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Middle Bronze Age land use practices in the north-western Alpine foreland – A multi-proxy study of colluvial deposits, archaeological features and peatbogs

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Final response to referee comments #1 and #2

Dear Mr. Homburg,

Thank you for handling our manuscript as topical editor within the SOIL review process. We have received two constructive comments from referee #1 and #2. We have answered their comments in consultation with all co-authors and we will revise the manuscript accordingly. If more information is needed prior to submission of the revised manuscript, please feel free to contact me for further discussion.

Sincerely,

Sascha Scherer and co-authors

Reply to comments from referee#1

One comment, I would suggest one or two sentences defining colluvial deposits in the manner in which they are used in this paper. I am aware that the term originated in Germany, but in North America it is used in a slightly different manner.

Thank you for this comment. We are aware of the different meanings of colluvial deposits, especially outside of Germany and we tried to consider this by defining and explaining colluvial deposits in the manner we use it in this paper (lines 74-83). For more clarity, we would change these lines to:

“With respect to the diverse definitions of colluvial deposits as hillslope sediments transported by either mass-gravity transport or slopewash type processes (Miller and Juilleret, 2020), we refer to the German term ‘Kolluvium’, which defines a colluvial deposit as the correlate sediment of human-induced soil erosion (e.g. Kadereit et al., 2010). In Central Europe, the human impact on the terrestrial ecosystem has distinctly increased since the Late Holocene and land use practices such as deforestation and soil tillage have left land use traces (e.g. charcoal, archaeobotanical and biogeochemical proxies) that are archived in colluvial deposits by impeding degradation processes through continuous sedimentation. Therefore, colluvial deposits can be considered one of the key archives for human-land interactions (Pietsch and Kühn, 2017; Zádorová, and Penížek, 2018) as they store not only the history of climate and sedimentation, but also proxies for past land use change and associated practices (Dreibrodt et al., 2010a;b; Scherer et al., 2021)."

The dating techniques are fairly standard, and I like the use of both OSL and radiocarbon dates, these two provide complimentary data. Dating can be tricky, and the context details often matter, but for a paper of this type it is not appropriate to present a detailed discussion of such contexts.

Since the dating of the colluvial deposits is the basis for the interpretation and discussion of the other analytical proxies related to the MBA we think it is necessary to include a short, but detailed discussion of the OSL and ^{14}C dating results to show the robustness of the chronostratigraphy.

The phytolith analysis contains very little specific identifications, say to the family or better. I am very familiar with phytoliths, is this because the remains are not identifiable or were these lumped for some reason? I don't find this problematic with phytoliths but am curious if this was a choice or the result of identification restraints.

We highlight the grass family and its sub-family (Pooid, C3, rondel-type) phytolith part of the assemblage as these were the most prevalent and identifiable within the monocot remains. Within the dicots, the only differences that could be understood were between the wood/bark and leaf components. Due to the preservation of the assemblages, further specificity was not possible within the identifications.

The micromorphology section (423-450) is one of the weaker parts of the paper. This is due in part to the small sample size of two. The section is an adequate description of the thin sections, but it lacks the inferential links to the main conclusions of the paper, which as I see it is human farming and landscape management in the MBA. This section should either include some statement of how it supports, or refutes, the primary arguments, of perhaps be eliminated. I like micromorph analysis, but it needs the right context to contribute meaningful results and that context is missing here.

Thank you for this comment. We used this micromorphological analysis to further prove an agricultural activity in the 2Apb horizon, which we so far interpreted only from field description. Micromorphology is the only analytical technique to prove that *in situ*. The sample size of two seems to be small, but each sample has a size of 60 x 90 mm (see methods section). Thus, it covers with 180 mm in length nearly the complete 2Apb horizon. The micromorphological results are supported by the results from phytolith analysis and the archaeobotanical determination of charred plant macro-remains.

For more clarity and inferential links to the main conclusion of the paper, we would add/change:

- Line 421 ff.: "Two thin sections from the 2Apb horizon at the ABR SA2 profile were analyzed to further support MBA ploughing activity. The thin section from the upper part of the 2Apb horizon (183-193 cm) had a […]"
- Line 710 ff.: "So far, MBA plow marks and soil-structural features indicating tillage have not been published for the north-western Alpine foreland at inland sites. Therefore, the micromorphological analysis of thin section from a buried plough horizon offers an unique opportunity of identifying prehistoric tillage practices. Evidence of arable

farming derived from phytolith analysis is further supported by micromorphological analysis of two thin sections at the 2Apb horizon (ABR SA2), which indicates the use of ards that were widely spread since the Late Neolithic (see Tegtmeier, 1987). Very dusty clay coatings […]"

The animal bones are another direct source of information on human subsistence at this site. The low level of identification is attributed to fragmentation of the bone. Is this a preservation issue or was it a result of bone processing? Given the species represented bone processing to extract marrow is a very real possibility and would render (no pun intended) the collection largely unidentifiable to species. Cut marks are rare, but were the bones intentionally split with spiral fracturing and impact fractures being present?
The low level of identification was a preservation issue common to sites in southwestern Germany with aerobic soil conditions and soil erosion and sedimentation processes (Stephan, 2016). Though the extraction of marrow seems interesting and has been also discussed in studies of the north-western Alpine foreland (Köninger, 2006), our zooarchaeological colleague would exclude it for two reasons:

- Bone processing like cut marks and spiral fractures were only marginally identified (0.3%, line 521).
- Cut marks and spiral fractures are commonly identified at long-bones; however, we mainly identified ribs and skull fragments as well as plate-bone fragments.

Also, these data suggest that goats/sheep and cattle were more common than pigs, so how does that fit into the landscape management model? Finally, by the nature of their much larger size I would think that cattle were the most important meat source, so landscape management in favor of cattle might also make sense, although cattle feed might be partly imbedded in the cereal grain production cycle.
Thank you for this interesting comment. We will change chapter *4.2.3 Livestock husbandry* according your comment and our response to it.
The data from archaeozoological and faecal biomarker analysis may seem to be contradictory, but we understand it in a way that they complement one another, if we consider different input pathways of animal bones and faecal biomarker into the soil. Assuming that every adult animal is used as meat source, the animal bone record is dominated by cattle (30%) and goat/sheep (27%), while pig played a significant but less important role (17%). However, based on the results from faecal biomarker analysis and the considerable occurrence of 5ß-stanole in the colluvial deposits north of the settlement area, it can be assumed that mainly pigs may have been kept there. Apart from this, herbivorous biomarkers identified mainly in colluvial deposits south of the settlement area suggests grazing on fallow land and/or manure production. In summary, the animal bone record provides quantitative information on general livestock and wildlife species distribution in the MBA settlement catchment, while the faecal biomarker allow for a spatial information of livestock husbandry. Taken together, the two data sets suggest a diverse landscape management model.
We would also consider pigs as important meat source, since cattle may have been served as draft animals and for milk production as well (Stephan, 2016).

First, the pollen diagram seems to indicate that the area is largely forested throughout the Bronze Age, or I would expect even higher percentages of grass pollen. In North America I am used to seeing dramatic grass increases as forests decrease, although this is usually attributed to climate and less to human manipulation. Are the posited pastures relatively small compared to the forested areas? If so is this a function of lower population density?
In this area of the north-western Alpine foreland, the potential natural vegetation is beech forests and grass pollen would only increase by significant opening of the landscape. However, we cannot comment on the comparison with North America, as more geographical context would be required. The pastures are indeed relatively small - compared to latter periods like Iron and Roman Age, also compared with the those periods the population density is lower.

I am a little surprised that with the clear ubiquity of the cereal grains that we don't see more grass pollen. I am curious if this indicates that while there is an increase of humans and human influence in the Alpine forelands during the Middle Bronze Age, that the populations were still relatively low.
Thank you for this comment. For a better understanding, we will change chapter *4.3 Offsite vegetation signals in relation to land use practices at the ABR site* accordingly.
The peatbogs from which the offsite pollen records originate are not in immediate vicinity of the study site, but in the wider catchment area. Therefore, they indicate only a general vegetation change and no onsite human impact. Through "closed" forested landscapes, the distribution of the pollen of cultivated cereals is hindered by the filtering effect of the forests. Due to their size (ca. 40-55 µm), the spread of the Cerealia-type pollen is hindered if they are not in the immediate surrounding of the depositional environment. Moreover the pollination of the cereal crops (*Triticum, Hordeum*) used in this period is cleistogamic - this means the flower is closed – which also contributes to a limited pollen spread. All these factors are reducing the chance to get a lot of cereal pollen in the peatbogs considered.
However, the Hardtsee pollen profile shows a massive increase in non-arboreal pollen around 1450 BCE, which is probably the result of a nearby MBA settlement (Hald, 2016). This is in line with other pollen profiles from peatbogs in the north-western Alpine foreland. During the Neolithic, the whole area is mainly forested, while the initial and sustainable opening of the landscape is observed since the Bronze Age (Lechterbeck 2016).

Reply to comments from referee#2

Since SOIL is an international journal it would be nice to see these soils keyed out using the U.S. soil taxonomic system. Perhaps not all the way to the family level but at least to the great group.
Thanks for this comment. We agree that for a better understanding of the soils, basic information from the U.S. Soil Taxonomy classification are helpful. Based on our field description and laboratory analysis, we have classified the soils as *Dystrudepts*. We will include this information in the header of table 1.

While the limited discussion of the geochronology is appropriate for this type of publication, I do have a few general comments:
OSL dating of colluvial/slope wash deposits is often problematic due to incomplete bleaching and short transport distances. Scatter in equivalent dose distribution plots from such contexts

typically show a significant number of high-dose rate grains making accurate age determinations difficult. I would be interested in learning more about how the dose rate was determined considering many of the samples were collected from soil horizons near stratigraphic boundaries.

We agree that bleaching can be an issue when dating colluvial samples using OSL. The samples measured for this study exemplarily show that bleaching can be site specific. While OSL samples from section ABR-W1 and ABR-W2 showed a high degree of bleaching, expressed in low overdispersion values, many samples from ABR-M and ABR-SA indeed showed signs for incomplete bleaching, expressed in a high overdispersion and positively skewed distributions. Factors such as transport distance, sedimentation rates and shadowing of the slope probably play an important role on the degree of bleaching of slope deposits. In case incomplete bleaching was identified, we applied a minimum age model to address this problem.

We are also aware that dose rate determination is complex when samples are taken near stratigraphic boundaries. However, dose rates of the samples investigated in this study are very uniform. Furthermore, when taking samples for radionuclide determination, we try to stay well away from stratigraphic boundaries. If this is not possible, we take samples representative of the 30 cm radius surrounding the sample, hence we include material from the neighboring stratigraphic unit.

For more information, we will add the D_e-distribution in the supplementary.

Concerning the radiocarbon chronology, while the summed probability density of the calibrated ages is compelling, the sample population is relatively small and consequently, the shape of the SPD plot is likely related (at least partially) to the shape of the calibration curve. The 400 BC radiocarbon plateau (Hallstatt plateau), although post bronze age, is particularly problematic and may be responsible for the peak in the SPD. Just a note of caution when interpreting the summed distributions.

Thank you for this comment. Certainly, the SPDs may be related to the shape of the calibration curve, which is particularly challenging with respect to the Hallstatt plateau. However, several archaeological structures (including charcoals and finds) were dated to the MBA as well (Höpfer et al., accepted). Since reviewer#1 has proposed to shorten the discussion of the dating results, we refrain from adding some extra information to the existing manuscript to balance the two referee comments.

References to be additionally included in the manuscript

Miller, B. A. and Juilleret, J.: The colluvium and alluvium problem: Historical review and current state of definitions. Earth Sci Reviews, https://doi.org/10.1016/j.earscirev.2020.103316, 2020.

Tegtmeier, U.: Neolithische und bronzezeitliche Pflugspuren in Norddeutschland und in den Niederlanden. Archäologische Informationen, 10(1), 1987.

Lechterbeck, J.: Quantitative analysis of pollen data for the assessment of human impact in the Neolithic of Lake Constance - a comparison of methods. In: Kerig, T., Nowak, K. and Roth, G. (eds.): Alles was zählt. Festschrift für Andreas Zimmermann, Universitätsforschungen zur prähistorischen Archäologie 285, 299 – 314, 2020.