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Review of “Last 2400 yrs. Environmental changes and human activity recorded in the gyttja-type bottom sediments of the Mlynek Lake (Warmia and Masuria Region, northern Poland)” by Welc *et al.*

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This manuscript reports a late-Holocene palaeolimnological reconstruction for a lake in northeastern Poland. It presents an impressive compilation of techniques and proxies and I commend the authors for the amount of work that has been invested. The evaluation of proxy data and neighbouring archaeological evidence is a neat approach and this paper acts as a solid case study. The overall reconstruction of landscape history seems well-reasoned but certain proxy interpretations were less convincing, and the narrative was quite difficult to follow. In particular, there is some duplication of content and inconsistent structuring across the Results and Discussion. The text is sufficiently clear for general meanings to be conveyed but it does not flow particularly smoothly, with odd word choices, inconsistent verb tenses and clunky phrasing throughout. I suggest the authors explore opportunities through colleagues or a professional copy-editing service (such as that offered by Copernicus https://publications.copernicus.org/services/copy_editing_for_english.html) to tighten up the prose. Overall, a significant amount of work is needed to re-order and clarify key aspects of the paper but the findings are, in my view, worthy of publication in due course.

Comment #1: Focus of the paper

Choosing a lake that is known from archaeological evidence to have a shoreline settlement is a neat approach, but I suggest the authors make the purpose of their investigation clearer from the outset. The introduction is very general and many of the key points made in the abstract are not introduced in this section. The second paragraph in the Introduction (Lines 72 – 81) begins to be more focused but needs to stipulate the specific objectives of this research. I could not determine whether the paper intended to assess how well the lake sediment record captures the story of known local settlement or to assess the comparative influence of human impact and climate variability on lake evolution, or both.

Comment #2: More details needed on the study area

I was surprised that “Section 2. Study Area” is so short. It presents some basic physiographic details but much more information is needed. Some reasons for choosing that particular study site are outlined in the Introduction and Abstract and these characteristics ought to be spelled out in Section 2. For example, from the LIDAR imagery (Figure 1D) it appears to me that there is an inflow at the northern end of the lake, but much of the text in the paper suggests it is disconnected from a fluvial network. This will have significant implications for interpreting signals of terrestrial inputs. Geochemistry is used as a key proxy but no details on underlying geology are provided, which is an oversight. Some information on the lake’s hydrological

dynamics should also be included. For example, the GPR data indicates it is frozen during the winter, which is likely to affect sedimentation patterns. It would be worth the authors browsing other palaeolimnological studies published in *Climate of the Past* to get a sense for the level of detail that would be useful to incorporate.

Comment #3: Spatial pattern of sedimentation

As I mentioned above, it is crucial the authors clarify whether there is an inflow at the northern end of the lake. Figure 2B (the MS curves) show good repeatability, with much higher rates of sedimentation in the central zone (M1 and M2). This presumably reflects quite intense sediment focusing, but, if there fluvial sedimentation, the interpretation could well be different. Furthermore, how the effects of sediment focusing influence your choice of coring site should be addressed – especially in light of the assertion on Line 108 that M1 was chosen because it was the longest. It is in terms of sediment length but, eye-balling the relative accumulation rates suggests M4 probably extends further back in time.

Comment #4: Structure of the Results and Discussion

I found it quite challenging to follow the narrative through Sections 4 and 5. One of the main reasons is that the content of the Results and Discussion sections is quite inconsistent. Section 4 Results generally describes the different proxies, which seems appropriate, but in places interpretation is also incorporated into that section, and this seems out of place. For example, Lines 258-267 offers some assessment of TOC source areas, which is unexpected. Similarly, Section 4.6 is a mixture of describing the proxy profiles and, in places, interpreting the geochemical signals. I think the Results would flow much more smoothly if the authors maintained a clear distinction between describing the data in the Results and analysing and interpreting their environmental significance in the Discussion section. I also think the description of the data could be more streamlined. My sense is that it isn't necessary to describe multiple peaks and troughs for each proxy (e.g., Lines 237-239, 250-254, etc.).

Comment #5: Terrestrial source areas

TOC and certain geochemical profiles are frequently interpreted in terms of allochthonous and autochthonous inputs. In principle, such interpretation is reasonable, but mechanistic explanations were not adequately provided, in my view. Firstly, this relates to my earlier points about fluvial connectivity. If the lake is isolated from any river network, I presume the authors define their terrestrial inputs as material cascading off the slopes immediately around the lake? The authors at times assert higher lithogenic inputs indicate heavy rainfall – presumably this is slope wash? I wouldn't describe that process as a flood, as on Line 292. The reader needs to know more about the physical characteristics of the lake basin to understand the source of terrigenous mineral material and the mechanism(s) by which it reaches the lake bed. Similarly, the interpretation of Ca and Fe/Ca ratio will be strongly affected by underlying geology. Are there Ca-rich rocks in the vicinity?

I was also unconvinced by the assertion that low TOC and high minerogenic indicators was best explained by episodic deforestation. Is it possible that vegetation clearance would have mobilised and deposited allochthonous soil carbon? Some clarity on this point would be useful.

Lines 114-115: on what basis was the organic material judged to be a mixture of aquatic and terrestrial? Carbon isotopic ratios or microscopy?

Comment #6: Inconsistent interpretation of individual proxies

While the overall narrative and evidence for periodic human impacts are reasonably convincing, in my view, a number of assertions about particular proxy changes to be inconsistent or less convincing. For example, I don't see are the "high frequency peaks of Al, K, Ca, etc" (Line 402) in Phase 2, which is concerning. Similarly, Lines 290-292 assess the significance of peaks in elemental concentrations that occur around 3-m depth but Figure 6 seems to show a single dip at that depth – is that what the authors are referring to? It's not clear why that would indicate greater mineral content.

I was also unclear how the authors use the terms "shallow" and "high lake level". Figure 11 shows high lake level in Phase 5 but on Line 373 it is described as "quite shallow". These, to me, read in contradiction? Lines 410-412 also seem rather contradictory as the climate at the time is described as "more dry" and "relatively warm and wet". Similarly, on Lines 486-487, do the authors really mean human activity was influencing climate at that time? This seems a bold claim. Perhaps it was mis-typed.

It could well be that tightening up the wording will clarify many of these issues. I suggest the authors move away from attempting to describe many small fluctuations in the data and focus on the big – more akin to the summary diagram in Figure 11.

Comment #7: Inferring climate variability from the proxy reconstruction

As stated above, I find the interpretation of human impacts to be generally sound. Aspects of the climate reconstruction are less convincing, however. For example, on multiple occasions the authors infer a shift in precipitation from TOC and/or geochemical data, for example Lines 449-451. I'm unconvinced a change in precipitation is necessary to produce such a response.

The authors may be able to produce a more streamlined narrative by focusing on major changes. For example, assertions such as Lines 487-488 that climatic warming led to shallowing of the lake and increased sedimentation rate is a bold claim. I suspect other interpretations are possible and indeed plausible and the data are probably not adequate to be certain which was the dominant driver. But such specific assertions are not necessary; better would be to focus on the overall narrative and assess, for each phase, what evidence there is for human-driven landscape change and what evidence points towards climate impacts being dominant.

Similarly, the assertion on Lines 505-507 that "a change in lake sedimentation around 1500 [AD] may be associated with the...LIA" is another bold claim that is (a) tenuous and not really justifiable but (b) not really necessary, in my opinion. Better to focus on the main narrative, which is a story of local human impact overprinting longer-term climate variability.

Comment #8: Merging the archaeological data

I really like the idea of amalgamating palaeoenvironmental data with the archaeological record but I suggest the authors consider presenting more of the latter data. Readers of *Climate of the Past* may have less exposure and background to archaeological evidence. Given its

prominence in the narrative around phased human impact, is it possible to provide more details on how the timing of settlement at Janiki Wielkie hillfort is known? At the very least, Figure 11 could be expanded to summarise the archaeological data for each zone.

Comment #9: Multiple zone classifications

Whilst I recognise different proxies will respond to different drivers at different times, I found it difficult to follow the narrative because multiple zone classifications are used. The lithology, pollen data and diatom assemblages were each ascribed a series of zones but their numbers is often mis-matched. It would really improve the accessibility of the manuscript if the authors were more consistent and explained at an early stage. It may also help if the lithology was not described numerically.

Comment #10: Improvement needed to the figures and tables

General point: figure and table captions are generally quite vague. I recommend the authors re-write most, if not all, such that each figure or table can be interpreted as a stand-alone object.

General point: there is a tendency to duplicate images and data in multiple plots. For example, could the aerial photograph of the lake (figures 1C and 2A) be merged? Does the magnetic susceptibility data need to be presented multiple times? Similarly, for Tables 2A and 2B, the reasoning behind presenting two tables is unclear. Focusing only on the proxies discussed in the text would be preferable so Table 2A could be removed, in my view.

I presume the text box on Page 12, Lines 363-364 is intended to be a table? It's unclear why it isn't labelled accordingly.

Figure 5: why is one segment of the core image presented? Better might be to present imagery for each of the four lithological units, if the authors feel this is essential information?

Figures 5 and 11: why doesn't the sediment accumulation rate curve extend to the bottom of the graph? Figure 4 shows a basal radiocarbon date so there shouldn't be a concern about chronological extrapolation?

Figure 8: this is effectively a re-production of the data presented in Table 2B plus TOC. One or the other might be preferable?

Figure 11: the age labels are missing their units. Presumably each number should be multiplied by 100 and the lowest to values need a negative sign? This should be written on the graph or at the very least in the figure caption.