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Interactive comment on "Natural versus anthropogenic genesis of mardels (closed depressions) on the Gutland plateau (Luxembourg); archaeometrical and palynological evidence of Roman clay excavation from mardels" *by* J. M. van Mourik et al.

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I am not going to provide a summary of the paper, Gary Huckleberry provides a fairly clear and concise summary in his comments, and the authors provide an abstract. I do want to applaud the authors for applying multiple lines of evidence from several disciplines to address the issue. I think that this makes this paper appropriate for this journal.

General Comments

I did find the paper to be a bit difficult to follow in places. Crucial pieces of information are just thrown out without adequate discussion of why they will be important. For example the four spikes in the Fagus pollen in Dauwelsmauer (F1-F4) are presented with dates on Page 11 Line 20 as part of the description of the pollen sequence. Later during the discussion of each of the other mardels these spikes are simply referred to as F1, F2, etc. These will come back later to play an important role in the discussion of the filling and pollen sequence in the mardels, and used as one of the primary dating methods. I found this frustrating and confusing since initially I wasn't sure what these designations meant; I had to spend a fair amount of time looking back over the paper to find them.

I think in the interest of people that might be unfamiliar with the Roman occupation of the region a quick summary is in order. This area was brought under Roman rule by Julius Caesar in 53 B.C during his conquest of greater Gaul. Initially it would have been little changed by the Romans, but as the region was assimilated into the Roman Empire it took on an increasingly Roman character – Roads, trade, villas, and commercial enterprises. This should have included pottery production, but it should also have included the production one of the most ubiquitous of Roman building materials – brick. As the Franks began to push into the region, by then the Roman province of Gallia Belgica, they forced the Romans to abandon the region by A.D. 406. If the mardels were quarried by the Romans, 53 B.C. to A.D. 406 is the time frame of interest for the anthropogenic origins by the Romans. If the authors are going to try to make the case that the Romans quarried the mardels they need to be more explicit on the time frame.

The paper lacks explicit assumptions and needs to be more explicit in its conclusions. It was fairly clear to me that the authors are relying not only on the dating of Daulwelsmuer but also cross-correlating their results with what is generally known about the climate and palynology of Western Europe, but I don't recall that this is ever explicitly stated as such. The same could be said about the significance of the trun-

cated paleosols. A truncated paleosol is a discontinuity in the soil sequence and is a result of either erosion or the Romans having removed that material. I would like to see more discussion on this discontinuity since I think the anthropogenic argument hinges on this. The authors are at least explicit in stating the overlying sediments and subsequent soil development is post-Roman, and I think they are probably correct in their assertions.

Huckelberry's discussion of the dating issues is fairly thorough and I agree with most of his comments. I do think that cross-dating using the pollen diagrams is acceptable, however it is a weak case argument because it is not very precise, and there is no control over the rates of sedimentation. In the Daulwelsmuer pollen diagram it is difficult to determine just where the Roman occupation would fit in the diagram. The dates go from 4260 BP (about 1700 years before the start of the Roman Republic) to 555 BP (1000 years after the Romans abandoned Gallia Belgica). I think the authors are using the F1 spike in Fagus pollen to mark the Roman period, but they aren't specific about using that as their marker. This brings me to another point. There is a long discussion of the pollen diagrams and how they relate to the climatic sequence and palynological indicators. I thought it was fairly well presented, documented and referenced, and not quite to the point. The article is about whether mardels are anthropogenic, specifically Roman, or natural phenomenon, not the climatic sequence of Europe and its effects on the environment. Discussions of the pollen from the Little Ice Age or the early Holocene don't appear to be particularly relevant, other than to provide confidence in the dating by cross-referencing pollen diagrams. I think the discussion of the pollen could be tightened up and again made more explicit about how it applies to the topic at hand.

As for the archaeometric analysis it doesn't seem like an afterthought to me, I think it is an important piece of the research. As Huckelberry correctly points out it is hampered by a small sample size, it can suggest conclusions but they are not well supported. I think their case can be made without detailed sediment descriptions and information

on precisely where samples were collected, however, such information would greatly strengthen their results. The archaeometric study says it uses Roman ceramics. As a North American archaeologist that means pottery to me, but it could mean ceramic tiles, bricks or other items in this context, the authors should be specific as to what types of ceramic. My guess is that it is pottery. At least this analysis produced an explicit conclusion (page 20, lines 18-20) that Beaufort and Berdorf are not potential sources for local ceramics. Again, a more representative sample size would increase confidence in this result.

If I could make a suggestion – if the authors want to pursue this line of evidence in the future, and I think they should, try using Roman bricks, if available, rather than pottery for testing. Pottery tends to be traded and can move very long distances for many reasons. Bricks, on the other hand, are bulky, heavy items that are economically unfeasible to move long distances (except possibly by ship). The Romans used brick so ubiquitously that there must be Roman brick buildings near these mardels. The brick from these buildings are less likely to be imported and more likely locally produced than the ceramics, and if the mardels are used to produce ceramics, then there is a high probability that it would be bricks.

I am going to conclude by saying I liked the approach to this problem and I think it meets the scientific significance of the journal. I think the scientific quality is mixed – the understanding of the methods and documentation and referencing is good to excellent, but the discussion tends to be unfocused with respect to the stated problem and sample size and dating issues make the arguments weak and poorly supported. The presentation quality needs work, I would say it was fair, it would stand with a major revision (see Huckleberry's comments). I read this paper and made my notes prior to looking at Huckelberry's comments. The two of us independently had many of the same issues, I refer to Huckleberry's comments where we agreed rather than reiterate them. I disagree with Huckelberry that the paper should not be published, I think the authors should be allowed a chance to revise their presentation. However, I think even

if they do rewrite the paper, their conclusions will still be tentative because I don't see that they can address the issues of sample size and dating without more research.

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