

Review of *ESSD*-manuscript “Last Interglacial sea-level data points from Northwest Europe” (Cohen et al.)

Cohen et al. synthesize sea-level data from the last interglacial (MIS 5e) in NW Europe by means of the standardized and publicly available WALIS database. Despite the long history of relevant research in this region, to the best of my knowledge this is the first systematic compilation of LIG sea-level data from NW Europe. As the authors point out, the ice-marginal setting of the study area makes it an interesting complement to the more established LIG datasets from far-field regions. Clearly, the authors have made an effort to capture the full literature on this subject, stretching back into the 1800s, which I appreciate. However, the presentation needs a lot of work. My review focuses mainly on conceptual issues and less on regional specifics (Sections 2 and 5). I also am not an expert on last interglacial chronology (Section 6), which is an important issue given the paucity of numerical ages and the critical role of stratigraphic correlation/interpretation.

My main concerns revolve around the following issues: (1) an apparent *a priori* interpretation of the RSL history; (2) terminology, definitions, and illustrations; and (3) the length of the manuscript.

[1] The authors quite deliberately work toward a transgression/highstand/regression interpretation of LIG RSL change that culminates in Section 5. I have several concerns here. First off, and fundamentally, these terms and their sequence-stratigraphic connotation (lines 764-765) concern shoreline change as a function of RSL change **AND** sediment supply. In other words, the link with RSL is not necessarily straightforward. My larger concern is that such an interpretation reeks of preconceived notions, i.e., that one single RSL cycle (i.e., rise, highstand, fall) occurred during the LIG. As they are well aware (e.g., Barlow et al., 2018, *NG*; somewhat curiously not cited in this manuscript), alternative views with multiple m-scale LIG RSL cycles have been advanced in the literature (e.g., O’Leary et al., 2013). I am not saying that I necessarily buy into these alternative views and/or that the authors are incorrect. However, using this assumption at the outset flies in the face of the goal of a sea-level database: to synthesize data as objectively as possible and to leave the door open for testing competing hypotheses. This is especially true for a dataset like the present one, with relatively large chronological and vertical uncertainties. On a somewhat related note, in lines 1238-1239 the authors refer to MIS 5 as a whole as “falling stage.” This is a gross oversimplification, given the major subsequent highstands (MIS 5c and 5a) as documented from many localities worldwide (and note the contradiction with lines 1250-1251). In conclusion, I think there are multiple reasons to back away from the transgression/highstand/regression model as presently used.

[2] Section 3 features a discussion of sea-level indicators, including a few that are newly introduced. This is where accurate and consistent terminology, along with illustration, is critical. So why not include a cartoon with a schematic cross section and sedimentary logs that illustrate the various sea-level indicators, their stratigraphic position, and their

interpretation (SLIPs versus limiting data)? Quite frankly, I would consider that more important than some of the panels in Figs. 2 and 3.

On key terminology, is Relative Water Level something different than the widely used Reference Water Level? Also, with respect to the RWL and IR calculations it would be helpful to clarify why MSL is used, as opposed to the commonly used MTL (e.g., Shennan, 1986). I partly bring these things up because I was struck by the fact that the indicative range for most of the sea-level indicators is the same, which raises the question to what extent they are mutually exclusive.

With regard to basal peat, it is fine to initially interpret this as a GWL indicator, but (1) it is unclear what the depth range information in line 497 is based on; and (2) by not including the next step (i.e., how basal peats can become SLIPs) it isn't clear how these could be anything more than limiting data. The main element that is missing here is paleoenvironmental information (e.g., biological, geochemical) that is necessary to determine whether basal peat is intertidal. From this perspective, it is unfortunate that biological information ("marine fauna") is divorced from the other sea-level indicators. Absent this type of evidence, an interpretation where the initial stage of basal peat formation occurred in a fresh environment (lines 489-491) becomes rather speculative. Separate from this, note that basal peat is commonly defined as resting immediately on a largely compaction-free substrate, so other than thickness loss of the sampled interval, there would be no vertical displacement. For tops of peat beds this is of course different but referring to those as basal peats (lines 671-672) is bound to lead to confusion.

I was unfamiliar with the concept of estuarine terraces (Section 3.2.4). Elsewhere (line 672) I came across the term "estuarine tidal flat tops" and the spreadsheet mentions "preserved tidal flat surface." What is needed here, is a clear explanation of what this would constitute in a modern tidal/estuarine environment. Were these tidal flats? And if so, how exactly is it defined? Note that it is not uncommon to restrict tidal flats to mudflats that reside between MTL and MLW (or MLLW), i.e., excluding marsh platforms (e.g., Fagherazzi et al., 2006, *PNAS*). The formulas indicate otherwise, however, placing this category in the same elevation range as coastal marshes... so what is the difference? As mentioned before, a cartoon would be of great help here – it would enable the authors to more effectively communicate the relationships between the different sea-level indicators. As mentioned, the different categories must be mutually exclusive, but are they? Along similar lines, is there a difference between marine terraces and abrasion platforms (lines 1032 and 1034)?

With regard to elevation (Table 5 and associated text), cross sections are not a measurement technique, merely a means to illustrate data. What matters here is how elevations as shown in a cross section were determined. Also, what does "±10% of elevation measurement" mean? For example, if an elevation of 10 cm above MTL is obtained, the error would be 1 cm, but if it is 1 m, the error would be 10 cm? I doubt that this is what the authors meant. A larger question is whether elevations are related to modern MTL or simply to a geodetic datum. If the former, how do the authors account for the fact that MSL is subject to fairly rapid change, often of opposite sign as in Britain

(lines 642-645)? Finally, vertical uncertainties of only ± 1 m (line 1243) for the LIG seem unrealistically low. That would place them among the best RSL data even within much of the Holocene. I find it difficult to see how adding >100 kyr would not inevitably result in larger errors. Am I missing something?

[3] Portions of the text (notably Sections 2 and 5) tend to get lost in lengthy regional stratigraphic details, with associated nomenclature that many if not most readers will be unfamiliar with. For example, the authors repeatedly mention the Lusitanian (a chronostratigraphic unit?) that I couldn't find in Table 1. Lincolnshire is also mentioned multiple times but not shown on a map. Note that these are merely examples. Trimming down this section would make the paper much more accessible. Would it be possible to move most of the regionally specific text to a supplement?

I came across tons of grammatical errors. The nature of the work is such that accurate communication is essential. I know that this is more about bookkeeping than imagination (and thus not particularly exciting) but failing to do so might cause more confusion than resolution. Put differently, other authors should be able to reproduce the work. In its current form, the writing is quite rough, and at many occasions difficult to follow. For a small selection of examples, see lines 147, 213, 300-301, 334, 398-399, 435, 625, 727-728, and 1091. Without a doubt, an effort by all authors can address this, not least the native English speakers on the team.

Some more specific comments regarding the text:

Line 55: I suggest adding Shennan (1982, *Proc Geol Assoc*) here. Also, if the journal guidelines allow it, I would recommend listing references chronologically.

Line 80: there is a widespread preference for “numerical ages” over “absolute ages” (see Colman et al., 1987, *QR*).

Line 99: not sure what “superregional” means.

Line 104: you probably mean “paleoceanographic” here.

Line 115: why not just stick to “sea-level data points” as in the title?

Line 125: better to say “unlike the present interglacial.”

Line 143: I suppose this must be cm?

Lines 194-195: not clear what a “sea-level site” is.

Line 290: you define *Cyprina* Clay here, but it has already been mentioned earlier (line 239) without a definition.

Line 308: here and elsewhere (wherever appropriate) I suggest you use “succession” rather than “sequence” unless it explicitly refers to sequence stratigraphy.

Line 313: please try to be more precise with terminology. Note that littoral environments occur in lakes as well, and lacustrine is the more commonly used term rather than limnic. Why not just referring to this as lacustrine versus marine (or shallow marine)?

Line 447: here and elsewhere, the authors use the term “generic” (or “non-generic”) but it isn't particularly clear what this means.

Line 451: “subaquatic” should be “subaqueous.”

Lines 461-465: what is said here about tidal amplification/dissipation matters a great deal for sea-level reconstruction and needs to be supported by one or more references.

Line 481: the definition that is provided here is debatable; basal peats typically overlie the transgressive surface that represents the upper boundary of the lowstand (or falling stage) systems tract (e.g., Zaitlin et al., 1994, *SEPM Spec Pub*).

Lines 485-487: the paleosol interpretation is tenuous, especially in the absence of references. The evidence I'm familiar with (e.g., Vetter et al., 2017, *G3*) shows that paleosols formed immediately prior to basal peat accumulation. Or are the authors referring to entirely different paleosols? Please clarify.

Line 571: the RWL should be defined more precisely; I presume it is this range divided by two?

Line 597: "footer" should be "footnote."

Line 620: is the next section really about the English Channel? It certainly doesn't appear to be about chronostratigraphy?

Lines 634-636: are you sure "subtract" is correct here? Shouldn't this number be added to bring an elevation from low tide level to mean tide level?

Lines 665-671: Keogh et al. (2021, *JGR-ES*) discuss the time-dependent nature of compaction of organic-rich coastal deposits, including its implications for sea-level reconstruction.

Line 678: is the nature and thickness of overburden included in the database?

Line 696: note that VLM strictly includes compaction, but that is clearly not how the authors define things in this work. This needs to be clarified somewhere.

Lines 701-703: I have trouble following this sentence; what does "earlier applied basin subsidence" mean?

Lines 731-733: using uncertainties is good, but it raises the question why no uncertainties are used for areas outside of the North Sea Basin where VLM appears to be less well understood (also see my earlier comment about vertical uncertainties). This must be addressed.

Line 1202: expressions like "5* LIG SLIPs" are not really appropriate for a paper like this one. If there is a ranking of data point quality, this should be clarified.

Lines 1231-1233: the authors seemingly intend to make a significant point here, but I had trouble understanding what that point is. Are they highlighting the differences or the similarities between WALIS and Holocene RSL databases?

Line 1281: this link took me to a southern African database. I suppose something else was intended here?

The quality of the figures needs work; for example, the lower portions of Figs. 5 and 6 are very difficult to read. A few more specific suggestions regarding figures and tables:

Fig. 1: the differentiation between small and large rivers is not meaningful (I can't tell the difference in the map). The depocenter VLM information from Fig. 4 shouldn't be repeated here. Instead, make sure that all geographic names mentioned in the text can be found in the maps.

Fig. 2 is not of the greatest quality and is every panel referred to in the text?

Fig. 3: please show the location of these areas in Fig. 1.

Table 1: Last Interglacial covers all of MIS 5, which seems inconsistent with the LIG definition used throughout the paper (MIS 5e or Eemian; also see line 100).

Table 2 needs a lot of attention; it contains a ton of text, and numerous acronyms that are not explained (note that it should be possible to read the table content without having to consult the main text). It would probably help to simplify things a bit. For example, could a finite list of geomorphic/stratigraphic contexts be provided and show which ones occur in a given geographic region? Maybe the table is simply too large, and this aspect should be separated from the numbers of data points in each geographic region.

Table 3: within this table there is a header that says “Paper Author added as part of this study.” I had to read this several times, but I’m still not sure what it means. Also, please specify which other WALIS Special Issue papers are being referenced here.

Table 4: the title is awkward; please reword. And what exactly is meant with “Duration constraint”? Is this the maximum duration? I looked at the various numbers in the table but couldn’t make sense of this.

Torbjörn Törnqvist