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Interactive comment on "Ground surface temperatures indicate the presence of permafrost in North Africa (Djebel Toubkal, High Atlas, Morocco)" *by* Gonçalo Vieira et al.

Gonçalo Vieira et al.

vieira@campus.ul.pt

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Comments posted by reviewer 2, Dr Benno Staub.

GENERAL COMMENTS

The research article "Ground surface temperatures indicate the presence of permafrost in North Africa (Djebel Toubkal, High Atlas, Morocco)" presents new observations and thoughts on the permafrost occurrence in the High Atlas. Due to the particularly poor data basis concerning permafrost and related phenomena in North Africa, any new measurements and findings on the potential permafrost distribution are of high importance, not only for the research community but also from an environmental and

socio-economic perspective.

The gap in research and the knowledge from previous studies are well described in the introduction and the topic is nicely put into a larger context illustrating the characteristics of arid and semi-arid environments. Moreover, the environmental conditions and the geological setting of the studied region are compared with other high mountain areas. The overall objective, to contribute to the question of permafrost distribution in North Africa, is very ambitious. Probably too ambitious because recent studies and direct observations are missing and the acquired GST time series only cover one year of data for a small number of spots. Although the authors grade their work as an "exploratory step towards an in-depth assessment aiming at the characterization and modelling of permafrost in the High Atlas", the main critique on this article could be: Why didn't the authors wait two years longer to publish their work together with a sound data basis, consisting of at least three years of continuous GST data and e.g. some complementary geophysical investigations?

The general approach combining weather station data series, remote sensing data and a geomorphological interpretation of the landforms is certainly a good starting point to maximise the informative value of the GST data. However, although the authors clearly point out that the few observations must be interpreted very carefully, the measurement setup and some of the results seem somehow on the limit of being scientifically reasonable. Personally, I doubt that only four miniature loggers can provide a meaningful 'altitudinal gradient', and I would avoid making linear regressions (P7_L3 & Fig. 8) out of it nor extrapolate these results to a larger area. On the other hand, the publication can be justified because almost no permafrost observations are available for the High Atlas, and because this paper may motivate the mountain permafrost research community to put particular emphasis on that region. Moreover, the article is a nice complement to other mountain permafrost papers enriching the TC special issue "The evolution of permafrost in mountain regions". Therefore, I recommend this research article to be published with minor corrections. The following comments could help to improve the article, mainly concerning the interpretation of the GST data. Most important, I would like to encourage the authors to keep on measuring and observing the permafrost in the High Atlas!

SPECIFIC COMMENTS

Title: The experience from the European Alps showed that one year of GST measurements does not provide reliable results on the ground thermal regime because of the high inter-annual variability of weather and snow conditions. In this regard, I suggest rethinking the title of the manuscript, e.g. towards a more neutral formulation “New observations indicate the occurrence of permafrost in the High Atlas mountain range (Djebel Toubkal, Morocco)”.

Interpretation of GST data: The interpretation of the GST data as a ‘BTS signal’ is only valid if a thermally insulating snow cover is present for longer than just a few weeks. It seems like logger T3 fulfils this criterion around end of February 2016 (Fig. 10). At T1 and T2, the active layer is likely not in a thermal equilibrium with the permafrost base, these GST records characterise an integral of the recent atmospheric conditions with some modification by a temporarily snow cover. Depending on the terrain roughness and the snow density, about 50-100 cm of snow are required to effectively insulate the ground surface from air temperature variations (e.g. Keller and Gubler (1993); Zhang (2005); Staub and Delaloye (2016)). If there is less snow under winter conditions, the ground is likely colder at its surface than a few meters below. Although the potentially snow-covered period is shorter and snow heights are lower on average in the Toubkal massif than e.g. in the European Alps, the timing and duration of the snow cover probably play a key role for sporadic permafrost occurrence also in the High Atlas – as described by the authors. In comparison to the permafrost areas in the Alps, where the conditions during the winter season are often more important for inter-annual ground temperature variations than the summer warming (cf. PERMOS (2016)), snow disappearance is up to three months earlier in the Toubkal massif (despite of persisting snow patches), even at 3500 m asl. This means that the ground is usually snow free

during the entire period of maximal insulation. The local effects of shading could be very important. Probably a GIS-analysis on topo-climatic parameters such as potential incoming solar radiation, slope and curvature could help to characterise the acquired GST data and putting it into the spatial context. Moreover, it might be interesting to quantify ground thawing and freezing degree day sums for the summer and winter period.

Measurement setup: A future GST measurement setup around Djebel Toubkal could be installed similarly as described by Gubler et al. (2011) to provide some observational evidence on the GST variability considering different ground materials and topoclimatic situations at least for a few years. I am fully aware of the high financial and logistical effort for such permafrost observations in the remote High Atlas, but I think that such a data basis is required for any further steps towards permafrost mapping and modelling. At best, such spatially distributed GST measurements would be complemented by ERT surveys and geomorphological mapping. Building up a rock glacier and frozen debris lobe inventory. Weather and climate data: The authors characterise that particular year with GST observations in the climatological context by using meteorological data (Sect. 4.1, Figs. 3-5, 11 and 13). This is clearly a challenging task regarding the sparse data available, but the spatial transfer of air temperature data over âĹij3500 m elevation between Menara at Marrakesh to the Djebel Toubkal mountain is not satisfactory from a scientific point of view. Although the lapse rates provided and calculated seem plausible, these lapse rates likely vary over the season and the weather conditions can be very different in the mountains to what is measured in Marrakesh. The “significant correlation” of monthly air temperature values between Neltner and Sidi Chamarouch (P6_L14 and Fig. 3) is likely a result of the high seasonal temperature amplitudes. However, the comparison to other quantitative and qualitative data sources could be extended. For example, the snow climatology could be analysed over the entire period of available satellite imagery. Also satellite-derived land surface temperature data could enhance the comparison of the period 2015-16 in a larger temporal and spatial context – of course with limitations due to the lack of validation data and the difficulties in mountainous terrain. Maybe c even RCM reanalysis data could help to assess the regional climate history.

P7_L20: Clarify that you mean daily maxima in the sentence “A plateau in the maxima. . .”

P7_L23-24: I would not state the relationship between elevation and MAGST of these four locations as “statistically significant” and rather try to quantify the uncertainty of each data point. The uncertainty of MAGST is likely much higher than ±0.4âŮęC/100m. Observations from the Swiss Alps show that elevation can be a poor proxy for MAGST, depending on the terrain and snow characteristics (boulder size, terrain roughness, solar irradiation, exposure to wind, and accumulation of snow by wind or avalanches) and regional weather patterns (e.g. Gubler et al (2011)).

Fig. 5: Clarify, that the dashed line is the extrapolation for the summit of Djebel Toubkal. If possible, add an uncertainty estimate (e.g. using a range of lapse rates).

Fig. 6: Add readable point labels and a legend for the colours.

Fig. 7: What are “daily hourly maxima”?

Fig. 8: See comment above. Maybe add an uncertainty estimate to each point?

Fig. 10: GST data series can be calibrated during the zero curtain period. It is visually not clear, if this calibration was done or if the dashed line is not really at 0âŮęC at some of the time series.

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REPLY TO DR BENNO STAUB,

Dear Dr Benno Staub,

Thank you very much for your insightful and detailed review with very valuable and important comments. Below we have selected the various questions and remarks that you pose and we address them. We will submit the manuscript following your suggestions in the next few days.

Best wishes,

Gonçalo Vieira, Carla Mora and Ali Faleh

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Comment by Dr Benno Staub : Âń The overall objective, to contribute to the question of permafrost distribution in North Africa, is very ambitious. Probably too ambitious because recent studies and direct observations are missing and the acquired GST time series only cover one year of data for a small number of spots. Although the authors grade their work as an "exploratory step towards an in-depth assessment aiming at the characterization and modelling of permafrost in the High Atlas", the main critique on this article could be: Why didn't the authors wait two years longer to publish their work together with a sound data basis, consisting of at least three years of continuous GST data and e.g. some complementary geophysical investigations? Âń

Reply by the authors : we agree with this criticism, but this was the only solution we had available, especially following the possibility to promote bilateral cooperation Portugal-Morocco. The question of the possible presence of permafrost in the High Atlas has not really been addressed previously in the literature and only scattered observations are found, mainly mentioning periglacial landforms, as we explain in the literature review. This being so, attracting significant funding in a competitive call for a mid-term (3 year project) would be almost impossible, since it would only be sustained by the meagre literature and broad working hypotheses. The option was to do an exploratory approach and for that, the funding available were bilateral agreements that partially fund travelling expenses (a few days) and no equipment. The scarce funding, remoteness of the sites and lack of possibility to check on the instrumentation, limited the experimental design to the one we follow in the manuscript. If the hypothesis of permafrost presence was to be confirmed, sustained by peer-reviewed result publication, then a full project application could follow. This is the rationale on the base of our approach. Bilateral projects are 1+1 year (depending of results of year 1) and the funding cycle has driven the science produced. However, we clearly agree that a longer time series is needed and we expect to implement a much better network in the near-future. Comment by Dr Benno Staub : Âń ... However, although the authors clearly point out that the few observations must be interpreted very carefully, the measurement setup and some of the results seem somehow on the limit of being scientifically reasonable. Personally, I doubt that only four miniature loggers can provide a meaningful 'altitudinal gradient', and I would avoid making linear regressions (P7_L3 & Fig. 8) out of it nor extrapolate

these results to a larger area. On the other hand, the publication can be justified because almost no permafrost observations are available for the High Atlas, and because this paper may motivate the mountain permafrost research community to put particular emphasis on that region.

Reply by the authors : We understand the concerns of the reviewer, but we think that we didn't go beyond the scientific reasonability of our results. Across the manuscript we try to balance the fact that the area is almost unknown and not go too far in our conclusions. In what concerns to the comments relating to P7_L3 and Fig.8, the issues are different. In P7_L3 we present results on altitudinal lapse rates from monthly air temperatures. The values that we have obtained are close to the ones from other authors and can be useful also for comparison with other mountain ranges, as for example is presented in well-known synthesis such as Barry or Geiger on mountain and local climates. In Fig. 8, we agree that conditions with soil are very much dependent on micro and toposcale factors, but in our approach we have carefully selected the sites so that they could minimize such influences and maximize the influence of altitude on GST. The exception was T3, which despite the similar overall micro-scale conditions, was in a concave in a valley slope, where snow showed a prevailing influence. This fact is also responsible by the larger residuals. We will follow the suggestions indicated below in the specific comments in order to include uncertainty in the data analysis. We have now also found that we forgot to refer to Fig. 8 in that same paragraph.

SPECIFIC COMMENTS (BY DR BENNO STAUB)

Comment by Dr Benno Staub : Âń Title: The experience from the European Alps showed that one year of GST measurements does not provide reliable results on the ground thermal regime because of the high inter-annual variability of weather and snow conditions. In this regard, I suggest rethinking the title of the manuscript, e.g. towards a more neutral formulation "New observations indicate the occurrence of permafrost in the High Atlas mountain range (Djebel Toubkal, Morocco)". Âń

Reply by the authors : We agree and we will change them title as suggested.

Comment by Dr Benno Staub : Âń Interpretation of GST data: The interpretation of the GST data as a 'BTS signal' is only valid if a thermally insulating snow cover is present for longer than just a few weeks. It seems like logger T3 fulfils this criterion around end of February 2016 (Fig. 10). At T1 and T2, the active layer is likely not in a thermal equilibrium with the permafrost base, these GST records characterise an integral of the recent atmospheric conditions with some modification by a temporarily snow cover. Depending on the terrain roughness and the snow density, about 50-100 cm of snow are required to effectively insulate the ground surface from air temperature variations (e.g. Keller and Gubler (1993); Zhang (2005); Staub and Delaloye (2016)). If there is less snow under winter conditions, the ground is likely colder at its surface than a few meters below. Although the potentially snow-covered period is shorter and snow heights are lower on average in the Toubkal massif than e.g. in the European Alps, the timing and duration of the snow cover probably play a key role for sporadic permafrost occurrence also in the High Atlas – as described by the authors. In comparison to the permafrost areas in the Alps, where the conditions during the winter season are often more important for inter-annual ground temperature variations than the summer warming (cf. PERMOS (2016)), snow disappearance is up to three months earlier in the Toubkal massif (despite of persisting snow patches), even at 3500 m asl. This means that the ground is usually snow free during the entire period of maximal insulation. The local effects of shading could be very important. Probably a GIS-analysis on topo-climatic parameters such as potential incoming solar radiation, slope and curvature could help to characterise the acquired GST data and putting it into the spatial context. Moreover, it might be interesting to quantify ground thawing and freezing degree day sums for the summer and winter period. Âż

Reply by the authors : This is an issue which was well thought during the preparation of the manuscript and we have decided to leave it as we present it. We have weighted well the terminology in order to be objective and stick to the data and to minimize

interpretations. Data shows that T3 is the only site where the Âń BTS Âż assumptions are valid and is the only site where data supports the occurrence of permafrost. For the highest sites, there is no indication that permafrost is present and probably it is not. The shadow effect and also the snow cover and its timing should be the key factors conditioning permafrost distribution in the High Atlas. We thought about using a GIS-based radiation modelling approach, but the points are few and not variable enough (e.g. T1 and T2 are in ridge position with comparable potential radiation, while T3 will have less radiation, but there is no real reason to sustain such a quantitative approach based on potential radiation, with this small sampling sites). The freezing and thawing degree days have been calculated for all sites and we have discussed within the team and with other specialists on their inclusion in the manuscript. However, this indexes were derived essentially for the Polar latitudes and we have decided to stick with the observed data. If needed, we can easily accomodate them in the manuscript, but we would prefer to use this, together with the empirico-statistical modelling approach in a forthcoming study with a much larger number of miniloggers.

Comment by Dr Benno Staub : Âń Measurement setup: A future GST measurement setup around Djebel Toubkal could be installed similarly as described by Gubler et al. (2011) to provide some observational evidence on the GST variability considering different ground materials and topoclimatic situations at least for a few years. I am fully aware of the high financial and logistical effort for such permafrost observations in the remote High Atlas, but I think that such a data basis is required for any further steps towards permafrost mapping and modelling. At best, such spatially distributed GST measurements would be complemented by ERT surveys and geomorphological mapping. Building up a rock glacier and frozen debris lobe inventory. Âż

Reply by the authors : You are right. The setup by Gubler et al (2011) is well-known by our team and we don't really know how we have missed it in the state of the art. It will surely ne one of the experimental setups that we will follow, together with a better altitudinal and aspect design. We will make sure we will accomodate the reference to

Gubler et al. (2011) in the manuscript.

Comment by Dr Benno Staub : Âń Weather and climate data: The authors characterise that particular year with GST observations in the climatological context by using meteorological data (Sect. 4.1, Figs. 3-5, 11 and 13). This is clearly a challenging task regarding the sparse data available, but the spatial transfer of air temperature data over âĹij3500 m elevation between Menara at Marrakesh to the Djebel Toubkal mountain is not satisfactory from a scientific point of view. Although the lapse rates provided and calculated seem plausible, these lapse rates likely vary over the season and the weather conditions can be very different in the mountains to what is measured in Marrakesh. The "significant correlation" of monthly air temperature values between Neltner and Sidi Chamarouch (P6_L14 and Fig. 3) is likely a result of the high seasonal temperature amplitudes. However, the comparison to other quantitative and qualitative data sources could be extended. For example, the snow climatology could be analysed over the entire period of available satellite imagery. Also satellite-derived land surface temperature data could enhance the comparison of the period 2015-16 in a larger temporal and spatial context – of course with limitations due to the lack of validation data and the difficulties in mountainous terrain. Maybe c even RCM reanalysis data could help to assess the regional climate history.

Reply by the authors : we agree with your comments. We will approach the regional climate variability using grided reanalysis data and evaluate how it improves the current approach. Snow climatology from remote sensing data would be the scope of a whole new approach, especially due to winter cloudiness and we will not do it here. The same applies to Land surface temperatures which are very much dependent on cloudiness and time of the day and there is not too much validation data. We will improve this part and change it in the next few days.

P7_L20: Clarify that you mean daily maxima in the sentence "A plateau in the maxima. . ."

Reply: Right. We will change it to ÂżA plateau in the daily maxima. . . Âż

P7_L23-24: I would not state the relationship between elevation and MAGST of these four locations as "statistically significant" and rather try to quantify the uncertainty of each data point. The uncertainty of MAGST is likely much higher than ±0.4âŮęC/100m. Observations from the Swiss Alps show that elevation can be a poor proxy for MAGST, depending on the terrain and snow characteristics (boulder size, terrain roughness, solar irradiation, exposure to wind, and accumulation of snow by wind or avalanches) and regional weather patterns (e.g. Gubler et al (2011)).

Reply : we will rephrase this sentence and will review this approach. The sites were selected in order to have a larger effect of altitude than of other factors, but including uncertainty in the analysis is an important point.

Fig. 5: Clarify, that the dashed line is the extrapolation for the summit of Djebel Toubkal. If possible, add an uncertainty estimate (e.g. using a range of lapse rates).

Reply : OK, we will do that. Somehow it is missing in the legend.

Fig. 6: Add readable point labels and a legend for the colours.

Reply : OK, we will do as suggested.

Fig. 7: What are "daily hourly maxima"?

Reply : the caption will be corrected : Âń Extremes are absolute monthly maximum and minimum temperatures.

Fig. 8: See comment above. Maybe add an uncertainty estimate to each point?

Reply : Yes, we will do as suggested.

Fig. 10: GST data series can be calibrated during the zero curtain period. It is visually not clear, if this calibration was done or if the dashed line is not really at 0âŮęC at some of the time series.

Reply : The calibration was not done, but in fact when preparing the figure, somehow the dashed line moved away from 0°C in some of the figures. We will correct this.

Interactive comment on The Cryosphere Discuss., doi:10.5194/tc-2016-234, 2016.