Earth Syst. Sci. Data Discuss., author comment AC2
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Reply on EC1

Woo Hun Ryang et al.

Author comment on "Last interglacial sea-level proxies in the Korean Peninsula" by Woo Hun Ryang et al., Earth Syst. Sci. Data Discuss., https://doi.org/10.5194/essd-2021-205-AC2, 2021

We cordially appreciate the comments by Drs. Colin V. Murray-Wallace and Dan Muhs. They meticulously reviewed our manuscript and provided valuable and helpful comments. The followings are in response to their comments in sequence. The red font indicates newly added or corrected sentences in the revised manuscript.

Commented [MDR1]: The Kopp et al. study contains a complex mix of tectonically active and stable coasts, dated and undated coasts, etc. The first real recognition of the higher-than-present LIG sea level was H.H. Veeh, in his 1966 JGR paper (and actually, not much has changed since that groundbreaking paper).

Reply [1]: Thanks for your comments, we cited Veeh's (1966) JGR paper.

Commented [MDR2]: What do you mean by "compaction" here? Sediment compaction?

Reply [2]: It is rewritten to 'sediment compaction'.

Commented [MDR3]: Are these marine sands and gravels? Terrestrial? Both?

Reply [3]: It is rewritten to '~ unconsolidated marine and aeolian sands and well-rounded beach gravels ~'.

Commented [MDR4]: Very nice map!

Reply [4]: Thank you for your comments. It was modified after Chough et al. (2000; 2004) and Cummings et al. (2016).

Commented [MDR5]: Also a very nice map, but I suggest that here, as in later figures, use WHITE for land (as you did in Figure 1) and use BLUE (whether one shade or more) for the ocean.

Reply [5]: We redrew Figure 2 referring to the drawing style in Figure 1.

Commented [MDR6]: No need for the second sentence (legend is understood). Can you state what the contour interval is? Also, if you make the terrace shades partially transparent, readers can see the contour lines.

Reply [6]: We deleted the second sentence and stated the contour interval of 10 m in the captions of Figures 3, 4, and 5. Unfortunately, we could not make the terrace shades partially transparent because the figure was covered by the terrace shades in the original paper.

Commented [MDR7]: Please give the contour interval on this map.

Reply [7]: We stated the contour interval of 10 m in the figure caption.

Commented [MDR8]: I do not see the "palaeo-beach pebble gravel" unit in either section. For the NQ t1 terrace, what is the yellow unit?

Reply [8]: We found that marks of the paleo-beach pebble gravel were covered by the green and blue shades in the columnar sections when it was modified. We corrected it. For the NQt_1 terrace, a sentence was added in the figure caption as follows: Sites YH09 and YH10 in the NQt_1 terrace (yellow) were dated to 0.09±0.01 ka and 0.11±0.01 ka by OSL, respectively (J.H. Choi et al., 2009).

Commented [MDR9]: say either "aeolian sand" [deposits] or "dunes" [landforms], but not both. Same comment for the next sentence.

Reply [9]: The word 'dunes' was deleted in the two sentences.

Commented [MDR10]: How about showing a photograph of these terraces?

Reply [10]: Unfortunately, the original paper of J.H. Choi (2009) in Quaternary International does not show any photograph of these terraces.

Commented [MDR11]: A major concern here (and this applies to equations (1) and (2), which you discuss later. As you show here, the "Paleo Seacliff Onlap Elevation" is indeed a maximum-limiting elevation for the shoreline angle elevation, complemented by the minimum-limiting platform elevation. However, I have never seen a situation in the field that looks like this. What you have shown here is what appears to be a marine deposit of ONE age draped over wave-cut platforms of TWO ages, including the paleo-sea cliff itself. In 40 years of studying marine terraces on three continents and many islands, I have never encountered such a situation. Now, it is very common for NON-marine deposits to be draped over two or more platforms and the intervening paleo-sea cliff, but not this. What is usually found, however, is a paleo-sea cliff back a platform, and the maximum-limiting elevation is the lowest exposed bedrock elevation on that paleo-sea cliff. That measurement, plus the elevation of the lower wave-cut platform will indeed bracket the elevation of the shoreline angle if it is not exposed.

Reply [11]: Thank you so much for your kind and detailed comments. Your comments on the overlying deposits are right. We had an error in drawing the schematic diagram, so Figure 6 was redrawn adding draped aeolian sands and nonmarine deposits over the marine deposits and paleo wave-cut platform.

In our study area, unfortunately, the paleo-sea cliff back a platform was rarely exposed due to a gradual topographic slope. However, seismic profiles on land showed a subsurface paleo-sea cliff, not exposed on the surface (Figure 5; J.W. Kim et al., 2007a). Thus, we had no choice to use the lowest exposed bedrock elevation on the paleo-sea cliff but should use the paleo seacliff onlap elevation (PSOE) in Figure 6 and equations 1 and 2.

Commented [MDR12]: The same comment as before: make the land white and the ocean blue.

Reply [12]: We redrew Figure 7 referring to the drawing style in Figure 1.

Commented [MDR13]: Here you need to state that the tops of the fluvial terrace fills have elevations that correspond to (i.e., can be traced to) the shoreline angles of marine terraces nearer to the coast. Otherwise, there is no confidence that the fluvial terraces are eustatically controlled.

Reply [13]: Although S.G. Choi (1993; 1996) insisted that the fluvial terrace fills with LIG ages correspond to the shoreline angles of nearby marine terraces, he did not present any scientific evidence of the correspondence in his papers. So, we include his contribution only as historically published data. This work presented the first two numerical LIG ages for coastal areas of the Korean Peninsula, but the ages were not included in our dataset of Table 3 and the open-access spreadsheet (Ryang and Simms, 2021).

Commented [MDR14]: Please don't use the phrase "age dating", which is redundant.

Reply [14]: We corrected the phrase.

Commented [MDR15]: Here, and throughout the manuscript, please delete all occurrences of the phrase "absolute age" and instead please use the term "numerical age", which is preferred by geochronologists.

Reply [15]: We changed the phrase ‘absolute age’ to ‘numerical age’ throughout the revised manuscript.

Commented [MDR16]: Are there no marine fossils in the marine terrace deposits of the Korean Peninsula anywhere? Is there no possibility of amino acid geochronology? If that is the case, you need to state this.

Reply [16]: Unfortunately, we are not aware of any geochronological studies of the marine fossils in the marine terrace deposits of the Korean Peninsula.

Commented [MDR17]: For equations (1) and (2), please see the comment made in the caption for Figure 6. This is a MAJOR issue.

Reply [17]: You can refer to Reply [11] for Figure 6.

Commented [MDR18]: For the age uncertainties, are these ONE sigma or TWO sigma? This needs to be stated explicitly.

Reply [18]: We inserted ‘error range’ of ± 1σ SE or ± 2σ SE into the last column of age uncertainty of Table 3, and the corresponding error range was written in each cell of the age uncertainty.

Commented [MDR19]: I believe you said all this earlier.

Reply [19]: We wrote this already as the first sentence in the 2.2 Overview section. This sentence is restated for the 4.1 and 4.2 sections.

Commented [MDR20]: Again, are these ONE sigma or TWO sigma errors? You need to be clear about this before going into all the site-specific details of the ages.

Reply [20]: We inserted ‘error range’ of ± 1σ SE or ± 2σ SE into the last column of age uncertainty of Table 3, and the corresponding error range was written in each cell of the age uncertainty. We also inserted the following sentence at the end of 3.3.1 Optically stimulated luminescence subsection: the error range had ± 1σ SE or ± 2σ SE according to

measurement methods.

Commented [MDR21]: Here, and in the previous paragraphs, why do you say this? Obviously, there has been uplift…some readers are going to be confused by this discussion and assume you really think that sea level during the LIG was actually this high.

Reply [21]: We rewrote these sentences in terms of the SA elevation in subsites of Gangneung-Anin, Donghae-Eadal-dong, Pohang-Yonghan-2, Pohang-Masan-ri, and Gyeongju-Jinri. Table 2 and Figure 14 were also revised.

Commented [MDR22]: What sort of shells? Marine? Is this the only place they are found?

Reply [22]: Unfortunately, the few shell fragments are too small in both size and quantity to identify in the core.

Commented [MDR23]: Any geochronology on these shells? Any indications of paleozoogeography that could be useful checks on the OSL ages?

Reply [23]: Unfortunately, no paleontological descriptions and studies are using the shell beds in the original paper.

Commented [MDR24]: "swaley" is not a word

Reply [24]: The phrase 'faint swaley cross-bedding' was rewritten to 'swaley cross strata'. From page 643 of AGI's 4th edition of the Glossary of Geology: "swaley cross strata: cross strata resembling hummocky cross strata but dominated by convave-up strata deposited in swales instead of by convex-up strata deposited on hummocks."

Commented [MDR25]: What species of oysters? Any useful paleozoogeographic information?

Reply [25]: Unfortunately, no paleontological descriptions and studies are using the oysters in the original paper.

Commented [MDR26]: Why not give some specific examples and some possible uplift rate calculations? For example, in the Subsite Yonghan-2 site of the Pohang area, it looks like the NQt3 terrace has a shoreline angle elevation of ~35 m above sea level and dates to MIS 5e. Using a suite of possible LIG paleo-sea levels (+3 m, +6 m, +9 m, etc.), you could present a range of possible uplift rates. Is NQt1 Holocene? If so, is that a storm deposit or a coseismically uplifted terrace?

Reply [26]: At the end of the 5.1 Uplift section, we wrote a paragraph for the uplift rate contents adding Eq. (12) and Table 4. For the NQt_1 terrace, a sentence was added in Figure 4 caption as follows: YH09 and YH10 sampling sites in the yellow NQt_1 terrace were dated to 0.09±0.01 ka and 0.11±0.01 ka by OSL, respectively (J.H. Choi et al., 2009).

Commented [MDR27]: These are not really "relative sea levels," they are present elevations of marine terraces.

Reply [27]: To address this comment, we revised Table 2 and Figure 14 in terms of the SA elevations. We also rewrote the related sentences in subsites of Gangneung-Anin, Donghae-Eadal-dong, Pohang-Yonghan-2, Pohang-Masan-ri, and Gyeongju-Jinri.

Commented [MDR28]: I am puzzled here. The entire paper is on the subject of LIG sea level and your summary of it all is two sentences? Why not say something about the

range of ages you have and compare them to U-series-dated and OSL-dated marine deposits elsewhere?

Reply [28]: The section title of LIG sea-level fluctuation means fine-scale sea-level fluctuations during LIG, which shares the common section title in many papers of the WALIS special issue. We think our data in the Korean Peninsula are too coarse to constrain fine-scale fluctuations in LIG sea levels.

Commented [MDR29]: You still haven't said whether these errors are one sigma or two sigma, so this discussion is not going to be easy to interpret, I'm afraid.

Reply [29]: We inserted 'error range' of ± 1σ SE or ± 2σ SE into the last column of age uncertainty of Table 3, and the corresponding error range was written in each cell of the age uncertainty.

Commented [MDR30]: Rephrase, removing "distorted" and "contamination", please.

Reply [30]: We removed them in the revised manuscript.

Commented [MDR31]: What do you mean by "coined"?

Reply [31]: The word was rewritten to 'initiated'.

Please also note the supplement to this comment:
https://essd.copernicus.org/preprints/essd-2021-205/essd-2021-205-AC2-supplement.pdf