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Interactive comment on “Interglacial and glacial variability from the last 800 ka in marine, ice and terrestrial archives” by N. Lang and E. W. Wolff

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We thank the reviewer for his positive endorsement that this will be a useful paper for modellers, and for his careful reading of our paper. He has correctly identified many places where we can be more precise to the benefit of the paper. Comments by the referee are labelled (R), and followed by the authors' response (A).

R: The authors provide an extensive discussion of marine, ice and terrestrial records covering the last 800 ka, which satisfy a number of criteria such as continuity, resolution and length. They concentrate on the strengths of glacial and interglacial periods according to the usual MIS nomenclature, but they also partly critique this nomenclature, based on a more thorough discussion of the dynamics of MIS 7, 13 and 15. The authors are generally very lucid about the potential caveats of their analyses (for example,

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the synchronisation of records). The resulting article is well balanced, well documented and, I believe, will constitute a high value reference to scientists interested in modelling the past state of the atmosphere and ocean with general circulation models, as well as those more interested in the dynamics of glacial-interglacial cycles. Hopefully, the authors will be able to release this compilation in the public domain in the form of a digital file.

A: We agree. We are checking with people who provided data that is not already publicly available, and hope to attach a file as supplementary data.

R: There is just one think that really annoyed me. The paper mentions at place 'estimated uncertainties', 'analytical uncertainties', 'pooled uncertainties' and these refer to intervals (e.g. : ± 1 ka); a statistician would prefer to speak about a 'confidence interval' (frequentist) or 'credible interval' (Bayesian). For example, rather than analytical uncertainty, the statistician would mention "an error on the measure with a credible interval (66 %) of ± 1 ka". The purpose of this comment is to draw the attention of the authors about the fact that the language used to express uncertainties is very loose in palaeoclimatology and any effort to get closer to standard practices in statistics will make our life easier in the future.

A: The reviewer is correct that the terminology in palaeoclimate is very loose (as is the practice for estimating confidence intervals). In our defence, we have in most cases simply used the terminology that the authors of original papers used. Because they have not treated their uncertainties in the same way we need to choose any common language carefully. To try to be as precise as possible, we have therefore now used the terminology "quoted confidence intervals" where appropriate, with the word "quoted" emphasising that we do not necessarily endorse the way in which authors have derived these.

R: Apart from this point which, I concede, cannot entirely be addressed within the context of this paper, I have no major comment. However, there are editorial weaknesses

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at a number of places. In the following, 'Edit again' means that I estimated the language to be too informal/not enough specific for a scientific paper.

R: p. 2224, l. 15: show some tendency edit again

A: Edited. "Strong interglacials, particularly in the marine isotopic records, tend to follow strong glacials"

R: p. 2224, l. : The climate of the recent third of the Quaternary : the saw-tooth pattern is only really obvious over the last 4 glacial-interglacial cycles. Before the mid-Brünhes, cycles were more sine-like.

A: True. We have removed "long" and shorter" from the first sentence and added after the sentence: "In the 4 most recent climate cycles, the glacial periods are much longer than the interglacials".

R: p. 2225, l. 10: "model glacial cycle": do you mean a 'typical' glacial cycle?

A: Yes. Altered.

R: p. 2225, l. 16: the Antarctic ice core record

A: Corrected.

R: p. 2225, l. 21: in Antarctic temperature every glacial is different : edit again.

A: Changed to "reveals that there are differences between every interglacial in the Antarctic ice core record"

R: p. 2225, l. 28: the global pattern of response: This is an interesting point. Response implies that there is an identifiable cause, such as the astronomical forcing. Yet, it is reckoned that a certain level of stochasticity exists in climatic dynamics, even at glacial-interglacial time scale. This stochasticity may explain, in part, differences among different interglacials. Consequently, definite causes of a given climate state at a given time cannot necessarily be identified in a deterministic fashion. One possible

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recommendation is to speak about ‘climate signatures’, or ‘patterns of climate changes’ rather than ‘climate response’. The same remark applies for the conclusion lines.

A: We agree. We now use the phrase “global spatial signature”

R: p. 2226, l. 5: ‘state-of-the-art model’: well; we, modellers, all pretend to be at the state of the art of our discipline, don’t we? But we chose to model different things. I would therefore recommend to be more explicit and speak about ‘general circulation models of the ocean and atmosphere’

A: Edited.

R: p. 2226, l. 20: ‘discover’: I do not understand the use of this word in the present context.

A: While we think discover is an acceptable usage, we have changed it to “identify”.

R: p. 2227, l. 14: ‘parameters’: ‘variables’ is to be preferred.

A: Edited.

R: p. 2227, ll. 21-24: ideally the resolution should be reasonably homogeneous throughout the record to avoid biases in the intercomparison of different interglacials. Can you briefly comment on this?

A: This is true, where resolution is low the interglacial or glacial peak could be absent from the record, hence our selection of records with resolution better than 3-4ka. Resolution is fairly homogenous - particularly for the ice core and terrestrial records. For the marine records there is greater variability, but there isn’t a consistent pattern of e.g. lower resolution in the earlier parts of the records. Typically resolution isn’t lower than 5ka in the lowest resolution sections of the records; the only exceptions are the early parts of ODP 982 sst, MD97-2140 sst, and parts of DSDP 607 bwt. If interested, the reader could check resolution at various parts of the records in the supplemental data table we plan to add.

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R: p. 2228, l. 16: are precession and obliquity also taken from Laskar? If yes then the Laskar reference should best come after 'precession'.

A: Yes. Done.

R: p. 2229, l. 5: tuned 'by' -> tuned 'to' p. 2230, l. 9: analysed 'from' -> analysed 'in' ?

A: Changed to "tuned to" and "analysed for"

R: p. 2230, l. 11 saturates at high sea-ice extent. Is it the proxy saturating at high sea-ice extent, or sea-ice saturating at high continental ice volume, or both?

A: It is saturating at high sea ice extents. The reason is (we believe but have still to test quantitatively) that, when sea ice is already extended far from the continent, additional ice is so far from the ice core site that it contributes little sea salt that can reach Antarctica. Thus the additional proxy response falls off as ice extent increases.

R: p. 2231, l. 1: 'very interesting' seems superfluous. The following discussion is enough to communicate to the reader that you indeed found it interesting.

A: Ok, removed.

R: p. 2232, l. 8: 'use these records in conjunction with other climate proxies from the same record': edit again. Do you mean: 'these proxies in conjunction with other climate proxies from the same archive'?

A: The interpretation of magnetic susceptibility is sufficiently difficult that we hesitate to call it a proxy (which raises the question "a proxy for what?"). Changed to "these data in conjunction with other datasets from the same sequences".

R: p. 2232, l. 23: 'ka-averaged': please explain

A: Each data point in LR04 stack is an average of all the 57 records' points in that 1ka interval.

R: p. 2232, l. 28: 'mid-transitions': are these the mid-points of the terminations?

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A: Corrected

R: p. 2233, l. 4: 17 $\delta^{18}\text{O}$ records: are these those of the LR04 stack?

A: 10 of the 17 $\delta^{18}\text{O}$ records are benthic; these 10 were included in the LR04 stack.

R: p. 2233, l. 21-23: is the linear interpolation needed because there are planktic data at depths for which there is no benthic data, and thus no LR04 age? Could you please clarify/confirm this?

A: This is on page 2234. Yes, this is correct, in some of the records benthic and planktonic samples were not collected from the same depth. The interpolation uses those depths for which there are LR04 ages as control points.

R: p. 2236, l. 9: some garbage left.

A: Corrected. In proof, not original manuscript.

R: p. 2229, l. 16: a section of data with a small deviation from a constant level. I do not understand this. RAMPFIT requires a data section in which there is a ramp, so a large deviation from a constant level.

A: Page 2236. First part corrected. And yes, this should read "the criterion for the selected interval was that the start and end points should be within a section of data with a small deviation from a constant level", i.e. the interglacial and glacial sections have a small standard deviation.

R: p. 2229, l. 23: 'blue for glacial, and purple for amplitude': edit again

A: This is on page 2236. We assume the referee is asking for clarification, thus "(dark blue to light blue for the strength of glacials, and dark purple to light purple for the amplitude of terminations)"

R: p. 2237, l. 12: 'more sensitive proxies' : same remark as for sea-salt : is this that the proxy is more sensitive to SST, or is this that SST is more sensitive to... to what in

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fact? To changes in ice volume, astronomical forcing, or to a sort of natural variability among interglacials (see my earlier comment)

A: Interesting point. We meant “more sensitive” in the sense that they are sensitive to the overall state of the system, but we accept that this is a little circular. Changed to “when considering different glacials, . . . are more variable than other proxies”.

R: p. 2241, l. 14: ‘collective view in the community’: I would recommend to be a bit more neutral; for example: SST records contrast with the perception emanating from the isotopic records, according to which MIS 16 is a particularly strong glacial.

A: Agreed. Wording changed accordingly.

R: p. 2242, l. 12: ‘Again with this pattern TVII somewhat breaks the pattern’: edit again

A: Line 17: “within this pattern” removed.

R: p. 2243, l. 15: well resolved -> well-resolved. p. 2243, l. 16 : ‘if 7.3 had been taken as the true interglacial at Baikal, then its strength would be greater than MIS 19’ : is this a complicated way of saying that in the Baikal record 7.3 is stronger than MIS 19, or did I miss something?

A: Yes, you didn’t miss anything. Text edited to clarify.

R: l. 2254, Table 1: give the unit of the resolution in the legend (ka) and briefly explain what ‘ka-average’ is.

A: Done

R: l. 2255, Table 2: based on. . . what ?

A: Rephrased for clarification.

R: Figure 4 does not read very well. Some better-positioned legend could increase its legibility. Think this is the kind of graphic one would like to insert in a slide-show: it has to speak by itself.

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A: The figure has been reformatting to, we hope, a better format for a slide, with a repositioned legend.

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R: Final remark: experts recommend to use 'astronomical forcing' rather than 'orbital forcing' because obliquity is not an orbital element.

A: A good point; this has been altered in the manuscript.

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Interactive comment on Clim. Past Discuss., 6, 2223, 2010.

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