

Interactive comment on “7300 years of vegetation history and climate for NW Malta: a Holocene perspective” by B. Gambin et al.

Anonymous Referee #2

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The manuscript by Gabin and co-authors presents a new insight to the vegetation history of Maltese Archipelago and its climatic implications. An evaluation of human impact on vegetation is attempted by the parallel discussion of pollen and NPP record with cotemporary archeological ones. Additionally, the first pollen-based quantitative climate-reconstruction for Malta’s Holocene, based on a multimethod approach, is a significant input to the paleoclimatic record of the Mediterranean due to the key location of the area.

Nevertheless a number of issues should be addressed in order to enhance the robustness of manuscript:

I totally agree with the comment of C. Hunt about the age model and its restrictions, yet I know that finding material for dating in such deposits is not easy. A careful use of

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the interpolated ages is therefore very important especially when comparing with other records.

The methodology used for the determination of different depositional environments of the core is unclear. The lithology of the sedimentary units alone is not a secure way to disseminate such marginal depositional environments.

The calculation, presentation and discussion of microcharcoal assemblages as percentages of terrestrial pollen sum and not concentrations or influx is not in accordance with the existing scientific literature (e.g. Callo et al. 2012, Sadori et al. 2004, 2008, Di Rita & Magri 2009, Tinner et al. 2009, Noti et al. 2009).

Even though it is tempting to believe that with the study of one-alone proxy record both climate and human impact can be considered and differentiated, I am not convinced. It looks too much of a cyclic argument to interpret the same vegetation dataset both in terms of human impact and climate change. It is well established that in the Mediterranean the transition from a climate-controlled vegetation to a human disturbed one is very complex and the role of both factors should be carefully examined (e.g. Roberts et al 2011). The climate reconstruction methods presented here are of high quality but if the vegetation itself during certain periods is affected by human activity, how is it possible to differentiate the climate signal alone. Under that perspective it would be useful to discuss and compare the results of the paleoclimate with independent records from the Mediterranean (stalagmites, marine geo- and bio-indices, SST, e.g. Finne et al 2011 and references therein)

Further corrections 1) Page 4 line 7 the use of ha should be avoided and replaced by SI units

2) Page 4 line 8 define the “five main types” of sediments deposited, or sedimentary rocks occurring in the island

3) Page 6 line 3 *Lycopodium* should be in italics

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4) Page 20 line 2 *Pinus* should be in italics

5) Both the terms charcoal (e.g. page 8 line 13 and 27, page 9 line 9, 24 and 31, . . .) and microcharcoal (e.g. page 6 line 18, page 14 line 7,) are used to describe the same thing.

6) Some of the references of fig 9 are not mentioned in the text and/or included in the reference list (Bar-Matthew et al. 1998, Pross et al. 2009, Magny et al. 2007, Kouli et al. 2012)

7) The humidity index featured in figure 9 is, neither explained, nor discussed in the text. As the term has been used before to describe relative humidity in marine pollen records of the Mediterranean (Triantaphyllou et al. 2009, Triantaphyllou et al. 2014, Kouli et al. 2012) it should be used with caution.

Interactive comment on Clim. Past Discuss., 11, 4505, 2015.

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