nature portfolio

Peer Review File

High interstadial sea levels over the past 420ka from the Huon Peninsula, Papua New Guinea

Web links to the author's journal account have been redacted from the decision letters as indicated to maintain confidentiality.

5th Jan 22

Dear Dr De Gelder,

Please allow me to apologise for the long delay in sending a decision on your manuscript titled "High interstadial sea levels over the past 420ka from Huon terraces (Papua New Guinea)". It has now been seen by 3 reviewers, whose comments are appended below. You will see that they find your work of some potential interest. However, they have raised quite substantial concerns that must be addressed. In light of these comments, we cannot accept the manuscript for publication, but would be interested in considering a revised version that fully addresses these serious concerns.

In addition to the responses to reviewer comments we ask that you meet the following editorial thresholds.

* Present a thorough re-analysis of sea-level variation over the past 420ka at the Huon Peninsula which includes a synthesis and comparison with palaeotemperature, CO2 and ice sheets histories from other studies.

* Fully justify each of your assumptions in your reanalysis of the Huon Peninsular rate of uplift and provide a clear indication of where your work advances the field beyond previous studies highlighted by reviewer 3

* Provide a thorough comparison of your findings to the many field studies with detailed terrace elevations from previous publications.

We hope you will find the reviewers' comments useful as you decide how to proceed. Should additional work allow you to address these criticisms, we would be happy to look at a substantially revised manuscript. If you choose to take up this option, please either highlight all changes in the manuscript text file, or provide a list of the changes to the manuscript with your responses to the reviewers.

Please bear in mind that we will be reluctant to approach the reviewers again in the absence of substantial revisions.

If the revision process takes significantly longer than three months, we will be happy to reconsider your paper at a later date, as long as nothing similar has been accepted for publication at Communications Earth & Environment or published elsewhere in the meantime.

We understand that due to the current global situation, the time required for revision may be longer than usual. We would appreciate it if you could keep us informed about an estimated timescale for resubmission, to facilitate our planning. Of course, if you are unable to estimate, we are happy to accommodate necessary extensions nevertheless.

We are committed to providing a fair and constructive peer-review process. Please do not hesitate to contact us if you wish to discuss the revision in more detail.

Please use the following link to submit your revised manuscript, point-by-point response to the reviewers' comments with a list of your changes to the manuscript text (which should be in a separate document to any cover letter) and any completed checklist:

[link redacted]

** This url links to your confidential home page and associated information about manuscripts you may have submitted or be reviewing for us. If you wish to forward this email to co-authors, please delete the link to your homepage first **

Please do not hesitate to contact me if you have any questions or would like to discuss the required revisions further. Thank you for the opportunity to review your work.

Best regards,

Adam Switzer, PhD
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Joe Aslin
Senior Editor
Communications Earth & Environment

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REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

Dear Editor,
I have now finished my assessment of the MS entitled "High interstadial sea levels over the past 420ka from Huon terraces (Papua New Guinea)" by De Gelder et al.
The paper re-analyses a classic pleistocene reef sequence in Papua New Guinea using previously

dated corals and high-resolution Digital Elevation Models. In my opinon, the authors do an excellent job in correcting for differential tectonic uplift across the coast,and adequately discusses the challenges of such correction (e.g, considering uplift constant through time). They make a smart use of reef stratigraphic models to cross-validate the terrace elevations observed in the study area. To me, this paper should be published. I have only one request for clarification and one major comment to offer the authors. I think that addressing them might help improving the paper.
1) I could not understand if, to model the reef terrace formation, you corrected Spratt and Lisiecky for GIA. If you didn't, I think you should. Can you please explain?
2) I think that you should add, as a final figure, a Dutton-like one (2015, science), where you sum up the numbers you get for each MIS, comparison with other studies (if any), and, if available, some considerations on paleotemperatures, Co2 and ice sheets. I think this will require some work, but it will surely widen the potential audience of the paper. Maybe this figure could be accompanied by one or two (short) paragraphs to widen the paleoclimate implications of your work.
I really have no other observations to make: the paper is clear, figures are nicely drawn and the study design make this paper interesting and meaningful.

Reviewer #2 (Remarks to the Author):

High interstadial sea levels over the past 420 ka from Huon terraces (Papua New Guinea) - Gino de Gelder et al.

This paper offers a new analysis of the Huon coral reef terraces in Papua New Guinea using high-resolution topographic data and applying novel geometric approaches to better understand terrace deformation pattern. This paper posits the following new interpretations for the Huon terraces: a) general northward tectonic tilt, as opposed to the previously reported northwestward tilt; b) recognized 31 Late Pleistocene terraces compared to the ~20 terraces previously documented; c) estimated relative sea level (RSL) corrected for glacial isostatic adjustment (GIA); and d) illustrated the Huon terrace sequence using numerical models (coral reef modeling). With this, the authors suggest that δ18O-derived global mean sea-level curves systematically underestimate sea level during interstadial periods, by up to ~20m, and this discrepancy is either an effect of incorrect oxygen isotope curve calibrations or δ18O does not fully capture short-lived sea-level variations.

This paper is overall well written and clearly structured. It demonstrates a novel approach on analyzing high-resolution topographic data to examine deformation patterns of reef terraces and derive RSL estimates. I think the paper did a good job in explaining the geometric analysis done and coupling it with coral reef modeling. The paper also presents the first RSL data for the ~420-125 ka period, improves elevation and age estimates for the ~125-0 ka period, and provides the first RSL estimates for interstadial and lowstands in Huon peninsula. With the new derived RSL data, the authors are able to provide an explanation about the discrepancy between the interstadial sea level in Huon and in other sites. With all this, I believe that this paper falls within the scope of the journal and will make a good contribution to Quaternary RSL studies. As it also offers insights for future work, this paper will become a useful reference for researchers working on sea level especially in tectonically active regions.

I have one main comment though and this has something to do with the use of shoreline angle in the analysis of the Huon coral reef terraces. The shoreline angle, situated at the junction between a terrace flat and its corresponding paleo-seacliff landwards, is designated by the authors as the morphological approximation of paleo-RSL. I did not find on the paper an explanation of the relationship of the shoreline angle with respect to sea level. As a reader, I would appreciate a brief illustration of how the shoreline angle forms on a coral reef terrace and how the shoreline angle represents paleo-sea level (in this case, in Huon peninsula). I think this is important since the new Huon RSL estimates were derived directly from terrace geometry—and thus from shoreline angle delineation. Notwithstanding this suggestion, I believe that this paper deserves publication with only some minor comments for the authors' consideration.

Minor comments:

Line 26: change to "relative sea level (RSL)"

Line 38: change to "global mean sea level (GMSL)"

Line 60: change to "we used"

Line 86: change to "we used"

Line 87: change to "we used"

Line 91: To better refer to the figures, I would suggest to make Figure 3 into Figure 2 since the description for Figure 3 (on Line 91) went first than for Figure 2 (on Line 96).

Line 99: N359E – Is this bearing? This is also not shown on the figure caption.

Line 104: change to "Inferred relative sea levels"

Line 123: change to "we updated"

Line 127: change to "other sites, which show"

Lines 142-146: Please rephrase; awkward wording

Line 150: change to "lower sea levels"…. "higher sea levels"

Line 180-181: where is this (clusters of ages around ~115 ka and ~ka) on the models (Figure 4)?

Line 208: change to "sea level"

Line 598: For, the MIS peaks which we have multiple RSL…

Reviewer #3 (Remarks to the Author):

<B>de Gelder et al., "High interstadial sea levels over the past 420ka from Huon terraces (Papua New Guinea)"</B>

<B>Summary</B>: de Gelder et al., present a reassessment of the tectonic deformation of the Huon

Peninsula, and a reinterpretation of the sea-level record based on fossil terrace records. The preserved terrace records from the Huon Peninsula (Papua New Guinea, PNG) provide an extraordinary record of changing sea levels, however, untangling the uplift and sea-level change is difficult due to the lack of independent estimates of uplift rates for the location. De Gelder et al, reassess the uplift rate by fitting a surface to the elevations of shoreline angle (i.e., the sea cliff base), derived from a digital surface elevation model. This showed a N rather the NW orientation of tilt. Based on this, and a reinterpreted uplift rate, past sea level was reconstructed and compared to the stack of Spratt and Lisiecki (2016). The authors then use the differences between these to suggest problems with isotopic methods of reconstructing past sea levels.

This has the beginnings of a very nice paper but in its current form, submission is a little premature, and the conclusions are not well supported, nor particularly novel. For example, the claim (lines 56, 63 etc.) that this the first record of sea level beyond ~130 ka is overstated. There is a wealth of data that extend beyond 130 ka (Andersen et al., 2008; Bard et al., 1996; de Boer et al., 2014; Dumitru et al., 2019; Edwards et al., 1997; Elderfield et al., 2012; Gallup et al., 1994; Grant et al., 2019, 2014; Kennedy et al., 2012; Rohling et al., 2014, 1998; Siddall et al., 2006; Sosdian and Rosenthal, 2009; Yokoyama et al., 2007; Zazo et al., 2007). Ok, many of these are fragmentary/sparse corals records (but not all!). The value of the Huon Peninsular is the rapid uplift, making it one of the few sites where there is such a complete record of sea-level highstands. de Gelder et al., provide a valuable, and much needed revaluation, but this is not the first time that sea levels for low stands, or periods older than LIG have been presented. Similarly, the conclusions that the isotopic methods have their deficiencies is not new either; (Adkins et al., 2002; Chappell and Shackleton, 1986; Schrag et al., 1996; Shackleton, 1987; Skinner and Shackleton, 2005; Spratt and Lisiecki, 2016; Waelbroeck et al., 2002) nor that change in uplift rate will affect the sea levels reconstructed for terraces prior and subsequent to the LIG (see Creveling et al 2015).

The analysis depends on a couple of assumptions: (i) that we know what sea level was during the Last Interglacial (LIG) (this underpins the uplift rate calculation) (line 76), and; (ii) the fidelity of sea-level records. The first is not well supported; there is still considerable debate within the community as to the maximum magnitude, timing and the geometry of melt during the LIG sea-level highstand (e.g., Barlow et al., 2018; Dendy et al., 2017; Dutton et al., 2015; Dyer et al., 2021; Hearty et al., 2007; Long et al., 2015; Rohling et al., 2019, 2017), these will all have an impact on the recalculated rate of uplift, (already discussed in e.g., Creveling et al., 2015; Düsterhus et al., 2016a, 2016b; Hibbert et al., 2016; Yokoyama and Purcell, 2021). The second is not acknowledged in the manuscript, but does have ramifications for the conclusions (i.e., the offsets between the fossil terraces, and the open ocean oxygen isotope stack, Spratt and Lisiecki, 2016, SL16). There is no perfect record of past sea-level change; fossil terraces 'need' favourable conditions for formation (i.e., periods of relatively stable sea level) and preservation etc. (Camoin and Webster, 2015; Murray-Wallace and Woodroffe, 2014; Woodroffe and Webster, 2014; Yokoyama and Purcell, 2021), and open ocean δ18O is a mixed record (sea, level, temperature, local overprinting etc., as acknowledged by the authors). The way the SL16 stack was constructed, aligning all records to the LR04 stack (Lisiecki and Raymo, 2005) ignores the potential for significant timing differences in the benthic δ18O signal between the different ocean basins (Skinner and Shackleton, 2005; Waelbroeck et al., 2002), and the careful work by authors on age constraints (tuning to LR04 results in average age uncertainties of ~4 ka in this time interval). Both of these could have quite an impact on the magnitude of the reconstructed sea levels if the age models are not correctly aligned. The records used in the SL16 stack are first order sea-level estimates derived from by subtracting estimates of the temperature component on the δ¹⁸O (δ¹⁸Omineral =

δ¹⁸Oseawater + ΔTseawater, and where δ¹⁸Oseawater reflects both global ice volumes and local influences). The relative contribution of global ice volumes and temperature to foraminiferal oxygen isotopes is complex and subject to substantial uncertainties. Notwithstanding, the new sea level record overlaps within uncertainties of SL16 (bar MIS 6b and the MIS3/2transition), and so I'm not sure you can draw any firm conclusions, given the uncertainties of the records.

The new terrace analysis is great – the level of detail is impressive and hints at new, and potentially short-lived, features within the Huon terraces. Sadly, these weren't 'ground truth-ed' (see other comments, below). The Huon Peninsula 'flight' of terraces, and rapid uplift, mean that this site has the potential to unravel past sea levels, if the uplift rate can be constrained. Currently, your estimates are a minor advance, as they are still subject to various assumptions about LIG sea levels (cf., Creveling et al., 2015) and are not independently verified. You have a germ of a very nice paper, but I'm unconvinced that your results are novel, or well supported by the data, and I wasn't convinced it advances our understanding of the system. In summary, this is a nice idea but it is a little premature. As such I would not recommend publication in Nature Communications, Earth and Environment.

<B>Other comments</B>:
PNG has a very complex tectonic setting (several microplates and deforming zones). Is the rate of uplift the same along the coast, given the rapid relative deformation of the Huon Peninsula (i.e., rotational component of movement of the South Bismarck Plate; e.g., Wallace et al., 2004)? How does the orientation obtained by fitting your surface to the shoreline angles compare to estimates from geodetic monitoring network e.g., Tregoning, 2002; Tregoning et al., 1999). Is the uplift rate derived from fitting of the surface the same for the different transects (i.e., Sialum, Kanazaura, Bobongara), and how do these compare to previous estimates (Chappell et al., 1996a, 1996b)? From your DSM analysis, are the terraces flat? Could you use this probe the deformation further?

The recalculation of the uplift rate doesn't really advance our understanding. Your estimates are still subject to the same assumptions; you're still using the elevation of the LIG terrace to tell you about the uplift rates BUT we don't know what this was, not any consensus on the timing or melt geometries of the peak etc.

Ground-truthing: Unfortunately, you weren't able to compare your analysis to the previously recognised features. I would have expected some attempt to compare your elevations with the detailed terrace elevations (including the elevations of sea cliffs) from previous publications. For example, Chappell et al. (2016) contains some very detailed maps of the terraces (their figs 11&12). Perhaps something for the supplementary information? Could you use you uplift rates to 'predict' the elevations of other features? This would give greater confidence in your recalculated rate. I'm not sure why I would use your new uplift rates compared to that derived by previous authors.

Why only use the MIS5e terrace elevations? how is your fit affected using other terraces (e.g., Holocene T1 where there are several publications with detailed age and elevation information)?

Your uplift rate spans a large range, in part stemming from the choice of max. LIG sea levels for the Huon Peninsula (7 ± 5 m) – why not use the relative sea-level prediction from your GIA modelling of

LIG highstand at the Huon Peninsula?

Is uplift constant over the whole ~420 ka record (line 210)? Previous work suggests that this is the case for the last 130 ka (Chappell et al., 1996b; Ota et al., 1993) when averaged over long timescales (1000 years). An alternative explanation could be episodic uplift events (i.e., coseismic uplift; Ota and Chappell, 1996)? Similarly, the terraces may also not capture short-term fluctuations in sea level (Camoin and Webster, 2015), or may not have chance to form if rapid sea-level change (e.g., Murray-Wallace & Webster, 2014).

GIA modelling: Choice sea-level curve for ice hist in GIA modelling… what is the influence of using alternative ice histories (e.g., based on de Boer, Sosdian and Rosenthal, or Elderfield et al 2012). Also, assumption of LGM-like for the older glacials could result in an underestimate of several meters for the following interglacial sea level highstands (Dendy et al., 2017; Rohling et al., 2017). Could you ‘test’ the ‘new’ RSL curve you have generated using GIA predictions of the sea level curve at Huon?

Line 137 to 142: “3d, shows a remarkable first-order similarity in highstand ages and sea-level elevations” – you’re not comparing like with like here (relative sea level and global mean sea level). The original ages and elevations of the fossil corals also ‘fit’ equally well (your figure 3).

Line 141: please use the references for the original studies, rather than the Dutton et al., 2015 synthesis.

References:

→ Adkins, J.F., et al., 2002. The Salinity , Temperature , and δ18O of the Glacial Deep Ocean. Science 298, 1769–1773.

Andersen, M.B., et al., 2008. High-precision U-series measurements of more than 500,000 year old fossil corals. Earth Planet. Sci. Lett. 265, 229–245.

→ Bard, E., et al., 1996. Pleistocene sea levels and tectonic uplift based on dating of corals from Sumba Island, Indonesia. Geophys. Res. Lett. 23, 1473–1476.

→ Barlow, N.L.M., et al., 2018. Lack of evidence for a substantial sea-level fluctuation within the Last Interglacial. Nat. Geosci. 11, 627–634.

→ Camoin, G.F., Webster, J.M., 2015. Coral reef response to Quaternary sea-level and environmental changes: State of the science. Sedimentology 62, 401–428.

→ Chappell, J., et al., 1996a. Reconciliaion of Late Quaternary sea levels derived from coral terraces at Huon Peninsula with deep sea oxygen isotope records. Earth Planet. Sci. Lett. 141, 227–236.

→ Chappell, J., Ota, Y., Berryman, K., 1996b. Late Quaternary coseismic uplift history of Huon Peninsula, Papua New Guinea. Quat. Sci. Rev. 15, 7–22.

→ Chappell, J., Shackleton, N.J., 1986. Oxygen isotopes and sea level. Nature 324, 137–140. https://doi.org/10.1038/324137a0

→ Creveling, J.R., et al., 2015. Revisiting tectonic corrections applied to Pleistocene sea-level highstands. Quat. Sci. Rev. 111, 72–80.

→ de Boer, B., Stocchi, P., van de Wal, R.S.W., 2014. A fully coupled 3-D ice-sheet–sea-level model: algorithm and applications. Geosci. Model Dev. 7, 2141–2156.

→ Dendy, S., et al., 2017. Sensitivity of Last Interglacial sea-level high stands to ice sheet configuration during Marine Isotope Stage 6. Quat. Sci. Rev. 171, 234–244.

→ Dumitru, O.A., et al., 2019. Constraints on global mean sea level during Pliocene warmth. Nature

574, 233–236.
→ Düsterhus, A., et al., 2016a. Palaeo-sea-level and palaeo-ice-sheet databases: problems, strategies, and perspectives. Clim. Past 12, 911–921.
→ Düsterhus, A., Tamisiea, M.E., Jevrejeva, S., 2016b. Estimating the sea level highstand during the last interglacial: a probabilistic massive ensemble approach. Geophys. J. Int. 206, 900–920.
→ Dutton, A., et al., 2015. Tropical tales of polar ice: evidence of Last Interglacial polar ice sheet retreat recorded by fossil reefs of the granitic Seychelles islands. Quat. Sci. Rev. 107, 182–196.
→ Dyer, B., et al., 2021. Sea-level trends across The Bahamas constrain peak last interglacial ice melt. Proc. Natl. Acad. Sci. 118, e2026839118.
→ Edwards, R.L., et al., 1997. Protactinium-231 dating of carbonates by thermal ionization mass\rspectrometry: Implications for Quaternary climate change. Science. 276, 782–786.
→ Elderfield, H., et al., 2012. Evolution of Ocean Temperature and Ice Volume Through the Mid-Pleistocene Climate Transition. Science. 337, 704–709.
→ Gallup, C.D., Edwards, R.L., Johnson, R.G., 1994. The Timing of High Sea Levels Over the Past 200,000 Years. Science. 263, 796–800.
→ Grant, G.R., et al., 2019. The amplitude and origin of sea-level variability during the Pliocene epoch. Nature 574, 237–241.
→ Grant, K.M., et al., 2014. Sea-level variability over five glacial cycles. Nat. Commun. 5, 5076.
→ Hearty, P.J., et al., 2007. Global sea-level fluctuations during the Last Interglaciation (MIS 5e). Quat. Sci. Rev. 26, 2090–2112.
→ Hibbert, F.D., et al., 2016. Coral indicators of past sea-level change: A global repository of U-series dated benchmarks. Quat. Sci. Rev. 145, 1–56.
→ Kennedy, D.M., et al., 2012. Shore platform development on an uplifting limestone island over multiple sea-level cycles, Niue, South Pacific. Geomorphology 141–142, 170–182.
→ Lisiecki, L.E., Raymo, M.E., 2005. A Pliocene-Pleistocene stack of 57 globally distributed benthic δ18O records. Paleoceanography 20, PA1003.
→ Long, A.J., et al., 2015. Near-field sea-level variability in northwest Europe and ice sheet stability during the last interglacial. Quat. Sci. Rev. 126, 26–40.
→ Murray-Wallace, C. V., Woodroffe, C.D., 2014. Quaternary Sea-Level Changes. Cambridge University Press, Cambridge.
→ Ota, Y., Chappell, J., 1996. Late Quaternary coseismic uplift events on the Huon Peninsula, Papua New Guinea, deduced from coral terrace data. J. Geophys. Res. Solid Earth 101, 6071–6082.
→ Ota, Y., et al., 1993. Holocene Coral Reef Terraces and Coseismic Uplift of Huon Peninsula, Papua New Guinea. Quat. Res. 40, 177–188.
→ Rohling, E.J., et al., 1998. Magnitudes of sea-level lowstands of the past 500,000 years. Nature 394, 162–165.
→ Rohling, E.J., et al., 2014. Sea-level and deep-sea-temperature variability over the past 5.3 million years. Nature 508, 477-82.
→ Rohling, E.J., et al., 2019. Asynchronous Antarctic and Greenland ice-volume contributions to the last interglacial sea-level highstand. Nat. Commun. 10, 5040.
→ Rohling, E.J., et al., 2017. Differences between the last two glacial maxima and implications for ice-sheet, δ18O, and sea-level reconstructions. Quat. Sci. Rev. 176, 1–28
→ Schrag, D.P., Hampt, G., Murray, D.W., 1996. Pore Fluid Constraints on the Temperature and Oxygen Isotopic Composition of the Glacial Ocean. Science. 272, 1930–1932.
→ Shackleton, N.J., 1987. Oxygen isotopes, ice volume and sea level. Quat. Sci. Rev. 6, 183–190
→ Siddall, M., et al., 2006. Sea-level reversal during termination II. Geology 34, 817–820.
→ Skinner, L.C., Shackleton, N.J., 2005. An Atlantic lead over Pacific deep-water change across Termination I: Implications for the application of the marine isotope stage stratigraphy. Quat. Sci.

Rev. 24, 571–580.
→ Sosdian, S.M., Rosenthal, Y., 2009. Deep-Sea Temperature and Ice Volume Changes Across the Pliocene-Pleistocene Climate Transitions. Science. 325, 306–310.
→ Spratt, R.M., Lisiecki, L.E., 2016. A Late Pleistocene sea level stack. Clim. Past 12, 1079–1092. https://doi.org/10.5194/cp-12-1079-2016
→ Tregoning, P., 2002. Plate kinematics in the western Pacific derived from geodetic observations. J. Geophys. Res. Solid Earth 107, ECV 7-1-ECV 7-8.
→ Tregoning, P., et al., 1999. Motion of the South Bismarck Plate, Papua New Guinea. Geophys. Res. Lett. 26, 3517–3520.
→ Waelbroeck, C., et al., 2002. Sea-level and deep water temperature changes derived from benthic foraminifera isotopic records. Quat. Sci. Rev. 21, 295–305.
→ Wallace, L.M., et al., 2004. GPS and seismological constraints on active tectonics and arc-continent collision in Papua New Guinea: Implications for mechanics of microplate rotations in a plate boundary zone. J. Geophys. Res. Solid Earth 109.
→ Woodroffe, C.D., Webster, J.M., 2014. Coral reefs and sea-level change. Mar. Geol. 352, 248–267.
→ Yokoyama, Y., et al., 2007. Japan Sea oxygen isotope stratigraphy and global sea-level changes for the last 50,000 years recorded in sediment cores from the → Oki Ridge. Palaeogeogr. Palaeoclimatol. Palaeoecol. 247, 5–17.
→ Yokoyama, Y., Purcell, A., 2021. On the geophysical processes impacting palaeo-sea-level observations. Geosci. Lett. 8, 13.
→ Zazo, C., et al., 2007. Quaternary marine terraces on Sal Island (Cape Verde archipelago). Quat. Sci. Rev. 26, 876–893.

Dear Reviewers,

We provide hereunder an overview of how we address the comments, with explanations and references, on a point-by-point basis. Reviewers' comments are in black and our response in blue. In the manuscript, all the new/changed text is highlighted in yellow.

Reviewer(s)' Comments to Author:

Reviewer 1:

Dear Editor,
I have now finished my assessment of the MS entitled "High interstadial sea levels over the past 420ka from Huon terraces (Papua New Guinea)" by De Gelder et al.
The paper re-analyses a classic pleistocene reef sequence in Papua New Guinea using previously dated corals and high-resolution Digital Elevation Models. In my opinon, the authors do an excellent job in correcting for differential tectonic uplift across the coast,and adequately discusses the challenges of such correction (e.g, considering uplift constant through time). They make a smart use of reef stratigraphic models to cross-validate the terrace elevations observed in the study area. To me, this paper should be published. I have only one request for clarification and one major comment to offer the authors. I think that addressing them might help improving the paper.

We very much appreciate the time and effort by the reviewer to go through our manuscript, as well as the positive assessment of the manuscript.

1) I could not understand if, to model the reef terrace formation, you corrected Spratt and Lisiecky for GIA. If you didn't, I think you should. Can you please explain?

Response 1.1: For the reef modeling we did not correct the Spratt & Lisiecki curve for GIA, The reason for this is that the differences with the corrected curve are very small (<5 m, Supplementary Fig. 6), and this way the effect of highstand-adjustment is slightly more pronounced and easier to observe. We clarified this now in Lines 550-553 in the Methods section.

2) I think that you should add, as a final figure, a Dutton-like one (2015, science), where you sum up the numbers you get for each MIS, comparison with other studies (if any), and, if available, some considerations on paleotemperatures, Co2 and ice sheets. I think this will require some work, but it will surely widen the potential audience of the paper. Maybe this figure could be accompanied by one or two (short) paragraphs to widen the paleoclimate implications of your work.
I really have no other observations to make: the paper is clear, figures are nicely drawn and the study design make this paper interesting and meaningful.

Response 1.2: We think this is an excellent suggestion, and have added two figures along those lines (Fig. 5 and Supplementary Fig. 9). Given the amount of MIS data, we chose a continuous graph representation rather than the separate boxes as in Dutton et al. (2015). We also added an extra paragraph to discuss the new figure, and the relationship between sea-level, temperature, summer insolation and CO2 in general (Lines 280-300).

Reviewer 2:

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This paper offers a new analysis of the Huon coral reef terraces in Papua New Guinea using high-resolution topographic data and applying novel geometric approaches to better understand terrace deformation pattern. This paper posits the following new interpretations for the Huon terraces: a) general northward tectonic tilt, as opposed to the previously reported northwestward tilt; b) recognized 31 Late Pleistocene terraces compared to the ~20 terraces previously documented; c) estimated relative sea level (RSL) corrected for glacial isostatic adjustment (GIA); and d) illustrated the Huon terrace sequence using numerical models (coral reef modeling). With this, the authors suggest that δ18O-derived global mean sea-level curves systematically underestimate sea level during interstadial periods, by up to ~20m, and this discrepancy is either an effect of incorrect oxygen isotope curve calibrations or δ18O does not fully capture short-lived sea-level variations.

This paper is overall well written and clearly structured. It demonstrates a novel approach on analyzing high-resolution topographic data to examine deformation patterns of reef terraces and derive RSL estimates. I think the paper did a good job in explaining the geometric analysis done and coupling it with coral reef modeling. The paper also presents the first RSL data for the ~420-125 ka period, improves elevation and age estimates for the ~125-0 ka period, and provides the first RSL estimates for interstadial and lowstands in Huon peninsula. With the new derived RSL data, the authors are able to provide an explanation about the discrepancy between the interstadial sea level in Huon and in other sites. With all this, I believe that this paper falls within the scope of the journal and will make a good contribution to Quaternary RSL studies. As it also offers insights for future work, this paper will become a useful reference for researchers working on sea level especially in tectonically active regions.

We very much appreciate the time and effort by the reviewer to go through our manuscript, as well as the positive assessment of the manuscript.

I have one main comment though and this has something to do with the use of shoreline angle in the analysis of the Huon coral reef terraces. The shoreline angle, situated at the junction between a terrace flat and its corresponding paleo-seacliff landwards, is designated by the authors as the morphological approximation of paleo-RSL. I did not find on the paper an explanation of the relationship of the shoreline angle with respect to sea level. As a reader, I would appreciate a brief illustration of how the shoreline angle forms on a coral reef terrace and how the shoreline angle represents paleo-sea level (in this case, in Huon peninsula). I think this is important since the new Huon RSL estimates were derived directly from terrace geometry—and thus from shoreline angle delineation. Notwithstanding this suggestion, I believe that this paper deserves publication with only some minor comments for the authors' consideration.

Response 2.1: We understand the reviewer's concern here, and have included a panel in Figure 2 to illustrate what we mean exactly with the shoreline angle. This panel is based on the original field observations as published in Chappell (1974). We think this clarifies the relationship between shoreline angle and sea-level.

Minor comments:

Line 26: change to "relative sea level (RSL)"

Response 2.2: Changed as suggested (Line 24).

Line 38: change to "global mean sea level (GMSL)"

Response 2.3: Changed as suggested (Line 35).

Line 60: change to "we used"

Response 2.3: Changed as suggested (Line 59).

Line 86: change to "we used"

Response 2.4: Changed as suggested (Line 87).

Line 87: change to "we used"

Response 2.5: Changed as suggested (Line 95).

Line 91: To better refer to the figures, I would suggest to make Figure 3 into Figure 2 since the description for Figure 3 (on Line 91) went first than for Figure 2 (on Line 96).

Response 2.6: We prefer not to make this switch, as we feel it makes more sense to first show how the tilt direction was obtained, and then the RSL calculations based on the 'ideal' tilt direction. We did remove the reference to Fig. 3 on Line 91 though, for consistency.

Line 99: N359E – Is this bearing? This is also not shown on the figure caption.

Response 2.7: We've added 'dip directions' now for clarity (Line 106). Furthermore, we realised that the numbers were switched: N002E is for the MIS 5a terrace and N359 is for the MIS 5e terrace. We corrected this both in the Fig. 2, Supplementary Fig. 5 and the caption of Fig. 2.

Line 104: change to "Inferred relative sea levels"

Response 2.8: Changed as suggested (Line 117).

Line 123: change to "we updated"

Response 2.9: Changed as suggested (Line 139).

Line 127: change to "other sites, which show"

Response 2.10: Changed as suggested (Line 144).

Lines 142-146: Please rephrase; awkward wording

Response 2.11: Rephrased for clarity (Lines 161-164).

Line 150: change to "lower sea levels"…. "higher sea levels"

Response 2.12: Changed as suggested (Line 168).

Line 180-181: where is this (clusters of ages around ~115 ka and ~ka) on the models (Figure 4)?

Response 2.13: In Supplementary Table 1, as we specified now (Lines 207-208).

Line 208: change to "sea level"

Response 2.14: Changed as suggested (Line 236).

Line 598: For, the MIS peaks which we have multiple RSL…

Response 2.15: Changed to 'For the MIS peaks characterized by' to clarify this (Line 558).

Reviewer 3:

de Gelder et al., "High interstadial sea levels over the past 420ka from Huon terraces (Papua New Guinea)"

Summary: de Gelder et al., present a reassessment of the tectonic deformation of the Huon Peninsula, and a reinterpretation of the sea-level record based on fossil terrace records. The preserved terrace records from the Huon Peninsula (Papua New Guinea, PNG) provide an extraordinary record of changing sea levels, however, untangling the uplift and sea-level change is difficult due to the lack of independent estimates of uplift rates for the location. De Gelder et al, reassess the uplift rate by fitting a surface to the elevations of shoreline angle (i.e., the sea cliff base), derived from a digital surface elevation model. This showed a N rather the NW orientation of tilt. Based on this, and a reinterpreted uplift rate, past sea level was reconstructed and compared to the stack of Spratt and Lisiecki (2016). The authors then use the differences between these to suggest problems with isotopic methods of reconstructing past sea levels.

This has the beginnings of a very nice paper but in its current form, submission is a little premature, and the conclusions are not well supported, nor particularly novel. For example, the claim (lines 56, 63 etc.) that this the first record of sea level beyond ~130 ka is overstated. There is a wealth of data that extend beyond 130 ka (Andersen et al., 2008; Bard et al., 1996; de Boer et al., 2014; Dumitru et al., 2019; Edwards et al., 1997; Elderfield et al., 2012; Gallup et al., 1994; Grant et al., 2019, 2014; Kennedy et al., 2012; Rohling et al., 2014, 1998; Siddall et al., 2006; Sosdian and Rosenthal, 2009; Yokoyama et al., 2007; Zazo et al., 2007). Ok, many of these are fragmentary/sparse corals records (but not all!). The value of the Huon Peninsular is the rapid uplift, making it one of the few sites where there is such a complete record of sea-level highstands. de Gelder et al., provide a valuable, and much needed revaluation, but this is not the first time that sea levels
for low stands, or periods older than LIG have been presented.

Response 3.1: we understand the point that the reviewer is making here, and have made several changes to clarify how our RSL data fits within the framework of previous findings. We've added a new supplementary figure (Supplementary Fig. 1) that compares our data in more detail with previous studies; specifically with most of the continuous sea-level curves (Sup. Fig. 1a-d) and fragmentary coral data (Sup. Fig. 1e-f) that are mentioned by the reviewer. With respect to the studies mentioned by the reviewer, we didn't compare our findings with continuous sea-level curves that only span a portion of the studied time range (like Yokoyama et al., 2007; ~50-0 ka), or data older than 450 ka (like Andersen et al., 2008 and Grant et al., 2019 mentioned above).

Furthermore, in the text we changed the following lines:
Line 48: changed "RSL estimates of lowstands - the troughs between peaks - and interstadials older than 130 ka are largely unknown" to "Geological RSL indicators of lowstands - the troughs between peaks - and interstadials older than 130 ka are largely unknown or have error bars of 10s of m (Supplementary Fig. 1)" to emphasize that we are only referring to the geological RSL indicators.

Line 54: changed "Here we provide the first RSL estimates for such periods" to "Here we provide the first geological RSL estimates for several of such periods", to emphasize again we're only referring to geological indicators, and to specify as well that we are not per se the first study to provide **any** estimate for lowstands or interstadials prior to 130 ka (although for several periods we think we are).

Line 62: changed "we provide RSL data for the ~420-125 ka period for the first time while improving elevation and age estimates of previously documented sea-level highstands" to "we provide the most complete geological RSL record for the ~420-125 ka period anywhere (Supplementary Fig. 1), while improving elevation and age estimates of previously documented sea-level highstands at Huon" along the same lines with the changes above.

Similarly, the conclusions that the isotopic methods have their deficiencies is not new either; (Adkins et al., 2002; Chappell and Shackleton, 1986; Schrag et al., 1996; Shackleton, 1987; Skinner and Shackleton, 2005; Spratt and Lisiecki, 2016; Waelbroeck et al., 2002)

Response 3.2: we agree with the reviewer, and have clarified this by adding a sentence in Line 218 "Previous studies highlighted several problems in the conversion from $\delta^{18}O$ to sea-level (e.g. Shackleton 1987; Adkins et al., 2002; Waelbroeck et al., 2002; Spratt and Lisiecki 2016). Along the lines of those studies, we envision…"

nor that change in uplift rate will affect the sea levels reconstructed for terraces prior and subsequent to the LIG (see Creveling et al 2015).

Response 3.3: We do not claim this as a new conclusion in a general sense, just in the sense that our new assessment of the Huon uplift rates changes the RSL estimates for Huon with respect to previous interpretations (Lines 118-122). We come back to the Creveling et al., 2015 paper in response 3.4 below.

The analysis depends on a couple of assumptions: (i) that we know what sea level was during the Last Interglacial (LIG) (this underpins the uplift rate calculation) (line 76), and; (ii) the fidelity of sea-level records. The first is not well supported; there is still considerable debate within the community as to the maximum magnitude, timing and the geometry of melt during the LIG sea-level highstand (e.g., Barlow et al., 2018; Dendy et al., 2017; Dutton et al., 2015; Dyer et al., 2021; Hearty et al., 2007; Long et al., 2015; Rohling et al., 2019, 2017), these will all have an impact on the recalculated rate of uplift, (already discussed in e.g., Creveling et al., 2015; Düsterhus et al., 2016a, 2016b; Hibbert et al., 2016; Yokoyama and Purcell, 2021).

Response 3.4: We agree that the sea-level during the LIG is not very well constrained, which is exactly why we picked such a broad range for the most elevated MIS 5e terrace in Huon (2-12m). We went through all the papers suggested by the reviewers and find indeed that our proposed range adequately covers the range of possibilities suggested by others: for the early stages of MIS 5e (125-129 ka), GMSL estimates range from ~2-3 m (Hearty et al., 2007; Dyer et al., 2021) to ~7.5 m (Kopp et al., 2009; Dutton et al., 2015) whereas the study of Creveling et al., 2015 proposed a GIA-corrected RSL at Huon of 12 m elevation, assuming a GMSL of 8 m in the early interglacial. We have clarified this in the text (Lines 133-138) by replacing

"as a compromise between reasonable MIS 5e minimum and maximum estimates of 2 m (GMSL[3]) and 12 m (GIA-corrected Huon RSL[23]), respectively."

With

"Within the considerable debate on the elevation and timing of MIS 5e sea-level at different locations (Kopp et al., 2009; Creveling et al., 2015; Barlow et al., 2018; Rohling et al., 2019; Dyer et al., 2021) we find the range of 2 m to 12 m a reasonable compromise between the lower end of early MIS 5e GMSL estimates (Hearty et al., 2007; Dyer et al., 2021), if GIA effects are negligible at Huon, and the upper end of early MIS 5e GMSL estimates (Kopp et al., 2009; Dutton et al., 2015), if GIA corrections at Huon are on the order of +4 m (Creveling et al., 2015)."

The second is not acknowledged in the manuscript, but does have ramifications for the conclusions (i.e., the offsets between the fossil terraces, and the open ocean oxygen isotope stack, Spratt and Lisiecki, 2016, SL16). There is no perfect record of past sea-level change; fossil terraces 'need' favourable conditions for formation (i.e., periods of relatively stable sea level) and preservation etc. (Camoin and Webster, 2015; Murray-Wallace and Woodroffe, 2014; Woodroffe and Webster, 2014; Yokoyama and Purcell, 2021), and open ocean δ18O is a mixed record (sea, level, temperature, local overprinting etc., as acknowledged by the authors). The way the SL16 stack was constructed, aligning all records to the LR04 stack (Lisiecki and Raymo, 2005) ignores the potential for significant timing differences in the benthic δ18O signal between the different ocean basins (Skinner and Shackleton, 2005; Waelbroeck et al., 2002), and the careful work by authors on age constraints (tuning to LR04 results in average age uncertainties of ~4 ka in this time interval). Both of these could have quite an impact on the magnitude of the reconstructed sea levels if the age models are not correctly aligned. The records used in the SL16 stack are first order sea-level estimates derived from by subtracting estimates of the temperature component on the $\delta^{18}O$ ($\delta^{18}O_{mineral}$ = $\delta^{18}O_{seawater}$ + ΔTseawater, and where $\delta^{18}O_{seawater}$ reflects both global ice volumes and local influences). The relative contribution of global ice volumes and temperature to foraminiferal oxygen isotopes is complex and subject to substantial uncertainties. Notwithstanding, the new sea level record overlaps within uncertainties of SL16 (bar MIS 6b and the MIS3/2transition), and so I'm not sure you can draw any firm conclusions, given the uncertainties of the records.

Response 3.5: Several points of critique are brought forward here, as a response we highlight the following:

- Concerning the point that terraces need favorable conditions for formation, we agree with that, which is part of the motivation of the terrace modeling. For that, we take into account realistic reef growth rates calibrated with well-dated Huon Holocene corals for a realistic range of sea-level changes (see Methods, Lines 560-562). Within the modeling, we also checked specifically how much the 'imperfection' of coral reef growth as a sea-level recorder could have biased our record (Lines 217-233). In the main text we added now "the rate of sea-level changes and on" to also emphasize the influences of the rate of sea-level changes (Line 222).
- Concerning the aligning of records in oxygen isotope records, yes, this is a good point. We lightly hinted at that on Line 268 "$\delta^{18}O$ records, which are often stacked or averaged", but we extended that now, by replacing that sentence with "Such records are often stacked or averaged, ignoring potentially important differences in timing between oceanic basins (Waelbroeck et al., 2002; Skinner and Shackleton, 2005), and smoothing out rapid sea-level changes (Spratt and Lisiecki, 2016)".
- Concerning the overlap in uncertainty between our Huon RSL data and the 2.5-97.5% confidence interval of the Spratt and Lisiecki, yes, we agree that most of the individual points overlap. However, given that all of our preferred estimates on 21 interstadial RSL data points plot above the most likely sea-level proposed by SL16, without exception, we contend that we can draw firm conclusions on that. To be more considerate of other sea-level curves than SL16, we also added a comparison to 11 other curves in Supplementary Fig. 1. We added a sentence in the text about this on Line 168: "We compared our results to an additional 11 $\delta^{18}O$-derived sea-level curves (Supplementary Fig. 1), and even though there are exceptions for some interstadials in some curves, the overall trend of interstadial Huon RSL at higher elevations remains" and added a reference to this new figure on Line 237.

The new terrace analysis is great – the level of detail is impressive and hints at new, and potentially short-lived, features within the Huon terraces. Sadly, these weren't 'ground truth-ed' (see other comments, below). The Huon Peninsula 'flight' of terraces, and rapid uplift, mean that this site has the potential to unravel past sea levels, if the uplift rate can be constrained. Currently, your estimates are a minor advance, as they are still subject to various assumptions about LIG sea levels (cf., Creveling et al., 2015) and are not independently verified.

Response 3.6: We appreciate the compliments on the analysis and agree that the potentially short-lived features are exciting. Concerning the critics mentioned here, we highlight the following changes:

- We made improvements on the 'ground truthing' with a supplementary figure comparing profiles (see response 3.13 below), and by adding more comparisons to previous work (Supplementary Fig. 1, see responses 3.1 and 3.5 above as well). We think this element has now improved sufficiently.
- Concerning the uplift rate constraints, we considered a broad range of possibilities (see details above in response 3.4 as well), making our results as robust as can be reasonably expected. Our re-evaluation of uplift rates has changed Huon RSL estimates for the past ~130 ka, which could be considered a minor advance, but the RSL constraints for the period 130-410 ka are new, and nowhere else has such a complete geological RSL record been derived. As such, given the exceptional status of the Huon peninsula in sea level studies, we do think that those represent a major advance in our understanding of sea-level and its relation with $\delta^{18}O$ records. We think we added further depth and significance to this work by adding Figure 5 (see response 1.2 as well), and adding a paragraph of discussion on broader climatic implications (Lines 280-300). We hope we have convinced the reviewer with this.

You have a germ of a very nice paper, but I'm unconvinced that your results are novel, or well supported by the data, and I wasn't convinced it advances our understanding of the system. In summary, this is a nice idea but it is a little premature. As such I would not recommend publication in Nature Communications, Earth and Environment.

Response 3.7: We hope that with the changes that we have made, both the ones detailed above and below, the reviewer is more convinced of this work now.

Other comments:
PNG has a very complex tectonic setting (several microplates and deforming zones). Is the rate of uplift the same along the coast, given the rapid relative deformation of the Huon Peninsula (i.e., rotational component of movement of the South Bismarck Plate; e.g., Wallace et al., 2004)?

Response 3.8: We think that from Figures 1, 2, 3 and 4, Supplementary Figs. 3, 5, Supplementary Data 2, 3 and the text (specifically section 'New interpretation of Huon coral reef terrace deformation') it should be clear that the rate of uplift is not the same along the coast. We emphasized it now in the introduction as well to make it even more clear (Line 56-57).
Bringing up the rotational component is an interesting suggestion, but for the rotational component to have an effect on our calculations, it should affect the relative motion between the Huon Peninsula and the source of uplift. We note that 1) the plate-scale rotation is likely taken up along the plate boundary, and not along the Wonga Thrust that we suspect to be the main source of uplift (Line 110) 2) the proposed ~8°/Ma (Tregoning et al., 1999) rotation between the S-Bismarck Plate and the Australian Plate would imply only ~3° over 400 ka, and 3) the deformation pattern does visibly differ comparing the lower terraces from the upper terraces (Supplementary Fig. 3). We've added a sentence now to mention this (Line 112) "Theoretically, microplate rotation of ~8°/Ma (Tregoning et al., 1999) could have a minor effect on the deformation pattern through time, but as the deformation pattern does not visibly differ comparing the lower terraces from the upper terraces (Supplementary Fig. 3), we assume that this effect is negligible in the following analysis."

How does the orientation obtained by fitting your surface to the shoreline angles compare to estimates from geodetic monitoring network e.g., Tregoning, 2002; Tregoning et al., 1999).

Response 3.9: That is a good point. We think Wallace et al., 2004 gives a better account of GPS velocities in the region, but both that study and the work by Tregoning suggest an overall NNE-SSW convergence between Huon and the New Guinea Highlangs (Fig. 1), which fits much better with a N-directed tilt than with the previously proposed NW-directed tilt. We've added a sentence on this now (Line 111), and added the GPS velocities to the inset of Fig. 1.

Is the uplift rate derived from fitting of the surface the same for the different transects (i.e., Sialum, Kanazaura, Bobongara), and how do these compare to previous estimates (Chappell et al., 1996a, 1996b)?

Response 3.10: As should be clear from Fig. 1 and Fig. 3 and Fig. 4, no the uplift rates are not the same for the different profiles. We've made a comparison to uplift rates to some of the previously mapped profiles in Supplementary Fig. 2, and referred to that in the text on Line 119.

From your DSM analysis, are the terraces flat? Could you use this probe the deformation further?

Response 3.11: Not always, and for that reason we preferred to focus on the shoreline angles instead. We clarified that now in the Methods section (Line 434): "As terraces are not always flat (e.g. example in Fig. 2b), either because of the way they formed and/or how they were eroded afterwards, we chose to focus our evaluation of the deformation on shoreline angles (Fig. 2)."

The recalculation of the uplift rate doesn't really advance our understanding. Your estimates are still subject to the same assumptions; you're still using the elevation of the LIG terrace to tell you about the uplift rates BUT we don't know what this was, not any consensus on the timing or melt geometries of the peak etc.

Response 3.12: As argued above (particularly in response 3.6) we think the uplift rate recalculation makes for a (smaller) step forward in better understanding of sea-level over the past ~130 ka, but a big step forward in terms of interstadial sea-level elevations for the past ~410 ka, which stems directly from the uplift rate calculations. Considering LIG sea-level considerations that we made, we refer to response 3.4 in which we elaborated on our assumptions.

Ground-truthing: Unfortunately, you weren't able to compare your analysis to the previously recognised features. I would have expected some attempt to compare your elevations with the detailed terrace elevations (including the elevations of sea cliffs) from previous publications. For example, Chappell et al. (2016) contains some very detailed maps of the terraces (their figs 11&12). Perhaps something for the supplementary information?

Response 3.13: Following this suggestion, we've included a supplementary figure comparing profile elevations (Supplementary Fig. 2), and in the text included the following sentences (Line 88): "Comparison with previously measured profiles (Stein et al., 1993; Chappell et al., 1996) shows that our DSM gives similar elevations within a few m (Supplementary Fig. 2). Although field-based estimates of coral reef terraces would typically give smaller error margins for individual transects, the main advantage of using a DSM instead is that it allows for a continuous evaluation with practically unlimited topographic profiles. This averages out local peculiarities and avoids the corresponding bias that discrete profiles would give, thus providing a more objective assessment of terrace elevations".

Could you use you uplift rates to 'predict' the elevations of other features? This would give greater confidence in your recalculated rate. I'm not sure why I would use your new uplift rates compared to that derived by previous authors.

Response 3.14: In the reef modeling exercise (section Terrace sequence modeling) we used the new uplift rates to 'predict' elevations of reef features. We emphasized that now (Lines 187-188).

We would recommend using our new uplift rates because they have been derived with a large-scale geometrical analysis using high-resolution digital topography, both using a qualitative (Supplementary Fig. 3) and a quantitative analysis (Fig. 2; Supplementary Figs. 4, 5). It is based on both 1) the transects that were used in previous study, but also on 2) several other transects that were previously inaccessible, so our analysis is more comprehensive. We specified now that we consider this a better way to quantify km-scale deformation compared to field research, which is often complicated given the lack of large-scale overview at any given location (Lines 83-84). Furthermore, as mentioned in Lines 109-112, the spatial pattern of our uplift rates fits better with the regional tectonic context, given "(i) the N-S compression indicated by focal mechanisms of the region's largest earthquakes, and (ii) the E-W orientation of the Wonga Thrust immediately S of the Huon Peninsula and (iii) overall NNE-SSW convergence from GPS constraints (Wallace et al., 2004)."

Why only use the MIS5e terrace elevations? how is your fit affected using other terraces (e.g., Holocene T1 where there are several publications with detailed age and elevation information)?

Response 3.15: Assuming this question concerns the uplift rate calculation: even though LIG elevation is relatively uncertain, it is still the best-constrained highstand prior to the Holocene, and the terrace is well-preserved throughout most of the area, even with lower uplift rates. It would be possible to calculate uplift rates from the Holocene terrace, but as those terraces are very young, and not very elevated (yet), relatively small vertical elevation uncertainties would have a relatively strong influence on uplift rate calculations. Furthermore, it is much more likely to be affected by seismic cycles and coseismic uplift (e.g. Chappell et al., 1996; Ota and Chappell, 1996), so to quantify the long-term deformation pattern, as we intend, the MIS5e terrace is more appropriate. Seismic cycles are probably also affecting the MIS5e terrace (and we correct for that, see Methods Lines 475-480), but should affect the average uplift rate much less than over the timespan of the Holocene.

Your uplift rate spans a large range, in part stemming from the choice of max. LIG sea levels for the Huon Peninsula (7 ± 5 m) – why not use the relative sea-level prediction from your GIA modelling of LIG highstand at the Huon Peninsula?

Response 3.16: We are quite surprised with this comment, as it seems in direct contradiction with the previous comments stating that there is a lot of disagreement on LIG sea level. As such, we refer again to response 3.4 for our viewpoint on this.

Is uplift constant over the whole ~420 ka record (line 210)? Previous work suggests that this is the case for the last 130 ka (Chappell et al., 1996b; Ota et al., 1993) when averaged over long timescales (1000 years). An alternative explanation could be episodic uplift events (i.e., coseismic uplift; Ota and Chappell, 1996)?

Response 3.17: Concerning the first question; yes it seems like (see Lines 161-164 in the 'inferred sea-levels' section, and Lines 237-241 in the 'Implications for global mean sea-level variations' section). Concerning the second part, yes that is in an interesting point. We've expanded on that now on Lines 241-244 "Even though 0-3 m episodic uplift events at timesteps of 200-1900 years have probably occurred throughout the Late Pleistocene (Ota and Chappell, 1996), it seems unlikely that clusters of tectonic uplift events would systematically occur much more frequent during interstadial periods."

Similarly, the terraces may also not capture short-term fluctuations in sea level (Camoin and Webster, 2015), or may not have chance to form if rapid sea-level change (e.g., Murray-Wallace & Webster, 2014).

Response 3.18: This is a fair point, and also one of the reasons we used coral reef modeling in our analysis. We clarified that now on Lines 183-186: "Models provide a way to precisely incorporate the geomorphic responses of coral reef sequences to sea-level changes, thus allowing for the possibility that rapid sea-level changes and short term fluctuations may not be recorded in fossil reefs (Camoin and Webster, 2015; Pastier et al., 2019)."

GIA modelling: Choice sea-level curve for ice hist in GIA modelling… what is the influence of using alternative ice histories (e.g., based on de Boer, Sosdian and Rosenthal, or Elderfield et al 2012).

Response 3.19: The purpose of the GIA-modelling was just to test the sensitivity of Huon's location, i.e. how much a Huon-based RSL curve would deviate from a GMSL curve. Given that this is only a few meters, we do not find it necessary to test alternative ice histories.

Also, assumption of LGM-like for the older glacials could result in an underestimate of several meters for the following interglacial sea level highstands (Dendy et al., 2017; Rohling et al., 2017). Could you 'test' the 'new' RSL curve you have generated using GIA predictions of the sea level curve at Huon?

Response 3.20: We believe the reviewer is referencing the fact that the size of ice sheets at MIS 6 and the rate of deglaciation will have an impact on RSL at MIS 5e. However the focus of our paper is on GMSL highstands during the glacial phase, which is constrained by our datasets, not deglacial-interglacial transitions. Thus we did not explore the sensitivity of RSL to the size or distribution of glacial maxima. Because our datasets center on time periods after ~100 ka, the GIA effects due to ice configurations during the MIS 6 deglacial are expected to be negligible.

Line 137 to 142: "3d, shows a remarkable first-order similarity in highstand ages and sea-level elevations" – you're not comparing like with like here (relative sea level and global mean sea level). The original ages and elevations of the fossil corals also 'fit' equally well (your figure 3).

Response 3.21: We changed this part (Lines 155-158) to "Fig 3d, shows that highstand ages correspond well, and interglacial highstands are consistently at higher elevations than interstadial highstands. Absolute elevations for the interglacial highstands (5e, 7e, 9e, 11c) are similar for both GMSL and Huon RSL," to clarify what we mean here.

Line 141: please use the references for the original studies, rather than the Dutton et al., 2015 synthesis.

Response 3.22: Changed as suggested (Line 160)

References:

→ Adkins, J.F., et al., 2002. The Salinity , Temperature , and δ18O of the Glacial Deep Ocean. Science 298, 1769–1773.
Andersen, M.B., et al., 2008. High-precision U-series measurements of more than 500,000 year old fossil corals. Earth Planet. Sci. Lett. 265, 229–245.
→ Bard, E., et al., 1996. Pleistocene sea levels and tectonic uplift based on dating of corals from Sumba Island, Indonesia. Geophys. Res. Lett. 23, 1473–1476.
→ Barlow, N.L.M., et al., 2018. Lack of evidence for a substantial sea-level fluctuation within the Last Interglacial. Nat. Geosci. 11, 627–634.
→ Camoin, G.F., Webster, J.M., 2015. Coral reef response to Quaternary sea-level and environmental changes: State of the science. Sedimentology 62, 401–428.
→ Chappell, J., et al., 1996a. Reconciliaion of Late Quaternary sea levels derived from coral terraces at Huon Peninsula with deep sea oxygen isotope records. Earth Planet. Sci. Lett. 141, 227–236.
→ Chappell, J., Ota, Y., Berryman, K., 1996b. Late Quaternary coseismic uplift history of Huon Peninsula, Papua New Guinea. Quat. Sci. Rev. 15, 7–22.
→ Chappell, J., Shackleton, N.J., 1986. Oxygen isotopes and sea level. Nature 324, 137–140. https://doi.org/10.1038/324137a0
→ Creveling, J.R., et al., 2015. Revisiting tectonic corrections applied to Pleistocene sea-level highstands. Quat. Sci. Rev. 111, 72–80.
→ de Boer, B., Stocchi, P., van de Wal, R.S.W., 2014. A fully coupled 3-D ice-sheet–sea-level model: algorithm and applications. Geosci. Model Dev. 7, 2141–2156.
→ Dendy, S., et al., 2017. Sensitivity of Last Interglacial sea-level high stands to ice sheet configuration during Marine Isotope Stage 6. Quat. Sci. Rev. 171, 234–244.
→ Dumitru, O.A., et al., 2019. Constraints on global mean sea level during Pliocene warmth. Nature 574, 233–236.
→ Düsterhus, A., et al., 2016a. Palaeo-sea-level and palaeo-ice-sheet databases: problems, strategies, and perspectives. Clim. Past 12, 911–921.
→ Düsterhus, A., Tamisiea, M.E., Jevrejeva, S., 2016b. Estimating the sea level highstand during the last interglacial: a probabilistic massive ensemble approach. Geophys. J. Int. 206, 900–920.
→ Dutton, A., et al., 2015. Tropical tales of polar ice: evidence of Last Interglacial polar ice sheet retreat recorded by fossil reefs of the granitic Seychelles islands. Quat. Sci. Rev. 107, 182–196.
→ Dyer, B., et al., 2021. Sea-level trends across The Bahamas constrain peak last interglacial ice melt. Proc. Natl. Acad. Sci. 118, e2026839118.
→ Edwards, R.L., et al., 1997. Protactinium-231 dating of carbonates by thermal ionization mass\rspectrometry: Implications for Quaternary climate change. Science. 276, 782–786.
→ Elderfield, H., et al., 2012. Evolution of Ocean Temperature and Ice Volume Through the Mid-Pleistocene Climate Transition. Science. 337, 704–709.
→ Gallup, C.D., Edwards, R.L., Johnson, R.G., 1994. The Timing of High Sea Levels Over the Past 200,000 Years. Science. 263, 796–800.
→ Grant, G.R., et al., 2019. The amplitude and origin of sea-level variability during the Pliocene epoch. Nature 574, 237–241.
→ Grant, K.M., et al., 2014. Sea-level variability over five glacial cycles. Nat. Commun. 5, 5076.
→ Hearty, P.J., et al., 2007. Global sea-level fluctuations during the Last Interglaciation (MIS 5e). Quat. Sci. Rev. 26, 2090–2112.
→ Hibbert, F.D., et al., 2016. Coral indicators of past sea-level change: A global repository of U-series dated benchmarks. Quat. Sci. Rev. 145, 1–56.
→ Kennedy, D.M., et al., 2012. Shore platform development on an uplifting limestone island over multiple sea-level cycles, Niue, South Pacific. Geomorphology 141–142, 170–182.
→ Lisiecki, L.E., Raymo, M.E., 2005. A Pliocene-Pleistocene stack of 57 globally distributed benthic δ18O records. Paleoceanography 20, PA1003.

→ Long, A.J., et al., 2015. Near-field sea-level variability in northwest Europe and ice sheet stability during the last interglacial. Quat. Sci. Rev. 126, 26–40.
→ Murray-Wallace, C. V., Woodroffe, C.D., 2014. Quaternary Sea-Level Changes. Cambridge University Press, Cambridge.
→ Ota, Y., Chappell, J., 1996. Late Quaternary coseismic uplift events on the Huon Peninsula, Papua New Guinea, deduced from coral terrace data. J. Geophys. Res. Solid Earth 101, 6071–6082.
→ Ota, Y., et al., 1993. Holocene Coral Reef Terraces and Coseismic Uplift of Huon Peninsula, Papua New Guinea. Quat. Res. 40, 177–188.
→ Rohling, E.J., et al., 1998. Magnitudes of sea-level lowstands of the past 500,000 years. Nature 394, 162–165.
→ Rohling, E.J., et al., 2014. Sea-level and deep-sea-temperature variability over the past 5.3 million years. Nature 508, 477-82.
→ Rohling, E.J., et al., 2019. Asynchronous Antarctic and Greenland ice-volume contributions to the last interglacial sea-level highstand. Nat. Commun. 10, 5040.
→ Rohling, E.J., et al., 2017. Differences between the last two glacial maxima and implications for ice-sheet, δ18O, and sea-level reconstructions. Quat. Sci. Rev. 176, 1–28
→ Schrag, D.P., Hampt, G., Murray, D.W., 1996. Pore Fluid Constraints on the Temperature and Oxygen Isotopic Composition of the Glacial Ocean. Science. 272, 1930–1932.
→ Shackleton, N.J., 1987. Oxygen isotopes, ice volume and sea level. Quat. Sci. Rev. 6, 183–190
→ Siddall, M., et al., 2006. Sea-level reversal during termination II. Geology 34, 817–820.
→ Skinner, L.C., Shackleton, N.J., 2005. An Atlantic lead over Pacific deep-water change across Termination I: Implications for the application of the marine isotope stage stratigraphy. Quat. Sci. Rev. 24, 571–580.
→ Sosdian, S.M., Rosenthal, Y., 2009. Deep-Sea Temperature and Ice Volume Changes Across the Pliocene-Pleistocene Climate Transitions. Science. 325, 306–310.
→ Spratt, R.M., Lisiecki, L.E., 2016. A Late Pleistocene sea level stack. Clim. Past 12, 1079–1092. https://doi.org/10.5194/cp-12-1079-2016
→ Tregoning, P., 2002. Plate kinematics in the western Pacific derived from geodetic observations. J. Geophys. Res. Solid Earth 107, ECV 7-1-ECV 7-8.
→ Tregoning, P., et al., 1999. Motion of the South Bismarck Plate, Papua New Guinea. Geophys. Res. Lett. 26, 3517–3520.
→ Waelbroeck, C., et al., 2002. Sea-level and deep water temperature changes derived from benthic foraminifera isotopic records. Quat. Sci. Rev. 21, 295–305.
→ Wallace, L.M., et al., 2004. GPS and seismological constraints on active tectonics and arc-continent collision in Papua New Guinea: Implications for mechanics of microplate rotations in a plate boundary zone. J. Geophys. Res. Solid Earth 109.
→ Woodroffe, C.D., Webster, J.M., 2014. Coral reefs and sea-level change. Mar. Geol. 352, 248–267.
→ Yokoyama, Y., et al., 2007. Japan Sea oxygen isotope stratigraphy and global sea-level changes for the last 50,000 years recorded in sediment cores from the → Oki Ridge. Palaeogeogr. Palaeoclimatol. Palaeoecol. 247, 5–17.
→ Yokoyama, Y., Purcell, A., 2021. On the geophysical processes impacting palaeo-sea-level observations. Geosci. Lett. 8, 13.
→ Zazo, C., et al., 2007. Quaternary marine terraces on Sal Island (Cape Verde archipelago). Quat. Sci. Rev. 26, 876–893.

We thank the reviewer for these interesting references, some of which we did not know yet. We have added several of these references to the manuscript and/or supplementary information as well (see details above).

We look forward to hearing back from you and the reviewers, and hope the manuscript is now acceptable for publication.

Kind regards,

Gino De Gelder, Laurent Husson, Anne-Morwenn Pastier, David Fernández-Blanco, Tamara Pico, Denovan Chauveau, Christine Authemayou and Kevin Pedoja

15th Jun 22

Dear Dr De Gelder,

Please allow me to apologise for the delay in sending a decision on your manuscript titled "High interstadial sea levels over the past 420ka from the Huon Peninsula (Papua New Guinea)". It has now been seen again by our reviewers, whose comments appear below. In light of their advice I am delighted to say that we are happy, in principle, to publish a suitably revised version in Communications Earth & Environment under the open access CC BY license (Creative Commons Attribution v4.0 International License).

We therefore invite you to revise your paper one last time to address the remaining concerns of our reviewers. At the same time we ask that you edit your manuscript to comply with our format requirements and to maximise the accessibility and therefore the impact of your work.

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Best regards,

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REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

Dear authors,
I have now assessed the revised version of your MS, and I have no further comments. Thank you for addressing my concerns, the paper reads well.

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The authors did very well in improving the manuscript "High interstadial sea levels over the past 420ka from the Huon Peninsula (Papua New Guinea)." The revised version of the paper fully addressed the comments from my review and I am very happy to suggest the manuscript to be accepted for publication.

Reviewer #3 (Remarks to the Author):

(see attached review)

de Gelder et al., "*High interstadial sea levels over the past 420ka from Huon terraces (Papua New Guinea)*"

This is a reassessment of the manuscript following revisions by the authors. The manuscript is much improved, with far better integration and "ground-truthing" with the existing literature on the fossil coral terraces of the Huon Peninsula. The additional text, figures and work looking at the consistency between your new estimates and the dated fossil terraces strengthens your re-evaluation of this iconic and valuable sea-level record. This careful work, reef modelling and greater clarity in methodology (i.e., assumptions made) has significantly strengthened the paper.

In summary, the paper rests on new terrace analysis which is impressive in its detail, allowing km-scale evaluation of setting and uplift. Based on this (and supported by reef modelling etc.) the authors makes a number of good contributions to the field:

1) Quantification of deformation pattern: ~350 measured shoreline angles confirm a N-ward trend, rather than the previously assumed NW trend.
2) Reassessment of uplift rates: this is very valuable, and the explanation of assumptions (i.e., lack of consensus on the Last Interglacial highstand timing or melt geometries) in the revised manuscript is much improved (and appreciated).
3) Re-interpretation of the dated coral sea-level indicators, and sea-level history: using the updated uplift rates, the authors have recalculated sea levels for some of the previously dated samples (Chappell, 1974; Lambeck and Chappell, 2001). The new terrace morphology (and reef modelling) also hints at new, and potentially short-lived, features within the Huon terraces.

The authors have addressed my (Reviewer 3) previous comments satisfactorily, but there remains some minor issues (detailed below). Once these have been addressed, I would see this manuscript making a good contribution to the sea-level community and beyond, and therefore recommend publication in *Nature Communications Earth and Environment.*

Comments:

1. Isotopic methods: there is a slight confusion here. I appreciate you have added additional records to the Spratt and Lisiecki (2016) stack (Bintanja et al., 2005; Elderfield et al., 2012; Rohling et al., 2014) ***BUT*** these use different methodologies (indirect modelling, $\delta^{18}O$ deconvolution using Mg/Ca, and marginal basin respectively) and are subject to different assumptions and uncertainties. Rather than detailed explanation, pleased ***add*** a sentence (or two) highlighting that what you term "isotopic methods" comprises several different methodologies.

The addition of SOM figure 1 is especially welcomed – thank you.

For information:

Open ocean $\delta^{18}O$ deconvolution (Elderfield et al., 2012; Sosdian and Rosenthal, 2009; Waelbroeck et al., 2002): Thank you for updating the manuscript to acknowledge the complexities associated with Waelbroeck et al. (2002) record (note that the vertical uncertainties ~ ± 13 m). Elderfield et al. (2012)/Sosdian and Rosenthal (2009) subtract the temperature component from North Atlantic/Pacific benthic $\delta^{18}O$ records using MgCa. Potential MgCa calibration and other complicating factors (e.g., Yu and Broecker, 2010) are associated with these records, and they have vertical uncertainties approx.. ± 35 m (1 sigma).

Marginal basin method: (a) Red Sea (Grant et al., 2014, 2012; Rohling et al., 2008, 1998; Siddall et al., 2003) and (b) Mediterranean (Rohling et al., 2014) utilises hydraulic control on water mass exchange, residence time, salinity and $\delta^{18}O$ (Siddall et al., 2004, 2003). The Red Sea method relies upon the hydraulic control of water mass exchange through the narrow Bab-el-Mandab Strait into the semi-enclosed (and highly evaporative) basin. The constriction of flow between a semi-enclosed basin and the open ocean with reduced sea levels, thereby increases the residence time of water in the basin, leading to pronounced salinity and $\delta^{18}O_{sw}$ changes in the basin. Hydrographic processes, and their effect on $\delta^{18}O_c$, of the Mediterranean Sea is more complex with large riverine inputs, temperate and African Monsoon climatic influences, as well as a larger strait. Probabilistic assessment gives vertical uncert. ± 3.5 m/6.3 m (Red/Med) at 95 % confidence interval.

Inverse forward modelling approach (i.e., joint ice sheet, temperature and $\delta^{18}O$ modelling) (Bintanja et al., 2005; de Boer et al., 2014a) use a coupled model of ice sheets and temperature (Bintanja et al., 2002; de Boer et al., 2014b) matched to an oxygen isotope record to quantify temperature and sea-level contributions simultaneously (uncert ~±10 m).

2. Discrepancy between 'isotopic' and other sea-level indicators: Personally, I feel this is still overstated given that this is not a novel conclusion. However, you present a very nice corroboration from the dated Huon fossil corals, although the re-evaluated fossil coral data presented in Figure 3 (bar MIS 6b and the MIS3/2transition) overlap within uncertainties with error envelope of Spratt and Lisiecki (2016), and most likely with the other records in SOM Fig 1. The additional discussion added is very welcome, and has placed your new study within the wider (and extensive literature) on these topics – i.e., the discrepancy in magnitude (Dalton et al., 2019; Duplessy et al., 2002; Pico et al., 2016; Waelbroeck et al., 2002) and deficiencies in the open ocean methods (Adkins et al., 2002; Chappell and Shackleton, 1986; Schrag et al., 1996; Shackleton, 1987; Skinner and Shackleton, 2005; Spratt and Lisiecki, 2016; Waelbroeck et al., 2002).

The new discussion added in this iteration on the secular variation (Fig 5 and SOM) is interesting, but again this builds upon a much wider body of work (Abe-Ouchi et al., 2013; Berends et al., 2021; Chappell and Shackleton, 1986; Clark and Mix, 2002; Foster and Rohling, 2013; Gasson et al., 2012; Mix and Ruddiman, 1984; Raymo et al., 2018; Spratt and Lisiecki, 2016; Thompson and Goldstein, 2006). I'm not sure you need this, nor what it really adds to the paper, but if you keep this section (& I leave this decision with the authors), adding a few additional references to acknowledge some of the existing work (to complement the current reliance on Shakun et al., 2016) is needed.

3. Uncertainties: please add error estimates for ages of the MIS 5a and 5c highstands, both the original authors and those you derive (line 148)

3. Recalculation of RSL for dated corals (Chappell, 1974; Lambeck and Chappell, 2001):
3.1. This really is the elevation, not RSL as you haven't included palaeo water depth (this can be up to 15 m, cf. Yokoyama et al 2001). You don't need to change this, as it's fairly obvious, but it is useful to keep in mind that the revised elevations would be a minimum sea-level estimate.
3.2. Why only use only some of the available data? What about others - e.g., Cutler et al., 2003; Esat et al., 1999; Stein et al., 1993; Yokoyama et al., 2001?

4. Adding a table to the Supplementary Information with the recalculated uplift rates (and uncertainties) for the principal locations of the dated fossil terraces would be very useful to the community (they are in the figures but it would take very little effort to add a table with the propagated uncertainties).

6. SOM figure 1 and line 169: This should be 10 rather than 11 methods – The Grant et al. (2014) and Rohling et al. (2009) record differ only in their chronologies, and the Grant'14 assessment is the most up to date assessment and should be used in preference to Rohling et al. (2009).

For background: The original Red Sea record was presented in Rohling et al. (1998) and Siddall et al. (2003), extended to 500 ka (Rohling et al., 2009), with additional Last Interglacial analyses (Rohling et al., 2008). The chronology for the last 150 ka was updated (Grant et al., 2012) and subsequently extended to the full record (Grant et al., 2014). The Grant et al. (2014) is currently this group's most up-to-date assessment of the Red Sea sea-level record, and should be used in preference to the earlier versions.

7. **What are the typical differences between the minimum and maximum shoreline angles**? Could you add a range to the *Methods* section so readers can have a sense of the magnitude of the difference in elevation.

8. **Clarification** line 485: "following careful assessment of dated sample" – what did this involve?

9. **Typos**:
replace "CO2" with "CO_2" (lines 281, 285 etc);
extra 'character' (box) in "Pa s" lines 537, and 538;
line 539 change "earth" to "Earth"

References:

Abe-Ouchi, A., Saito, F., Kawamura, K., Raymo, M.E., Okuno, J., Takahashi, K., Blatter, H., 2013. Insolation-driven 100,000-year glacial cycles and hysteresis of ice-sheet volume. Nature 500, 190–193. https://doi.org/10.1038/nature12374

Adkins, J.F., Mcintyre, K., Schrag, D.P., 2002. The Salinity , Temperature , and d 18 O of the Glacial Deep Ocean. Science (80-.). 298, 1769–1773. https://doi.org/10.1126/science.1076252

Berends, C.J., de Boer, B., van de Wal, R.S.W., 2021. Reconstructing the evolution of ice sheets, sea level, and atmospheric CO_2during the past 3.6 million years. Clim. Past 17, 361–377. https://doi.org/10.5194/cp-17-361-2021

Bintanja, R., van de Wal, R.S.., Oerlemans, J., 2002. Global ice volume variations through the last glacial cycle simulated by a 3-D ice-dynamical model. Quat. Int. 95–96, 11–23. https://doi.org/10.1016/S1040-6182(02)00023-X

Bintanja, R., van de Wal, R.S.W., Oerlemans, J., 2005. Modelled atmospheric temperatures and global sea levels over the past million years. Nature 437, 125–8. https://doi.org/10.1038/nature03975

Chappell, J., 1974. Geology of coral terraces, Huon Peninsula, New Guinea: A study of Quaternary tectonic movements and sea-Level changes. Geol. Soc. Am. Bull. 85, 553–570.

Chappell, J., Shackleton, N.J., 1986. Oxygen isotopes and sea level. Nature 324, 137–140. https://doi.org/10.1038/324137a0

Clark, P.U., Mix, A.C., 2002. Ice sheets and sea level of the Last Glacial Maximum. Quat. Sci. Rev. 21, 1–7. https://doi.org/10.1016/S0277-3791(01)00118-4

Cutler, K.B., Edwards, R.L., Taylor, F.W., Cheng, H., Adkins, J.F., Gallup, C.D., Cutler, P.M., Burr, G.S., Bloom, A.L., 2003. Rapid sea-level fall and deep-ocean temperature change since the last interglacial period. Earth Planet. Sci. Lett. 206, 253–271.

Dalton, A.S., Finkelstein, S.A., Forman, S.L., Barnett, P.J., Pico, T., Mitrovica, J.X., 2019. Was the Laurentide Ice Sheet significantly reduced during Marine Isotope Stage 3? Geology 47, 111–114. https://doi.org/10.1130/G45335.1

de Boer, B., Lourens, L.J., van de Wal, R.S.W., 2014a. Persistent 400,000-year variability of Antarctic ice volume and the carbon cycle is revealed throughout the Plio-Pleistocene. Nat. Commun. 5, 2999. https://doi.org/10.1038/ncomms3999

de Boer, B., Stocchi, P., van de Wal, R.S.W., 2014b. A fully coupled 3-D ice-sheet–sea-level model: algorithm and applications. Geosci. Model Dev. 7, 2141–2156.

Duplessy, J.C., Labeyrie, L., Waelbroeck, C., 2002. Constraints on the ocean oxygen isotopic enrichment between the last glacial maximum and the holocene: Paleoceanographic implications. Quat. Sci. Rev. 21, 315–330. https://doi.org/10.1016/S0277-3791(01)00107-X

Elderfield, H., Ferretti, P., Greaves, M., Crowhurst, S., McCave, I.N., Hodell, D., Piotrowski, A.M., 2012. Evolution of Ocean Temperature and Ice Volume Through the Mid-Pleistocene Climate Transition. Science (80-.). 337, 704–709. https://doi.org/10.1126/science.1221294

Esat, T.M., McCulloch, M.T., Chappell, J., Pillans, B., Omura, A., 1999. Rapid Fluctuations in Sea Level Recorded at Huon

Peninsula During the Penultimate Deglaciation. Science. 283, 197–201. https://doi.org/10.1126/science.283.5399.197

Foster, G.L., Rohling, E.J., 2013. Relationship between sea level and climate forcing by CO_2 on geological timescales. Proc. Natl. Acad. Sci. 110, 1209–1214. https://doi.org/10.1073/pnas.1216073110

Gasson, E., Siddall, M., Lunt, D.J., Rackham, O.J.L., Lear, C.H., Pollard, D., 2012. Exploring uncertainties in the relationship between temperature and sea level over the past 50 million years. Europe 1–35. https://doi.org/10.1029/2011RG000358.1.INTRODUCTION

Grant, K.M., Rohling, E.J., Bar-Matthews, M., Ayalon, A., Medina-Elizalde, M., Bronk Ramsey, C., Satow, C., Roberts, A.P., 2012. Rapid coupling between ice volume and polar temperature over the past 150,000 years. Nature 491, 744–747. https://doi.org/10.1038/nature11593

Grant, K.M., Rohling, E.J., Bronk Ramsey, C., Cheng, H., Edwards, R.L., Florindo, F., Heslop, D., Marra, F., Roberts, A.P., Tamisiea, M.E., Williams, F.H., 2014. Sea-level variability over five glacial cycles. Nat. Commun. 5, 5076. https://doi.org/10.1038/ncomms6076

Lambeck, K., Chappell, J., 2001. Sea level change through the last glacial cycle. Science (80-.). 292, 679–686.

Mix, A.C., Ruddiman, W.F., 1984. Oxygen-Isotope Analyses and Pleistocene Ice Volumes. Quat. Res. 21, 1–20. https://doi.org/10.1016/0033-5894(84)90085-1

Pico, T., Mitrovica, J.X., Ferrier, K.L., Braun, J., 2016. Global ice volume during MIS 3 inferred from a sea-level analysis of sedimentary core records in the Yellow River Delta. Quat. Sci. Rev. 152, 72–79. https://doi.org/10.1016/j.quascirev.2016.09.012

Raymo, M.E., Kozdon, R., Evans, D., Lisiecki, L., Ford, H.L., 2018. The accuracy of mid-Pliocene δ18O-based ice volume and sea level reconstructions. Earth-Science Rev. 177, 291–302. https://doi.org/10.1016/j.earscirev.2017.11.022

Rohling, E.J., Fenton, M., Jorissen, F.J., Bertrand, P., Ganssen, G., Caulet, J.P., 1998. Magnitudes of sea-level lowstands of the past 500,000 years. Nature 394, 162–165.

Rohling, E.J., Foster, G.L., Grant, K.M., Marino, G., Roberts, A.P., Tamisiea, M.E., Williams, F.H., 2014. Sea-level and deep-sea-temperature variability over the past 5.3 million years. Nature 508, 477–82. https://doi.org/10.1038/nature13230

Rohling, E.J., Grant, K.M., Bolshaw, M., Roberts, A.P., Siddall, M., Hemleben, C., Kucera, M., 2009. Antarctic temperature and global sea level closely coupled over the past five glacial cycles. Nat. Geosci. 2, 500–504.

Rohling, E.J., Grant, K.M., Hemleben, C., Siddall, M., Hoogakker, B.A.A., Bolshaw, M., Kucera, M., 2008. High rates of sea-level rise during the last interglacial period. Nat. Geosci. 1, 38–42.

Schrag, D.P., Hampt, G., Murray, D.W., 1996. Pore Fluid Constraints on the Temperature and Oxygen Isotopic Composition of the Glacial Ocean. Science. 272, 1930–1932. https://doi.org/10.1126/science.272.5270.1930

Shackleton, N.J., 1987. Oxygen isotopes, ice volume and sea level. Quat. Sci. Rev. 6, 183–190. https://doi.org/10.1016/0277-3791(87)90003-5

Siddall, M., Rohling, E.J., Almogi-Labin, A., Hemleben, C., Meischner, D., Schmelzer, I., Smeed, D.A., 2003. Sea-level fluctuations during the last glacial cycle. Nature 423, 853–858. https://doi.org/10.1038/nature01687.1.

Siddall, M., Smeed, D.A., Hemleben, C., Rohling, E.J., Schmelzer, I., Peltier, W.R., 2004. Understanding the Red Sea response to sea level. Earth Planet. Sci. Lett. 225, 421–434. https://doi.org/10.1016/j.epsl.2004.06.008

Skinner, L.C., Shackleton, N.J., 2005. An Atlantic lead over Pacific deep-water change across Termination I: Implications for the application of the marine isotope stage stratigraphy. Quat. Sci. Rev. 24, 571–580. https://doi.org/10.1016/j.quascirev.2004.11.008

Sosdian, S.M., Rosenthal, Y., 2009. Deep-Sea Temperature and Ice Volume Changes Across the Pliocene-Pleistocene Climate Transitions. Science (80-.). 325, 306–310. https://doi.org/10.1126/science.1229223

Spratt, R.M., Lisiecki, L.E., 2016. A Late Pleistocene sea level stack. Clim. Past 12, 1079–1092. https://doi.org/10.5194/cp-12-1079-2016

Stein, M., Wasserburg, G.J., Aharon, P., Chen, J.H., Zhu, Z.R., Bloom, A.L., Chappell, J., 1993. TIMS U-series dating and stable isotopes of the last interglacial event in Papua New Guinea. Geochim. Cosmochim. Acta 57, 2541–2554.

Thompson, W.G., Goldstein, S.L., 2006. A radiometric calibration of the SPECMAP timescale. Quat. Sci. Rev. 25, 3207–3215. https://doi.org/10.1016/j.quascirev.2006.02.007

Waelbroeck, C., Labeyrie, L., Michel, E., Duplessy, J.C., McManus, J.F., Lambeck, K., Balbon, E., Labracherie, M., 2002. Sea-level and deep water temperature changes derived from benthic foraminifera isotopic records. Quat. Sci. Rev. 21, 295–305. https://doi.org/10.1016/S0277-3791(01)00101-9

Yokoyama, Y., Esat, T.M., Lambeck, K., 2001. Coupled climate and sea-level changes deduced from Huon Peninsula coral terraces of the last ice age. Earth Planet. Sci. Lett. 193, 579–587. https://doi.org/10.1016/S0012-821X(01)00515-5

Yu, J., Broecker, W.S., 2010. Comment on "Pliocene-Pleistocene Climate Transitions ". Science. 328, 1480.

Dear Reviewers,

We provide hereunder an overview of how we address the comments, with explanations and references, on a point-by-point basis. Reviewers' comments are in black and our response in blue. In the manuscript, all the new/changed text is highlighted in yellow.

Reviewer(s)' Comments to Author:

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- 1) Quantification of deformation pattern: ~350 measured shoreline angles confirm a N-ward trend, rather than the previously assumed NW trend. □
- 2) Reassessment of uplift rates: this is very valuable, and the explanation of assumptions (i.e., lack of consensus on the Last Interglacial highstand timing or melt geometries) in the revised manuscript is much improved (and appreciated). □
- 3) Re-interpretation of the dated coral sea-level indicators, and sea-level history: using the updated uplift rates, the authors have recalculated sea levels for some of the previously dated samples (Chappell, 1974; Lambeck and Chappell, 2001). The new terrace morphology (and reef modelling) also hints at new, and potentially short-lived, features within the Huon terraces. □

The authors have addressed my (Reviewer 3) previous comments satisfactorily, but there remains some minor issues (detailed below). Once these have been addressed, I would see this manuscript making a good contribution to the sea-level community and beyond, and therefore recommend publication in *Nature Communications Earth and Environment*.

Again, we very much appreciate the time and effort by the reviewer to go through our manuscript in great detail, as well as the positive assessment of the manuscript.

Comments:
1. Isotopic methods: there is a slight confusion here. I appreciate you have added additional records to the Spratt and Lisiecki (2016) stack (Bintanja et al., 2005; Elderfield et al., 2012; Rohling et al., 2014) *BUT* these use different methodologies (indirect modelling, δ18O deconvolution using Mg/Ca, and marginal basin respectively) and are subject to different assumptions and uncertainties. Rather than detailed explanation, pleased *add* a sentence (or two) highlighting that what you term "isotopic methods" comprises several different methodologies.

The addition of SOM figure 1 is especially welcomed – thank you.

For information: ***Open ocean δ18O deconvolution*** (Elderfield et al., 2012; Sosdian and Rosenthal, 2009; Waelbroeck et al., 2002): Thank you for updating the manuscript to acknowledge the complexities associated with Waelbroeck et al. (2002) record (note that the vertical uncertainties ~ ± 13 m). Elderfield et al. (2012)/Sosdian and Rosenthal (2009) subtract the temperature component from North Atlantic/Pacific benthic δ18O records using MgCa. Potential MgCa calibration and other complicating factors (e.g., Yu and Broecker, 2010) are associated with these records, and they have vertical uncertainties approx.. ± 35 m (1 sigma).

Marginal basin method: (a) Red Sea (Grant et al., 2014, 2012; Rohling et al., 2008, 1998; Siddall et al., 2003) and (b) Mediterranean (Rohling et al., 2014) utilises hydraulic control on water mass
exchange, residence time, salinity and δ18O (Siddall et al., 2004, 2003). The Red Sea method relies upon the hydraulic control of water mass exchange through the narrow Bab-el-Mandab Strait into the semi-enclosed (and highly evaporative) basin. The constriction of flow between a semi- enclosed basin and the open ocean with reduced sea levels, thereby increases the residence time of water in the basin, leading to pronounced salinity and δ18Osw changes in the basin. Hydrographic processes, and their effect on δ18Oc, of the Mediterranean Sea is more complex with large riverine inputs, temperate and African Monsoon climatic influences, as well as a larger strait. Probabilistic assessment gives vertical uncert. ± 3.5 m/6.3 m (Red/Med) at 95 % confidence interval.

Inverse forward modelling approach (i.e., joint ice sheet, temperature and δ18O modelling) (Bintanja et al., 2005; de Boer et al., 2014a) use a coupled model of ice sheets and temperature (Bintanja et al., 2002; de Boer et al., 2014b) matched to an oxygen isotope record to quantify temperature and sea-level contributions simultaneously (uncert ~±10 m).

We did not feel like adding a lot of text on this topic, as we already mention in the introduction: "many calibration techniques have been proposed for the derivation of GMSL curves from δ^{18}O-records, which has led to a broad range of GMSL estimates" (lines 42-45), but we added on line 177 "obtained with several different methodologies relying on different assumptions," to emphasize that the δ^{18}O-derived sea-level curves are not all of the same type.

2. Discrepancy between 'isotopic' and other sea-level indicators: Personally, I feel this is still overstated given that this is not a novel conclusion. However, you present a very nice corroboration from the dated Huon fossil corals, although the re-evaluated fossil coral data presented in Figure 3 (bar MIS 6b and the MIS3/2transition) overlap within uncertainties with error envelope of Spratt and Lisiecki (2016), and most likely with the other records in SOM Fig 1. The additional discussion added is very welcome, and has placed your new study within the wider (and extensive literature) on these topics – i.e., the discrepancy in magnitude (Dalton et al., 2019; Duplessy et al., 2002; Pico et al., 2016; Waelbroeck et al., 2002) and deficiencies in the open ocean methods (Adkins et al., 2002; Chappell and Shackleton, 1986; Schrag et al., 1996; Shackleton, 1987; Skinner and Shackleton, 2005; Spratt and Lisiecki, 2016; Waelbroeck et al., 2002).

The new discussion added in this iteration on the secular variation (Fig 5 and SOM) is interesting, but again this builds upon a much wider body of work (Abe-Ouchi et al., 2013; Berends et al., 2021; Chappell and Shackleton, 1986; Clark and Mix, 2002; Foster and Rohling, 2013; Gasson et al., 2012; Mix and Ruddiman, 1984; Raymo et al., 2018; Spratt and Lisiecki, 2016; Thompson and Goldstein, 2006). I'm not sure you need this, nor what it really adds to the paper, but if you keep this section (& I leave this decision with the authors), adding a few additional references to acknowledge some of the existing work (to complement the current reliance on Shakun et al., 2016) is needed.

We prefer to keep this section and the figure, as it provides some context on larger climatic implications, but agree that a few additional references would be a fair addition. We have now changed

"The non-linear relationship between GMSL and paleotemperature suggests that within each glacial cycle, cooling of the globe preconditions the Earth so that major ice-sheets can grow[62]. As our Huon RSL estimates suggest an even stronger degree of non-linearity between GMSL and paleotemperature/CO2 (solid arrows in Fig. 5a), such a temperature-preconditioned response of ice sheets to global cooling may thus have been more pronounced than previously imagined."

to

"This non-linear relationship between GMSL and paleotemperature has been described before, and can be attributed to (a combination of) delayed glacial isostatic rebound (Oerlemans, 1980; Pollard, 1982; Abe-Ouchi et al., 2013), differences in glacial tresholds of the different ice-sheets (Gasson et al. 2012; De Boer et al., 2012), and cooling of the globe as a precondition for ice sheet growth (Shakun et al., 2016). Whichever of these mechanisms is dominant to the relationship between GMSL and paleotemperature/CO_2 (solid arrows in Fig. 5a), our Huon RSL estimates suggest that the effect of non-linear processes has been more pronounced than previously imagined."

(Lines 293-300).

3. Uncertainties: please add error estimates for ages of the MIS 5a and 5c highstands, both the original authors and those you derive (line 148)

We added uncertainties here now as suggested (lines 154-155).

4. Recalculation of RSL for dated corals (Chappell, 1974; Lambeck and Chappell, 2001):4.1. This really is the elevation, not RSL as you haven't included palaeo water depth (this can be up to 15 m, cf. Yokoyama et al 2001). You don't need to change this, as it's fairly obvious, but it is useful to keep in mind that the revised elevations would be a minimum sea-level estimate. 4.2. Why only use only some of the available data? What about others - e.g., Cutler et al., 2003; Esat et al., 1999; Stein et al., 1993; Yokoyama et al., 2001?

4.1: we did take water depth of coral reef terraces into account (lines 491-492), which should be around ~1 m according to the indicative meaning calculator of Lorscheid et al. (2019). We agree that individual coral samples may be formed many more meters below sea-level (as in Yokoyama et al., 2001), but that the shoreline angles of coral reef terraces should be much closer to past RSL (see Fig. 2a for example). Part of the motivation to carry out terrace sequence modeling section was to investigate the possibility of terraces forming several meters below sea-level (lines 224-240), and we found that this possibility "could have lead to misinterpretations in Huon RSL highstand elevation, up to ~10-15 m, and age, up to ~10 ka, for the T31 (MIS 11c) terrace, and that values gradually increase with age." (lines 234-236).

4.2: we are not sure to understand the question. We did not use dated coral reef samples to obtain RSL elevations, we merely used a reasonable (and broad) range of possible ages and RSL elevations for the upper MIS 5e terrace, and then used the well-preserved coral reef terrace morphology to calculate RSL for all the other terraces. We compare our data with Lambeck and Chappell (2001) in Fig. 3, who based their RSL elevations on all of the references (and more) mentioned by the reviewer, as well as with Chappell and Shackleton (1986) for the lowstands. We showed the data of Cutler et al. (2003), Chappell et al. (1996) and Yokoyama et al. (2001) in Figure 4 to compare the reef models with independent ages. In Supplementary Table 1, we compare our proposed terrace ages with published ages of all the references mentioned by the reviewer. We think further comparison between our data and published coral ages is not necessary.

5. Adding a table to the Supplementary Information with the recalculated uplift rates (and uncertainties) for the principal locations of the dated fossil terraces would be very useful to the community (they are in the figures but it would take very little effort to add a table with the propagated uncertainties).

Good suggestion, we have made such a table, which is the new Supplementary Data 2, and refer to it in lines 159-160: "We give updated uplift rate estimates for the locations of the dated samples in Supplementary Data 2."

6. SOM figure 1 and line 169: This should be 10 rather than 11 methods – The Grant et al. (2014) and Rohling et al. (2009) record differ only in their chronologies, and the Grant'14 assessment is the most up to date assessment and should be used in preference to Rohling et al. (2009).

For background: The original Red Sea record was presented in Rohling et al. (1998) and Siddall et al. (2003), extended to 500 ka (Rohling et al., 2009), with additional Last Interglacial analyses (Rohling et al., 2008). The chronology for the last 150 ka was updated (Grant et al., 2012) and subsequently extended to the full record (Grant et al., 2014). The Grant et al. (2014) is currently this group's most up-to-date assessment of the Red Sea sea-level record, and should be used in preference to the earlier versions.

Fair point, we changed the text to 10 methods (line 176), and changed Supplementary Fig. 1 accordingly, also in the caption.

7. What are the typical differences between the minimum and maximum shoreline angles? Could you add a range to the *Methods* section so readers can have a sense of the magnitude of the difference in elevation.

We now added "Typical differences between minimum and maximum shoreline angle elevations are on the order of ~2-10 m, with some extremes of a few 10s of m for the older coral reef terraces (Supplementary Data 2, 3)" on lines 486-489.

8. Clarification line 485: "following careful assessment of dated sample" – what did this involve?

From the Lambeck and Chappel (2001) paper, reference 57: *"The ice-volume equivalent sea level estimates in Fig. 3, A and B, are based on a variety of previously published and some unpublished results. The results from 125,000 to about 5000 years ago are from sources listed in (15–19, 27, 30–35, 38, 39, 94)."*

We do not think it is necessary to repeat all those references in our paper, especially since we do not know which ones exactly were considered to obtain the 127±2 ka age for the upper MIS 5e terrace. We changed this sentence to: "following assessment of dated samples from both Huon and other sites (see their paper for details)." (line 502)

9. Typos: replace "CO2" with "CO_2" (lines 281, 285 etc);
extra 'character' (box) in "Pa s" lines 537, and 538;
line 539 change "earth" to "Earth"

Changed as suggested.

References:

Abe-Ouchi, A., Saito, F., Kawamura, K., Raymo, M.E., Okuno, J., Takahashi, K., Blatter, H., 2013. Insolation-driven 100,000- year glacial cycles and hysteresis of ice-sheet volume. Nature 500, 190–193. https://doi.org/10.1038/nature12374

Adkins, J.F., Mcintyre, K., Schrag, D.P., 2002. The Salinity , Temperature , and d 18 O of the Glacial Deep Ocean. Science (80-.). 298, 1769–1773. https://doi.org/10.1126/science.1076252

Berends, C.J., de Boer, B., van de Wal, R.S.W., 2021. Reconstructing the evolution of ice sheets, sea level, and atmospheric CO2during the past 3.6 million years. Clim. Past 17, 361–377. https://doi.org/10.5194/cp-17-361-2021

Bintanja, R., van de Wal, R.S.., Oerlemans, J., 2002. Global ice volume variations through the last glacial cycle simulated by a 3-D ice-dynamical model. Quat. Int. 95–96, 11–23. https://doi.org/10.1016/S1040-6182(02)00023-X

Bintanja, R., van de Wal, R.S.W., Oerlemans, J., 2005. Modelled atmospheric temperatures and global sea levels over the past million years. Nature 437, 125–8. https://doi.org/10.1038/nature03975

Chappell, J., 1974. Geology of coral terraces, Huon Peninsula, New Guinea: A study of Quaternary tectonic movements and sea-Level changes. Geol. Soc. Am. Bull. 85, 553–570.

Chappell, J., Shackleton, N.J., 1986. Oxygen isotopes and sea level. Nature 324, 137–140 https://doi.org/10.1038/324137a0

Clark, P.U., Mix, A.C., 2002. Ice sheets and sea level of the Last Glacial Maximum. Quat. Sci. Rev. 21, 1–7. https://doi.org/10.1016/S0277-3791(01)00118-4

Cutler, K.B., Edwards, R.L., Taylor, F.W., Cheng, H., Adkins, J.F., Gallup, C.D., Cutler, P.M., Burr, G.S., Bloom, A.L., 2003. Rapid sea-level fall and deep-ocean temperature change since the last interglacial period. Earth Planet. Sci. Lett. 206, 253–271.

Dalton, A.S., Finkelstein, S.A., Forman, S.L., Barnett, P.J., Pico, T., Mitrovica, J.X., 2019. Was the Laurentide Ice Sheet significantly reduced during Marine Isotope Stage 3? Geology 47, 111–114. https://doi.org/10.1130/G45335.1

de Boer, B., Lourens, L.J., van de Wal, R.S.W., 2014a. Persistent 400,000-year variability of Antarctic ice volume and the carbon cycle is revealed throughout the Plio-Pleistocene. Nat. Commun. 5, 2999. https://doi.org/10.1038/ncomms3999

de Boer, B., Stocchi, P., van de Wal, R.S.W., 2014b. A fully coupled 3-D ice-sheet–sea-level model: algorithm and applications. Geosci. Model Dev. 7, 2141–2156.

Duplessy, J.C., Labeyrie, L., Waelbroeck, C., 2002. Constraints on the ocean oxygen isotopic enrichment between the last glacial maximum and the holocene: Paleoceanographic implications. Quat. Sci. Rev. 21, 315–330. https://doi.org/10.1016/S0277-3791(01)00107-X

Elderfield, H., Ferretti, P., Greaves, M., Crowhurst, S., McCave, I.N., Hodell, D., Piotrowski, A.M., 2012. Evolution of Ocean Temperature and Ice Volume Through the Mid-Pleistocene Climate Transition. Science (80-.). 337, 704–709. https://doi.org/10.1126/science.1221294

Esat, T.M., McCulloch, M.T., Chappell, J., Pillans, B., Omura, A., 1999. Rapid Fluctuations in Sea Level Recorded at Huon Peninsula During the Penultimate Deglaciation. Science. 283, 197–201. https://doi.org/10.1126/science.283.5399.197

Foster, G.L., Rohling, E.J., 2013. Relationship between sea level and climate forcing by CO2 on geological timescales. Proc. Natl. Acad. Sci. 110, 1209–1214. https://doi.org/10.1073/pnas.1216073110

Gasson, E., Siddall, M., Lunt, D.J., Rackham, O.J.L., Lear, C.H., Pollard, D., 2012. Exploring uncertainties in the relationship between temperature and sea level over the past 50 million years. Europe 1–35. https://doi.org/10.1029/2011RG000358.1.INTRODUCTION

Grant, K.M., Rohling, E.J., Bar-Matthews, M., Ayalon, A., Medina-Elizalde, M., Bronk Ramsey, C., Satow, C., Roberts, A.P., 2012. Rapid coupling between ice volume and polar temperature over the past 150,000 years. Nature 491, 744–747. https://doi.org/10.1038/nature11593

Grant, K.M., Rohling, E.J., Bronk Ramsey, C., Cheng, H., Edwards, R.L., Florindo, F., Heslop, D., Marra, F., Roberts, A.P., Tamisiea, M.E., Williams, F.H., 2014. Sea-level variability over five glacial cycles. Nat. Commun. 5, 5076. https://doi.org/10.1038/ncomms6076

Lambeck, K., Chappell, J., 2001. Sea level change through the last glacial cycle. Science (80- .). 292, 679–686. Mix, A.C., Ruddiman, W.F., 1984. Oxygen-Isotope Analyses and Pleistocene Ice Volumes. Quat. Res. 21, 1–20. https://doi.org/10.1016/0033-5894(84)90085-1

Pico, T., Mitrovica, J.X., Ferrier, K.L., Braun, J., 2016. Global ice volume during MIS 3 inferred from a sea-level analysis of sedimentary core records in the Yellow River Delta. Quat. Sci. Rev. 152, 72–79. https://doi.org/10.1016/j.quascirev.2016.09.012

Raymo, M.E., Kozdon, R., Evans, D., Lisiecki, L., Ford, H.L., 2018. The accuracy of mid-Pliocene δ18O-based ice volume and sea level reconstructions. Earth-Science Rev. 177, 291–302. https://doi.org/10.1016/j.earscirev.2017.11.022

Rohling, E.J., Fenton, M., Jorissen, F.J., Bertrand, P., Ganssen, G., Caulet, J.P., 1998. Magnitudes of sea-level lowstands of the past 500,000 years. Nature 394, 162–165.

Rohling, E.J., Foster, G.L., Grant, K.M., Marino, G., Roberts, A.P., Tamisiea, M.E., Williams, F.H., 2014. Sea-level and deep-sea-temperature variability over the past 5.3 million years. Nature 508, 477–82. https://doi.org/10.1038/nature13230

Rohling, E.J., Grant, K.M., Bolshaw, M., Roberts, A.P., Siddall, M., Hemleben, C., Kucera, M., 2009. Antarctic temperature and global sea level closely coupled over the past five glacial cycles. Nat. Geosci. 2, 500–504.

Rohling, E.J., Grant, K.M., Hemleben, C., Siddall, M., Hoogakker, B.A.A., Bolshaw, M., Kucera, M., 2008. High rates of sea-level rise during the last interglacial period. Nat. Geosci. 1, 38–42.

Schrag, D.P., Hampt, G., Murray, D.W., 1996. Pore Fluid Constraints on the Temperature and Oxygen Isotopic Composition of the Glacial Ocean. Science. 272, 1930–1932. https://doi.org/10.1126/science.272.5270.1930

Shackleton, N.J., 1987. Oxygen isotopes, ice volume and sea level. Quat. Sci. Rev. 6, 183–190. https://doi.org/10.1016/0277-3791(87)90003-5

Siddall, M., Rohling, E.J., Almogi-Labin, A., Hemleben, C., Meischner, D., Schmelzer, I., Smeed, D.A., 2003. Sea-level fluctuations during the last glacial cycle. Nature 423, 853–858. https://doi.org/10.1038/nature01687.1.

Siddall, M., Smeed, D.A., Hemleben, C., Rohling, E.J., Schmelzer, I., Peltier, W.R., 2004. Understanding the Red Sea response to sea level. Earth Planet. Sci. Lett. 225, 421–434. https://doi.org/10.1016/j.epsl.2004.06.008

Skinner, L.C., Shackleton, N.J., 2005. An Atlantic lead over Pacific deep-water change across Termination I: Implications for the application of the marine isotope stage stratigraphy. Quat. Sci. Rev. 24, 571–580. https://doi.org/10.1016/j.quascirev.2004.11.008

Sosdian, S.M., Rosenthal, Y., 2009. Deep-Sea Temperature and Ice Volume Changes Across the Pliocene-Pleistocene Climate Transitions. Science (80-.). 325, 306–310. https://doi.org/10.1126/science.1229223

Spratt, R.M., Lisiecki, L.E., 2016. A Late Pleistocene sea level stack. Clim. Past 12, 1079–1092. https://doi.org/10.5194/cp-12-1079-2016

Stein, M., Wasserburg, G.J., Aharon, P., Chen, J.H., Zhu, Z.R., Bloom, A.L., Chappell, J., 1993. TIMS U-series dating and stable isotopes of the last interglacial event in Papua New Guinea. Geochim. Cosmochim. Acta 57, 2541–2554.

Thompson, W.G., Goldstein, S.L., 2006. A radiometric calibration of the SPECMAP timescale. Quat. Sci. Rev. 25, 3207– 3215. https://doi.org/10.1016/j.quascirev.2006.02.007

Waelbroeck, C., Labeyrie, L., Michel, E., Duplessy, J.C., McManus, J.F., Lambeck, K., Balbon, E., Labracherie, M., 2002. Sea-level and deep water temperature changes derived from benthic foraminifera isotopic records. Quat. Sci. Rev. 21, 295–305. https://doi.org/10.1016/S0277-3791(01)00101-9

Yokoyama, Y., Esat, T.M., Lambeck, K., 2001. Coupled climate and sea-level changes deduced from Huon Peninsula coral terraces of the last ice age. Earth Planet. Sci. Lett. 193, 579–587. https://doi.org/10.1016/S0012- 821X(01)00515-5

Yu, J., Broecker, W.S., 2010. Comment on "Pliocene-Pleistocene Climate Transitions ". Science. 328, 1480.

We thank the reviewer for these interesting references, some of which we did not know yet. We hope the manuscript is now acceptable for publication.

Kind regards,

Gino De Gelder, Laurent Husson, Anne-Morwenn Pastier, David Fernández-Blanco, Tamara Pico, Denovan Chauveau, Christine Authemayou and Kevin Pedoja