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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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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. How-

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ever, I do have a number of suggestions for further improvement that may improve the manuscript's structure and its interdisciplinary and international appeal.

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.

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.

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 vegetation change as being forced by climate.

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.

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.

6. Overall, I found this a very interesting manuscript and a novel contribution to the literature on the antiquity and dynamics of temperate grasslands.

Technical comments by line number:

31 – place a comma after "taxa" 32 – "Maximum tree cover. . . between 4200 and 2500" here, but "greatest tree cover. . . between 6000 and 2500" in line 30. . . this seems like a mistake since the ranges overlap 35 – "mid-Holocene forest maximum" seems to refer to conditions from 4200-2500 cal. yr BP, which is usually termed late Holocene.

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See also comments about the use of “forest”, line 305. 38 – “highlighting recurring anthropogenic pressure” – is this indeed cyclical, or due to more-or-less continuous anthropogenic pressure? 39 – “was in between that in” – consider “falls in between that of” 42 – “reflects” – change to “reflect” to agree with woodlands 44 – the comment about pollen preservation seems to indicate that this pollen record may be adversely affected by taphonomy... is that the case? Perhaps taphonomy could be addressed elsewhere in the paper? 47 – delete “The” at start of sentence 49-51 – this sentence sounds like it concerns the present-day environment (i.e. ecological studies), but all the references are palaeo studies. More precise wording would avoid misleading readers, or modern ecological studies could be incorporated to increase the interdisciplinarity of the manuscript 54 – the definition of “lowlands” in this paper is not very clear, since it seems to imply that lowlands are “drier” than “mesic areas of Europe”. A more precise term like “steppic grasslands” or even a map showing the current extent (not potential extent) of the ecosystem might help, since readers could easily confuse the steppic lowlands they are referring to with the lowlands of the Netherlands, for example, which are certainly quite different. 65 – the idea of grasslands being richer than rainforests seemed surprising, but the original reference states “at small spatial scales vascular plant diversity of certain European grasslands even exceeds tropical rainforests”. It would be good to specify what scale is meant here, since ‘alpha diversity’ is a vague term encompassing a large range of spatial scales (Whittaker et al. 2001, J. Biogeog. 28: 453-470). 67 – this sentence refers to temperate grasslands (it may be different for tropical grasslands) 71 – unclear what you mean by “cultural rise” – what is it and what is the evidence for it? 87 – remove comma after “although”. I found this sentence difficult to understand. Are you saying that recent land-cover models tend to support the pollen-based reconstructions, whereas the old ones didn’t? 92 – this sentence is very important in the paper, but is very complex and methodological. I feel as though it would be more logical to have it appear after the aim and research questions, rather than before. 96 – these are very good research questions and would benefit from stronger links to the preceding introductory material. Why is it important to know what

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is “natural” here? Is that a current concern for biodiversity conservation in the region? Why would we expect moisture-demanding taxa in past forests? Has this been hypothesised on some basis? There seems to be a word missing in “When did this area undergo the most marked land cover”... land-cover change, perhaps? 103 – what is the hypothesis mentioned here? How can it be assessed using pollen data which are poorly taxonomically resolved for many grassland taxa? 105 – please explain these desertification phenomena earlier in the introduction to provide context 114 – what is the source of the “excessively temperate continental” category? Could you give a Köppen classification as well? 118 – comma after “eroded” 120 – it’s unclear to me why the potential natural vegetation is being presented here, given that the paper aims to reconstruct the past vegetation and land cover. I feel that PNV should be a model to be tested, rather than stated as a fact, as it is here. 126 – the pollen source area is stated as being 20 km. How was this determined? 162 – what is the minimum pollen sum? 177 – it would be good to justify the statement that n-alkanes reflect climate conditions here – what is the evidence? 199 – this statement about trees may need revision to specify deciduous trees or angiosperms, since n-alkanes appear to be less successful in detecting coniferous trees (Diefendorf et al. 2015. Geochim. Cosmochim. Acta 170: 145-156) 220-225 – please provide a reference justifying Zr and Fe:Mn as proxies for erosion and anoxia 273 – the subtitle here is confusing and unclear. Are you trying to say it’s about both periods or the transition from one to the other? Perhaps “Transition from forest steppe (6000-4200 cal. BP) to maximum tree cover (4200-2500 cal. BP)” would be clearer? Also, can we be certain C. orientalis was a ‘tree’? 276 – change “open woodland forest” to “open woodland” 279 – change “shrub” to “shrubs” (or make grasses and forbs singular) 280 – change “Coeval to” to “Coeval with” 282 – is it possible to disentangle the climatic and vegetation signals in the n-alkanes, or is it not possible to say whether they were caused by one or the other? Perhaps the pollen record suggests that vegetation thickening was the main cause of the n-alkanes variations? Vegetation thickening might be linked to declining fire activity, changes in agricultural practices (cropping, grazing) and/or climatic drivers. 286 – how are aquatic plants re-

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flected in the n-alkane record? The absence of aquatic/wetland taxa (e.g. Cyperaceae) in the pollen record (Appendix A1) is striking – were aquatics not counted? 288 – is it possible that there was a higher lake level when this lake is connected hydrologically to the Danube? This would imply the Danube also having a higher level, wouldn't it? Could the potential anoxia be linked to lake trophic status or to aquatic vegetation? 305 – the term "forest cover" is a little bit misleading as the authors' reconstructions do not show the existence of a forest, but of a forest-steppe. There are several instances through the text where this confusion could arise, including line 34 of the Abstract. In the context of forest-steppe, I suggest "tree cover" rather than "forest cover" and perhaps "steppe expansion" rather than "forest loss". 311 – change "impact of" to "impact on" 325 – remove the "." after "Plantago" 329 – the mention of uncertainties is welcome; however, the authors could help the reader to better understand the implications of the uncertainties... do the interpretations change if we consider these uncertainties, or are the uncertainties minor? 353 – replace "modelled-based" with "model-based" 392 – unclear sentence 398 – change "extend" to "extent" 399 – this is an interesting idea about the climatic suitability of the region for forest, though it is maintained relatively open through disturbances like cropping and grazing – a 'plagioclimax'. Perhaps this idea could be expanded a bit more, given its relevance to landscape restoration and conservation, as well to understanding human interactions with the biota of the region 403 – delete "the" before "SE Europe" 411 – comma after "millennia" 412 – the idea that deforestation contributed to aridification would benefit from some discussion and justification in the Discussion section, not just in the Conclusion. 416 – there is a slight inconsistency in the argument here about n-alkanes. The authors say that they track the vegetation changes in the pollen, so that makes them a reliable indicator of past vegetation change, but earlier in the paper the n-alkanes are interpreted as a climate proxy. Is it that n-alkanes are a proxy for climate directly, or are they, like pollen, a proxy for vegetation, which can be influenced by climate? Readers like myself will be grateful for the additional explanation! 418 – please help the reader to understand how this single record might be able to be used to test land-cover models – this would add greatly

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to the application and relevance of the study 419 – what is meant by “an earlier impact than in the reconstructions is also true”? This issue is not really discussed in the paper and seems not to really relate to the material presented. Perhaps after the research questions are refined, the conclusion could be restructured slightly to address those? 425 – replace “design” with “designed” 431 – replace “grating” with “granting” (also line 433) Fig. 5 – please add some indication of the pollen zones from Fig. 4 Table 2 – check spelling of Plantago lanceolata Appendix A1 – please add wetland/aquatic taxa to the pollen diagrams

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