

Discussion

Reply to the comments by Mauz, B. and Antonioli, F. on “Sea Level and Climate Changes during OIS 5e in the Western Mediterranean”

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1. Introduction

We really appreciate the comments by Mauz and Antonioli on our paper ([Bardají et al., 2009](#)), but here we respond to the erroneous statements made in their critical reading of our work.

We agree with the authors' historic review of the problems raised when effects of tectonics and sea level oscillations are observed in the record of relative sea-level changes in Western Mediterranean. We should not forget that the Mediterranean itself constitutes a collision plate boundary, with few stable areas within it. Therefore, the study of the Last Interglacial shorelines cannot be simply analysed according to altitude, as they say. What we do not share with Mauz and Antonioli is that “the only solution to this seemed to be determining the age of a coastal deposit using radiometric, dosimetric and chemical dating techniques”.

It is obvious that we need a chronological approach to be as accurate as possible. First of all, however, an exhaustive field analysis is needed, and this is overlooked by Mauz and Antonioli at least in the Spanish Mediterranean sector. To begin with, field analysis would yield a deeper wider geological knowledge, not only of the precise sections but also on a regional scope. Second, a geomorphologic understanding of the selected sections is necessary. Geomorphology is the best tool that enables us to understand how many different sea level highstands or intervals we are dealing with in terms of their relation to former and younger sea level indicators (i.e. 3D stratigraphic architecture). Sampling for dating purposes can only be done when the sedimentary sequence is clear, and after detailed geomorphologic mapping. Unfortunately,

geomorphologic maps are often lacking in studies related to the reconstruction of past sea level changes. In most cases these maps would be the only way of reconstructing past sea level oscillations (see for example, [Radtke and Schellmann, 2004](#)), becoming the best way to build robust working hypothesis regarding the age of deposits you are dealing with.

Based on their comments, Mauz and Antonioli pay much more attention to the dating than to field knowledge. Indeed, the major failings found by Mauz and Antonioli in our paper relate first to the U-series dating of mollusc shells. They don't agree with our assumed cementation and climate parameters. Secondly, our use of the U-series dating technique itself is criticised. Thirdly, the role and appearance of Senegalese fauna in the Mediterranean are questioned. Finally, they also find problems with the oolitic facies and recorded sea level changes.

2. U-series dating of mollusc shells

It is unfortunate that Mauz and Antonioli dedicated so much energy in lengthy comments combining a variety of arguments to explain why they simply do not accept our findings. They make use of sentences and phrases extracted “out of context” from published literature (including our own) but curiously miss a key paper.

2.1. U-series data

A few examples with respect to U-series data will illustrate both improper and biased use of extracts.

a. “All but one sample of [Zazo et al. \(2003\)](#), all samples of [Hillaire-Marcel et al. \(1996\)](#) and most samples of Hillaire-Marcel et al. (1986) show initial [²³⁴U/²³⁸U] activity ratios higher than those expected from a closed system.”

The $^{234}\text{U}/^{238}\text{U}$ ratio does not, per se, indicate the “closure” of a radioactive system, especially when this system corresponds to the initial incorporation of diagenetic (i.e., post-depositional) uranium in the vadose zone of beach deposits (i.e., with continental U-inputs showing variable ^{234}U -excesses). This is a totally ungrounded argument here. However, we think that the discussion on the correction models for open U-series systems was not the aim of this paper, but was one of the main concerns of a previous paper ([Goy et al., 2006](#)) that should have been consulted by Mauz and Antonioli.

b. Mauz and Antonioli cite [Eggins et al. \(2005\)](#): “These results are notable for contradicting established wisdom, which assumes that the most reliable ages can be obtained from the inner layer of mollusc shells due to the arrest of U-uptake following a phase of early U-uptake (e.g. [Hillaire-Marcel et al., 1996](#)).”

It is indeed true but, as explained in many of our papers, any geological setting and diagenetic situation leads to its own specific geochemical characteristics. Thus, Mauz and Antonioli should also have cited here extracts from other papers of our group illustrating the large array of geochemical behaviour of U-series in beach deposits that we have observed. These range from totally “open system” where late diagenetic, possibly modern U-uptake occurs, to systems rapidly “closed” following an early diagenetic phase of U-uptake (e.g., [Zazo et al., 1999](#) and [Goy et al., 2006](#)). They should also have referred here [Kaufman et al. \(1996\)](#) and [Labonne and Hillaire-Marcel \(2000\)](#); two papers where post-depositional U-uptake in mollusc shells are discussed in relation with organic lining decay.

c. “[Zazo et al. \(2003\)](#) ascribe the U-series age of the coral (178 ± 10 ka) to MIS 7.1, but according to the astronomical ages of the marine isotope stages ([Martinson et al., 1987](#)) this age spans from MIS 6.4 to MIS 6.6. This reveals that (i) the age estimate is inaccurate because the Senegalese fauna was not in the Mediterranean Sea during cool climate periods and (ii) the interpretation of the datum is wrong.” Well aside from a too hasty linkage of high sea-levels to the SPECMAP timescale (an issue that would require a full paper by itself), and a naive statement

about the improbability of the presence of Senegalese fauna during “cold intervals” in the Mediterranean Sea (which we have never claimed), the authors show some bias in comparing the 178 ± 10 ka datum with the age of a maximum insolation in the Northern Hemisphere. They ignore the fact that all U-series “measurements” obtained in the cited study were used with a U-flux-correction model to discuss the age of the fossil “coral–uranium” (and not that “of the coral”; see Table 4 and Appendix in [Goy et al., 2006](#)).

2.2. Climate

Aridity in SE Spain is a fact, not an opinion. South-eastern Spain exhibits the most arid climate in Europe, more than any other place in Mediterranean European countries. This is not a freely taken assumption, but it can be referenced in any meteorological or climatic classification of Europe (see for instance [Peel et al., 2007](#)). Almeria falls within the Bwh (desert) climate and Murcia in the Bwh (steppic) one, after the Köppen classification, and they experience arid or semiarid climates after the Thornthwaite aridity index.

Mauz and Antonioli refer to [Tzedakis \(2007\)](#) to justify their climate assumptions for SE Spain, but although this author makes a wonderful palaeoenvironmental review of the Mediterranean for the last few million years, the references used for SE Spain do not reach the Last Interglacial. Indeed, they all relate to the Holocene–uppermost Pleistocene ([Pons and Reille, 1988](#) and [PaKaufman et al. \(1996\)](#) [ntKaufman et al. \(1996\)](#) [aleón-Cano et al., 2003](#)) or at most to MIS 3 ([Cacho et al., 1999](#) and [Cacho et al., 2006](#)). Likewise, the pollen records that Mauz and Antonioli, assume as generalized for the entire Mediterranean during MIS 5e, are from other Mediterranean areas, such as NW Greece, and not from SE Spain. Indeed, [Tzedakis \(2007\)](#) suggests “specific regional patterns in the character of the interglacial successions, reflecting climate and local intrinsic properties”, specifically talking about the drier locations of southernmost Europe, suggesting that these drier areas have experienced a unique evolutionary history.

2.3. Cementation

Taking into account these climatic characteristics, which lead to very little leaching of carbonates, and the availability of CaCO₃ coming from widespread calcareous substratum, rapid cementation would not be unexpected.

As far as we know, beachrock definitions mention more specifically the morphology and precise location of the deposits rather than their petrography. They are coastal deposits rapidly cemented by calcium carbonate in the intertidal (foreshore) zone, characterized by the preservation of syndimentary structures. Cementation of beachrocks can certainly be varied, and is generally characterized by high-Mg micrite or aragonite. However, beachrock cementation is not limited to those types ([[Milliman et al., 1974](#)], [[Neumeier, 1998](#)] and [[GischlKaufman et al. \(1996\)er, 2007](#)]).

Regarding our MIS 5 deposits, and independently of whether or not they are real beachrocks, the preservation of the prograding bodies within the highly cemented littoral deposits leads us to assume an early stage cementation.

3. Time of Senegalese fauna immigration

Mauz and Antonioli found great problems assuming that *S. bubonius* entered into the Mediterranean earlier than MIS 5e. They again go back to the problems found in U-series ages of molluscs, but they overlook the field evidence. In our paper ([Bardají et al., 2009](#)) we made an extensive report on the occurrence of *S. bubonius* in Western Mediterranean MIS 5 levels, and, in our opinion, the exclusive association of *S. bubonius* to MIS 5e deposits can be ruled out, or at least there should be a reasonable doubt about such an association. This species is more abundant in MIS 5e deposits but these are not the only deposit yielding *S. bubonius*. On Mallorca Island, for example, *S. bubonius* appear in a marine terrace deposited during a highstand older (and higher) than MIS 5. [Zazo et al. \(2003\)](#) report a marine unit older than MIS 5 deposits (see Fig. 15 in that paper), located at + 10 m, where [Cuerda \(1989\)](#) and [Cuerda and Sacares \(1992\)](#) reported the occurrence of Senegalese fauna, including *S. bubonius*.

In his turn, [Hearty \(1987\)](#) found *Strombus* fauna associated with deposits producing aminozone E and mixed E and F–G ratios in Campo de Tiro (Mallorca). These ratios of aminozone F–G are reworked populations in the last Interglacial deposits or indigenous populations in older deposits. He also found *Strombus* in a unit ascribed to MIS 5.3/5.1 (see Fig. 7 therein). Similarly, [Hearty \(1986\)](#), in a work described as pioneering by Mauz and Antonioli, stated that “Senegalese (or *Strombus*) community...is thought to have immigrated into the Mediterranean during the middle and/or late Pleistocene”.

Mollusc species can migrate in a larval form, and they develop only if they find suitable temperature, salinity and other ecologic conditions for their survival. In this sense, alkenone unsaturation index from the Alboran Sea ([Martrat et al., 2004](#)) show higher than present SST not only during MIS 5 but also during MIS 7.

4. Sedimentary facies

We are very surprised with the statements that Mauz and Antonioli make about the sedimentary facies of MIS 5 deposits from Mediterranean Spanish coasts. Sadly, these are incorrect.

We are astonished to read “To our knowledge the last ooid formation at the Iberian Peninsula took place during the last Messinian in the Neogene basins of the Betic mountains (“Terminal Carbonate Complex, [Riding et al., 1991](#))”, and again when it is said that we find “oolitic sand at the coast adjacent to these Neogene Basins”, suggesting that oolitic facies in MIS 5e deposits from SE Spain are reworked from Neogene deposits. This is simply not true.

4.1. Wind regime during the Last Interglacial

We infer a certain wind regime for several reasons, not only from the presence of oolites. First, and the most important point, is that we always find oolitic sediments forming wide dune belts, reaching in some cases heights of more than 20 m (El Altet, Alicante, in [Montenat, 1973](#)), where sedimentary structures clearly point to an aeolian origin. The second point is that for oolite formation high energy is needed, and we again cite [Wanless and Tedesco \(1993\)](#) who made

an extensive report on present day oolite formation in Bahamas. However, Mauz and Antonioli cite the work of [Simone \(1980\)](#) to say that oolites can form both under “calm or agitated water”, but what she actually says is that tangential oolites need, in general, high energy, while radial oolites form preferentially in calm environments.

Assuming as certain the fact that we need some energy for oolite (tangential oolites) formation, the energy requirements can come either from wind-wave agitation or from tides. This is why, together with the extended dune development that does not prevail today, we assume a wind regime different from the present for most of the oolitic settings. Stronger winds from the east are the only way to explain the precise location of these outcrops, always found in east-facing coastal sectors. Furthermore, the present day wind regime (see Fig. 2B) points to enhanced easterly winds during the warmest season, surprisingly affecting those western Mediterranean sectors where MIS 5e oolites are found.

Here, we emphasize that our palaeoenvironmental interpretations do not come exclusively from petrographic characteristics, but primarily from geomorphologic disposition, sedimentary structures and chronological evolution of sedimentary bodies.

4.2. Reworking from Neogene oolites

We accept that maybe oolites are not currently forming on the Egypt and Tunisia coasts. We have not studied them and we only make such an affirmation based on literature ([Lucas, 1955](#)). However, Mauz and Antonioli state that oolites from Tunisia and Alexandria are reworked from MIS 5 and early Holocene deposits, assuming oolite formation during MIS 5 in Tunisia.

References given by Mauz and Antonioli ([Fabricius et al., 1970](#)), ([Strasser et al., 1989](#)) and ([El-Sammak and Tucker, 2002](#)) show the petrographic differences (among others, such as mechanical abrasion, boring by organisms, broken oolite envelopes) between “in situ” and reworked ooids. The percentage of oolite particles within the sediment is also reported as an indicator for “in situ” oolites ([Strasser et al., 1989](#)),

who considered 60–80% of oolites a good indicator of “in situ” formation.

Regarding the southeastern Spain oolites, the first regional geological study done in Neogene and Quaternary deposits ([Montenat, 1973](#)) already considered the possibility of a reworking origin for the Last Interglacial (then called Tyrrhenian) deposits. Petrographic analyses were conclusive: the Neogene oolites were bigger than Pleistocene ones, with a much thicker cortex, but with only a few preserving their original envelopes; most of them appeared recrystallized and with the nucleus and/or envelopes almost completely dissolved. Pleistocene oolites, on the other hand, preserve their aragonite lamellae and display (under polarized light) the black cross, an anomalous birefringence typical of a tangential arrangement of aragonite crystals. These characteristics therefore eliminate a Neogene origin. Likewise, petrographic analyses done in samples taken in some of the reported oolitic aeolian dunes always show a percentage of oolites that lies within the limits stated ([Strasser et al., 1989](#)) as an indicator for “in situ” oolites.

Further, assuming that all MIS 5 oolites are reworked from Neogene deposits implies that all Neogene basins contain oolitic facies of either Terminal Carbonate Complex (Late Messinian) or older units. This contention is most unfortunate and untrue (see [Montenat, 1990](#), for a complete review of Neogene basins from SE Spain, or even the available geological maps of Spain, IGME). If we consider some of the most outstanding examples of oolitic dunes from MIS 5, we can see that there is no possibility of such a reworking origin.

Probably, the most outstanding example of the disconnection between the TCC and MIS 5 is found in the Cope Basin, a tiny basin just north of Cape Cope (Aguilas, Murcia) filled with marine Pliocene to recent sedimentary rocks and having a Betic substratum mainly composed of metamorphic rocks (schists, phyllites, etc). There (see Figs. 4 and 5 in [Bardají et al., 2009](#)) oolites accumulate in coastal dunes and upper foreshore deposits of MIS 5 age, but no TCC or other oolitic rocks have been found or cited so far. Therefore, no evidence of reworking of

pre-MIS 5 oolitic deposits can be invoked in this example.

Coated grains and oolites are also found in La Marina (El Pinet, Alicante), here again there are no Neogene oolitic facies directly connected with the coastal sector during MIS 5. The same occurs with the wide dune complexes developed in Callblanque or La Manga del Mar Menor (Murcia), where no oolitic rocks have been found in the Neogene sequences.

In Almeria, aeolian oolitic dunes found just north of Gata Cape (Los Genoveses, San José, La Isleta, etc.) are placed on the large volcanic unit of Gata Cape, where no TCC or other oolitic facies are found. Of course it might be argued that those oolites come from eroded TCC rocks that originally capped the volcanic rocks, such as near Carboneras (Mesa de Roldán), but we must take into consideration that there are no oolitic facies where the Neogene units are still preserved. Even more, the ruggedness of the coastal cliffs makes transport of oolites by longshore currents to small isolated bays extremely improbable. Finally, reworking from the post-evaporite Messinian oolites outcropping in Sorbas, Almeria–Nijar or Polopos basins also seems physically impossible if we take into consideration several points: the long distance from these basins to these coastal sectors; lack of geographical connection, now and during last Interglacial times, between Almeria–Nijar basin and eastern Gata Cape littoral, due to the elevations of La Serrata range and the Miocene volcanic outcrops of Gata; absence of oolites anywhere between these basins and the Gata Cape oolite dunes; degree of preservation of oolites in MIS 5 deposits, without any sign of abrasion or boring or broken lamellae; and percentage of oolites.

We sincerely think that prior to making such assertions, Mauz and Antonioli, should have acquired closer and more accurate geological and geomorphological field knowledge of the studied area.

5. Sea level interpretation

Mauz and Antonioli question our data and interpretation of sea level highstands.

In the Campo de Tiro section (in the Balearic Islands) three different littoral deposits from MIS 5.e, defining three different high positions of sea level, have been reported by all the authors that have studied this sequence ([\[Butzer and Cuerda, 1962\]](#), [\[Hearty, 1987\]](#), [\[Cuerda, 1989\]](#), [\[Goy et al., 1997\]](#) and [\[Zazo et al., 2003\]](#)). The ages given by more than 30 samples taken in the same section ([\[Hillaire-Marcel et al., 1996\]](#)) are in complete agreement not only with other sea level interpretations of the Mallorca littoral (see for instance [Hearty, 1987](#), or [Tuccimei et al., 2006](#)) but also for sea level intervals defined for MIS 5.e all over the world (see the review done recently by [Hearty et al., 2007](#)). These authors report “significant climatic changes occurred thereafter, midway through and continuing to the end of MIS 5e...”, with a marked oceanographic reorganization and an increase in storminess associated with the end of MIS 5.e. We find the same record of three high sea level indicators for MIS 5e, not only in Mallorca Island but also in other littoral sections from SE Spain ([Zazo et al., 2003](#)). These indicators record different sea level positions but also they show different sedimentary facies indicating significant climatic changes and an increase in storminess to the end of MIS 5e.

Mauz and Antonioli argue that we should have compared our sites with tectonically stable areas in the Mediterranean, providing an example of tectonically stable areas in Sardinia or Sicily. Indeed, Sardinia is reported by [Ferranti et al. \(2006\)](#) as the reference site for MIS 5.e sea level elevation, arguing that it behaves as a tectonically stable area. We question how an island where MIS 5e markers have been described at elevations ranging from 1.8 to more than 10 m above present sea level can be considered stable? Likewise, several positive sea level oscillations within MIS 5 have been reported in Sardinia ([Lecca and Carboni, 2007](#)).

[Antonioli et al. \(2006\)](#) report fossil tidal notches from different sites of the Tyrrhenian coast of Italy at elevations ranging from 3.8 to 10.8 m, stating that “these elevations are very close to the uncertainty range for the eustatic elevation of MIS 5.e highstand”, and so they conclude that the studied sites are located in almost tectonically stable areas. In

Sicily (one of the reported sites for tidal notches) there are sea level indicators for MIS 5.e at elevations between 2 and 175 m above mean sea level ([Ferranti et al., 2006](#)).

[Antonioli et al. \(2006\)](#) report a steady sea level between 127 and 119 ka, explaining the differences in altitude of tidal notches as due to glacio-hydro-isostatic subsidence, given the fact that the areas reported are “tectonically stable”.

As we see, these authors always refer to MIS 5.e sea level position, but which sea level? The exhaustive review done by [Hearty et al. \(2007\)](#) report on many places around the world, defining up to six different sea-level intervals for MIS 5, three of them corresponding to high positions of sea level at different heights (+ 2.5 ± 1 m between ca. 132 and 125 ka; above + 3 m between 124 and 121 ka; and between + 6 and + 9 m around 121–119 ka), with at least one “short regression” around 125–124 ka.

There are many references to different highstands, or perhaps it is more accurate to say high sea level oscillations during the last interglacial (MIS 5.e), many of them reported in the previously mentioned work by [Hearty et al. \(2007\)](#) and many others not reported therein but included in our work ([Bardají et al., 2009](#)). Regarding MIS 5.3 or 5.1, the first one is usually reported to have a lower sea level than present, but during MIS 5.1 most authors seem to widely agree about a similar or slightly higher than present sea level ([\[Hearty et al., 1986\]](#), [\[Hearty and Kaufman, 2000\]](#), [\[Vessica et al., 2000\]](#), [\[Murray-Wallace et al., 2001\]](#), [\[Hearty, 2002\]](#), [\[Muhs et al., 2002\]](#), [\[Potter, 2004\]](#), [\[Wehmiller et al., 2004\]](#), [\[Tuccimei et al., 2006\]](#) and [\[Doar and Kendal, 2008\]](#)). So, Mauz and Antonioli should not lightly state that there is no record of sea level above the modern position after 114 ka because there are worldwide examples and references to such a sea level.

Finally, we are well aware of the difficulty of reconstructing a sea level curve in the Mediterranean, and this is why we have not attempted it. We do not infer the global sea level from our sites given in Fig. 7 of our paper (as Mauz and Antonioli suggest). This figure is taken without any modification from the

work by [Ginés et al. \(2001\)](#), based on phreatic speleothems in Mallorca Island (Balears). A more recent and complete sea level curve for Mallorca Island is given by some of the authors in [Tuccimei et al. \(2006\)](#). However, this curve agrees with our data for the same island and the same sector of the island, both in the number of highstands and in ages. This is why we decided to include it in our work.

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