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Interactive comment on "A standardized database of MIS 5e sea-level proxies in southern Africa (Angola, Namibia and South Africa)" *by* J. Andrew G. Cooper and Andrew N. Green

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This is an important addition to the LIG sea-level databasing efforts and therefore a valuable dataset and publication. However, I have significant questions about the reference water levels and indicative meanings given in Table 1, and the conclusions drawn from the data. Therefore this paper requires major revision. My comments below largely follow the order of presentation in the manuscript.

Do you only include those locations with absolute chronological control? It appears from the introduction that there are some useful sites which do not have absolute dating, but would still be useful markers e.g. could have a 1 or 2* age control value given.

The vast majority of data points have 4 and 5* age control (which is great), but including other sites without direct age control (but, for example, relative chronostratigraphic controls) allow you to fill some spatial gaps, for example, Namibia as you discuss in 5.4? By focusing only on those absolute dating locations it feels restricted in scope, and therefore its use. If there is a reason for this, it needs stating.

Lagoons are often brackish (as you disused in section 3). However, in the abstract, you state they are given marine limiting status (as per the abstract)? On inspection of the 'WALIS' data file I see all those RSL indicators listed as lagoon as tagged as a sea-level indicator. Therefore lagoon should be removed as a marine limiting point from the abstract (or this needs clarification that you are using lagoon in the same sense as defined by in WALIS).

Line 17 – ating spelling mistake

Line 45 – urther spelling mistake

Line 62 – replace with 'previous dating. . ..'

Line 69 – Capital for Last Interglacial as per other parts of the manuscript. Needs checking for consistency throughout (e.g. lines 78, 87, 90. . .).

Line 67 – is it important these are 'only stone tools' or that that they are 'stone tools only of the Sangaon culture'?

Lines 72-76 – This needs to be clearer for readers not experts in local fauna. What are the indicative taxa of the LIG in the region which allows macrofossils to be used to ascribe chronology? As per my point above, can these locations be included in the database if there is an absence of absolute chronology?

In the background section e.g. line 80 onwards, elevations are given in m, but no datum is ascribed. Is this relative to MSL (and if so where/what is MSL) or a local reference datum? This query applies throughout; though I see this is considered for section 5, which is good. However, for some elevations in section 5 amsl is stated and for others just m is given. It would be useful to be very clear (maybe at the start of section 5) which are known with respect to a datum, and which are assumed relative to MSL.

Line 103 – formatting issue

Lines 175-179 – advice from the Editor (A. Rovere) suggest the paper should not include discussion of processes. Though it provides justification for a reappraisal of previous work, presentation of the age/altitude/indicative meaning of the RSL data is the purpose of this publication. This part reads as a RSL review rather than a database publication. Suggest this is removed.

Section 3 – needs the addition of references to other publications which ascribe indicative meanings (and reference water levels) of indicators such as beachrock.

The reference water level descriptions in table 1 are very vague and not as per the definition of a reference water level, which is one value and often the midpoint of the indicative range (see Shennan et al 2015 Sea Level Handbook, Rovere et al 2016 and WALIS guidance notes). Furthermore, which element of low tide e.g. MLWN, MLW, LAT? (See Woodroffe chapter (11) in Handbook of Sea Level Research (Shennan et al.) for guidance). The indicative range which is the vertical range over which the indicator's modern analogue exits (for example MHWS to MTL) (see again Sea Level Handbook) also is not correct as presented in Table 1. In the 'RSL indicators' tab in the WALIS file this information is correct (for both the reference water level, and the indicative meaning) as taken from the standardised WALIS framework – this is the information which should be included in table 1. I believe that the indicator references column in table 1 and the spreadsheet should also replicate each other. Table 1 needs significant updating to reflect the supplementary database file (and cross-checking that the indicative ranges given in the WALIS framework apply to the indicators included here).

A specific concern in table 1: Beach swash zone given relative water level is given as 'low tide to ?10 m' (see note above that this should be one value, not a range – this

is the indicative range). What is the ? – plus/minus? Likely minus given it is subtidal, but therefore this should be a limiting data point as the lower range of the indicative meaning is unknown (as presented here currently). The only beach swash datapoint in the presented 'WALIS' file is #417, but this is given as a sea level indicator; but based on the description in the table, I do not see how. Given that you suggest this indicator could extend to -10 m (?) below sea level (and stating later in the paper that the tidal range typically in this region is 2 m) are you confident this is a beach swash indicator (as defined in WALIS), or should it simply be a marine limiting data point?

Line 254-260 – remove discussion of 1 or 2 peaks in this section. This should focus on the data points to constrain RSL, not a discussion about the number/type of sea-level highstands which forms data interpretation.

Line 326 and Figure 4 – References in figure caption are missing from the reference list. If the black line is sea level from Grant et al. (2014) Nature Comms 5 (5076) this is RSL in the Red Sea. This makes no sense to plot this against data from southern Africa as solid earth processes etc would mean this RSL should be different from southern Africa when not corrected for these processes (which is outside the scope of this paper). It is also not clear what has been plotted in the grey as this would be compilation of modelled GMSL (e.g. Kopp et al 2009) v data-based Red Sea RSL (Rohling et al. 2009) v modelled West Australia RSL (O'Leary et al. 2013) (if my understanding is correct given lack of references) which confuses global and local sea level (these are not global eustatic curves as stated in line 326), and there is no explanation how this is produced. This figure should be removed.

Given the above re Figure 4, the opening of section 6.1 needs revision.

Line 336 – you make reference to a data point from Bateman et al (2004) which is not presented above or in the database files. There is no context of where this data point is from. Given the data point is from South Africa, it is not clear why it is not presented in the database.

Lines 344-345 and Figure 5 – I see no basis, based on the plot in figure 5, for defining two sea-level highstands. There is simply a wide spread of data between -5 and +10 m over the duration of the LIG. One data point at -5.5 m (which I understand from the database is likely #396, foreshore deposits with $\sim$17 m vertical range) cannot be used as the basis for a mid-interglacial lowstand and therefore 2 sea-level peaks, especially given the large geographic spread of the data as shown in Figure 1 (the South Africa data appears to cover $\sim$20 degrees of longitude), and when not corrected for spatially variable solid earth process (which is beyond the scope of this work).

Figure 5 – This is appropriate. However, have you plotted the elevation of the indicator (as the y-axis label suggests) or the RSL (m)? It would be useful to show similar plots for the other data presented, by region (in a sea level sense, not geopolitically).

Figure 6 and lines 348-350 – What is the unpublished data by Green as given in the figure caption. This appears critical to the conclusion drawn here and makes it difficult for the reader to evaluate the implications (in this case a double-peaked highstand). In the lack of full dating control it is not possible to argue for two sea-level peaks in MIS 5e - it would be worth referring to Mauz, et al. "No evidence from the eastern Mediterranean for a MIS 5e double peak sea-level highstand." Quaternary Research 89.2 (2018): 505-510. who revisit and date similar sections in the Mediterranean and show that the previously considered double MIS 5e sea-level peak is actually from two interstadials (MIS 5e and 5a). In the absence of direct chronological controls at the sites presented in Figure 6, this is a viable alternative hypothesis.

Section 6.5 is useful, but some of these points need to clear earlier (e.g. level of assumptions) so the data/graphs etc can be evaluated as presented, with this background knowledge.

Lines 410-412 – I see no compelling evidence for two sea-level highstands (see various comments above) and this should be removed, and revisions made to the wider conclusions

Lines 434-438 - This comes from nowhere and has no context with the rest of the paper. Delete.

Figure 1 – longitudinal labels need checking

Database - There are two files held at the online directory. It appears much of the '5e shoreline data' file repeats the 'WALIS' datafile, but not everything. This is really confusing and took me time to work out. Only one supporting database file should be presented.

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