

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

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CPD reply to Reviewer 2 (ANON)

We would like to thank our anonymous reviewer for the positive feedback and constructive comments on our manuscript. Comments and responses are addressed below:

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

Answer: Yes, we agree that the chronology must be treated with caution, although there is good correlation between cores BM2 and BM1. Reworking processes in low-energy

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ria environments such as these tend to be low. To overcome reservoir problems we have dated charcoal and short-lived plant material. Changes in sedimentation rates will essentially be mediated by shifts in fluvial sediment supply (climate or human-induced erosion episodes). In this paper, our interpretations are based on a chronological time scale established according to four radiocarbon dates. As stated above, we assume that the sedimentation rate in the intervals between dating points have remained relatively constant. However, we do not exclude the possibility that in some depths, some changes in sedimentation rate may have occurred leading to slightly different ages for the observed environmental changes.

2) 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.

Answer: The methodology used to define the sedimentary environments is based on high-resolution sedimentological and palaeoecological data (ostracods and marine molluscs). We refer the reader to (Marriner, N., Gambin, T., Djamali, M., Morhange, C., Spiteri, M. (2012). *Geoarchaeology of the Burmarrad ria and early Holocene human impacts in western Malta. Palaeogeography, Palaeoclimatology, Palaeoecology*, 339-341, 52–65) for further details.

3) 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).

Answer: Noted. Both methods (percentage or concentrations/influx) provide the same results/trends; it is also acknowledged that trends observed, however they are obtained, must always be interpreted very carefully in term of fire events.

4) 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 con-

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vinced. 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).

Answer: We agree with this point which was highlighted on page 4535 line 24/5 whereby we state: “We also cannot exclude for this period a possible bias in our climate reconstructions due to the increasing human impact”. This is a very important point. Therefore we have added the following sentence in the text to add further emphases: ‘Human impacts have affected the natural vegetation of the Mediterranean since the mid Holocene, but disentangling climatic and anthropogenic causes of vegetation change is complex. Our climatic reconstruction seems consistent with independent records from the Mediterranean such as lake-levels from Sicily (Fig 8) or speleothems from Israel (Magny et al., 2012, Bar-Matthews and Ayalon, 2011), and large scales paleoclimate reconstruction (Mauri et al., 2015).’ A comparison with the independent Mediterranean speleothems records has been added in Fig. 9.

5) Further corrections a) Page 4 line 7 the use of ha should be avoided and replaced by SI units b) Page 4 line 8 define the “five main types” of sediments deposited, or sedimentary rocks occurring in the island c) Page 6 line 3 Lycopodium should be in italics d) Page 20 line 2 Pinus should be in italics e) 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. f) Some of the references of fig 9 are not mentioned in the text and/or included in the reference list

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(Bar-Matthew et al. 1998, Pross et al. 2009, Magny et al. 2007, Kouli et al. 2012) g) 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.

Answer: Amendments to typos will be made and requested information added, thank you.

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