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# *Interactive comment on* "Arctic soil development on a series of marine terraces on Central Spitsbergen, Svalbard: a combined geochronology, fieldwork and modelling approach" *by* W. M. van der Meij et al.

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Dear reviewer,

We would like to thank you for your time and effort to review our article on Arctic soil development. The comments you provided are very useful and will certainly improve the article. Here we respond to the comments.

Major concerns



*1) Ages of the terraces The presentation of the age constraints is quite confusing. Surface ages are reported to a certain extent already in the text, in chapter 2.1. Some references are given and the reader is referred to Fig. 1. The authors report about the 6 terraces but no real ages are shown (for the individual terraces). A correlation of ages with altitude is shown in Fig. 3, but much later on – but no relation to the terraces is given. So, the reader is fed portion-wise with surface age data – and this makes the lecture of the manuscript quite difficult. I furthermore did not find out the ages of all terraces (even after having read the whole manuscript). Please create an additional table where each terrace is assigned to a specific age or age range.*

RESPONSE: In the revised manuscript we paid more attention to the existing ages of the different terrace levels, in order to make it a more coherent story. We did not date all major terrace levels, but the combination of our OSL dates with earlier radiocarbon dates cover most of them. Please note that the major terrace levels contain smaller ridge and valley sequences, thus several ages. With the relation between altitude and age we have derived the age constraints of each major terrace level, but this is one of the results of the study, just like the OSL ages, and is thus reported in the Results section (in Table 3, as suggested).

*2) Soil data: The soil dataset should be presented in a better legible way. What is the use of Table 3? It only shows the average (±SD) of the entire dataset of some soil parameters. Why not presenting this dataset for each terrace? The reader would then have the possibility to see how these parameters are changing as a function of time. Or include at least the parameters such as the CaCO3 content, BD and horizon thickness in Figure 5 and give an earlier reference to this figure in the text (I however would prefer a table). Furthermore, Fig. 5 suggests that all soils have the same horizons. Is this really true?*

RESPONSE: With Table 3 we aimed to show summary statistics of the observed soil properties for the study area. We have now extended the table and present soil properties per horizon per terrace level. Fig. 5 acts as a visualization of Table 3. In our opinion, this visualization is helpful in better understanding possible trends in the soil properties. We have extended this Figure with bulk density and horizon thickness for a more complete picture. We have also added the number of occurrences for each box in Fig. 5, to show that not every horizon is found in each soil.

*In addition, the horizon designation is slightly confusing. Here maybe some more explanations could be given in the methods section) I know which principle has been employed (it is explained in the text). E.g. Fig. 4: The 'typical' soil left seems to have an aeolian deposit on top (this seems to be the C-material, right?). If 1 now stands for aeolian material and 2 for marine material, should the horizon sequence not be the following: 1C, 1bA, 2bA, 2bB . . .? In addition, the horizon 'B/' appears (which was obviously proposed by Forman and Miller, 1984). This seems to be a designation that neither exists in the WRB nor in the Soil Taxonomy. It seems to stand for 'silt illuviation'. But when having a look at Fig. 5, I do not see a higher silt concentration in the B/ horizon. (furthermore, part of the figure is cut off . . . silt fraction of T3). What are the process for silt illuviation?*

RESPONSE: We did not adapt the formal WRB horizon designation, because it is easier to compare similar horizons when they are given the same name. Therefore we named all marine horizons 2A, 2B, 2Bl or 2BC, although indeed only some of them are buried below the aeolian deposits. We have explained this better in the text, where we also mention the correct horizon designations. Organic matter can be found throughout the sandy aeolian cover. Therefore we previously named it an A horizon. However, you have convinced us that the designation 1AC is more appropriate, and we have used it in the revised paper. Evidence for silt eluviation are the increase of silt concentration of Bl horizons with age, while the concentration in 2A horizons decreases (Fig. 5). Next to that, the belly shaped curves in Fig. 7 also show an enrichment of silt in the subsurface. The very porous gravelly soils enable percolation of water, which can transport silt. In the field we could see evidence of this process by layers of silt on top of rocks and soil layers where the matrix was enriched with silt (Bl horizons). The small case italic L (*l*)



indicates the pedogenic accumulation of silt. We have better explained the processes behind silt eluviation in the manuscript.

*3) more details about OSL dating: How were the samples taken? Were really marine samples analysed? Why should such a material be suitable for dating? How should bleaching have occurred? . . . Or did you sample the loess deposits? This needs to be better explained.*

RESPONSE: Thank you for pointing out that details about the OSL dating were missing. We have now elaborated on the OSL dating in the Methods section. We analyzed the sand fraction of the marine material, which is probably completely bleached due to reworking in the swash zone. The lab results also indicated no evidence of partial bleaching.

Minor points

*p. 1347, L. 25: what is 'relative' physical weathering?*

RESPONSE: This referred to the relative contribution of physical and chemical weathering. We have rephrased this.

*p. 1349, L. 9-23: if terraces are so complicated – why did you choose them for your investigation?*

RESPONSE: We chose marine terraces because they exhibit a chronosequence of long-term Arctic soil development. The mentioned complicating factors distort the temporal signal, but they give valuable information on spatial drivers of soil formation. Therefore the marine terraces are suitable for the study of long term Arctic soil development on a landscape scale. We elaborated on this in the manuscript.

*p. 1350, L. 6-17: should maybe be moved to the methods section.*

RESPONSE: We used this paragraph to introduce the structure of the paper, like a reading guide. In the Methods section we elaborate on all mentioned methods. Therefore we believe it is suited for the Introduction.

*p. 1351, L. 19-22: this sentence is not understandable.*

RESPONSE: We have removed this sentence and elaborated more on the results of Long et al. (2012), as mentioned earlier in this response.

*p. 1355, L. 18: '. . .. horizons were not cryic'. This is difficult to believe for such an environment. Please explain. p. 1361, L. 20-21: Phaeozems and Chernozems. Sure? They would testify quite a different climate that obviously had existed in the past. p. 1361, L. 17-24: Several soil units are mentioned here but the reader cannot allocate them to the terraces. As mentioned already above, the soil data should be better presented. Instead of Fig. 5 a table showing all parameters (see above) per terrace unit (average values ± SD) and soil units should be presented.*

RESPONSE: we omitted the fact that all soils were probably Cryosols, in order to better describe the variation in observed soils. However, as this leads to confusion, e.g. on the genesis of the soils, we have now used the proper WRB classification and used qualifiers to indicate the observed soil diversity. The variation in soil units mainly depends on morphological setting instead of age, as is mentioned in the text. This is in fact a main result of the paper. Therefore it would not be useful to include the soil units in Table 3, which shows temporal variation. We now better describe how the different soils are positioned in the landscape.

*p. 1356, L. 5: ANOVA → do you have a normal distribution of the datasets?*

RESPONSE: The residuals of a linear model with soil properties and terrace level, soil horizon and morphological position were normally distributed, according to the Shapiro-Wilk test. Therefore the use of ANOVA was justified. For the silt fraction, we had to apply a log transformation to achieve normality. This is now updated in the new version.

*p., L. 19: why a log function? Published data that substantiate such an assumption?*



RESPONSE: The physical weathering equation in LORICA results in an exponential decay of coarse material. Therefore we used this formulation to calculate the weathering rate. Fig. 5 also suggests an exponential decay of gravel fraction for the 2Bl and 2BC horizon. We have explained this better in the revision.

*p. 1361, L. 12: ' . . .approximately 14393 years old' . . .. I think you know what I mean . . .*

RESPONSE: That age is indeed not very approximate. As this age serves as input for the model, we left out the 'approximate' and only noted the age (13300 years, following from the new uplift curve).

*p. 1362, L. 20: R2 = 0.29: is this significant? p. 1367, L. 6-7: where is this regression presented?*

RESPONSE: The p-value of this regression is 0.007, and can therefore be considered significant. This is the same regression as referred to later in the text (second comment). We clarified this in this section.

*p. 1362, L. 23: where is this number coming from? How was it determined? (please show it in a way that it is traceable for the reader).*

RESPONSE: This number follows from Eq. 4 and the procedure explained on page 1359, L. 22-25. We have explained this now better in the main text.

*p. 1366, L. 11-15: what about permafrost? I assume that there is permafrost. How deep is the active layer?*

RESPONSE: With the impermeable layer mentioned in Section 5.2 we indeed mean permafrost table (and to a lesser extent the active layer). We have now clarified this. The thickness of the active layer in the vicinity of the study area varies between 0.3 and 2.5 m (Gibas et al., 2005), with thaw depths on the marine terraces ranging from 0.45 to 1.20 m below the surface (Rachlewicz and Szczuciński, 2008) (this information is also added to the manuscript).

*p. 1367, L. 2: two times 'from the simulated'*

RESPONSE: One time removed.

*p. 1367, L. 28: Fig. 7 does not show a spatial distribution.*

RESPONSE: Changed to: "This expected variation in weathering between different morphological settings was observed in particle size distributions (Fig. 7),. . .".

*p. 1368, L. 4: physical weathering? (if calculated from the gravel fraction. . .).*

RESPONSE: Correct, we have changed it.

*Technical corrections: Table 3 (in my opinion not that useful). But if used, please use more common units for BD, such as t/m3, kg/dm3 or g/cm3*

RESPONSE: We have adapted to g/cm3.

References:

Gibas, J., Rachlewicz, G., and Szczuciński, W.: Application of DC resistivity soundings and geomorphological surveys in studies of modern Arctic glacier marginal zones, Petuniabukta, Spitsbergen, Polish Polar Research, 26, 239-258, 2005.

Long, A. J., Strzelecki, M. C., Lloyd, J. M., and Bryant, C. L.: Dating High Arctic Holocene relative sea level changes using juvenile articulated marine shells in raised beaches, Quaternary Science Reviews, 48, 61-66, 2012.

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