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Log book
Anders Bornfalk Back, Scorched earth: a posthole approach to Iron Age warfare

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Reviewer 1

Review of "Scorched earth: a posthole approach to Iron Age warfare"

The paper uses settlement archaeology data from development-led archaeology in the Uppsala Plain region to analyze the presence and abundance of evidence for burning of houses. The burning of houses is used as a proxy for Iron Age warfare, based on the argument that heavily burnt buildings most probably represent aggressions rather than accidents. The evidence for burning is discussed in relation to experimental observations from Denmark and Sweden and related to a discussion of source critical factors of importance for the understanding of settlement archaeology remains in heavily ploughed regions. From the empirical investigation of 57 houses with evidence of burning, two anomalies with high numbers of burnt houses are identified chronologically and discussed in relation to the development of mid-Iron Age society in general.

Overall, I think this is a well-written and valuable contribution, with a novel approach to an interesting problem. The arguments rest on a solid empirical material that is properly and transparently presented, and the results and conclusions pave way for some interesting future prospects in the field of settlement archaeology and studies of Iron Age conflict and warfare. There are some issues that could be further problematized, and in some cases elaborated or complemented, as exemplified in the following:

In the introduction, it is stated that there are only two Iron Age sites in Sweden where warfare as an activity is evidently represented. This, of course, is a matter of definition, and in my view, there are several categories of sites that would qualify at least to be considered in this context (such as hillforts, settlements with human remains displaying trauma, etc.). The argument is also weakened by the fact that later in the text, several places are in fact mentioned with rather clear indications of warfare (e.g. strongholds with evidence of heavily burnt walls and ramparts, pp. 3–4, or the settlement of Björkgärdet mentioned on p. 4 where numerous arrowheads were found and interpreted as evidence for an attack).

Also mentioned in the introduction is the issue of preservation and its importance for our possibilities to detect warfare. The two sites highlighted as unquestionable evidence for warfare,

the Garrison at Birka and Sandby borg respectively, are both put forward as unique in terms of preservation which, it is argued, results in a bias. This, I would argue, is not entirely true. While the preservation on these sites is indeed exceptional in relation to the settlement material in the Uppsala Plain region, they are by no means unique, and they do not contain categories of archaeological material that are not normally preserved. Rather, it is the sequence of events at these sites, including those in later times, that has created some rather unusual archaeological circumstances. But they are not unique; there are quite a few sites excavated that fill these same criteria (e.g. Uppåkra, and most houses excavated on Öland and Gotland, and more). At many of these sites, burning of both one and several phases of houses is evident. And additionally, in several such cases violent activities in connection with the burning are also likely (e.g. Uppåkra, Vallhagar). Concerning Sandby borg, the main reason that the massacre can be identified today is probably the fact that the bodies were not taken care of. Had they been collected and buried after the massacre, the violent event would probably be difficult to detect archaeologically.

A central proposition for the study is that heavy burning of a house can result in below-ground charred posts and/or reddened soil. This, however, remains to be fully understood it seems, as shown e.g. by the experimental burnings cited in the study where no such processes could be detected. While these traces commonly occur in settlement archaeology contexts, more research is needed (experimental and/or comparative) in order to fully understand what processes they represent.

From the observation that in the experimentally burnt houses, the roof supports were seemingly not affected by fire below ground, it is argued on p. 7 that in some cases, the presence of seemingly unburned posts in postholes can be seen as indirect evidence for burning. While some of these houses could well have been burnt down, it becomes a circular argument to state that the presence of unburned posts indicates that the house burned. An addition to this argument would perhaps be the (speculative but still) statement on p. 8 that disassembly for reuse was a standard procedure, hence the presence of posts in postholes per se could indicate that the house was not disassembled but destroyed, supposedly then by fire.

In future studies, comparative analyses of houses with better conditions for preservation could add value to the argument and enhance our understanding. Hence, while the results presented here are promising and indicative of a field within settlement archaeology inquiry with large potential, more research is needed concerning what the parameters included in the study actually represent. This, I think, could be highlighted as a source critical aspect to consider.

This said, I regard this as an original and highly interesting study that should definitely be published. The above suggestions are not to be seen as strict requirements in order to lift the paper to an acceptable standard, rather they are intended to make the arguments of an already well-written and high-quality paper more stringent and streamlined.

2022-06-03

Reviewer 2

Scorched earth: a posthole approach to Iron Age warfare.

The paper provides interesting results and a useful method of approaching iron age warfare in a new way, by focusing on the possible wartime practice of burning houses and its visibility in the archaeological record. The paper addresses the several source critical issues in a considerate manner. However, there are some issues that can be strengthened before it is published.

1). It is focusing on the Uppsala plain as a case study, which provides solid material, however, I do not think it manages to lift the gaze and connect Uppsala plain with other examples in a very thoughtful way. The garrison of Birka and Sandby borg are mentioned, but in a quite superficial way. For example, the following is written on page 3: "Perhaps not too surprisingly, existing evidence strongly suggests that destruction of individual houses, settlements and fortifications by fire was an integral part of Iron Age warfare. The two sites mentioned above [Birka and Sandby borg] on which there is little or no doubt that they were scenes of lethal conflict, were both destroyed in this way." That last statement is far from certain and does not hold up to the source criticism that the author upholds to the material in Uppsala plain, see page 7: "As indicated, there are two methodological aspects to consider with the current approach. Firstly, how can we conclude that a specific house burned because of an act of aggression? A first step is to include only buildings which were heavily burned (i.e. destroyed) and exclude houses where the traces suggest a minor "everyday" fire." It is not certain that the fire in house 40 of Sandby borg was deliberate and it did not consume or destroy the whole building (heavily burned), or any complete bodies. Most houses and their contents are untouched by fire. It is in fact possible that the fire in Sandby borg was accidental, which is indeed also highlighted by the author of this article later in the same paper. The narrow geographical focus is not an issue, since it provides a solid case study, but it does become a problem when the connections to other cases outside of Uppsala plain are few and quite superficially discussed.

2). I think the paper would benefit from including more anthropological theory concerning warfare, which is lacking in the paper now. It would be interesting to see how such theory could expand the interpretations presented in the discussion, which now seem to be somewhat speculative and under-developed. The links made to the development of Old Uppsala as a central place are certainly intriguing and possible, but also quite tentative and assumptive. I do not find it properly argued for, and here perhaps anthropological theory could help find necessary links and arguments. I do think something is missing as it is. For example, on page 4, the following is written: "The specific motives behind burning as a strategy in warfare most likely varied. Ultimately, however, it was a policy of power. Whether the direct stimuli were tactical, punitive or symbolic, it was an act of dominance reflecting political and social relations." I would expect such a statement to be followed by a reference - most likely anthropological - but no such reference is there. I think this lack of theory is why some of the interpretations become hard to follow. For example, on page 12, it is written that: "Perhaps one should not rule out two parallel processes resulting in burnt farms, of which one could be the aforementioned, but one single process seem more likely." Why does it seem more likely with one single process? Based on empirical evidence or theory or both?

I also have a minor comment. On page 2, the following is written: "In combination with the previous – and to some extent still existing (Bornfalk Back 2016) – widespread lack of interest in conflict studies in Iron Age discourse, it is virtually impossible." I think this sentence needs to be clarified and nuanced – there are multiple Iron Age discourses, and some of them are certainly interested in conflicts (the popular discourse?).

Author's comments

I am indebted to the anonymous reviewers for reading the draft of my paper and offering suggestions to improve the final version. I have considered all of their comments, some of which I have included in the text, others I have thought appropriate to explore in future engagements with this research topic.

Reviewer 1 commented on my introductory remark on the scarcity of Iron Age sites accepted by archaeologists as places of battle. Personally, I clearly see the potential of approaching sites such as Uppåkra, Vallhagar and several hillforts as scenes of conflict. But in reality, and this was my point, this is rarely done in contemporary scholarship. Swedish archaeology was for a long time characterised by a widespread lack of interest in conflict studies and in my opinion, we are struggling with this legacy today. A case in point is the general demilitarisation of the martial architecture of c. AD 400-700 (*e.g.* forts, strongholds, ramparts etc.) which dominated the discourse from c. 1990 up to about ten years ago. Within this tradition of thought, relating these defensive structures to warfare was a myth obscuring an understanding of their true purpose as arenas of worship of different kind (Bornfalk Back 2016). Consequently, the state of knowledge of the fortification tradition of the Middle Iron Age has with few exceptions been stagnant since the 1980s. I will rework the paper's opening section so it becomes clear to the reader that my remark was on the attitudes of archaeologist, not on the empirical evidence as such.

Reviewer 1 likewise suggested that the rich empirical material of Sandby borg and the Garrison on Birka is not solely a matter of rare and fortunate preservation conditions but also due to post-battle processes. This is an important point and I will include this in the paper. The same reviewer also called for comparative and experimental research in the future to increase our understanding of what the different traces of fire in a burnt house actually represent. I agree and hopefully this paper will contribute to the interest of these matters.

Reviewer 2 suggested that detailed comparisons with materials outside the Uppsala plain, as well as applying anthropological theory on warfare, could strengthen the study. Since the aim of the paper is to present a specific method that makes use of development-led data, rather than to discuss martial tactics or strategy *per se*, too lengthy discussions on settlement archaeology of other regions or a superimposed theoretical framework would probably burden the text. However, as mentioned above, I do see a benefit in future studies to include comparative approaches, not least when exploring centralization processes of tribal Iron Age Sweden in more depth.

The excavation of the well-preserved House 40 at Sandby borg on Öland, mentioned by reviewer 2, can highlight the difficulties of identifying malicious burning in the archaeological record of ploughed out settlements. There is ample evidence that this building suffered a fire during an attack: partially burnt skeletal remains of humans laying on the floor, remains of burnt logs on the floor surface (according to the excavators possibly furnishing or part of a collapsed roof) and charred archaeobotanical remains also on the floor (probably from the burned turf roof according to the analysis) (Gunnarsson *et al.* 2016:25 pp; Alfsdotter *et al.* 2018:429). The house could have been torched deliberately or accidentally catching fire during the fighting. The fire seemed to have died out before consuming the entire house, leaving the part of the roof-bearing posts set underground unburnt. If the top 20 cm of this house had been ploughed out, most of the evidence of the attack and burning would not have been preserved. Therefore, the 57 heavily burnt houses of the Uppsala Plain that form the corpus of the study, all of which identified through the criteria of burnt roof-bearing posts, must be seen as a minimum of the actual number of buildings destroyed by fire in this region.

References

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