Response to reviewers ESSD

Reviewers' comments are in black and our response is in red.

Review: Hayley Cawthra

This is an important review and the historic overview of sea-level studies in South Africa is very well documented, but the last 10 years of this have several relevant studies that have been missed, and as such, a major revision is necessary. Some additional references to check, that contain ages and information on Last Interglacial deposits: [if Mozambique will be considered in the review?] Armitage, S.J., Botha, G.A., Duller, G.A.T., Wintle, A.G., Rebêlo, L.P. and Momade, F.J., 2006. The formation and evolution of the barrier islands of Inhaca and Bazaruto, Mozambique. Geomorphology, 82(3-4), 295-308.

Mozambique is covered in another contribution in the Special Issue

Roberts, D.L., Bateman, M.D., Murray-Wallace, C.V., Carr, A.S. and Holmes, P.J., 2009. West coast dune plumes: climate driven contrasts in dunefield morphogenesis along the western and southern South African coasts. Palaeogeography, Palaeoclimatology, Palaeoecology, 271(1-2), 24-38.

This has now been cited in the context of determining the age of deposits at Swartklip, but it does not contain any further information pertinent to this paper in terms of sea-level indicators

Fisher, E.C., Albert, R.M., Botha, G., Cawthra, H.C., Esteban, I., Harris, J., Jacobs, Z., Jerardino, A., Marean, C.W., Neumann, F.H. and Pargeter, J., 2013. Archaeological reconnaissance for middle stone age sites along the Pondoland Coast, South Africa. PaleoAnthropology, 104-137. [a review paper on dunes that includes a database as a supplement and may be helpful]

Thank you- this has now been included and cited.

Roberts, D., Cawthra, H. and Musekiwa, C., 2014. Dynamics of late Cenozoic aeolian deposition along the South African coast: a record of evolving climate and ecosystems. Geological Society, London, Special Publications 388(1), 353-387.

This has been added.

Morrissey, P., Knight, J. and Stratford, D.J., 2020. Early Marine Isotope Stage 5 sea levels, coastal dune palaeoenvironments, and human occupation on the southeast coast of South Africa. Quaternary Science Reviews 245, 106504.

This came out while the paper was under review. Its pertinent contents (an alternative interpretation of the Nahoon section) have now been incorporated

On 'future research directions', Swartklip and Nahoon are tagged as 'priority' sites but these have already been recently re-addressed. They are published in Roberts et al. 2009 (the paper on West Coast dune plumes) and Morrissey's 2019 Masters project that was published in 2020.

In response to new unpublished information on Swartklip communicated to us by John Pether, we have removed that site from consideration (it is much older). However, the recent reinterpretation of Nahoon (Morrissey et al., 2020) provides an alternative to that presented by Roberts. Both have been incorporated into a figure showing key sites for further investigation.

I found the figures to be of fairly poor quality and some of them (e.g., from Jacobs and Roberts, 2009) are not sufficiently modified from their original form.

The figures have been redrawn and modified

Runds (2017) is missing a reference in the reference list so I could not compare this to its source.
It is in the reference list

On coverage and completeness: was Mozambique deliberately left out of Southern Africa? There has been work done there on the Last Interglacial which I would think appropriate to include here.
This has been addressed with recognition that Mozambique is not part of the study

Not all studies described in the text are given equal consideration, and I felt that the dated sites that are presented in the database did not receive as much attention in the main text as undated sites with inferred or relative ages. This seems a bit odd and I think better cohesion between the two parts of this work is needed.

There are essentially two parts to the data presented in this paper. In the first, the history of sea-level investigations and the nature of sea-level indicators is reviewed in the "Literature overview". This necessarily is weighted toward the earlier works that often do not contain chronological information, but it does describe the context and nature of subsequent studies that do contain dated evidence. The second part (the database description) is in contrast, focussed fully on the chronological information whose context has also been covered in the review section.

My overall feeling is that this paper should not be published in its current form as it is missing too many relevant and recent studies. A lot of literature has been overlooked, and my impression is that if the authors were not aware of all research going on in other parts of Southern Africa, they could have approached a more diverse team of people working in this region to work with them in order to tell a fuller story.

This has been addressed with recognition that Mozambique is not part of the study

Specific comments/suggested edits:
Abstract
Line 7: Marine Isotopic Stage (MIS) Done
Line 17: dating Done
Line 23: expand LIG on first use – but I noted that throughout the paper, Last Interglacial is used, so perhaps just write it in full in the abstract We have adopted the Last Interglacial in full notation.

Line 24: expand relative sea level on first use I suggest stating in the abstract that there is a focus on cemented beach deposits and beachrock, more than on dunes. Although dunes are not good indicators of former sea level, much of the geochronological work has been carried out on dunes and these were included in the database. This is a little confusing to track between the manuscript and the paper database.
We have reworded the abstract to reflect the focus on sea-level indicators, but also note that some dune dates are included where they provide useful limiting data.

1. Introduction
Line 49: sea-level indicators Done

2. Literature review
Line 55: this citation list can include the recently published Hearty et al. (2020) paper: Hearty, P.J., Rovere, A., Sandstrom, M.R., O'Leary, M.J., Roberts, D. and Raymo, M.E., 2020. Pliocene-Pleistocene stratigraphy and sea-level estimates, Republic of South Africa with implications for a 400 ppmv CO2 world. Paleoceanography and Paleoclimatology 35(7).
Inserted

Line 109: Roberts et al (2009) later dated that sequence, and although the focus was mainly on aeolianites, the logged section does include beach deposits.

This has been inserted in a reappraisal of the importance of the Swartklip sequence.

Line 113: see also Morrissey et al. (2020) to supplement Nahoon interpretations.

This paper came out during the review process and has now been inserted- it is also discussed in the "Future Research Directions" section.

*although dune-related, Roberts et al. (2013) provided an overview and database of aeolianites that includes MIS 5e. Even if not relevant here where dunes as standalone geomorphic features are not being considered, there are ages in that database that are relevant to stacked MIS 5e sequences considered in this WALIS review that may be helpful.

This citation has been added under "Other interglacials"

**in an MIS 11-focused paper, Roberts et al. (2012) document a MIS 5e sequence that abuts the older deposits at Danabaai [Roberts, D.L., Karkanas, P., Jacobs, Z., Marean, C.W. and Roberts, R.G., 2012. Melting ice sheets 400,000 yr ago raised sea level by 13 m: Past analogue for future trends. Earth and Planetary Science Letters 357, 226-237.]

Thank you for drawing the above paper to our attention. We have consequently included three additional points in the database and included details from this paper in the review.

Line 143: a summary of more dates from Mossel Bay (onshore and offshore) is provided in figure 3 of Cawthra et al. (2020): Cawthra, H.C., Frenzel, P., Hahn, A., Compton, J.S., Gander, L. and Zabel, M., 2020. Seismic stratigraphy of the inner to mid Agulhas bank, South Africa. Quaternary Science Reviews 235, 105979.

We have added a reference to this text

Line 185: For neotectonics in Namaqualand, see de Beer (2012): De Beer, C.H., 2012. Evidence of Neogene to Quaternary faulting and seismogenic deformation along the Namaqualand coast, South Africa. South African Journal of Geology 115(2), 117-136.

Reference to this article has been added

3. Sea-level indicators
Line 205: Roberts et al. (2012) and Cawthra et al. (2018) also use the contact between upper shoreface and foreshore deposits as a geologic indicator of former sea level, approximating Mean Sea Level. This differs slightly to the facies themselves, and in the South African Cape this is a relatively well-preserved indicator of MIS 5e sequences.

This sea-level indicator has been added to the paper and to the WALIS database

4. Elevation measurements In Table 2, 'echo sounder' should specifically be a 'multibeam echosounder'. This provides decimetre accuracy and the resolution allows for clear identification of contacts. The same is not possible using a singlebeam echosounder.

This has been added in Table 2 to the single beam echosounders that were used in earlier studies. We have also included in "elevation measurements" and an additional sentence has also been added to highlight the measurements undertaken by Roberts et al. (2012) as a model for future investigations.

5.1 Western Cape
Line 270 and entire paragraph: please link this chronology to the 2009 publication that provides chronology of that sequence.

This has been done

5.2 Eastern Cape
Line 280 and entire paragraph: please link to Morrissey et al. (2020) If dunes and palaeosols are included, please refer to Fisher et al. (2013) which provides a chronology of 'red dunes' at Msikaba.

We have added reference to Morrissey et al.

5.3 KwaZulu-Natal

The MIS 5e highstand is also documented geologically in Cawthra (2012): Cawthra, H.C., Uken, R. and Ovechkina, M.N., 2012. New insights into the geological evolution of the Durban Bluff and adjacent Blood Reef, South Africa. South African Journal of Geology 115(3), 291-308. but this was not dated.

Congratulations on assimilating the information that is published up to now and I look forward to seeing this paper and database published pending revision. The entire WALIS resource will be highly beneficial to Pleistocene researchers. From responses to my broad comments, I see that Mozambique is not part of this specific review, but it too, has MIS 5e deposits and sequences worth reporting in the paper of relevance to that region

Natasha Barlow:

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.
We have focussed in the database on those sites that have absolute dating control. We have reviewed the other sites and drawn attention to their potential in the paper, but given the controversies that have arisen in the absence of chronological control in the past (and still persist), the database has been limited to the sites where chronological control is available.

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.

Agreed, the inclusion of other sites would expand the database, but, (a) there are relatively few sites that have indirect age indicators and even then, with the presence of Holocene highstands and earlier Pleistocene highstands, certainty regarding dating is compromised without some absolute measure, and (b) the chronologically constrained datapoints are actually quite numerous.

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 sealevel 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).

Thanks for pointing this out- requisite edits have been undertaken in the abstract.

Line 17 – ating spelling mistake Done
Line 45 – urther spelling mistake Done
Line 62 – replace with 'previous dating. . ..' Done
Line 69 – Capital for Last Interglacial as per other parts of the manuscript. Needs checking for consistency throughout (e.g. lines 78, 87, 90. . .). This has been changed throughout
Line 67 – is it important these are 'only stone tools' or that that they are 'stone tools only of the Sangaon culture'? the latter- sentence modified accordingly
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?
Our wording has evidently caused some misunderstanding here. The macrofossils establish the age of Pliocene and Early Pleistocene deposits - they therefore exclude an MIS5 age. We have reworded the sentence for clarity.

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

We have inserted a sentence to this effect at the end of para 1 in Section 2. "unless otherwise stated, all elevations are expressed relative to MSL". In section 5 we have standardized the notation and inserted "Elevations cited in the following text are stated in relation to MSL unless there is explicit information to relate them to another datum"

Line 103 – formatting issue corrected

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.

Agreed, but (a) it is only 4 lines and (b) the information is critical to future understanding of Angolan sea-level data both now and in the future.

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.

Thank you for this comment. We have added the references mentioned. We have removed the repetition in this section and expanded the description of each indicator adding references from Table 1. This much improves the section. The text now correlates with Table 1 and the database.

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).
This has been done.

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?

To address this we have recompiled Table 1 taking the information directly from the WALIS database. In the text we augment the standard descriptions from the database with reference to region-specific information. There is indeed just one "swash" indicator and it does not have an associated absolute date. The several "foreshore" indicators we regard as less precise than swash zone as they may also include surf zone facies.

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.

We are here describing the stratigraphic context for the RSL indicators. We have reworded it to ensure it reads as description rather than 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.

On Fig 4. was not our intention to plot the Red Sea data against the southern African data, nor to infer any potential relationship between the two. We simply add the Red Sea curve to provide temporal context for the southern African data (without it, the data simply "float" on the graph). The caption has been updated to this effect. Additional information (indicated by the grey shading) has been removed and the caption updated. Similarly, the beginning of Section 6.1 has been amended and reference to a eustatic curve has been removed.

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.

This is in the database (RSL_361). We cited the wrong source- (it is Carr et al., 2010). This has been amended.

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 ~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 ~20 degrees of longitude), and when not corrected for spatially variable solid earth process (which is beyond the scope of this work).

We have removed discussion of regional evidence for sea-level variability during MIS5e

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).

The Y-axis refers to RSL. Thank you for pointing out the ambiguity. We have adjusted the caption accordingly. There are no suitable data to prepare similar plots for Namibia and Angola and the scarcity of data make regional subdivision untenable at this stage.

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.
Thank you, we have inserted words to this effect in Section 6.1.

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
Agreed- we have done this in editing section 6.1 and have removed the offending text in lines 410-412.

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

Figure 1 – longitudinal labels need checking Done

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.
The database and online directory files have been updated.