

Comment on Reviewer#2

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 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

The active tectonic zone of Greece, Turkey, etc was included in the literature overview, 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, eustasy and related GIA processes”).

“...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”

My reading of the WALIS project text is exactly this – pursue a research task by comparing GIA models with the database. The text says: “we are working with earth modelers towards obtaining a large array of models to predict vertical land motions caused by glacial isostatic adjustment and dynamic topography. Analyzing the database in light of these models will allow us to give a more precise answer to the question: “how high was sea level in the Last Interglacial?” See <https://cordis.europa.eu/project/id/802414/reporting> for details.

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.

I share these sceptical thoughts. Some arguments convinced me to include the sites in the database: northern Tunisia: the terrain is thrust folded. Quaternary terrestrial and marine deposit do not show deformation and overlie deformed Pliocene strata unconformably in graben, synclines and depressions. The LIG deposits are part of cliff sections, marine terraces are absent. Some data (e.g. striated faults, flexure, seismic; Essid et al., 2016, Tectonophysics 682; Melki et al 2011, J Geodyn. 52) suggest ongoing compression (not uplift!) but whether LIG coastal deposits are affected is not clear.

Marmara Sea: I guess the referee means ID 927 which is described in sec 4.1 (Black Sea). The site was included because tectonic land movements are quantified (Avsar et al 2017) and because its elevation is within the 5 ± 8 m as expected for the LIG (see sec 5.1 for details).

Carmel coast: looking at published data (e.g. Gvirtzman et al 1997; Porat et al, 2003) a SSW-directed tilt of the shelf seems obvious. Mauz et al 2013 show that around 20 km south of the Walis ID942 site the dip of the LIG deposit changes from gentle (1-4 degree) to steep (~10 degree) suggesting that the northernmost part of the Carmel coast is unaffected by the tilt.

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.

I appreciate this comment, the reviewer makes a good point. If proxy data are included in the database just because the indicator is situated close to an inferred eustatic position, the GIA modeler could perhaps adjust the melting history accordingly. However, this type of approach is prone to circular conclusion and serious modelling would not proceed in this way. Nevertheless, we should try and facilitate modelling work by include critical evaluation of the proxy data in our data compilation work. This includes, for instance, mentioning faults, even when their activity is unknown for the period of interest.

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.

In the second paragraph of this review the referee asked us to explain the data selection, here however inclusion of “much more data” is requested. I am therefore feeling a little unsettled about this comment. In addition, “discussion of processes that control elevation” is requested. I understand that the ESSD journal does not envisage discussion of data. In fact, the paper template does not include a “Discussion of data” section. This is what Alessio emailed the authors in September 2020: *“Scope of the paper. After communications with the Chief Editor of ESSD, David Carlson, we established that every paper should be framed as the description of a data product. The data product is the regional database you are assembling through our system. ... The MS should contain short overviews of the sites inserted in the database, also detailing the choices you make in including / excluding sites, recalculating uncertainties, assessing quality.”*

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?

On Galilei coast LIG deposits are found on a “abrasion platform” (Sivan et al., 2016). For further details see comment of co-authors.

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

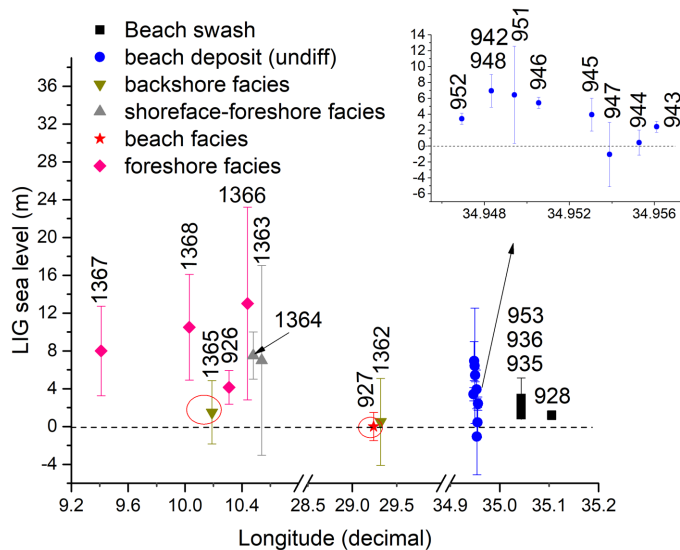
I understand, it is indeed not consistent. The zones were classified according to the dominant controlling factor. The Levant coast is definitely governed by the Nile cell and by the sediment load of the delta; the gulfs and embayments are best described as coastal lowlands because “Rift zone” (Gulf of Gabes, Gulf of Sirte) or “Graben” (Haifa Bay) would allude to tectonic processes which are not confirmed to be active in the late Quaternary. I wouldn’t know how to better classify the coastal zones other than not to classify at all.

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

Thank you for highlighting this inconsistency – it applies to Black Sea and north Tunisia. It will be ironed out.

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

Thanks for highlighting the weakness – The ID numbers are now added in the figure and the caption will be changed accordingly.



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).

Uncertainties are key to the WALIS aims and there is a lot to say about error estimation. As an author I can say that the error bars displayed in Fig. 6 are obtained from the Walis database software. The user has no influence on the calculation. The online platform does however outline the equation used for uncertainty calculation (simple quadratic square root rule) and using this simple equation I obtained identical uncertainty values up to one digit. And yes, the error bar represents the sum of all uncertainties. To assess the standardisation of indicators the IR error should be examined. The difference between IR-errors of the same indicator should reflect the tidal amplitude and if not, the standardisation is imperfect.

Specific comments and technical corrections

Thanks for these – much appreciated

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

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 – I believe the faults have to be mentioned because there is no LIG shoreline onshore in Haifa bay while the shoreline is evident adjacent to the bay at 1-2 m on Galilei coast and at 0-7 m on Carmel coast. Not mentioning the faults would look as

if the authors of the review haven't noticed the problem. For more details see comment of the co-authors.

Line 137: How do you know it is LIG shoreline? Giglia (1984) describes a "shallow marine-beach deposit", the stratigraphic context indicates "Tyrrhenian transgression". His map shows the distribution of the deposit in the coastal plain of the Sirte Gulf.

Line 171: where the amplitude is around 70 cm. at line 140 m you state the tide amplitude is 1.5 m – in line 171 the text is about tidal amplitude, in line 140 the text is about tidal range.

Line 184: where did you take these depths from? Add a reference – 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. – thanks for pointing this out. I shall make clear in the text that 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 – this comment is unclear to me. The text in line 321 summarises the results from Austermann et al.'s modelling work.

Line 326: Please specify time scale of fluctuations – the Walis project is about LIG and the Walis texts says: "Last Interglacial (here intended as MIS 5e, peaking 125 thousand years ago)".

Line 330: how much younger? I don't think this question relates to the manuscript under review. Please see the reference for details.

Line 331: Future research directions should be modified according to the suggests paper rearrangement. For this section Alessio's guideline says: "What is needed to improve the MIS 5e record in the area studied? Are there "hotspot sites"? I feel this is exactly what we did.