

Interactive comment on “Climate, people, fire and vegetation: new insights into vegetation dynamics in the Eastern Mediterranean since the 1st century AD” by J. Bakker et al.

Anonymous Referee #1

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The paper by J. Bakker et al. focuses on the last 2k years of human impact, fire and vegetation changes in the Taurus Mountains (Southwest Turkey), exploring the link between the observed changes and the role of climate variability. The authors combine palaeoecological data in a nice multi-proxy framework (pollen, sediment charcoal, NPP's and sediment properties) from two well documented archeological sites, to disentangle human impact and climate variability during an important period of landscape transformation and cultural development. The role of climate changes vs human impact in shaping Mediterranean ecosystems is a long-standing and debated question among palaeoecologists; and this study may improve our understanding of Holocene environmental changes in the Eastern Mediterranean, at least for the last 2k years.

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With that said, there are several relevant points that the authors should clarify in revising their manuscript. If the paper is refocused and the points below addressed, then this could be an interesting contribution to the research in the area.

Specific comments: 1) Throughout the manuscript, it is quite unclear which data are new and which are instead from previous published studies. For instance, in the abstract the authors claim that “two well dated palaeoecological records... provide a first relatively detailed record of vegetation dynamic from late Roman times”, but at least one sediment core used (Gravgaz site, SA06EP1) appears to be the same as in Bakker et al., 2012 (The Holocene), where also pollen, charcoal and sediment properties data are presented. The site Bereket is also mentioned in another paper from the same authors (Veg. Hist and Archeobotany, 2012, Table 2). Which are the new data, then? Please also note that in many parts of the discussion section (i.e. “vegetation dynamics”) you seem to explain the observed changes with the role of climatic variability, which in turn is inferred from pollen data (Bakker et al. 2012; even same data?). Even assuming that the climate fluctuations that you infer from pollen data are realistic (which for me is a long bow to draw, considering the human impact), you can’t explain vegetation dynamic using climate data inferred from pollen, but you can instead focus on the reliability of other (independent) climatic proxies. 2) My other concern is the presentation and interpretation of the charcoal data. First, it is important to understand that microscopic and macroscopic charcoal likely reflect biomass burning at different spatial scales (local vs regional), so it is advisable to interpret them separately. Most importantly, the identification of charcoal peaks is limited to the local fire proxy and requires contiguous sampling, not to miss any fire events; this is normally achieved on contiguous (sieved) samples, rather than from pollen slides. If you inferred charcoal concentrations from 50 (and 44) pollen slides, and every ca 7 or 14 cm, evidently you don’t have the resolution for peak detection analysis. What you are showing instead is a limited number of charcoal peaks detected by chance along the record. If this is the case, the estimation of fire frequency and their implication for the records should be avoided.

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