12.1.2021

To:
The editors of the Special Issue: WALIS - the World Atlas of Last Interglacial Shorelines
Following the reviewers comments accepted for the manuscript titled “MIS 5e sea-level proxies in the eastern Mediterranean coastal region” by Barbara Mauz et al.
MS No.: essd-2020-357

We would like to thank the two reviewers for the detailed and thorough reviews they performed. We agree with almost all of their remarks, and we believe that following their recommendations will considerably improve the article.

From the very beginning we (all authors) decided to share the responsibility; Sivan and Galili- the data from Israel and Cyprus while the data from north Africa, Greece and Turkey- Dr Mauz.

During the writing process we respected the decisions of the first author, although in many cases we disagree with them. We wrote the needed chapters related to Israel and Cyprus as agreed.

Following the comments the reviews made, we clarified to Dr Mauz that most of the comments are reasonable and that following them will considerably improve our paper. In our response to the “letter to the editor” draft sent to us by Dr. Mauz we have been trying to respect the first author decisions on one hand, and to follow most of the important recommendations of the reviewers, on the other hand. While doing this, Dr. Mauz already published a note stating that there is disagreement between the co-authors on the corrections needed.

Therefore, below you will find our brief point-by-point answers to the reviewers’ comments. This is, to the best of our understanding, the way in which the reviewers’ recommendations should be carried out, and we call Dr Mauz to follow them. The most significant issues required are: adding and citing the available, missing works and data from the studied areas, mainly from Cyprus and the works done in North Africa.

We are looking forward to see the final version of the article after the corrections, before it is submitted to the journal.

Please see our detailed answers to the reviewer’s comments (the comments are in red while our answers are in black):

Comment on Review#1

“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).”
We agree. We insisted on adding at least the data from Cyprus, an Island that contains large amount of MIS 5.5 data, large part of it with high quality elevation measurements and good quality of dating. We insisted on adding this data but the corresponding author decided not to include it.

“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.”

We agree and this is exactly what the manuscript intended to deliver: to present all data from various present-day elevations. A database that will allow further research to study the various mechanisms involved.

“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”As mentioned above, we believe that all data has to be presented in such a review paper. The tectonic and/or GIA contribution can be discussed in short but in general this kind of paper has to present all data available and the uncertainties it contains.

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

We agree. The issue of using at least French literature was discussed few times but Dr. Mauz didn’t accept our approach. The south and east Mediterranean was historically divided between French speaking authorities like most of north Africa, Syria and Lebanon, while other areas like Palestine (now Israel) and Cyprus were under English speaking authority. In Turkey there is a lot of research published in German. A review paper has to include all kind of reported scientific data. Unfortunately, the corresponding author didn’t share with us the same understanding.

“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.”

We prefer to add more data but we also agree to the less preferable option of changing the title.

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

This recommendation was accepted and the table was revised.

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.”

The corresponding author answered that “we have used Antonioli et al 2015 as reference” and we accept it.

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

We accept the detailed answer of the corresponding author.

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…”

Here too we accept the detailed answer made by the corresponding author.

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

Please see the corresponding author response.

authors should better define these IR in the methods
Please see Table 1.

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.
Please see the answer by B. Mauz.

Why authors did not report the large number of U-series available in Jedoui et al., 2003 on QSR?
The sites mentioned in the areas that Dr. Mauz reported on and she is the expert in OSL and therefore we account on her response.

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.
The Israeli data is not well presented in the paper. We had a lot of disagreements with the corresponding author. This bad representation leads to misunderstandings: in the Galilee type section of Rosh Hanikra (Sivan et al., 2016) we have at least three different kinds of RSL indicators: we have the Strombus bearing unit, the Vermetids that is to our understanding an index point and we have the bioclastic sandstone. This description has to be be added in order to show which part of the sequence is marine limiting (the bioclastic sandstone) and what are the index point (the Strombus and the vermetides units).

Comment on Review#2

I do not understand why, and the authors should explain it, much of the coasts on the northern side where regional tectonics strongly affect the present position of the LIG shoreline (Greece, Turkey), are excluded from the database.
This question was also addressed by the first reviewer. We agree and as we already mentioned above we think that the active tectonic zone of Greece, Turkey, and the coasts of Cyprus has to be included.

"…designedly exclude those sites affected by non-GIA processes, and this would be a good idea in case they want to pursue a research task and specifically to compare their elevation data to GIA predictions to test model scenarios"
To our opinion all sites from all kinds of coasts have to be presented so they can be used for all kinds of studies checking for tectonic and/or GIA rates and more.

This is obviously not the case here because they do include some sites in regions clearly experiencing active deformation such as northern Tunisia, the Marmara Sea and the Carmel coast.
The Carmel coast is part of the whole Israeli coast that is **relatively stable** (Galili et al., 2007; Sivan et al., 2016 and references therein).

The fact that for a number of reasons, including low displacement rates or geometric characteristics of the active faults in the selected areas (most faults are strike-slip or thrusts), the LIG shoreline does appear close to the eustatic position - unlike what happens for instance in the Corinth Gulf - is not a justification and actually could lead to wrong estimation of GIA model parameters.

We have to present the data and later we can suggest mechanisms for various elevations. Only in a few sites GIA model predictions for the site at LIG were carried out. One of them is in Rosh Hanikra Israel (Sivan et al., 2016) where few models were checked. The results give an envelope but are not decisive.

In my opinion, overview papers such this, which is related to an Atlas, should encompass all available data and not just a selection of them. The Authors should include much more published data and discuss what processes control the elevation of LIG shoreline in different sectors to make this paper more appealing to the community.

Agree. See our answers above.

I acknowledge that terraces are found mostly in the tectonic unstable zones, which are left out of the database; but then, why do you quote it in the paper?

Cyprus has elevated terraces with well dated LIG deposits but unfortunately it was not included. It has to be in the paper.

I have a difficulty in following the adopted criteria [regards organization of description of zones].

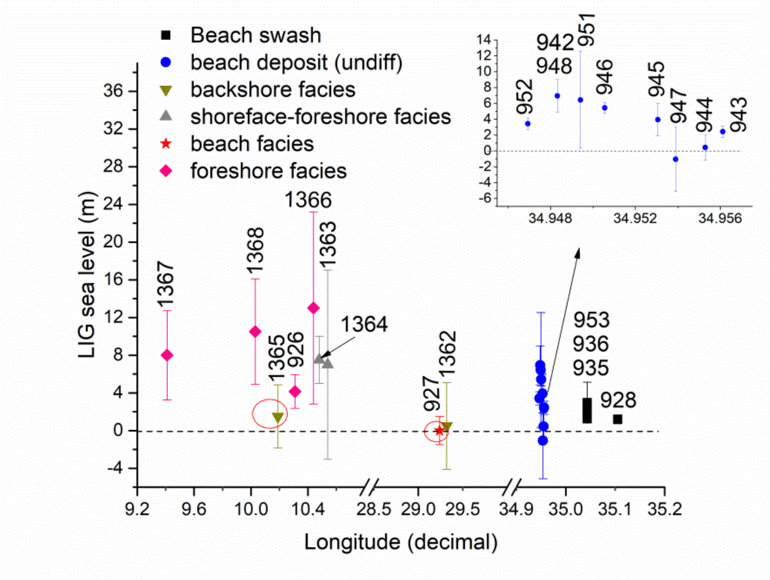
It has to be changes.

In section 4 (E Med RSL sites) you introduce zones different than the description in Literature Overview section

It has to be corrected.

Figure 6: It is quite difficult to relate this figure to the text and to the electronic database, in light of the lack of ID numbers in figure

It was improved by Dr. Mauz.



it is not easy to understand why error bars on same indicators (e. g. foreshore facies) are different. I presume they reflect the sum of uncertainties. A discussion on uncertainties is lacking in the paper (but this is a pitfall of the database as well, where the uncertainty estimation strategy is unclear).

The co-author used the calculations made by the online WALIS platform.

Specific comments and technical corrections

Strombus bubonius (LMK) is today identified as *Strombus (=Persististrombus) latus GMELIN* – what is today and what is the reference?

We accepted the old terminology based on Sivan et al., (2016) about the Galilee coast, Israel where it is written: "In the Mediterranean Sea, the key fossil indicator for MIS5e is the gastropod Strombus bubonius. It is now known as Persististrombus latus (Taviani, 2014), but we retain the old synonym for ease of comparison with referenced literature (e.g., Zazo et al., 2003, 2013 and references therein; Bardaji et al., 2009, Sivan et al., 1999)".

In Cyprus, Galili et al. (2015**)** wrote that previous names of this index fossil were Strombus bubonius (Gignoux 1913; Deperet 1918) and Lentigo latus, and, more recently, Kronenberg & Lee (2007) suggested the name Persististrombus latus. Since the term 'Strombus bubonius' appears to be very common in the literature, including articles quoted in here, we abbreviate its name as 'SB' throughout this work

The Ahihud fault separates the Rosh Hanikra platform from Haifa bay. Here and elsewhere, these local features are distracting the reader as long as they do not impact the position of the LIG shoreline, or they do it but they are not shown on a map

We didn't want to present these faults since they are misleading but the corresponding author did add them. The faults do not have any impact on the LIG RSL indicators. The paper deals with indications for sea levels not with coastlines. Therefore, the most important issue is the fact that the data from the Galilee and from the Carmel coast presents same LIG sea levels, in the frame of the uncertainties.

Line 137: How do you know it is LIG shoreline?

This question got an answer by Dr. Mauz.

Line 171: where the amplitude is around 70 cm. at line 140 m you state the tide amplitude is 1.5 m – Again, there is an answer by Dr. Mauz who was on charge of these sites.

Line 184: where did you take these depths from? Add a reference

Dr. Mauz already answered: the references are in table 1.

Line 240: 2.3 mm/a subsidence is a pretty high estimate...with this velocity the LIG shoreline should be 230 m below sea level. Please clarify.

Dr. Maus wrote that she'll make it clearer since the estimate is for the most recent period (2008-2014) of instrumental record

Line 321: local dynamic topography. How do you know is dynamic topography only and not unaccounted GIA effects or compaction or some local tectonics

Dr. Maus already answered that the text in line 321 summarises the results from Austermann et al.'s modelling work.

Line 326: Please specify time scale of fluctuations

In most cases there is no way to present time scale fluctuations. Even in Rosh Hanikra, Galilee coast, fluctuations were found based on field relations but with no way to date each one of them (Sivan et al., 2016).

Line 330: how much younger?

There is no way to know.

Line 331: Future research directions should be modified according to the suggests paper rearrangement.
Agree. It will be modified by the re-arrangement.

Sincerely,
Prof. Dorit Sivan   Dr. Ehud Galili

E. GALILI

Dorit Sivan.

Copy: Dr Barbara Mauz-corresponding author