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Comment on soil-2020-84

Steven Shelley (Referee)

Referee comment on "Middle Bronze Age land use practices in the north-western Alpine foreland - A multi-proxy study of colluvial deposits, archaeological features and peat bogs" by Sascha Scherer et al., SOIL Discuss., https://doi.org/10.5194/soil-2020-84-RC1, 2021

Scientific significance: I would rate this paper as a good contribution. It does presents multidisciplinary evidence for human land use, so much so that I had to consider whether it was appropriate for a Soil, but in the end, it does show how soil, and the physical and chemical remains within the soil, can be used in an interdisciplinary approach to gain an understanding of the past. I am a North American archaeologist who has done similar types of studies in the American Southwest, and thus was very interested in how they used so many lines of evidence to support their conclusions.

Scientific Quality: I think this paper is excellent in its use of a variety of scientific methods and it really is an interdisciplinary approach where the various disciplines support the final conclusions.

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. To a North American geologist or geoarchaeologist colluvium is unconsolidated sediments that have been deposited at the base of hillslopes, usually in the footslope or toeslope position, by either rainwash, sheetwash, slow continuous downslope creep, or through a combination of these processes. Since Soil is international, a couple of sentences discussing in slightly more detail how the term used here differs would save some initial confusion for some readers.

As with colluvium, the soils terminology and classification is different from the USDA system with which I am familiar, but there is so much similarity and overlap in terminology and classification that I had very little problem understanding the authors presentation.

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. That said, I found the dating to be reasonable. While there were a few anomalous dates, the dating curves are consistent with what we would expect in the type of depositional environment suggested by the soils and support the inference of a MBA component.

The onsite deposition of charred archaeobotanical remains and animal bones from archaeological features is typical of the data used by archaeologists to support the inferences concerning farming and the use of animals. Where this paper excels is in the use of polycyclic aromatic hydrocarbons (PAHs), charcoal spectra, phytoliths, soil microstructure, urease enzymatic activity, microbial biomass carbon (Cmic) and heavy metal contents, as proxies for onsite and near-site land use practices. This impressive array of supporting evidence is one reason I found this paper compelling. I have used many of these analyses in my own research, so I will comment on those with which I am familiar and feel qualify to make comments.

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.

The charcoal spectra have some gaps, which is a common issue with this type of analysis and is to be expected. The data presented are consistent with the inferences of change over time, and the supporting evidence from other studies bolsters these arguments.

The archaeobotanical remains are especially compelling since the contexts from which they are recovered show unambiguous evidence that these plants were used by humans. The sample size is not overly large, but sufficient in size to reveal the primary plants being used. I suspect that if the forests were being managed to increase *Quercus* that acorns were also used by humans. Since most uses of acorns that I am familiar with involve grinding them into flour, which doesn't preserve in most archaeological contexts, so the absence of the species in this record may be due to preservation. Again, this is the value of the interdisciplinary approach used here, *Quercus* may not preserve in the archaeobotanical remains, but will be present in pollen and phytolith evidence.

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.

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? 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.

The pollen evidence is well presented, probably because much of the paleoenvironmental reconstruction is so dependent upon this evidence and the presentation is fairly standardized. I am going to stipulate that I believe that general trends are well represented but have a couple of questions.

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?

Overall, this paper is well written, well edited, and as concise as it could be with so many different analyses. Each section had enough data and appropriate illustrations and tables. Almost all of the different lines of evidence support the basic conclusions of human modification of the landscape in favor of oak forests, and I know from personal experience that pigs love acorns, and black snakes, although I don't think anyone modifies the landscape to increase them. 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.