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Earth Surface Dynamics Discussions

Interactive comment on "Implications of present ground temperatures and relict stone stripes in the Ethiopian Highlands for the palaeoclimate of the tropics" *by* Alexander R. Groos et al.

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Referee report on: Implications of present ground temperatures and relict stone stripes in the Ethiopian Highlands for the palaeoclimate of the tropics Authors: Groos et al.

General comments I am pleased to see that the Bale Mnts are receiving this geomorphic attention it deserves. I have worked on a variety of periglacial landforms in various African environments over many years now - but to me the large scale sorted stone stripes presented in this paper from the Sanetti Plateau, are the most special and unique periglacial landforms I have yet seen in Africa. They are truly special and must rank amongst the largest and best examples globally, I would think. So work on

these is certainly called for and important to publish. I think the big challenge with these amazing landforms is ascertaining when they developed, how long they were actively forming for and when they might have become inactive (relict) periglacial phenomena. The second great challenge is ascertaining how they formed, because any misunderstanding as to their formation has serious implications to any climatic controls we attach to their genesis. I think these challenges are very real for this paper and when I read the work I see that this is where the paper has its struggles. I have several major concerns with this paper, which I will outline next, but at the same time wish to also assist with providing suggestions that might help rework this paper into something that might be publishable.

Major concerns to address 1. The paper is far too long and tries to tackle too many things with too much detail, such that the connections between the various bits of collected data/information, become somewhat muddled and lost in the discussion/conclusion. The extent of detail to such things as instrumentation and story behind the logger battery issues etc may be valuable to place in a technical report or PhD thesis, but is not suitable for a journal publication. The text requires substantial trimming down and tightening up throughout. 2. Although the written style is relatively uncomplicated and for the most part satisfactory, there is a tendency towards colloquial language style, which is not suitable. The written style thus requires considerable improvement for publication. 3. The Scientific methods are notable and impressive for such a region given the logistical hurdles. However, great or large quantities of data may not always be the most useful or necessary data for the study objectives. - While it is great having 36Cl results for the landforms, these raise more questions than provide answers. These do not necessarily inform us when the landforms first developed, or how long they were actively forming for, or when they became 'periglacially inactive'. So, despite all the efforts in obtaining exposure ages, the authors are still left with merely assuming that the landforms are of Late Glacial age. Such an assumption might be reached without the exposure ages and have in fact also been made for other openwork block deposits (e.g. block streams) in the high Drakensberg (southern

Africa) – see for e.g. Boelhouwers et al. (2002). Without any real sense of timeframe, it is impossible to use the landforms for any palaeo-climate reconstructions. - Great effort was also undertaken with the Ground-penetrating radar measurements; something not previously done for periglacial landforms in Africa. However, the results to me do not show much that is of significance - and so does not add enough value to provide for anything noteworthy to add to the discussion or meet the aim/objectives of this paper. I would like to be proven wrong here – so if the authors can indeed use these data in a way that enhances/strengthens the discussion, then that would be good. - The authors provide considerable temperature data (ground and air). In fact I think too much is attempted with these temperature records and in the process of trying too much with it (also too many graphs), the scientific value and merit is lost. I will elaborate on temperature data separately as this constitutes a major concern. 4. Temperature data: While the temperature data recorded at various localities might be used for various scientific purposes, I think the way in which the data have been used in this paper requires very careful reconsideration. - The work is built upon the presumption that the sorted stripes are a product of past seasonal or sporadic permafrost that would have required ground temps of -1°C . . .or a thermal reduction of around 12°C from those recorded more recently. And the authors argue on their modelling basis that air temps would thus have been lowered by around 7.6°C. This is all highly presumptuous and very controversial. In the first instance, ground temperatures were measured in the finer textured soil stripes and not within the openwork block deposits. When these features first formed, they may have formed in a scree of such open-work block deposition because of unique localized air flow (cooling) with depth through such openwork material, thus possibly creating 'pockets' of long lasting frozen ground phenomena (be it extended seasonal freeze or permafrost). So, the soils in which the authors have done their measurements may not have been as extensively frozen as for instance in the adjacent blocky material. Please see some published work which has shown enhanced cooling through blocky periglacial phenomena (e.g. Harris & Pedersen (1998) show much colder ground thermal conditions below blocky

materials than finer textured regolith cover). - A further point is that the authors have not considered the likely thermal impacts of snow in a palaeo-environmental context. It is thus impossible to begin modelling likely air temperature reductions unless we know 1) the actual palaeo-ground temperature at exactly the same site as the contemporary measurements were taken (which is assumed to have been below 0°C – but very much built on assumption as the stripes may have formed when there was deep freeze beneath the blocky material but only limited/shallow/seasonal freeze in the finer textured soil stripes), and 2) the depth, duration etc of snow which would have had an insulating effect – or maybe helped preserve cooling during particular times of the year etc. The distribution and thickness of snow across the landscape would almost certainly have had impacts on the spatial/temporal characteristics of ground freeze and thaw during past colder periods. In summary re the temperature data – it is 'stretching the data too far' to try and start modelling past air temperatures as the scientific context is far too simplistic in the way it has been presented here. In reality, the contexts are much more complicated than the authors make it out to be. At best, I think the authors can use contemporary ground temperature data to reflect on contemporary shallow soil frost phenomena.

More detailed technical matters to address: P3, line 23: How do you define 'alpine environment' . . .on what basis? Is it based on a Eurocentric view of 'alpine', or is it based on what has commonly been defined as the 'Afro-alpine' zone? I am not advocating any given view but the authors should define what they understand makes the Bale Mnts the largest African 'alpine' environment. . .as opposed to for instance the Atlas Mnts or high Drakensberg-Maloti mnt system in Lesotho, Southern Africa (in both these cases one might argue for extensive 'alpine and/or Afro-alpine' regions which are larger than that of the Bale).

P5, lines 13-14- the values of glacial extent mentioned here is according to who? Needs a reference.

P5, line 20: the authors say here that the large periglacial features are associated with

freeze-thaw processes. Unless the authors can verify that they have measured freezing and thawing dynamics here, and that these mechanisms produced these landforms, then this is a scientific assumption. So rather write as '. . ..features are likely associated with. . ..'

P 5, line 30 would read better to say are 'endemic to'

P13, line 11: Stone stripes apparently required a thick active layer. Why do you say it had to be thick? What do you understand to be 'thick' rather than 'thin'? What dimensions are we dealing with here? Can it be that the relict sorted stripe sorting depth might say something re to active layer thickness. . .or depth to which [periglacial] geomorphic mechanisms operated?

P14 – at the bottom of this page the authors list so called 'frost-induced phenomena' such as frozen waterfalls, needle ice, patterned ground and solifluction lobes. This is a bit confusing as it mixes geomorphic periglacial landform types (i.e. patterned ground and solifluction lobes) with ice types (massive ice as frozen waterfalls or needle ice developed in soil). Ground ice types might be seen as mechanistic agents, while the landforms might be seen as products of the former.

Figure 4: These are impressive photos and all valuable to add here. In photo g, I can see the patterned ground (blocky borders) – in fact they look impressive to me, but the dotted white line that the authors have placed to supposedly outline the borders (shape) do not correspond with the pattern border localities in the photo.

The caption to Figure 4 is a bit misleading I think. It informs the reader that these photos show us the 'Periglacial environment of the Bale Mnts'. In the first instance, it shows contemporary phenomena of a frozen waterfall and needle ice (i.e. the contemporary environment). These features do not qualify this to be labelled a contemporary periglacial environment as the ground temps show very temporally limited and shallow diurnal freeze only, and the contemporary active cryo-geomorphic environment has a negligible effect on the landscape/landforms today. However, the larger relict landforms

show us that this was indeed once a periglacial environment. So the caption could read something like 'Contemporary seasonal ice phenomena and relict periglacial landforms of the Bale Mnts'

Figure 5: It is problematic to show the location of only one needle ice site and only one frozen water fall locality. Firstly, there were likely other sites with needle ice at the time of observation. . .as also for frozen waterfalls or seepage out of rock at some localities. Secondly, the needle ice shown on the map is not a permanent feature at that locality, neither is the ice on the cliff face – which is hence problematic to show on a map. In Contrast, the other geomorphic phenomena mapped are permanent on the landscape (at least for the generation that will read this article) and thus suitable for mapping. Are the waterfalls (as shown in the photo) frozen every year? For how many months each year?

Figure 5b shows 3 exposure age locations but only one age given. '620' requires an indication of scale of age used. Why does the word saturated appear twice on the map? Is this not also a bit problematic. . .unless it is permanently saturated at that locality? Figure 5c three numeric values given. . ....what are these . . .age scale used?

P17, line 2: the authors say that the deposits are associated with so called 'frost weathering'. How do you know for certain that it was due to 'frost weathering' . . .and not maybe a combination of different weathering mechanisms of which freezing/thawing of water might be one? This would then also imply potential thermal stress (thermoclastis) as an additional weathering type. I think greater scientific caution and rigor is required with statements such as these.

Figure 7: When I examine your temperature records over the period 2017 to 2019 in this Figure, I am concerned that your 2cm and 10cm ground temperature data may not actually represent the temperatures at a fixed depth through time because I can see that their amplitudes (in both the positive and negative directions) increases progressively through time. This is of course typical to a situation where your thermistor has

shifted upwards through the soil profile.

For temperature measurements and discussion re temperatures – why do you interchange between Kelvin and °C? Please keep to °C.

Way forward I think that the greatest strength this paper has to offer is in showcasing the very unique large sorted stripes and possibly large sorted patterned ground. Showcasing these features and finding a way to show their environmental significance (in a scientifically robust manner), surely merits publication, albeit as a much shorter article than the one submitted currently. I suggest a much trimmed down version of this paper: 1) briefly describing contemporary soil frost dynamics and small-scale contemporary soil frost phenomena – where some of the temperature data could be included, and 2) showcasing the large relict features with mapping data and field based measurement data (I currently do not see the value of the 36Cl and ground penetrating radar data). From these, one could then build an interesting but focused and concise discussion (along the lines of some of the discussion on p25, lines 17-31 – which I quite like). I caution against trying to make too much inference from relict landforms for which we still know relatively little in terms of their mechanisms of formation and thus underlying ground and air climatic requirements. It would thus not be possible to say too much about palaeo-climates for this region, let alone the tropics as a whole as the title of the paper implies. It might be worth saying something about the geo-heritage & geo-tourism potential here given the rarity/uniqueness of the landforms.

References: Boelhouwers, J., Holness, S., Meiklejohn, I., & Sumner, P. (2002). Observations on a blockstream in the vicinity of Sani Pass, Lesotho Highlands, southern Africa. Permafrost and Periglacial Processes, 13(4), 251-257. Harris, S. A., & Pedersen, D. E. (1998). Thermal regimes beneath coarse blocky materials. Permafrost and Periglacial Processes, 9(2), 107-120.

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