Reply to Reviewer#1

I am sorry to say that the authors could not achieve agreement on how to reply to the review. The text below is therefore from me only. A separate comment from the co-authors may follow.

I thank the referee for their expert review and wish to reply as follows.

"*I do not think authors performed the review of last interglacial shorelines in the eastern Mediterranean, as they stated in title A large part of the eastern Mediterranean data is not included in the database (eg. Greece, Cyprus and much of the Turkish coast*)."

The title is about "MIS 5e sea-level proxies" which is not quite the same as "last interglacial shorelines". The active tectonic zone of Greece, Turkey, etc was included in the literature overview, hence the title "eastern Mediterranean" and later excluded from the database for reasons outlined in the introduction ("…*enables us to separate shoreline data generated to unravel tectonic processes from sea-level data generated to reconstruct the LIG sea level and the associated ice volumes, eustacy and related GIA processes"*). By separating the data I have been following two of the three aims of the WALIS project: first, address the uncertainties associated with the 6-10 m LIG highstand, and second, understand LIG sea-level oscillations. By cleaning the data from non-GIA components we followed the key objective of the project: analysing the database in the light of a large array of GIA models. See https://cordis.europa.eu/project/id/802414/reporting for details.

"*it is very important to have a clear picture of the whole basin. This is important to disentangle the different factors influencing the current elevation of LIG shorelines*."

I agree and this is exactly what the manuscript intended to deliver: differentiate between GIA and non-GIA affected coastal zones.

"*Authors focused a large portion of the text (section 1.1.1) on the active coastal zones. This part is floating in text because authors never approached the tectonic control on LIG shorelines in the remaining part of the manuscript*"

I am not sure if I fully understand this comment. What I can say is that the paper guideline provided by Alessio says about sec 1 (Literature overview): "*Give a brief overview of the historical development of MIS 5e sea level reports in the study area*." I feel this is what I did.

"*in Tunisia there is a large amount of literature in French that is never mentioned in text… authors cannot ignore this literature*"

A number of languages are spoken in the eastern Med (Greek, Turkish, Hebrew, Arabian…) and some papers on Quaternary geology are published in these languages. Also, a number of Europeans have been working in North Africa during the early to late 20th century and most of them published in their home language (French, Italian, German, Polish…). I am sorry to say that I am unable to read these languages and I believe this lack of ability applies to many colleagues. I cannot see why we should give preference to the French literature over, say, Turkish or Arabian papers. Because English is the scientific language, for the sake of reproducibility the review has to focus on literature published in English.

"If they decline to do this effort, they should change the title. Of course, the first option would be much important for the scientific community."

I appreciate the referee's opinion about what is important for the scientific community.

"in table 1 it is unclear what is considered sea-level index point and what is considered marine limiting point."

Thank you for highlighting this – the header of the table should say "RSL <u>proxies</u> reviewed in this study".

The nomenclature as well does not follow the international protocols (eg, Shennan et al., 2015). As an example, the coastal notch. The IR is MHW to MLW while the RWL (e.g., the midpoint of the IR) is (MHW to MLW)/2."

I am afraid, I do not find an IR definition for coastal notch in Shennan et al. 2015. In fact, we have used Antonioli et al 2015 as reference.

Also the other types of RSL indicators are not well explained. They should be standardized and a clear explanation of each indicator should be given. I know that, for high energy coast, the IR is dependent by the local hydrodynamics but I do think this issue was well addressed in the recent protocol provided by Rovere et al., 2016

I appreciate this comment – there is definitely a lack of standardisation in table 1. The referee recommends using local hydrodynamics as outlined in Rovere et al., 2016 where RWL of a beach deposit is half-way between wave breaking depth and berm. This is reasonable for the Holocene beach for which instrumental record is available. For earlier interglacials the berm (or similar landform) has to be reconstructed through sedimentary features and the breaking depth remains unknown as long as coastal topography is not perfectly reconstructed and wave climate is deduced from downscaled palaeo-climate models. For the purpose of the review and compilation of LIG sea-level data we need to find criteria beyond hydrodynamics.

it is unclear why you grouped in a single typology all the sedimentary facies. They are very different (eg., a lagoon is from a low energy environment while a carbonate sand…"

I do concur with the referee, single typology is indeed not sufficient to describe the IR of a coastal deposit. I will separate backshore, foreshore and shoreface deposits in the table and ascribe IRs to the deposits. For reasons outlined above the IRs will not be deduced from hydrodynamics but from bed forms and facies following descriptions in textbooks such as Reading (1986). Facies relationships are deduced from standardised principles of Walter's Law and Sequence stratigraphy as described by Nelson (2015) in the Handbook.

The section 2.3 is not exhaustive, because the reported IR (1 to 4 m for foreshore and 4-8 for shoreface) were only seldom applied on the data.

I am not sure if I understand this comment – I suspect that the reviewer is puzzled by some discrepancies between our facies description and the representation of the respective datapoint in the database. This is indeed a problem that we need to sort out together.

authors should better define these IR in the methods

A method section does not exist in the review paper. Table 1 summarises the IRs.

Why, for instance authors selected +1 to -3 m as IR for Ras Karboub? I am not saying that this is incorrect but I think authors should better define these IR in the methods. Similarly, why authors selected 1 to 3 m of depth for the El Max Abu Sir? This is also not explained in the methods. I accessed the database and this detailed description is available in the RSL indicator tab. I suggest to transfer this part also in the main text.

Table 1 summarises the IRs and list the references from which the IRs are deduced. I agree with the referee that there are discrepancies between our facies description and the RSL indicator description in the database. The problem is not solved by copying the database text into the review.

Why authors did not report the large number of U-series available in Jedoui et al., 2003 on QSR?

Jedoui et al dated the Jeffara coastal barrier using mollusc shells. This barrier is continuous from NW Libya to the Gulf of Gabes and then continuous in the Gulf of Hammamet up to Cap Bon. The barrier has been dated in several places by OSL technique which is, in this particular case, a better suited dating technique than U-series. Critical evaluation of analytical data (see U-activity ratio in Jedoui's Table 1) is a necessary consequence of known methodological problems (e.g., Kaufman et al. 1996; Geochem and Cosmochem Acta 60) and thus, why populating the database with data that do not survive state of the art screening?

Authors transformed them [Israeli dataset] in sea level indicator and not in marine limiting points. Authors should justify this choice because this is not in line with the methods of the manuscript and the guidelines of Walis.

See separate comment from co-authors.