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Interactive comment on "The transformation of the forest steppe in the lower Danube Plain of south-eastern Europe: 6000 years of vegetation and land use dynamic" *by* Angelica Feurdean et al.

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The transformation of the forest steppe in the lower Danube Plain of south-eastern Europe: 6000 years of vegetation and land use dynamic" by Angelica Feurdean et al. Simon Connor (Referee) simon.connor@anu.edu.au Received and published: 28 August 2020

This manuscript, by Angelica Feurdean and colleagues, is an interesting and professionally executed study of past conditions on the Lower Danube Plain. The authors

use a multiproxy approach and quantitative land-cover modelling to address questions about the past extent and dynamics of the forest-steppe ecotone in the Western Black Sea region. The manuscript is very well referenced, contains high quality data and is clearly written. The high temporal resolution of the sampling and the quantitative modelling aspects really make this paper stand out from all others in the region. I agree with the other reviewer (#1) that the manuscript presents no major problems. However, I do have a number of suggestions for further improvement that may improve the manuscript's structure and its interdisciplinary and international appeal.

R: Many thanks for your positive appreciation of our work.

Specific comments: 1.The Introduction would benefit from some careful restructuring to link the literature review to the research questions more closely. At the moment, the Introduction seems to be presenting many different aims and objectives: "determine lake catchment and in-lake changes", "explore the role of climate, natural and anthropogenic disturbances", test a hypothesis about the naturalness of the landscape, determine whether forests were more moisture-demanding, determine the timing of transformations, determine the ecosystem's sensitivity to climate and anthropogenic impacts, inform decision making about desertification and to test land-cover models. This seems like too many questions for one paper – the authors cannot hope to deliver on all of these with depth and certainty. Indeed, many of these themes are not revisited in the Discussion section. A more focussed introduction would clarify exactly what problems the authors are aiming to (and can) solve. Ideally, the research questions should arise from gaps or uncertainties in the literature.

R: We largely agree with this point. As this study is the first mid to late Holocene multi-proxy palaeoenvironmental and palaeovegetation reconstruction for this region, we highlighted all the main questions we felt we could answer with the current datasets. However, our aims are targeted at understanding the past vegetation composition and dynamics and their drivers (climate, humans and fire). In the revised version of the paper, we will streamline the aims to ensure a better fit with the themes addressed in

the Discussion.

2.The authors' use of Potential Natural Vegetation (PNV) as a baseline could be more critically assessed. PNVs are problematic since they are static in space and time, while pollen data and REVEALS reconstructions, like the results presented here, show that the past vegetation was spatio-temporally dynamic. There is an excellent paper exploring the mismatches between PNV and REVEALS in Czechia (Abraham et al. 2016, Preslia 88: 409-434) and I encourage the authors to consult it and other papers on the topic (e.g. Chiarucci et al. 2010, J. Veg. Sci. 21: 1172-1178; Rull 2015, J. Veg. Sci. 26: 603-607). My feeling is that the manuscript would be stronger if the authors reduced their reliance on the PNV map and instead used the REVEALS reconstruction as a test for PNV accuracy. This is important because PNV is commonly used as a baseline for restoration and it could be influencing current conservation efforts. The paper shows that there are several possibilities for the vegetation of the site – various types of forest-steppes, steppes and agro-pastoral landscapes, which are dynamic in space and time. This would make for much richer and more interesting conclusions.

R: This is a pertinent point and one that can be addressed with pollen records. Our study does not only rely on the accuracy of PNV map to determine the natural vegetation in the region but uses the pollen-based vegetation reconstruction to determine how much the current vegetation diverges from what should be there under the same climatic conditions. As this point appears to not have come across clearly in the submitted manuscript, we will work to incorporate the reviewer's suggestions and additionally highlight the mismatch between the vegetation composition from PNV map and the pollen-based vegetation reconstruction. We also thank the reviewer for the useful references he has provided.

3.Given the carbonate-rich geology and the fact that mostly shells were dated, it would be useful to briefly discuss potential reservoir effects. Reservoir effects will not change the modelling results, but may introduce some uncertainty about the timing of the major transitions identified in the land-cover reconstructions and the interpretations of vege-

tation change as being forced by climate.

R: We have determined that the hard water effect is about 1000 years and discuss the timing of major environmental changes assuming ages 1000 years younger than would have been calculated without taking into account this dating limitation (see S2). In the revised discussion we will also refer to the timing of major vegetation transition according to the uncorrected age depth.

4. A surprising omission in the manuscript is the aquatic pollen taxa. These taxa might help better interpret the n-alkane results, providing additional proxy for lake hydrological conditions. The authors' interpretations of anoxia and lake level change may find stronger support with the addition of aquatic pollen. The extent to which aquatic vegetation influenced the n-alkanes signal could also be explored.

R: Thank you for pointing this out. The pollen percentages of aquatic taxa and Cyperaceae do not show a clear pattern in their composition and abundance over time to provide any additional information on changes in hydrological conditions. However, in the revised paper we will present these taxa and attempt to link their dynamics to that of n-alkane variations.

5. The REVEALS modelling in this paper is very comprehensive. One small doubt concerns Carpinus orientalis. In my experience, this 'tree' is very often a shrub in forest-steppe landscapes. Is it correct to call it a 'forest' taxon in this region? Is it possible that the increase in C. orientalis around 4200 cal. yr BP represents a scrub expansion or the abandonment of coppicing? These changes may even relate to shifting land-use at the Neolithic–Bronze Age transition, a topic that would benefit from further exploration in the Discussion.

R: We have referred to Carpinus orientalis as a forest taxon based on its percentages and consistent presence in the uppermost canopy levels in our vegetation surveys of/within forest stands in the area. Upon a more thorough and linked investigation of our archaeological and palynological records, we will integrate our findings regarding

the dynamics of C. orientalis in the revised version of the manuscript.

In addition to the general comments, we found most of specific comments very useful and they will help us to improve the quality of the paper. We will therefore make every effort to incorporate the majority of them into the revised version of the paper.

Kind regards, Angelica on behalf of the co-authors

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