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Interactive comment on "Holocene vegetation dynamics in response to climate change and hydrological processes in the Bohai region" *by* Chen Jinxia et al.

Anonymous Referee #1

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This manuscript (MS) by Jinxia et al. describes the results of a palynological and sedimentological study based on a core taken from the Laizhou Bay in the Bohai Sea, eastern China. The MS is generally well written (some remarks concerning grammar etc. and also concerning redundancies are mentioned later), and I think the results are worth being presented since they are at least of regional importance. I am not completely sure though if the results are completely fitting with the scope of Climate of the Past. I have marked the scientific significance as 'good', but in the current stage it is rather between good and fair - if additional methods were used to generate climate data via the described record and if certain aspects would be discussed in more detail, 'good' would probably be fitting.

I must admit in this context that I am not familiar with the research region. It seems some of the authors have also contributed to additional studies from the Bohai region, and I was concerned that there might be some redundancy to these different studies. It seems though that a medium- to high resolution study comprising the Holocene has not yet been published for the region, and the earlier studies are incorporated into the discussion. Several palynological aspects are well incorporated into the manuscript, but a potential degradation of pollen grains is not appropriately discussed in my opinion. The authors correctly refer to the possible problem that there may be a transportation bias concerning bisaccate pollen (it should be considered that this may also affect Poaceae and perhaps Cyperaceae values). They also mention that the suspension of pollen grains may play an impotant role. But pollen concentration varies between less than 100 grains/g (which is quite low!) and several thousand grains/g in this record – this may also point to a degradation signal. Just for example, note that Pinus pollen, which is probably much more resistant to degradation than certain nonsaccate pollen such as Quercus or Chenopodiaceae (compare e.g. Cheddai & Rossignol-Strick 1995 or Havinga 1967), increases relatively in those sections of the core which are characterized by higher sand content (and thus probably are better oxiginated). I think the aspect of pollen preservation should be mentioned in detail at the onset of the discussion and considered throughout.

Another weakness is the age model, particularly in the upper part of the core. The authors dismiss three foraminifer-based ages in favor of one Cs age in the upper section of the core, which may be okay – but then it should be discussed in detail what may have caused that these ages are ca. 3000 yr too old, and how the reader can be sure that the other foraminifer-based ages are correct. It should be mentioned, considering the problems with the uppermost ages, which foraminifer species were chosen (probably benthics?). If the used ages are correct, the 35-cm-section between 25 and 60 cm comprises more than 3000 yr, but the lower 120 cm comprise also ca. 3000 yr. This is certainly tied to the sedimentological aspects which are discussed (particularly the shift(s) of the the Yellow River Channel) by the authors, but in my opinion there remain a lot of uncertainties concerning the ages particularly above 60 cm depth. Therefore, it seems problematic to me to mention quite precise ages for the uppermost 60 cm (as done e.g. in the abstract, see below). Consider also a possible problem in Tab. 1 (see below). And how have you dealt with the older age at 119 cm compared to the younger at 129 cm – could it be redeposition? Was one age excluded?

I cannot say much about the sedimentological interpretation. Concerning the palynology-related sections of the interpretation, I think they are quite well written (though the preservation and age model 'problems' should be considered more often, particularly when it comes to the interpretation of stages 2c to 3). I wondered in this context if the results of Li et al. 2019 would be worth being mentioned here since the record presented in the MS seems to cover the end of the Holocene climatic optimum.

What concerns me particularly when it comes to the relevance of this MS for Climate of the past is section 5.5, in which the authors link their own date to other climate records.

A) Since the authors show their own data vs age (Fig. 8), it should be clearly stated how the age model was composed (linear interpolation?), it is not enough to show the dates in Tab. 1. What a about the ages at 119 and 129 cm?

B) Quercus, as the authors explain, has several species in the region, therefore, the climatic susceptibility of this genus might be relatively low, and several factors, not only temperature, are influencing its relative occurrences in the pollen record. Quercus pollen is also quite susceptible to taphonomical bias (s.a.). For example, the Quercus curve of the Feng et al. (2017) record the authors cite is (naturally) completely different.

C) Two other studies with pollen-based climate reconstructions which are included in Fig. 8 work with quantitative reconstructions – if the authors want their own data to be directly comparable, they should also use such an approach.

The whole section 5.5 seems a little bit like an addition to make the paper a 'climate paper". This is also consistent with some inconsistencies concerning the related Fig.

8 (see below). In order to make this MS appropriate for Climate of the Past, I would suggest to use the pollen data as base for quantitative climate data. The results should be incorporated in the climate-related section. The other aspects I mentioned (taphonomy/degradation and discussion of the age model/interpolation) should be considered, too, and discussed appropriately.

Some detailed remarks follow below:

Abstract

LINES 14/15: 'Nevertheless… remain sparse.' This sentence implies that this is generally the case, but there are numerous studies from other regions regarding this aspect. Also 'long-term' may be confusing here since the presented record does not even span the whole Holocene. Sentences like this one might perhaps be completely removed.

LINE 29 and following: If I did not completely miss anything, the age model is quite unsure between 3000 and 0 years BP (see general comments), and Pinus (excluding on peak that might be a taphonomical signal, s.a.) seems to be decreasing, compare authors' own results (4.2.3).

LINE 30: I understand this that way that the authors call the Quercus percentages a 'temperature index', which is very keen!

1. Introduction LINES 59/60 While the abbreviations YR and BS are already explained in the abstract, maybe they should be explained again in the main text?

2.2. Climate and vegetation LINE 103 '… annual mean air temperature is 7.5-14.0 °C…' Quite a wide range for an average temperature. LINE 109 Perhaps 'Quercus dentata'?

3.2. Palynological and grain size sample analysis LINE 133: Lycopodium in italics LINE 134: Since KOH also degrades pollen, it should be mentioned how long it was used and if all samples were exposed for an identical time interval. LINE 137: 'palynomorph sum' – is the pollen sum meant? If also dinocysts and other palynomorphs have been counted, this should be mentioned here. LINE 137: 'exotic pollen method. . .' The whole sentence seems a little queer to me, and if Lycopod spores were used, I find the term 'pollen' misleading in this context.

4.1. Chronological model LINE 159: and following: It should be explained which objects were used for the dating (ideally the specific species should be mentioned). Either here or in the discussion it should be discussed what may have caused the discrepancies and why the authors trust the other AMS radiocarbon dates.

4.2.1 Palynological Zone 1 LINE 174: 'pollen' is a singular tantum, a plural may only be appropriate if one mentions different pollen types (but even then, 'pollens' should better be avoided – occurs again later in the MS). Line 178: The MS should be consistent concerning grains/g and grains g-1.

4.2.2 Palynological Zone 2 LINE 186: 'decline' (Plural) LINE 193: Here, NAPs may be appropriate, but I would still suggest to write NAP. LINE 197: 'percentage frequency' sounds/reads strange – percentage implies relative frequency. . . (occurs again later in the MS)

5.1. Key terrestrial. . . LINE 237: The second sentence sounds/reads strange, and phrases like 'It is worth noting' should be avoided – if it was not worth noting, why should one mention it. LINE 238: There have been many earlier studies which revealed this effect. Perhaps it would be good to add ', also for Asian regions' or something similar after 'Previous studies', or you should cite one older study dealing with the effect. LINE 257: The last sentence seems useless to me.

5.2. Sedimentary records. . . LINE 279: I think these sentences can be significantly condensed. And in this paragraph, the aspect of pollen grain degradation via oxidation would be worth mentioning. LINE 281: amount I have not checked the following paragraphs in detail – this should be done by a reviewer with sedimentological expertise. Concerning the interpretation itself, several parts are convincing and I appreciate how

the earlier studies are incorporated, but the aspects I discussed in the general remarks should be included. I am particularly surprised about the precise ages given in LINE 426 and LINE 439 – it is not clear to me how 1000 a BP have been determined.

Code/Data availability There are so many options to upload data in an appropriate way these days, but people can change positions, move to other countries or even change their career, therefore, it seems inappropriate to me to name one e-mail address here!

Author contribution Are all aspects mentioned here appropriate to justify being added as co-author?

Table 1: The ages at 119 and 129 have the same calibrated age (probably the one for 119 is wrong?).

Figures: It seems that genus and species names are not always in italics.

Figure 8: In addition to my problems with the age model of the core and the use of Quercus as 'temperature index', the labels in this figure are inconsistent. It should be added that the Quercus curve is from core CJ06-435 (if it is shown anyway after revision). It should be added where curve f is from. These are only a few example. . . all labels should say what it is shown and where the record is from (if the data is based on a specific core/region).

Additional references used for this review: Cheddadi, R. and Rossignol-Strick, M. 1995: Improved preservation of organic matter and pollen in Eastern Mediterranean sapropels. Paleoceanography 10, 301-309.

Havinga, A,J, 1967: Palynology and pollen preservation. Review of Palaeobotany and Palynology 2, 81-98.

Li M, Zhang S, Xu Q, Xiao J, Wen R. 2019. Spatial patterns of vegetation and climate in the North China Plain during the Last Glacial Maximum and Holocene climatic optimum. Science China Earth Sciences, 62: 1279–1287, https://doi.org/10.1007/s11430-018-9264-2

Apologies for some formatting probably getting lost in my comment (such as italics or superscript).

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