lake systems' (reviewer 2). The referees consider our manuscript a 'highly worthy contribution' (reviewer 1), 'ultimately worthy of publication' (reviewer 2). However, some important concerns are raised as well, which we would like to address below. Please note that reviewer comments are presented in italics and our responses in standard text.

<u>Reviewer 1</u>

- Summary: Application of the BIT index as a paleo-precipitation proxy at Lake Challa, East Africa has sparked a highly interesting and complex debate over the past several years. The proxy seems to say something important about hydroclimate on long timescales, but how it works remains a mystery. In this manuscript, Buckles et al. present a new analysis of branched GDGTs in sediment trap and sediment core data in order to clarify how the proxy works. The dataset and the analysis are a highly valuable contribution, and the paper is generally well-written (although the 'Results' section needs to be revised for a more general, non-organic geochemistry audience; see my comments below). However, my main issue is that the conclusions are strongly biased towards supporting the BIT index as a robust and consistent paleo-precipitation proxy in this setting, when the authors' own data show that it is, in fact, not. I strongly recommend that the authors remove the highly speculative conclusions toward the end of the paper (more information below) and focus, instead, on what their dataset does in fact show. The data are extremely informative and they add more pieces to the puzzle, but they do not come close to 'proving' that the BIT index "is a reliable precipitation proxy, at least in the Lake Challa system and on (multi-)decadal and longer timescales" (Conclusion, pg 1199). The authors should provide a more honest discussion of what we still need to learn in order to understand how the BIT index reflects hydroclimate at Lake Challa.

We understand that our conclusions are bold, although we believe them to be fully substantiated by our data. To accommodate concerns expressed by the reviewer, we will extend discussion of the 2,200 year record to include further interactions of the BIT index record with known climate events. We additionally propose that the primary conclusion should read: "The BIT index of Lake Challa sediments reflects the intensity of monsoon precipitation indirectly, in a similar manner as varve thickness is an indirect proxy of monsoon precipitation (Wolff et al. 2011; *Science*). Wolff et al. (2011) observed that varve thickness in Lake Challa sediments is primarily controlled by the thickness of the diatom layer deposited during austral winter, which is determined by the strength of seasonal deep mixing and therefore depends on wind stress during the austral winter. Inter-annual variation in local wind stress is inversely related to variability in precipitation associated with the ENSO cycle. We propose that the BIT index is primarily controlled by variation in the annual Thaumarchaeota bloom during austral summer, which is suppressed when excess nutrient input associated with occasional rainfall-driven soil-erosion events result in these Thaumarchaeota being outcompeted by nitrifying bacteria. The BIT index therefore can be considered to integrate the frequency of soil erosion events over time, which in the extremely seasonal and semi-arid tropical environment of Lake Challa is most likely proportional to total annual monsoon precipitation. However, since the mean frequency of such erosion events appears low at the (inter-) annual time scale, the positive relationship between BIT and rainfall will not manifest itself unless integrated over multi-decadal and longer time scales."

- A note on the writing style:

The topic of this paper (assessing the validity of the BIT index as a paleo-precipitation proxy in Lake Challa) is highly relevant to the broader paleoclimate community. However, the Results section (and parts of the Discussion prior to section 4.4) is written in such a way that it would be much better suited to an organic geochemistry journal. The authors should revise the paper to be more accessible to a non-organic geochemistry audience. For example, rather than focus on the technical details of every measurement they performed, the authors should give the results alongside a discussion of why these measurements were made in the first place. What is the importance of looking at core lipids and intact polar lipids? Why would someone do this? What new information does this provide? Why are there multiple indices for brGDGTs, and why bother comparing BIT to, e.g., MBT? These may be obvious to an organic geochemist, but it is totally unfamiliar territory to most Climate of the Past readers.

We agree with the reviewer that our paper presents an opportunity to explain the analytical approaches of state-of-the-art organic geochemistry to the Climate of the Past readership, and will be happy to include further explanation in the text to render it more accessible to non-geochemists.

- Detailed comments: Generally, I find the arguments presented in this section to be rather biased towards "support[ing] use of the BIT index as hydroclimatic proxy in this system (Verschuren et al., 2009)." It should not be assumed a-priori that the interpretation presented in Verschuren et al., 2009 is correct and should be supported, somehow, by the modern data.

The present study does not intend to 'support' the Verschuren et al. (2009) interpretation of long-term BIT index variation in the 25,000-year Lake Challa record. That interpretation is robust, on account of 1) it showing clear signatures of generally recognised widespread climate-change anomalies (such as Younger Dryas drought) at exactly the right time; 2) to the extent allowed by its lower (multi-century scale) resolution, it also shows clear signatures of the known regional climate anomalies, again at the right time; 3) it is in agreement with a second and fully independent hydroclimatic proxy, namely the low-resolution lake-level reconstruction based on seismic-reflection data (Moernaut et al., 2010); and 4) its relatively unique characteristics can be explained by reference to climate dynamics specific to the region of equatorial Africa beyond Atlantic Ocean influence. Therefore, that the Challa BIT index reflects long-term variability in that region's monsoon rainfall is no less certain than for any other, traditional or more novel, hydroclimate proxy. The purpose of the present study is to find a mechanistic explanation for <u>why</u> the BIT index in Lake Challa sediments reflects monsoon rainfall, given the recent finding that branched GDGTs (constituents of the BIT index) found in Lake Challa sediments primarily originate from production in the water column itself, rather than from the surrounding soils.

In fact, the data presented here show that BIT is an unreliable and inconsistent hydroclimate proxy in this system. It does seem to respond to ecological changes in the lacustrine system following the early-2008 erosion event, via suppression of Crenarchaeol production. However, this rainfall event was less intense (if Challa and Taveta are comparable) than a heavy rainfall event in early 2007, but that early 2007 event was not detected by BIT. Therefore, BIT does not in fact respond in a consistent manner to extreme rainfall events. If, in fact, the BIT index does respond to rainfall events that follow severe drought (as the authors postulate for the early-2008 event), then it is an indicator of erosion extremes, not precipitation extremes or seasonal (monsoonal) precipitation.

We do not argue that the BIT index is a one-on-one indicator of precipitation extremes or of seasonal precipitation amount. If it were, it could be used to trace rainfall variation at (sub-) annual to inter-annual time scales, which we repeatedly state it does not. As suggested by the reviewer, it is indeed an indicator of 'extreme' erosion events, which in this semi-arid tropical region have a threshold relationship with rainfall extremes. We will adjust the text to better clarify this.

Additionally, please note that our measured precipitation record originates from local hydrological monitoring and therefore was available only as summed precipitation per month. As the creek in the NW corner of the lake is only activated in periods of intense precipitation, it might represent the difference between all the month's rain falling in one day or spread over the full month.

The proxy of course could be detecting high-amplitude variability. It is very possible that in the 25,000 year record, the BIT index is not recording regular monsoonal rainfall but rather extreme flooding events that also follow extreme droughts.

That is exactly our proposition. However, it is important to note that over the past 25,000 years, the Lake Challa area has always experienced a semi-arid tropical climate with high propensity for both extreme drought episodes and extreme precipitation events. We can thus surmise that, integrated over time, the impact of extreme erosion events on the aquatic ecosystem of Lake Challa is proportional to longer-term trends in total rainfall.

The BIT signals get smoothed out and even shifted in time relative to the varve record, as is shown in Figure 3G.

We do not know which figure the reviewer is referring to, since Figure 3G shows only a comparison between two sets of BIT index data. He/she may refer to the comparison between BIT index and varve-thickness in Figure 7, but if so we do not agree with his/her assessment that BIT is shifted in time relative to varve thickness. These are two (largely) independent hydroclimatic proxies, with a similarly complex but different relationship with climate as the ultimate driver of <u>a major part of</u> their variation through time. This allows the existence of proxy-specific and time-scale dependent biases, and hence there is no reason why long-term trends in these two proxy records should look exactly the same, or why there would be a systematic phase shift between them.

Finally, the authors' conclusions that the BIT index may be reliable on decadal timescales even if it is not reliable on interannual timescales seems highly over- speculative. Conveniently, we do not have decades' worth of modern data to disprove this. But in fact, even in this multi-year dataset, the authors have only one single event on which this interpretation is based.

The importance of rare events in long-term system dynamics is a common element in most geological and ecological processes. As mentioned on page 1 of our response, we propose the application of the BIT index in Lake Challa solely on multi-decadal or longer timescales since we believe this allows for a low frequency of soil erosion events. In our multi-year dataset, the marked perturbation of the microbiological community in the suboxic water column (as measured in settling particles) for almost two years of a nearly four year dataset is clear and the soil erosion event is the only identifiable cause, despite the extensive ongoing monitoring of the lake, its water column and analysis of sediments that extend the 'status quo' GDGT composition to cover more than 6 years of deposition. The relatively low frequency but long-lasting effect of this event directly supports the conclusion that inter-annual timescales would be too biased by the low frequency of these events, but multi-decadal timescales would integrate these signals. Our trust in the reliability of this rainfall proxy is enhanced by the congruence of our resulting 2,200-year rainfall reconstruction with established (though still relatively scarce) knowledge of the wider region's hydroclimatic history, including three historically documented episodes of prolonged drought over the past 200 years. As the reviewer will agree, very few lake-based hydroclimatic proxies <u>from any location in the world</u> can claim this level of success. We therefore respectfully believe that our results represent a significant step forward in this field.

These findings are important for continuing to develop the interpretation of the Lake Challa BIT record. The authors should recognize that the interpretation of this proxy in this setting will continue to develop through time with new data, and it may even be revised quite thoroughly. Such is the purpose of collecting modern data to inform a proxy.

Our multi-year maintenance of a multi-parameter monthly monitoring programme on a small and remote African crater lake, at considerable time, logistic and financial investment, ought to be sufficient demonstration that we are aware of this and willing to meet the challenge.

The BIT index at Lake Challa does seem to say something about precipitation and erosion on long timescales. However, at this stage it is unclear what this proxy is telling us, and why it seems to work. This paper is an important first step, and additional modern observations will continue to clarify and develop the interpretation of this proxy and its application in other settings. However, I feel that a more laudable approach would be for the authors to explain what they have found and to honestly assess what is still not well-understood. They do not need to 'solve' the BIT proxy in this paper in order for the 25,000 year record to still be useful. In fact I feel the over-speculation weakens the overall findings of the paper, which in themselves are very interesting and a highly worthy contribution to the paleoclimate community.

We thank the reviewer for this appreciation of our work, and are sorry if we gave the erroneous impression that our primary aim is to 'solve' the BIT proxy. Our own publication record (Sinninghe Damsté et al., 2009; Sinninghe Damsté et al., 2012; Buckles et al., 2014a) should make clear that we have dissected the issue from all possible angles and we ourselves have published data casting initial doubt on the reliability of BIT index as rainfall proxy in this system. However, in combination with this previous research on the BIT index and its constituent compounds in Lake Challa (Sinninghe Damsté et al., 2009; Sinninghe Damsté et al., 2012; Buckles et al., 2014a) we believe that we now have a relatively comprehensive idea of the mechanisms that control its variation in Lake Challa. Although quantifying the relative influence of these mechanisms is beyond the scope of this paper and potentially impossible without multi-decadal field data, it does not diminish the congruence of our resulting 2,200-year rainfall reconstruction with established knowledge of the wider region's hydroclimatic history (see page 3).

- Line-by-line comments:

Pg 1180: Make it clear that Crenarchaeol = GDGT V

Agreed.

Pg 1183 Lines 5-10: Please include a figure with the age model. In the supplementary material please provide the 14C AMS dates and their 1- and 2-sigma uncertainties.

Both the ^{14}C- and varve-based age models have been published previously (Blaauw et al., 2011; Wolff et al., 2011) and we refer to these previous publications, where details of these age models and associated discussion are readily available.

Pg 1185, Equation 3: Define DC?

Degree of cyclisation; we will clarify in the revised manuscript.

Pg 1186, Line 5: Why these 'general guidelines?' How were these cut-offs chosen? 0.5 is very low to be considered "strongly correlated", especially since these are r-values and not r2.

Dancey and Reidy (2004) recommend the following characterisations for the interpretation of correlations and Pearson's *r*:

Correlation coefficient	**Strength of correlation**
1	Perfect
0.7-0.9	Strong
0.4-0.6	Moderate
0.1-0.3	Weak
0	Zero

Our cut-offs were chosen based on the above, with slightly lower boundary conditions enforced to reflect the assumption of a large number of confounding factors in the data (although our boundary conditions are still well within the accepted range). These factors include (but are not limited to):

- A relatively large number of GDGT measurements with zero or near-zero values;
- Potential small time offsets between the varve record and the geochemical record;
- The measurement of $\%C_{org}$ at two–centimetre intervals, while all other parameters are per centimetre or less;
- Those affecting the abundance and distribution of GDGTs such as different bacterial producers of branched GDGTs, limited knowledge of the ecology of GDGT producers in Lake Challa and their response to pH/temperature, changes in production/depth of production and allochthonous influxes over time, etc.; and
- The relatively low number of data points available (208).

Additionally, a slight reclassification of the strength of the correlation coefficients would have no impact on our discussion of the data or our conclusions. Each coefficient is interpreted by comparison with the rest of the dataset and their classification is by its nature arbitrary.

Pg 1186, What is SD? standard deviation? Please define Pg 1187: Does r=0.67 for [brGDGT] with both crenarchaeol and its regioisomer? Please confirm

SD is standard deviation and r=0.67 for both crenarchaeol and its regioisomer (see Table S2). This will be clarified in the revised manuscript.

Pg 1189 line 20: Do the gravity core samples have any actual age control points at the top? Otherwise, it is a strong assumption to say you know their timing down to the month.

Sinninghe Damsté et al. (2009) and Blaauw et al. (2011) state that the uncompacted top centimetre of sediment represents approximately 2 years of deposition. This is based on tie points in the visual fine lamination and magnetic susceptibility profiles of multiple gravity cores collected between 2003 and 2011, and confirmed by ^{210}Pb-dating of a gravity core collected in 1999; see Blaauw et al. (2011) for details. We simply use this data to demonstrate that the surface sediments collected do indeed reflect the composition of the descending particles collected in the sediment trap over that time period, so the assumption is tested and is not egregious.

Figure 6: Confused. I do not see GDGT-0 on this figure. I am only now realizing that GDGT-0 is the same as GDGT-I in the Appendix. This is very confusing. Can you please make this terminology very clear, repeating it throughout the paper so that the reader can follow.

In this figure, GDGT-I is GDGT-0. We understand that the mixed nomenclature can be confusing and it will be clarified in the revised text.

Figure 6: Why no error bars on CH07?

CH07 consists of two samples from 0.0-0.5 cm and 0.5-1.0 cm depth. For our purposes, we integrate these two samples (summed abundance rather than average) and take it to represent one sample of 0.0-1.0 cm depth. As it represents one sample, it does not have error bars; please also see Table S4.

Pg 1193 line 12: Why would it be true that brGDGTs/crenarchaeol are correlated, and hence brGDGT producers are heterotrophic bacteria? This connection is not clear, please explain.

Based on analysis by Buckles et al. (2014a), branched GDGTs and crenarchaeol are primarily exported to sediments from the suboxic zone of Lake Challa. The hypothesis is that branched GDGT-producing bacteria are involved in the degradation of organic matter produced by diatom blooms, while Thaumarchaeota are ammonium-oxidising archaea and therefore thrive on the degradation products of the diatom bloom. This would explain the correlation of crenarchaeol with branched GDGTs and will be clarified in the revised text.

Figure 2a: Because of the missing Challa precipitation data it is difficult to compare the magnitude of the precipitation that resulted in the erosion event with the other precipitation in the records. Please plot Taveta rainfall for the other events as well on Fig 2a, so that the comparison may be made.

This data is unfortunately not available, as the station is defunct and measurement was intermittent. We will continue to search for more precipitation data from November 2006 to August 2010 and will also examine TRMM data, as suggested by reviewer 2.

Pg 1194, line 19: The onset of the principal rainy season cannot be the only reason for erosion, because the other years' principal rainy seasons did not see similar erosion events.

This is a chronological description of events. Based on accounts of local fishermen the allochthonous influx to the lake was triggered by the onset of the principal rainy season as described, but not all principal rainy seasons will necessarily trigger a soil erosion event.

Pg 1197: "Since stronger austral-winter winds are associated with a weak southeasterly monsoon compromising the main rain season during March–May " Please provide citation for this mechanism.

Wolff et al. (2011; *Science*); Wolff, 2010 (*Thesis; Universität Potsdam*).

Reviewer 2

The aim of this study is to test the BIT index (ratio describing the proportion of branched GDGTs, of soil origin, versus isoprenoids GDGTs, of aquatic origin) as a proxy of precipitation in tropical Africa. Buckles et al. used data from a sediment trap, soils, and lake sediments combined to climate data to evaluate the BIT index in the Lake Challa area (Kenya/Tanzania). They found that brGDGTs were also produced in the lake water column and that the BIT index in Lake Challa sediments reflected the crenarchaeol abundance, rather than brGDGT abundance thus complicating the original interpretation of this proxy. Here, Buckles et al. proposed that pulses of Thaumarchaeota production during the driest and windiest years mostly control the BIT index in a lake system where allochthonous sedimentation is dwarfed by autochthonous sedimentation. I found their interpretation realistic for the modern/recent lake sediments but it is also possible that the proposed mechanism varied for the older sediments (cf. the 25,000 yr record). For example, the high BIT index (of 1) during the early Holocene may also be related to the increase of brGDGTs derived from soils (this period was significantly wetter compared to the present-day conditions). The data presented in this study are highly valuable, particularly since data derived from such a long-term monitoring program are very rare but remain necessary to understand the sedimentation processes in lake systems. Overall I found this paper interesting to read and ultimately worth for publication at Climate of the Past after substantial adjustments. Like the authors, I think that the BIT index remains a potential good proxy for paleohydrology, although the monitoring data presented here suggests that the behavior of the GDGTs and the exact meaning of this proxy remain still elusive in small lake systems. I disagree with the authors that their study 'validates' the use of the BIT index in such environments since the new mechanism they promote (i.e. high in situ brGDGT production combined to a production of crenarchaeol triggered by precipitation in the lake's catchment) to explain this proxy strongly differ with the initial one (i.e. soil versus aquatic origin of the GDGTs).

Moreover, they do not provide a way to evaluate which mechanisms (soil-derived brGDGTs versus in situ production of brGDGTs) can control the BIT index in the sediments. This would be necessary for an unambiguous interpretation of the sedimentary BIT index. For example, when looking at different time periods, both mechanisms could operate in the same lake system and their impact on the BIT index cannot be considered as identical (until proven). The authors should provide here a more balanced discussion and importantly they should also provide more ways to help future understanding of this proxy. Below are other important points that, in my opinion, need to be fully clarified prior to publication.

We agree that the mechanism controlling BIT variation may vary for the older sediments (cf. the 25,000 year record), but this problem can hardly be considered unique to this particular proxy. In fact, many paleoenvironmental proxies (even some supposedly robust traditional proxies) remain un-validated even in the modern-day system. Sinninghe Damsté et al. (2012) already discuss the longer BIT-index record in detail and therefore this is out of scope for our paper, although we do refer to this previous research in our discussion. Additionally, Buckles et al. (2014) demonstrate that the signature of modern-day branched GDGT influxes to Lake Challa is not only indistinguishable in the BIT index of settling particles in sediment-trap samples, but also that their distributions do not shift towards those found in soils. This is despite the lake reportedly 'turning brown' and associated changes in the Ti/Al ratio as described by Wolff (2010). Please also note that even with 40% more rainfall, the Lake Challa climate regime would will be semi-arid with similar seasonal and inter-annual variability of monsoon precipitation. We thus believe that despite significantly wetter conditions, the influx of soil-derived branched GDGTs is not the primary mechanism for the increase in the sedimentary BIT index.

The new mechanism we propose differs so markedly from the initial mechanism due to the now widespread knowledge that branched GDGTs can be produced in substantial amounts in lakes (e.g., Tierney and Russell, 2009; Tierney et al., 2010; Loomis et al., 2011), in addition to further research in Lake Challa specifically (Sinninghe Damsté et al., 2012; Buckles et al., 2014a). Before the sediment trap time series was expanded, Sinninghe Damsté et al. (2009) noted that influxes of branched GDGTs to the sediment trap appeared to correspond with the precipitation regime; however, extending this time series and combining it with additional analysis of intact polar lipids ('living' branched GDGTs) showed that branched GDGTs in sediments were primarily derived from the water column (Buckles et al., 2014a). This emphasises the value of long-term monitoring. However, please note that both mechanisms rely on influxes of soil to the lake affecting the sedimentary BIT index, either directly or indirectly.

Point (1). The modern data: settling particles.

Most of the sediment trap data presented by the authors derived from Sinninghe Damsté et al. (2009) and Buckles et al. (2014). The authors mentioned here that they "report additional results for GDGTs I to IV present in these samples". It is not clear reading this manuscript what is really new and what is derived from the former studies on this site. This should be better defined. The authors should emphasize more their discussion on the new findings.

In the results section, we specify which results have been presented elsewhere and collated within this manuscript, and which are new. The results focus on describing the new data: GDGT concentrations and GDGT-based indices from the 2,200-year sediment record and isoprenoid GDGTs from surface sediments and settling particles (excluding crenarchaeol and its regioisomer).

I wonder if the GDGTs data (and their indexes) from the sediment trap are contemporaneous with the weather events presented for comparison (temperature, precipitation: : :). Here are some open questions that should be discussed more in detail in this manuscript: - What is the estimated residence time of the GDGTs in both the Lake Challa water column and its watershed?

This is discussed in detail by Buckles et al. (2014a). We refer to this research where relevant.

- What is the velocity of settling particles within the water column of the Lake Challa? Does this velocity remain constant during a seasonal cycle? Looking at the Wolff et al. (2014) data, it seems that there is a systematic lag of 2-4 months between the deposition of Ti and the preceding main peak of precipitation. Does that also apply to the GDGTs?

As shown in our Figure 2, the peak in Ti/Al ratio is concurrent with the peak in precipitation. The velocity of sinking particles would depend on their size; GDGTs from the upper water column are likely to be exported to sediments relatively rapidly (Buckles et al., 2014a). However, the majority of GDGTs exported to sediments are produced in the vicinity of the sediment trap and thus their time to reach the sediment trap would likely be even shorter than that of soil-derived materials.

Point (2). The paleo-record: comparison of the BIT index and varve thickness during the last 2000 yr. The authors spent a large part of their manuscript to discuss the modern data, while a smaller part of it is devoted to the discussion of the paleo-record. The balance between the two parts could be improved.

We will extend the discussion of the 2,200-year record to include further comparisons of the BIT index record with regional climate records. However, our focus is on the discussion of modern data and on the validation of the proxy rather than interpreting regional climate dynamics as expressed by the BIT index record, which cannot be done without a full review of regional climate records with their respective merits and defects. The reviewer will agree that this requires a different kind of study.

Figure 7 shows the direct comparison of the BIT index and varve thickness during the last 2000 yr. The authors used 5-point and 7-point running average for the BIT index and varve thickness data, respectively, which were sampled with a different resolution. Instead, I would advise them to resample the varve thickness data using the exact sampling resolution as for the BIT index and to show a time series of varve thickness (with the mean and standard deviation for each sample interval) that is directly comparable with the BIT index data. Then, the same running averages could also be overlaid above the two records. Correlation plots with significance level would be also valuable. Does the correlation vary in time? The authors suggested that "the BIT index should not be used as a precipitation proxy on the interannual timescale. Rather, one data point per decade seems sensible, with a five-point moving average (Fig. 7b) providing a robust reconstruction of longer-term dry/wet trends." To validate their statement, they should calculate the correlation (and demonstrate that it is significant or not) between the BIT index and the varve data at different timescale (using for example a moving average or a band-pass filter): according to their statement, correlation should increase towards lower frequency variability of the BIT index. Buckles et al. compared their BIT index data with the total varve thickness. However, Wolff et al. (2011, 2014) demonstrated that "the total varve thickness is controlled by the thickness of the light layer: : : varve thickness mainly reflects the quantity of diatom frustules deposited during the dry season and in particular during April to September". Wolff et al. (2014) noted that "varve thickness can be used as a proxy to reconstruct paleo wind variations during the dry season." Thus the total varve thickness is not a direct proxy of precipitation. Instead, the small and organic dark varve layers in the sediments record monsoon precipitation (unfortunately biased by additional in situ lake precipitation products): "The darker layers represent the two rainy seasons (November to December and March to May) and the brief intervening dry season with amorphous organic matter derived from phytoplankton and calcite precipitation: : :" (Wolff et al. 2014). The authors should then also compare the BIT index data with the dark varve thickness data and provide a correlation plot as for the total varve thickness data. It would be important to know which varve thickness data (light or dark) provide the best correlation with the BIT index through time.

Wolff et al. (2011) proposed that varve thickness in Lake Challa sediments is primarily controlled by the layer of diatoms deposited during the austral winter, which is determined by the strength of seasonal deep mixing and therefore depends on wind stress during the austral winter. Wolff et al. (2011) also show that "El Niño (La Niña) events are associated with wetter (drier) conditions in East Africa and decreased (increased) surface wind speeds." The interpretation of the variation in annual varve thickness data in fact reflects ENSO variability, which is on a shorter timescale to the decadal resolution of our BIT index record. As such, they are not directly comparable by the method described by the reviewer as dark varve layers are deposited during the two rainy seasons but are not a proxy for monsoon precipitation. However, we will further explore correlations between the BIT index and varve thickness on various timescales, such as during the past 200 years, in our revised manuscript.

Additionally, the reviewer suggests that the varve thickness data be resampled to the depth resolution of the BIT index for comparison. However, the depth axis is not independent of varve thickness, i.e. a different number of varves (and parts of varves) must be summed to achieve 1 cm resolution.

Other points:

- Although I am totally confident with the GDGTs data produced by the authors and the calculated BIT index for the different substrates presented in this study, I am not very confident with the precipitation data they used for comparison. Buckles et al. (2014) first showed the precipitation data and indicated

that it derived from a governmental agricultural station "immediately north of Lake Challa". In the current paper Buckles et al. showed the same data but discriminated the data from "Challa" and "Taveta". The time series looks weird, it is not seasonal but erratic, showing for example only one significant month of precipitation for 2007 with ca. 650 mm (extreme precipitation amount for March), and almost not a single drop of water during the rest of this year. What is even stranger is that other authors who also worked on the same material from the same sediment trap used other precipitation data for their comparisons: the record of Voi located 100 km to the East (Wolff et al., 2014). The Voi station is also not ideal since it is located too far away from the study site and may not have recorded some major events at Challa. Since the precipitation record from Voi show less gaps of precipitation (i.e. prolonged period of no precipitation) and since the mean annual precipitation is significantly lower in Voi compared to Taveta according to Sinninghe Damsté et al. (2009; Fig. 3f), the data presented here seem obviously incomplete/biased. According to the data of Voi (Wolff et al., 2014), there is no "long drought that stretched from May 2007 to February 2008 due to failure of the short rains in 2007" as Buckles et al. asserted. If the precipitation data derived from local ground-based stations have issues, the authors may check the remote sensing product for alternative solutions. For example, the data from the Tropical Rainfall Measuring Mission (TRMM) are easily available, and provide 3 hourly rainfall amounts with a spatial resolution of 5x5 km2 from 1997 to 2015. I am not arguing that a more robust precipitation record would provide a complete mechanism for interpreting the complex BIT signal but this will certainly help the data interpretations.

This analysis is fair and the comment helpful. Due to the limitations of the data we have not endeavoured to make detailed analysis of the precipitation (other than that it clearly rained followed by a soil erosion event); however, we would include TRMM data in any future submission.

- The nomenclatures of the GDGTs are not straightforward and will confuse many of the readers who are non-specialists. Referring to GDGT-0 for GDGT-I or to GDGT-1 for GDGT-II does not facilitate the understanding of complex ratios of molecules. It would be wise to state that this dual nomenclature exists at the beginning of this manuscript and to stick to one or the other all over the manuscript.

Agreed.

- I also found highly relevant and valuable the comments provided by the first reviewer.

Agreed.