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Stig Welinder: Comments

von Hackwitz, K. & Stenbäck, N., ”Changing Landscapes – a GIS analysis of Neolithic site location and shore displacement in Eastern Central Sweden”

If I had read the manuscript of the article before accepting to be a referee, I would not have accepted. The article is too much a testing and re-evaluation of an article by me from the mid 1970s. However, because of just that perhaps a comment by me on that very part of the present manuscript could be of interest to have included in the editorial archive of JAAH.

The number of scholars working on the Early and Middle Neolithic of Eastern Middle Sweden has always been fairly small until about 20 years ago. My background in the 1970s was the work by Axel Bagge and Sten Florin during the 1930s, 40s and 50s. They rested on the shoulders of the Uppsala landscape archaeology seminar, which was active around 1910. Since the 1970s several rescue archaeology units has excavated and analysed dozens of new sites using methods scarcely dreamt of in the 1970s. Amazingly little of this is seen in the article by Kim von Hackwitz and Niklas Stenbäck. They have chosen to copy my work with partly other or new methods, not trying to form new theories based on the full use of new data and methods. I find that a pity.

Of course, it had been more interesting to see new ideas on change and disappearance of the Pitted Ware sequence, not just a demonstration that my try from the 1970s was not flawless.

I had at my disposal a data set with 53 Pitted Ware sites. Kim von Hackwitz and Niklas Stenbäck analyse a selection of 39 sites out of these 53 sites. They declare that 53 sites were sparse data. Obviously, they had the choice between 39 sites, discarding 14 (26 %) of my sites, or perhaps 80–100 sites including what has been excavated since the 1970s. The former was chosen, because they wanted to test my theory. The latter is the self-evident basis for discussing new ideas. I hope that will be done in the near future.

The second difference between my analysis and the new one in the article is the choice of chronological system. I used a very simple system differentiating between early and late sites with the aid of just two ornament attributes. This left a rest group of sites, which had or had not an intermediate chronological position. This is very accurately described in the article by Kim von Hackwitz and Niklas Stenbäck, who themselves used the classic Fagervik chronology by Axel Bagge from the 1950s. Thus, we both in fact dated the sites to spans of around 400–500 years regardless of the actual lifespans of the sites. Accordingly, none of us should have referred the sites to one topographical landscape each but to a changing landscape during one or several centuries. This would have been rather hopeless in the 1970s; in the 2010s, it is easily done with sophisticated GIS applications – as demonstrated in the Norslunda example by Kim von Hackwitz and Niklas Stenbäck. I long to see that done for scores of sites.

In the present article, and that is the third difference, fascinating modern landscape reconstructions are used. I hope that they are based on shore-displacement studies with accuracy worthy of the inherent GIS systems and the high-resolution elevation data (NNH). My handicraft is well described in the article.

Therefore, Kim von Hackwitz and Niklas Stenbäck have placed a big question mark beside my view of the change in life-pattern of the people using Pitted Ware pottery in the changing landscape of Eastern Middle Sweden during the Neolithic. I have placed a few small question marks beside their test of it. I hope that it will not take another 30–35 years until the next voice is raised.

Kim von Hackwitz and Niklas Stenbäck hint at discussing the subsistence economy of Neolithic sites from their landscape setting – the good old days of the site catchment analysis of the Cambridge School of Economics. The latter analysed catchment areas with the radius 10 km for hunting-gathering sites. For practical reasons I sufficed myself with 4 km. Kim von Hackwitz and Niklas Stenbäck did the same in order to copy me in their test. In the future, that will not be necessary. In addition, they are advised to include ecofact data in their analyses, as I did in the 1970s. There are more of them, and e.g. isotope data, today.

Stig Welinder

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Anonymous Review

Kim von Hackwitz & Niklas Stenbäck: Changing landscapes – A GIS analysis of Neolithic site location and shore displacement in Eastern Central Sweden

Contents

The authors aim to use new techniques in reconstructing the prehistoric landscape "as accurately as possible". These new techniques will be used in two case studies, both involving the Neolithic and eastern Central Sweden. The first case study intends to test Stig Welinder's hypothesis concerning the "disappearance" of the Pitted Ware culture when their settlements were moved further and further out in the archipelago. The second case study is a reconstruction of the Neolithic landscape for a site, Norsunda in Uppland, which was excavated in connection with the construction of Arlanda stad, and in which the authors intend to use the "Nya Nationella höjdmodellen" (NNH) [New National Elevation model], which in turn builds upon LIDAR (Light Detection and Ranging). Specifically, the authors aim at seeing whether a better resolution of the prehistoric landscape would present a different picture of the Neolithic settlement locations in the spatial landscape.

The authors introduce the new technique that they intend to use for their case studies, i.e. a method for estimating the Neolithic shoreline considering both land elevation and differences in sea levels using regression analysis. They refer to a study by Sund (2010) and a type of parameter (Sund 5-11), the meaning of which you do not understand unless you have read Sund (2010). Neither do they state Sund's regressions equation for the calculation they intend to perform in their case studies. The authors also declare that they use GSD-data for the elevation data, without explaining to an uninitiated reader what that is. They subsequently state that there are source critical problems in using regression analysis for this type of studies, but maintain that such criticism is too technical to explain in this type of forum, instead referring to a unpublished article by Löwenborg, that is given no title in the reference list.

The first case study uses Welinder's 35 year-old hypothesis of the marginalisation of the Pitted Ware Culture in the outer archipelago as a "straw man". Somewhat surprisingly, here the authors also maintain that the hypothesis has prevailed until now, most likely in order to support their own case. They subsequently describe how Welinder has classified the pottery, how this is used to date the different sites and, additionally, they describe Welinder's definition of the outer and inner archipelago based on how large a percentage of the surroundings consists of open water. His conclusions are that early PWC sites are found in the inner archipelago and in intermediary locations, while late PWC sites are found in the inner archipelago, in intermediary locations and in the outer archipelago, but mainly in the outer archipelago. Initially, the authors intended to use the same sites as Welinder did in 1978, but this proved to be impossible, since the coordinates for some of Welinder's sites are uncertain; additionally, the regression equation used has some geographical limitations. These limitations are not accounted for. Furthermore, the authors use a different chronology than Welinder did in his article; while he used the Överåda chronology (o1-o6) and analysed 53 sites, the present authors use the Fagervik chronology (Fagervik I- Fagervik V) and analyse 39 sites. Both studies, however, base their discussion on three different groups in their analysis: Early, Mixed and Late PWC. The new method is applied in order to visualise the Neolithic landscape and classify the sites as inner archipelago, intermediate and outer archipelago.

The authors subsequently maintain that their results significantly deviate from Welinder's, since none of the sites analysed according to Welinder's definition (>65% surrounding water area) were located in the outer archipelago. Most of the authors' sites are in the inner-archipelago category. Further, the authors assert that the mean (in %) for the surrounding water area do not differ significantly between the categories of inner archipelago (24%) and mixed (29%). It is disturbing that there is no statistical evidence for this pronouncement of significant and non-significant deviances. Additionally, the authors conclude that as many as 10 of the early PWC sites (out of 39 sites) are surrounded by <10% water surface, while none of the late sites are surrounded by so little water. This is completely in line with Welinder's hypothesis, albeit that the sites are not in the outermost archipelago, but still further out than the early sites! However, the case study is concluded by the statement that there is "a certain amount of truth in Welinder's theory", (which by the way is a hypothesis), in that the early sites are foremost found in the inner archipelago. Nevertheless, since early PWC sites are also found in the outer archipelago, the hypothesis is not completely correct. I miss a source critical discussion on how the different chronologies used have influenced the results in the study. Statistical evidence for statements on differences is lacking. Fig 5 is not particularly clarifying; it would have been more useful with three histograms for the different categories of early sites, intermediary sites and late sites on the X-axis and percentage surrounding water on the Y-axis. Thus, it is impossible to confirm Welinder's hypothesis that late PWC sites are only found in the outer archipelago, and neither could Welinder's results. Most importantly, however, the hypothesis is not discarded, which is surely the very point of hypothesis testing. I miss an alternative hypothesis to Welinder's hypothesis.

Case study 2 is based on the initial questions from the excavation of the site in 2008, i.e. in which phase was the site shore bound and which activities can be discerned. The original result, based on shoreline displacement estimated from GSD, was that during the Middle Neolithic, the site was a hunting and fishing site located on a small island close to a larger camp on a larger island. At the onset of the Late Neolithic, Norslunda was still located on the shore with hunting and fishing as main activities, while at the end of the Neolithic, the site was located inland. Thus, the authors do a new reconstruction of the Neolithic landscape using NHH. Here the authors compare maps based on GSD and NHH, and I do not understand the sentence "However, if compared the difference between the maps created by GSD data and NNH data is not significant for the interpretations of the site from a landscape perspective." Apparently, the NHH data are discarded, since in the next sentence, Sund's regression with GSD data is used to recreate the shoreline at that time. Subsequently, the NHH data are used anyway; when the two maps are compared, the authors find that GSD data lose many details. The conclusions are mainly corresponding with the original analysis for the Middle Neolithic, in that the activities would have been the same during the different time periods, but that the size of the island changed and thus also a little of the closeness to the shore. For the Late Neolithic, however, the consequences are greater, since the site was located several kilometres inland in the new analysis.

The authors conclude by establishing that the paper has discussed the advantages of using regression analysis and the new NNH to reconstruct prehistoric landscape. Although I do not agree that they really discuss the regression analysis, since it is not presented at all, I agree with the fact that it provides a better picture of the prehistoric landscape. It would be shameful is such high-resolution data did not! Further, the authors state that they have provided a new understanding of the Neolithic landscape and the analysed sites, and here, I disagree. They claim that they are able to discard Welinder's hypothesis of the changing locations of PWC sites in the landscape. In fact, they have not. In the second case study, the authors do not develop the advantages and disadvantages in using GSD or NNH in a way that makes it understandable for the reader, and which could have guided the reader in further applications of the different databases.

Structure

Many things recur in the paper, e.g. Welinder's hypothesis is presented several times. The authors' original intention with the first case study may be questioned – is it to embarrass Welinder or is it to promote the new technique and show its functionality? The chapter on theory and method is somewhat confusing in that the theory for the new "method" is mixed with the theory behind Welinder's hypothesis. What is the most important purpose of the paper? Is it to discard Welinder's hypothesis or to promote the new technique? It must be more stringent. We find ourselves in the midst of GRK or not GRK when we thought we would be reading about the new GIS technology.

Originality

Yes, there is some degree of originality in the work since new techniques are used to illustrate old issues. The approach as such is not new – it is the same as used by Welinder in 1978 – but enhanced and developed technique is used.

Jan Apel: Review

Kim von Hackwitz & Niklas Stenbäck. Changing Landscapes – A GIS analysis of Neolithic site location and shore displacement in Eastern Central Sweden.

In their paper, Kim von Hackwitz and Niklas Stenbäck have applied new GIS techniques to test earlier interpretations of the stone age archaeology of Eastern Central Sweden: Stig Welinder's idea of the acculturation of the Pitted Ware Culture and the interpretation of prehistoric shorelines at the Neolithic site Norslunda, Arlanda Stad in Uppland. Both of these interpretations relied on data of the eastern central Swedish shore displacement. In this review, I will concern myself less with the cultural historical implications of the paper and instead concentrate on the methodological implications. I may be able to give some background to the paper based on my involvement in the rescue archaeological E4-project in which one of the new tools that the authors uses was developed. I will also discuss some of the results that are presented, especially, of course, in relation to the earlier research.

The shore displacement process that affected eastern central Sweden from the Early Holocene onwards has been important for archaeological interpretations in this area of Sweden. In fact, it is not an exaggeration to suggest that thanks to the shore displacement process, we have gained access to information on the spatial layout of Stone Age sites that would have been impossible in other parts of Europe. The combination of a continuous land-upheaval and isostatic sea level variations created by the varying amount of ice at the poles depending on climatic variations. Compared to the southern parts of Scandinavia, where for their chronological and chorological interpretations of archaeological sites archaeologists have to rely on the vertical stratigraphy of sites that have been used and re-used through millennia, archaeologists in Eastern central Sweden are frequently able to excavate coastal sites that have been used for a limited period of time, owing to the fact that the internal structure of the site is fairly well preserved. This is especially true for Stone Age sites, since the shore displacement was severe during parts of this period. Another lucky consequence is the shore displacement process. Thus, from an archaeological point of view, there are great advantages of working in an area that has been affected by a shore displacement process.

The problems with reconstructions of correct shorelines from the Stone Age in eastern central Sweden can be boiled down to two points:

(1) the accuracy of the available topographic data and
(2) the ability of geologists and archaeologists to correctly date prehistoric shorelines.

Since the case studies by Welinder and Stenbäck that are re-evaluated in the present paper were published, major advances in both these areas have been reached. The old topographic data that was available when the two papers were written was at best based on stereoscope evaluations of old aerial photographs and had an average standard error of 2.5 m of 2 m and in certain cases up to 7 m (Talts 1999). The new estimations, where the landscape has been digitally scanned, have virtually no meaningful error from an archaeological perspective so the first problem appears to have been eliminated.

Thus, any problem in estimating reasonably correct prehistoric shorelines can no longer be referred to errors in the topographical data but boils down the possibilities of dating prehistoric shorelines. From the geological perspective, the introduction of the AMS dating technique meant that the accuracy of the dating of different horizons in the cores was enhanced. Instead of relying on old relative dating techniques or conventional 14C-datings of bulk samples, it now became possible to date single pollen. However, even though the dating techniques improved, the annual variation of the Baltic sea-level (up to 2 m) meant that even with accurate topographic data the influx of salt water meant that salt water diatoms survived in the lakes even after it had been isolated. In the first volume of the E4-project published in 2007, Risberg *et al.* estimated the time lag of the AMS core dates to c. 300 years, i. e. the replacement of salt-water diatoms with fresh-water diatoms occurred 300 years after the lake had been isolated.

However, this estimation is based on dates of coastal archaeological sites and the important question is of course how archaeologists have argued when they interpret a site as coastal. In fact, one of the things I really like with the present paper is the tentative definition of a coastal site that is presented in the paper, because this kind of clear-cut definition (a minimal amount of surrounding water) has been lacking in many earlier studies. As is perfectly clear in the current paper by von Hackwitz and Stenbäck, in a flat landscape, such as northern Uppland, the arguments used to define coastal sites as opposed to inland sites have archaeological consequences.

From a macro-geographical perspective, one way of arguing for coastal sites during the Neolithic has been the clustering of sites that arguably is the consequence of periods with temporary stabile coastlines (see for instance Björck 1999 & 2000). When this data is combined with the analyses of maritime proxies in the archaeological and osteological data from the actual sites (see list in Risberg *et al.* 2007:110) a robust coastal interpretation is possible. A promising way of determining the exact location of a prehistoric coastline has turned out to be phosphate mapping (Sundström *et al.* 2006 Al & Ilves & Darmark 2010). This method is based on the empirical observation that phosphates tend to decrease at the lower level of an axis through a site perpendicular to the prehistoric shoreline.

I would finally like to add some comments on the importance of the local topography around the site and the impact that it may have on the usefulness of the results. When studying the maps made with the new method sites in more hilly landscapes, such as the sites in Östergötland and the northeastern parts of Södermanland are less affected by the land upheaval. It may be worth to consider that the annual variation of the sea level in the Baltic basin in this context. Sites that were placed close to the beach in flatter landscapes could only be used for short, maybe seasonal, occupation while sites positioned close to the water in hilly landscapes might have been used permanently since the net effect of the land upheaval process was less. Maybe this is the reason why the reinterpretation of the Norslunda site seems very reasonable, while the suggested reinterpretation of Welinder's hypothesis is less convincing? Because as I see it, the new results, based on a more robust reconstruction of the shoreline displacement, actually support Welinder's original idea.

The two main arguments for these reinterpretations of Welinder's idea that are presented in the paper is:

(1) That a smaller proportion of the earlier sites are more exposed to the sea while the majority of the earlier sites are located to the inland while the later sites mostly fall in-between.
(2) That seven of the early sites have continuity into the later period and therefore, according to the fact that Sweden has a consistent regression, and that these sites went from being located in the outer archipelago to the inner archipelago.

The first point does in fact not contradict Welinder's hypothesis, since what is important for the acculturation argument is that the inland sites are abandoned in period 2, not that outer archipelago sites are occasionally used. The second point is based on the fact that Sweden has a consistent regression and that, logically, this would mean that coastal sites during period 1 would naturally become inland sites during period 2. However, it is quite clear that most of the seven sites with continuity are still positioned at the coast in period 2, presumably because they are positioned in a hilly landscape where the regression has little effect (see Figs. 3 and 4; Tab. 1 and 2).

However, it has not been the author's main goal to reinterpret Welinder but to demonstrate the advantages of new techniques, and the maps presented in the paper clearly shows the advantages of this new technology.

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Authors' comments

The paper was commented on by Professor Stig Welinder, Dr Jan Apel and an anonymous reviewer. We are very grateful for the response and advice given and have carefully reconsidered every suggestion for change made by the reviewers. We will respond to their individual comments separately below. However, we first need to clarify a change in the arrangement of the paper that was made before we read the review comments.

After submitting the paper, an error in the use of the mathematical formula calculating the shorelines was detected by Daniel Löwenborg, who introduced the method within the *Rethinking Human Nature* seminar. This affected the produced shorelines and consequently the outcome of the results in the paper. For that reason, we had to make some changes in the structure.

As there are several users of Löwenborg's manual, we will describe the miscalculation here. The error is in the first step in the manual where the BP value is to be given in several A-columns in the Excel document, sheet 1 *Sund_lokaler_databas*. In one of the column, *A2*, the value shall be given as BP^2. For a BP value of 5000, that column will therefore get the value of 5000x5000 = 25000000. This will affect the results in the created field for each BP, that should be copied to sheet 2, *H BP*, and then integrated in ArcGIS to calculate the shorelines according to the manual.

For the present paper, the adjustment in the calculation led to a major difference in the produced shorelines and results in both cases:

In the first case – Norsunda, the originally calculated shorelines had a difference ranging up to 5 metres compared with the shorelines provided by SGU. This affected the reconstructed landscape and the situation of the sites considerably. Hence, we developed a discussion concerning the understanding of prehistoric landscape and land use with regard to the difference in the outcome in shoreline reconstructions between the current model (Sund) and SGU's model. However, after the correction in the calculation, the shorelines are more or less consistent with the shorelines produced by the SGU with a difference of about 0,2 m. The previous discussion has therefore been replaced with a more theoretical discussion concerning site placement in aspects of historicity and land use to better suit the outcome of the analysis. This case study is also reformulated to be a test of the regression method used in the analysis.

In the case concerning Welinders's thesis, the first analysis indicated that his conclusion concerning late PWC as more maritime could be questioned. However, when using the corrected calculation, the indication that the hypothesis was difficult to prove was actually strengthened, especially since it turned out that there were no inland locations – a parameter that disturbed the results in the first analysis and bothered the reviewers. Consequently, we reordered the structure of the paper, only discussing Welinder's thesis after we had evaluated the regression method.

The error and the subsequent changes were pointed out to the editors of the *Journal of Archaeology and Ancient History* 2013-02-26.

Authors' comments to the notes made by Stig Welinder

We are very thankful for Welinder's comments to the paper. However, his comments are of a more discursive nature and do not really include any actual changes but more suggestions for future research. We will however respond to his comments as we find the discussions significant.

First, we want to clarify that the current paper is an initial analysis meant to be further developed by both authors in different ways including additional sites, supplementary data and different GIS-analyses. However, as the authors do not agree in all questions concerning the Pitted Ware Culture, we decided not to discuss the culture on a more evaluated theoretical basis, but instead focus on an analysis that could be tested and evaluated.

We did not mean that the number of data set used in Welinder's analysis (59) was sparse. We were referring to the data from the sites such as radiocarbon dated sites, pollen, DNA etc. as well as the tools for reconstructing the prehistoric landscape to be able to analyse site locations. This is not a critique, but rather a description of the archaeological methods of the 1970s. This is also discussed in the paper.

Regarding Welinder's suggestion to use the 80–100 sites that are now known within the area, we point out the following: We wanted to test the theory based on early and late PWC sites, using FII and FIV pottery, and few sites include FIV pottery. Furthermore, all the known PWC sites in the area cannot be or have not been dated and are therefore not useful in this analysis. However, we think that the results indicate more than that the previous analysis performed by Welinder "was not flawless" and that this affects the understanding of the late Middle Neolithic landscape and land use, since our results, compared to Welinder's, show a different correlation between the early and late sites in relation to their situation in the archipelago. This is highly interesting. as this may be the result of intensification in the diversified land use.

We share Welinder's request for more sophisticated GIS-analyses, such as the Norslunda example, as both authors are involved in research concerning changing coastal landscapes including advanced GIS-analysis and Agent Based Modelling methods. We hope that these projects can be presented in the near future.

Welinder's last remark refers to a subsistence economy and site catchment. In this paper, we do not attempt to discuss subsistence economy and certainly not with the reference of a 4 km radius. As Welinder points out, such discussions should include ecofact data as well as different isotopic data.

Authors' comments to reviewer 1, Jan Apel

Apel's comments were very stimulating to read; not only does he pinpoint the problem related with shoreline models, but also some of the problems with Neolithic costal sites. Even though the history around shoreline modelling is known to the authors, we suggest that the readers of our paper also read Apel's comments to gain a better underrating of the history and problems of shoreline reconstructions.

Apel describe the results as important as they point out the importance of site locations and local topography. We agree with him and this topic was developed in the paper during the rewrite. However, he also says that the results actually support Welinder's thesis, as there were many early inland sites. As stated above, our calculation error had already been discovered, and the new analysis changed the result of that investigation.

Authors' comments to reviewer 2, Anonymous

First, we want to comment on the reviewer's statement in the end of the text, asking if the intention of the paper is an attempt to embarrass Welinder. We strongly contradict this statement as we have the highest respect for Welinder, not least since he has always integrated new methods in his work, leading to new understandings. This was also stated in the paper. We want to point out that as a researcher, there is a chance that your work will be evaluated even long after it was published. There is nothing extraordinary about that. Further, the reviewer does not agree that we have provided a new understanding of the Neolithic Landscape. Since s/he does not give any constructive suggestions of how to improve this, it is difficult to respond to the critique.

<u>Some of the comments made by the reviewer were considered during the rewrite of the paper:</u>

The reviewer's discussion concerning the results of the Welinder study is not taken under consideration, as the recalculation changed the outcome. As a result, the important complications stated by the reviewer have been resolved.

The commentator asks for an alternative hypothesis to Welinder's hypothesis. The rewritten manuscript contains an evaluated theoretical discussion concerning land use that can be seen pointing to an alternative understanding based on the new results. However, the aim of the paper was to test Welinder's thesis, not to provide a new theory.

The comment concerning the initial Case study 2 is no longer relevant, as we reworked that part of the paper, and have removed the parts that the reviewer found confusing.

<u>For the remaining comments (in order of appearance):</u>

Clarifying of what GSD-data is: We do have a description of the size of the grid when discussion the old GSD data (50+) and the new NNH data (described as 2+). Nevertheless, we have now included an extended description of the GSD with a separate reference in the initial text as well as explained the abbreviation.

The missing title for Löwenborg's paper in the reference list was highlighted with yellow, and has been brought up to date.

We are surprised that the reviewer wondered whether Welinder's thesis is still used and referred to, especially as we had several recent references to support this in the text. Additionally (even though this cannot be supported by references), when we have mentioned our work to well-known Swedish Stone Age researchers, several of them assume that Welinder's thesis is in fact correct. Hence, the statement was not "to strengthen our own case", but we simply wanted to reinvestigate the hypothesis.

The limits of the regression equation are now further described in the text.

The parameter (Sund 5-11) is a typo and should be a reference; (Sund 2010:5-11).

The reviewer thinks that it is disturbing that we do not present any statistical evidence for our statement that there were no significant difference in per cent of surrounding water areas between the categories of "inner archipelago and mixed". We do not understand the problem as we pointed out the difference in situation between the groups of early and late sites. These numbers are presented both in diagrams and individually in tables and can easily be measured.

The reviewer wants a critical discussion of how the different chronologies used in Welinder's and the current analysis (Överåda and Fagervik) can have affected the results. The main reason why such a comparison is not made is that the Överåda chronology cannot be used to distinguish late Pitted Ware pottery. We have developed and clarified that problem in the text.

Statistical evidence for differences between the current analysis and Welinder's analysis could not be provided, as Welinder's variables and ours are difficult to compare, and we now refer to this in the text. Nevertheless, we produced two tables and two diagrams based on the current analysis where the readers can obtain information concerning the analysis and the results.

The reviewer suggests that Fig 5 should include three histograms for three categories of sites; early, intermediary and late. However, the current analysis did not include intermediate sites, since they are of no interest to the discussion. Therefore, we have chosen not to change the charts. The purpose of the previous Fig 5 (now Fig. 9) is to show the difference between early and late sites according to situation and surroundings of water.