

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.

Anonymous Referee #1

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General comments:

The authors investigated a Holocene (late Pleistocene) soil and landscape chronosequence of gravelly raised marine terraces in an arctic environment. Besides surface dating using OSL and combining the results with existing C-14 data, soils were analysed and soil landscapes modelled using LORICA. The procedure and research questions are innovative and the results convincing. Although modelling is always bound to assumptions, negligence and consequently difficulties, the authors nicely show that arctic soil development is not straightforward and that modelling can contribute to our

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understanding of soil and landscape development. Consequently, this is an interesting paper and merits to be published. There are however several shortcomings that need to be addressed.

Specific 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. 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 (\pm 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? 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

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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? 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.

Minor points: p. 1347, L. 25: what is 'relative' physical weathering? p. 1349, L. 9-23: if terraces are so complicated – why did you choose them for your investigation? p. 1350, L. 6-17: should maybe be moved to the methods section. p. 1351, L. 19-22: this sentence is not understandable. p. 1355, L. 18: '.... horizons were not cryic'. This is difficult to believe for such an environment. Please explain. p. 1356, L. 5: ANOVA → do you have a normal distribution of the datasets? p. 1359, L. 19: why a log function? Published data that substantiate such an assumption? p. 1361, L. 12: '... approximately 14393 years old' I think you know what I mean ... 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 \pm SD) and soil units should be presented. p. 1361, L. 20-21: Phaeozems and Chernozems. Sure? They would testify quite a different climate that obviously had existed in the past. p. 1362, L. 20: $R^2 = 0.29$: is this significant? 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). p. 1366, L. 11-15: what about permafrost? I assume that there is permafrost. How deep is the active layer? p. 1367, L. 2: two times 'from the simulated' p. 1367, L. 6-7: where is this regression presented? p. 1367, L. 28: Fig. 7 does not show a spatial distribution. p. 1368, L. 4: physical weathering? (if calculated from the gravel fraction. ...).

Technical corrections:

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

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