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Interactive comment on "MIS 5e sea-level proxies in the eastern Mediterranean coastal region" *by* Barbara Mauz et al.

Anonymous Referee #2

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General comment

The paper by Mauz et al. provides an overview of sites in the eastern Mediterranean coasts where evidence of indicators of the Last Interglacial shoreline were previously published. The compilation was taken in the frame of the WALIS project - the World Atlas of Last Interglacial Shorelines and follows its protocol as supporting databases show. To my knowledge a modern compilation of LIG data from the eastern Mediterranean is lacking so the effort is welcomed.

However, I found the compilation necessary but not exhaustive. 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 authors state in section 4 that they

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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 or look for minimal coastal displacements unaccounted for by these models. 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 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.

On the same reasoning, I found confusing that they organize the literature overview description by following geodynamic provinces and do a great effort in describing coastal zones affected by active tectonics and with reported evidence of LIG indicators. This part is not utilized in their compilation so there is an apparent discrepancy between text and supplemental material. 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 databases, I found some conflicting definitions and typing. For example, the sea-level indicator that in one DB is defined “Beach swash deposit” is in some cases undistinguishable in terms of lithofacies from what in the other DB is labelled as “Foreshore” or “Coastal Barrier”. In some entries, there is confusion between chronostratigraphy and lithofacies description (e. g. Indicator description of row 11 in Israel database). In general, the database tables are hard to follow because of the large number of entries. I think this is a WALIS template problem and not specific to this paper, and I understand that authors may have problem in homogenising. However, tables in the

text paper should be more concise and to the points, and I find tables 1 useful but too generic, and table 2 useless. Table 1 lists some indicators that are not found in the provided databases (e. g. marine terraces, Cladocora reef, Lithophaga holes). 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? The sediment facies indicator, which is the most largely used one, is rather condensed in text table. In the text there is a description of bathymetric corrections, but I could not understand from the xsl tables whether they were applied or not (I understand they were not).

A further weak point regards organization of description of zones. In section 1.1 (overview) it is stated that the description follows division of zones relative to tectonic structures, but I have a difficulty in following the adopted criteria:

1.1.1. Active tectonic zones is based on tectonics

1.1.2 Nile littoral cell and delta is based on oceanography and morphology.

1.1.3 African passive margin is based on tectonics, but the Nile is part of it.

1.1.4 Coastal lowlands is based on morphology...could be either passive or active margins. The ones you identify are part of the African passive margin

In section 4 (E Med RSL sites) you introduce zones different than the description in Literature Overview section. For instance, Black Sea rift and Alpine orogenic belt (which are active tectonic zones) 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 (checking it with the Longitude is impossible). Also, 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).

Specific comments and technical corrections

Line 21: Specify in which reference frame Africa moves. The African plate is moving

generally NE in a hotspot reference frame, and is moving from NE to NW moving from E to W along its margin relative to Europe.

Line 43: Strombus bubonius (LMK) is today identified as Strombus (=Persististrombus) latus GMELIN

Line 123: Mauz et al 2009 is not in reference list

Line 126: Black Sea is included in Coastal lowlands but it is not shown like this in Fig. 1

Line 132: 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.

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

Line 139: Gharbi et al is not in reference list

Line 142: LIG deposit is part of a beach ridge stretching parallel to the modern coastline at ca 3 m altitude. Add reference.

Line 171: where the amplitude is around 70 cm. at line 140 m you state the tide amplitude is 1.5 m

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

Line 210: Bosellini et al is not in reference list

Line 225: In the Isreael database there are 5 more indicators for a total of 26, not 21

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

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

Line 326: Please specify time scale of fluctuations.

Line 330: how much younger?

Line 331: Future research directions should be modified according to the suggests paper rearrangement.

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