11/08/2021

Response letter to comments on *Hydroclimatic variability of opposing late Pleistocene climates in the Levant revealed by deep Dead Sea sediments* (CP-2020-161), by Ben Dor and colleagues

Dear Editor,

We wish to thank you and the reviewers for completing the review of this manuscript in a timely manner during those complicated times. After reading carefully through the comments, which we consider of prime importance for improving the manuscript, we have carried out our best efforts to address all the comments made by the reviewers in order to improve the manuscript's quality and to clarify its implications so that it meets the high standards of *Climate of the Past*.

Our impression from the comments is that the reviewers possess substantial knowledge within the scope of the paper and that their attitude towards the manuscript is overall positive. We have therefore followed their review and corrected the entire manuscript; expanding the text parts where required, recalculated the necessary analyses and rewritten any relevant part that addresses the updated results and their interpretation, and we hope that you would find the modified manuscript suitable for publication in *Climate of the Past*. Please find below our detailed response to the comments made by the reviewers.

On behalf of all authors,

Dr. Yoav Ben Dor

Response to Comment by Prof. Pierre Francus (Editor)

Response to general comments:

Dear Prof. Pierre Francus,

We wish to thank you and the reviewers for completing this review in a timely manner during those complicated times. After reading carefully through the comments, which we consider of prime importance for improving the manuscript, we believe that these comments have contributed substantially to the quality and clarity of the manuscript, and improved its implications for paleoclimate research in the eastern Mediterranean.

We have followed the comments and have recalculated the necessary analyses so that all parts of the discussion and conclusions are now backed by the proper analyses of the data and its interpretation.

We have corrected the entire manuscript, including calculations, text and figures, in accordance with the reviewers' comments and have further removed any questionable segments that were not robust enough, or that are insufficiently coherent. Please find our detailed response to the comments made by you and the reviewers.

On behalf of all authors,

Dr. Yoav Ben Dor

Response to specific comments:

Comment: As suggested in referee 3 comments (RC3), please reorganized the labelling of figures. The current numbering is confusing, especially at the beginning. Please verify the numbers in the call for figures in the text. I have the feeling that there are some inconsistencies.

Response: The labelling of the figures was corrected and is now in accordance with the text. We have additionally split the first figure into two figures, as suggested by the reviewers, and reduced the number of supplementary figures.

Comment: As suggested in RC2, the supplementary material is plentiful, and will be overwhelming for most of the readership of Climate of the Past. Try to remove what is not necessary.

Response: The supplementary was substantially reduced and now includes only figures of relevant calculations directly addressed in the modified manuscript.

Comment: I agree with RC3 that referring to "non-persistent periodic[al] components of 2-6 years" in ll. 369-370 appears more like wishful thinking than proper interpretation of the obtained results.

Response: Following these comments we have recalculated the wavelet spectra using an area-wise false positive estimation, which demonstrates that this is indeed the case. We agree with this notion, so the results and discussion sections were rephrased in accordance with the updated calculations and in accordance with additional comments made by the other reviewers.

Comment: The interpretation is based on the comparison between the "periodic components" of the current synoptic conditions (ll. 334-335 and 424-448), with the "periodic components" of your records. However, it is not discussed how sure we are that the current synoptic conditions are similar to the ones in isotopic stage 2, and even if the conditions were similar during the two time-intervals considered here for analysis, 18 ka and 27 ka. Indeed, during the Pleistocene, the presence of large polar ice caps has deflected the jet streams and many other systems towards the equator. This should be discussed.

Response: It is true that we cannot unambiguously determine which synoptic systems affected the eastern Mediterranean during the LGM. Nevertheless, there is no obvious reason to suggest that is should have been significantly different. Several studies are dealing with this question, and they support the general notion that synoptic circulation patterns in the past were similar to present (Greenbaum et al., 2006; Amit et al., 2011; Enzel et al., 2008), with some possible modifications of their spatial characteristics (e.g., Goldsmith et al., 2017). A section addressing this issue was added to the updated manuscript (see lines: 485-493, 596-606).

Response to technical corrections:

All technical corrections were be corrected accordingly.

Cited references:

Amit, R., Simhai, O., Ayalon, A., Enzel, Y., Matmon, A., Crouvi, O., Porat, N., and McDonald, E.: Transition from arid to hyper-arid environment in the southern Levant deserts as recorded by early Pleistocene cummulic Aridisols, Quaternary Science Reviews, 30, 312-323, 2011.

Enzel, Y., Amit, R., Dayan, U., Crouvi, O., Kahana, R., Ziv, B., and Sharon, D.: The climatic and physiographic controls of the eastern Mediterranean over the late Pleistocene climates in the southern Levant and its neighboring deserts, Global and Planetary Change, 60, 165-192, 2008.

Goldsmith, Y., Polissar, P., Ayalon, A., Bar-Matthews, M., and Broecker, W.: The modern and Last Glacial Maximum hydrological cycles of the Eastern Mediterranean and the Levant from a water isotope perspective, Earth Planet. Sci. Lett., 457, 302-312, 2017.

Greenbaum, N., Ben-Zvi, A., Haviv, I., and Enzel, Y.: The hydrology and paleohydrology of the Dead Sea tributaries, Geological Society of America Special Papers, 401, 63-93, 2006.

Response to Report #1

By Anonymous Referee #1

Response to general comments:

We appreciate the reviewer's comments and overall positive attitude towards the manuscript. We have adjusted the discussion to address the comments raised by the reviewer, such as elaborating on the possible role of different precipitation sources on the hydrologic and the isotopic signal of the studied sediments. We have substantially modified the introduction and the discussion so that the modes of laminae formation and water sources are explained, and have made sure that the conclusions are supported by the data and its analyses and are clearly distinct from conjectures. Considering the other reviews, we acknowledge the notion that no distinct periodic component is clearly identified in the records, and the discussion and conclusions accordingly were modified accordingly.

Response to specific comments:

Comment: This maybe in the other papers by the author team, but can you distinguish between a detrital laminae with sub-layers and a period with no aragonite laminae deposition? From this paper it appears the assumption is that you will always have an aragonite sub-layer?

Response: The nature of the sediments and the way that we understand their formation, according to modern analogues and previous detailed investigations of available exposures, suggest that detritus-aragonite couplets are deposited annually, thus forming varves. This is further supported by our microfacies analyses based on continuously sampled thin sections, in which we observe even slight changes in the sediments in details. This is further supported by previous studies of the Dead Sea sedimentary record (e.g., Stein et al., 1997; Marco et al., 1996), the study of modern lakes by monitoring and recent cores, and the agreement between laminae counting and independent radiometric dating such as ^{14}C and U-Th (Prasad et al., 2009; Haase-Schramm et al., 2004). Thus, because no deposition of alternating aragonite and detritus takes place under modern conditions in the Dead Sea (e.g., Ben Dor et al., 2021), the interpretation of alternating aragonite and detritus facies as annual deposits is, to some extent, a (pretty solid) assumption, as it cannot be directly and unambiguously determined for the studied interval of Lake Lisan (e.g., Prasad et al., 2004; See lines 167-184, 204-213; Ben Dor et al., 2019).

Comment: Please make it clear throughout which data are new here and which are from Ben Dor et al., 2018 e.g. Figure 1 looks very similar to figure panels from that paper.

Response: This is now clarified both in the methods section and in relevant figure captions. The only data that was previously published is the series of annual flood frequency (number of sublaminae), which was published by Ben Dor et al., 2018.

Comment: Is it possible to be more precise with the ages? This may be discussed in the other paper in more detail, but a bit more detail of the chronology would be useful for readers who approach your work through this paper.

Response: This is now elaborated in the text (see lines 150-154)

Comment: As I suggest above, I'm not convinced by the wavelet analysis presented in Figure 7 as a strong support for your hypotheses of persistent cycles, even in wetter periods.

Response: This section was fully revised following the technical comments on the appropriate false-positive detection methods, and now includes updated calculations based on an area-wise estimation (Schulte, 2016, 2019), rather than the original point-wise approach (Grinsted et al., 2004; Torrence and Compo, 1998). We agree that modified calculations of the wavelet analyses do not support significant periodic components, so this was revised accordingly (see lines 438-455).

Response to technical corrections:

All technical corrections were made.

Cited references:

Ben Dor, Y., Neugebauer, I., Enzel, Y., Schwab, M. J., Tjallingii, R., Erel, Y., and Brauer, A.: Varves of the Dead Sea sedimentary record, Quaternary Science Reviews, 215, 173-184, 2019.

Ben Dor, Y., Flax, T., Levitan, I., Enzel, Y., Brauer, A., and Erel, Y.: The paleohydrological implications of aragonite precipitation under contrasting climates in the endorheic Dead Sea and its precursors revealed by experimental investigations, Chemical Geology, 576, 10.1016/j.chemgeo.2021.120261, 2021.

Grinsted, A., Moore, J. C., and Jevrejeva, S.: Application of the cross wavelet transform and wavelet coherence to geophysical time series, Nonlinear processes in geophysics, 11, 561-566, https://doi.org/10.5194/npg-11-561-2004, 2004.

Haase-Schramm, A., Goldstein, S. L., and Stein, M.: U-Th dating of Lake Lisan (late Pleistocene dead sea) aragonite and implications for glacial east Mediterranean climate change, Geochim. Cosmochim. Acta, 68, 985-1005, http://dx.doi.org/10.1016/j.gca.2003.07.016, 2004.

Marco, S., Stein, M., Agnon, A., and Ron, H.: Long-term earthquake clustering: A 50,000-year paleoseismic record in the Dead Sea Graben, Journal of Geophysical Research: Solid Earth, 101, 6179-6191, 1996.

Prasad, S., Negendank, J., and Stein, M.: Varve counting reveals high resolution radiocarbon reservoir age variations in palaeolake Lisan, Journal of Quaternary Science: Published for the Quaternary Research Association, 24, 690-696, 2009.

Prasad, S., Vos, H., Negendank, J., Waldmann, N., Goldstein, S. L., and Stein, M.: Evidence from Lake Lisan of solar influence on decadal-to centennial-scale climate variability during marine oxygen isotope stage 2, Geology, 32, 581-584, 2004.

Schulte, J. A.: Cumulative areawise testing in wavelet analysis and its application to geophysical time series, Nonlinear Processes in Geophysics, 23, 45-57, 2016.

Schulte, J. A.: Statistical hypothesis testing in wavelet analysis: theoretical developments and applications to Indian rainfall, Nonlinear Processes in Geophysics, 26, 91-108, 2019.

Stein, M., Starinsky, A., Katz, A., Goldstein, S. L., Machlus, M., and Schramm, A.: Strontium isotopic, chemical, and sedimentological evidence for the evolution of Lake Lisan and the Dead Sea, Geochim. Cosmochim. Acta, 61, 3975-3992, 1997.

Torrence, C. and Compo, G. P.: A practical guide to wavelet analysis, Bulletin of the American Meteorological society, 79, 61-78, 1998.

Response to Report #2

By Anonymous Referee #2

Response to general comments:

We appreciate the reviewer's positive impression of the manuscript and the novelty of the presented data. Our general outline for addressing the general comments is provided below:

Comment: (i) in brief: why can these laminae couplets be used as annual record. At the same time, it's worth reflecting whether an annual character of the time-series is needed a priori for the proposed discussion, or if it can result from it.

Response: The nature of the sediments and the way that we understand their formation, according to modern analogues and previous detailed investigations of available exposures, suggest that detritus-aragonite couplets are deposited annually, thus forming varves. This is further supported by our microfacies analyses based on continuously sampled thin sections, in which we observe even slight changes in the sediments in details. This is also supported by previous studies of the Dead Sea sedimentary record (e.g., Stein et al., 1997; Marco et al., 1996), the study of modern lakes by monitoring and recent cores, and the agreement between laminae counting and independent radiometric dating such as ^{14}C and U-Th (Prasad et al., 2009; Haase-Schramm et al., 2004). Thus, because no deposition of alternating aragonite and detritus takes place under modern conditions in the Dead Sea (e.g., Ben Dor et al., 2021), the interpretation of alternating aragonite and detritus facies as annual deposits is, to some extent, a (pretty solid) assumption, as it cannot be directly determined for the studied interval Lake Lisan (e.g., Prasad et al., 2004; Ben Dor et al., 2019). Because we note that deciphering their sedimentation under the current conditions of the Dead Sea is not possible, and our microfacies analyses suggest that the similar structure of the studied sediments further supports their interpretation as varves. The delicate structure of detrital sublaminae, that contain graded bedding of detrital components and are covered by a thin layer of amorphous organic material strengthen the interpretation of their formation mechanism by individual flooding events that deliver detritus and trigger algal blooms. This is now elaborated in the introduction section (See lines 167-184, 204-213).

Comment: (ii) clear statements on the bicarbonate and alkalinity sources to the Dead Sea, that ultimately contribute to aragonite precipitation. Even under the simplest assumption that these are only hydrological in nature, aren't the floods themselves also a source of bicarbonate? Thus, how can the proxies be 'independent' as proposed in L110-115; L440-449?

Response: The contribution of floods to the overall hydrological (and alkalinity) budget of the Dead Sea (and likely to Lake Lisan) is negligible (Armon et al., 2019; Begin et al., 2004), which makes the proxies practically independent (see Ben Dor et al., 2021 for details). This is now elaborated in the introduction part (see lines 169-184).

Comment: (iii) Moreover, it was recently shown that calcite dust is an important bicarbonate source in the region, and excerpts control on aragonite precipitation in the Dead Sea. This of course adds complexity to the issue, given the logical implication would be that the thickness of the aragonite laminae is not exclusively under hydrological control.

Response: This aspect in clearly addressed in the revised manuscript according to our recent findings, and the reader is further referred to for more information on that elsewhere (Ben Dor et al., 2021). In short, it was shown that the potential contribution of dust directly settling on the lake is insufficient to support the deposition of aragonite laminae (e.g., Ganor and Foner, 1996; Kalderon-Asael et al., 2009). However, the dissolution and remobilization of accumulated dust from the watershed is indeed a potentially substantial source for bicarbonate that could increase the alkalinity of inflow (e.g., Crouvi et al., 2017; Belmaker et al., 2019), which would consequently affect the relationship between Ca-carbonate deposition into inflow. Although this cannot be directly addressed for the studied time intervals, we considered recent studies of the snow-affected Mt. Hermon region in Israel (Avni et al., 2018) and denudation rates in the Judea region (Ryb et al., 2014), which altogether suggest that the dissolution of bedrock could not have increased alkalinity inflow by a factor greater than two. Thus, although these aspects limit the extent of conclusions that can be directly drawn from the data, we consider that the likely relationship between inflow and aragonite deposition is probably monotonous, and increased inflow would result in increased aragonite thickness and vice versa (see lines 168-183).

Response to specific comments:

Comment: L17-18: “aragonite … serve as a proxy of annual inflow (…), whereas detrital laminae (…) record floods”: How can floods and the annual inflow be differentiated by the proxies given that the first also contribute to the ionic sources of aragonite precipitation? Also, what is the mineral composition of the detrital sub-layers, do they contain carbonates? Eventually treat the time-series as sub-sets, or inter-dependent?

Response: The reasoning behind these claims in now elaborated in the introduction (see lines 168-183). Additionally, it is supported by the relatively low Spearman rank correlation coefficient of flood frequency and aragonite thickness (Fig. 3; r = 0.52 and 0.61 for falling and rising lake levels, respectively). We note that our original expectation was to observe better correlation, which is not the case.

Comment: L14&54: briefly explain why these laminations can be regarded as having annual character. Or is this perhaps something to be explored, in face of recent discussions? One possibility would be treating the time-series as a floating chronology, and search for pattern-types that might support the annual character. Arguments supporting that arise for example from statements such as on L398-399 and L364 (however indepedent records), about the encountered periodicities.

Response: The reasoning supporting the interpretation of these sediments as varves is elaborated in the abovementioned response to the previous comment (i). This is now elaborated in lines 167-184, 204-213.

Comment: L80-90: This paragraph tries to connect the different microfacies with the different synoptic climate features. However, a distinct causality between aragonite/detrital sub-layers, and the Mediterranean cyclones/Red Sea troughs/subtropical jet streams, remains unclear. This is rather a subject for the discussion.

Response: In this section we review the previous knowledge on the sediment and water paths into the lake and published information on synoptic circulation systems. We do not provide any interpretation, but simply state that the delivery of detrital material into the lake requires floods that are statistically linked to certain weather patterns, whereas annual inflow is linked to others. These aspects are now elaborated in the revised manuscript (see lines: 119-129).

Comment: L110 -115: while the hydroclimate variables might be independent; the proxies obtained herein have some degree of dependency (aragonite sub-lamina thickness, and number of detrital pulses). Aren't the floods also sources of (bi)carbonate ions, and thus won't they contribute to the aragonite thickness? And this is regardless of the timing of aragonite precipitation. I think this is one of the aspects that needs some more reflection within the discussion below.

Response: Yes, this makes a good point that is addressed in lines 168-182.

Comment: L440-449: This part of the discussion would benefit from additional reflection/explanations.

Response: The entire discussion was revised and elaborated in accordance with the comments made by the reviewers.

Comment: L445: What properties? What frequency? Is the frequency relatable to the encountered flood frequency? Here it remains unclear why Red Sea troughs and active subtropical jet stream disturbances contribute to the flood frequency, but not to the annual inflow. While this might be true from a hydroclimate perspective, how does it translate to the sedimentary system?

Response: This part of the discussion was clarified an elaborated using available data and previous papers. The general understanding of precipitation patterns induced by different synoptic systems in the Dead Sea watershed depicts a "de-coupling" of annual inflow into the lake, which depends on annual precipitation over the northern parts of the watershed, and floods reaching the coring site. This is because the frequency and intensity of *eastern Mediterranean Lows*, which primarily affect the northern parts of the watershed, determines annual precipitation over the watershed (Saaroni et al., 2010) and affects flood frequency in the relevant ephemeral streams (Goldreich et al., 2004), whereas the contribution of other synoptic systems to annual precipitation is substantially lower, and is mostly relevant for the southern parts of the watershed (Armon et al., 2019). This is also evident by the low correlation ($r^2 = 0.086$) of major floods (return period >5 years) in the Negev Desert (Kahana et al., 2002) and precipitation in Jerusalem (Fig. R1), which found to be closely correlated with Dead Sea lake level, and hence with annual inflow into the lake (Enzel et al., 2003). Thus, although this cannot be directly proven for the LGM, we consider these modern observations as a tool to decipher the sedimentary record (e.g., Enzel et al., 2008; Goldsmith et al., 2017). See lines 486-495.

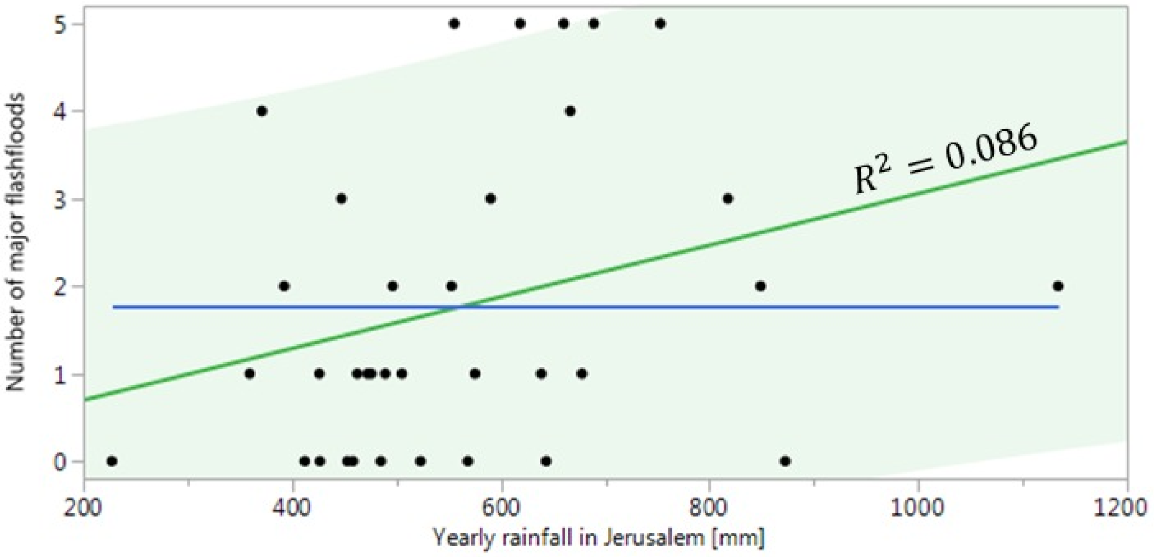

Figure R1 – Frequency of major floods in the Negev Desert (Kahana et al., 2002) and annual precipitation in Jerusalem. The low correlation indicates the decoupling of annual inflow into the lake and flood frequency, demonstrating the importance of different synoptic systems and their characteristics on these two hydrological properties.

Response to technical corrections:

All technical comments were corrected accordingly.

Cited references:

Armon, M., Morin, E., Enzel, Y., 2019. Overview of modern atmospheric patterns controlling rainfall and floods into the Dead Sea: Implications for the lake's sedimentology and paleohydrology. Quaternary Science Reviews 216, 58-73.

Avni, S., Joseph-Hai, N., Haviv, I., Matmon, A., Benedetti, L., Team, A., 2018. Patterns and rates of 103–105 yr denudation in carbonate terrains under subhumid to subalpine climatic gradient, Mount Hermon, Israel. Bulletin 131, 899-912.

Begin, Z. B., Stein, M., Katz, A., Machlus, M., Rosenfeld, A., Buchbinder, B., and Bartov, Y.: Southward migration of rain tracks during the last glacial, revealed by salinity gradient in Lake Lisan (Dead Sea rift), Quaternary Science Reviews, 23, 1627-1636, 10.1016/j.quascirev.2004.01.002, 2004.

Belmaker, R., Lazar, B., Stein, M., Taha, N., and Bookman, R.: Constraints on aragonite precipitation in the Dead Sea from geochemical measurements of flood plumes, Quaternary Science Reviews, 221, 105876, 2019.

Ben Dor, Y., Neugebauer, I., Enzel, Y., Schwab, M. J., Tjallingii, R., Erel, Y., and Brauer, A.: Varves of the Dead Sea sedimentary record, Quaternary Science Reviews, 215, 173-184, 2019.

Ben Dor, Y., Flax, T., Levitan, I., Enzel, Y., Brauer, A., and Erel, Y.: The paleohydrological implications of aragonite precipitation under contrasting climates in the endorheic Dead Sea and its precursors revealed by experimental investigations, Chemical Geology, 576, 10.1016/j.chemgeo.2021.120261, 2021.Goldreich, Y., Mozes, H., Rosenfeld, D., 2004. Radar analysis of cloud systems and their rainfall yield in Israel. Isr. J. Earth Sci 53, 63-76.

Crouvi, O., Amit, R., Ben Israel, M., and Enzel, Y.: Loess in the Negev desert: sources, loessial soils, palaeosols, and palaeoclimatic implications, in: Quaternary of the Levant: Environments, Climate Change, and Humans. Cambridge University Press, Cambridge, edited by: Enzel, Y., and Bar-Yosef, O., 471-482, 2017.

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Ganor, E. and Foner, H.: The mineralogical and chemical properties and the behaviour of aeolian Saharan dust over Israel, in: The impact of desert dust across the Mediterranean, Springer, 163-172, 1996.

Goldsmith, Y., Polissar, P., Ayalon, A., Bar-Matthews, M., and Broecker, W.: The modern and Last Glacial Maximum hydrological cycles of the Eastern Mediterranean and the Levant from a water isotope perspective, Earth Planet. Sci. Lett., 457, 302-312, 2017.

Haase-Schramm, A., Goldstein, S. L., and Stein, M.: U-Th dating of Lake Lisan (late Pleistocene dead sea) aragonite and implications for glacial east Mediterranean climate change, Geochim. Cosmochim. Acta, 68, 985-1005, http://dx.doi.org/10.1016/j.gca.2003.07.016, 2004.

Kahana, R., Ziv, B., Enzel, Y., and Dayan, U.: Synoptic climatology of major floods in the Negev Desert, Israel, International Journal of Climatology, 22, 867-882, 2002.

Kalderon-Asael, B., Erel, Y., Sandler, A., and Dayan, U.: Mineralogical and chemical characterization of suspended atmospheric particles over the east Mediterranean based on synoptic-scale circulation patterns, Atmospheric Environment, 43, 3963-3970, 2009.

Kalderon-Asael, B., Erel, Y., Sandler, A., and Dayan, U.: Mineralogical and chemical characterization of suspended atmospheric particles over the east Mediterranean based on synoptic-scale circulation patterns, Atmospheric Environment, 43, 3963-3970, 2009.

Marco, S., Stein, M., Agnon, A., and Ron, H.: Long-term earthquake clustering: A 50,000-year paleoseismic record in the Dead Sea Graben, Journal of Geophysical Research: Solid Earth, 101, 6179-6191, 1996.

Marco, S., Stein, M., Agnon, A., and Ron, H.: Long-term earthquake clustering: A 50,000-year paleoseismic record in the Dead Sea Graben, Journal of Geophysical Research: Solid Earth, 101, 6179-6191, 1996.

Prasad, S., Negendank, J., and Stein, M.: Varve counting reveals high resolution radiocarbon reservoir age variations in palaeolake Lisan, Journal of Quaternary Science: Published for the Quaternary Research Association, 24, 690-696, 2009.

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Ryb, U., Matmon, A., Erel, Y., Haviv, I., Benedetti, L., Hidy, A., 2014. Styles and rates of long-term denudation in carbonate terrains under a Mediterranean to hyper-arid climatic gradient. Earth Planet. Sci. Lett. 406, 142-152.

Saaroni, H., Halfon, N., Ziv, B., Alpert, P., Kutiel, H., 2010. Links between the rainfall regime in Israel and location and intensity of Cyprus lows. International Journal of Climatology 30, 1014-1025.

Stein, M., Starinsky, A., Katz, A., Goldstein, S. L., Machlus, M., and Schramm, A.: Strontium isotopic, chemical, and sedimentological evidence for the evolution of Lake Lisan and the Dead Sea, Geochim. Cosmochim. Acta, 61, 3975-3992, 1997.

Response to Report #3

By Prof. Reik Donner

Response to general comments:

We appreciate Prof. Donner's comments and very detailed suggestions, as we highly appreciate his experience with geoscientific time series analyses. We are grateful for the opportunity we were given to improve the manuscript according to these comments and suggestions. We have adjusted, removed and recalculated all the necessary analyses to make sure they comply with the provided comments and suggestions.

Additionally, because we agree with the notion that the original supplementary was too long and largely redundant, as was pointed out by the other reviewers. We have therefore substantially revised and reduced its size, and have additionally also made sure that its referenced in a clear and consistent way throughout the manuscript.

Response to major comments:

Comment 1: The presentation of the employed time series analysis methods (recurrence analysis, wavelet analysis, singular spectrum analyses) is very short and hardly assessable to non-specialists, which will form the vast majority of the readership of Climate of the Past. I think that a more detailed introduction (possibly as part of the supplement) would be justified.

Response 1: We have expanded and improved the presentation of the employed methods, and have further improved the referencing to relevant papers that provide the necessary background for the readers (see lines 234-358).

Comment 2: It is pretty unlikely that paleoclimate variability in the study area has undergone (exactly) periodic oscillations at interannual to multidecadal scales. Therefore, I do not agree with using the term "periodic components" (e.g., in l.65), yet would expect something like "narrow-banded oscillations" or similar. What can actually be characterized in the presented study is the relevance of certain "spectral bands" (interannual, decadal, multi-decadal) and how their respective spectral power may differ among the two study periods. I will further comment on this point below when addressing the performed wavelet analyses and the interpretation of the corresponding results.

Response 2: We agree with that comment, so we have rephrased the relevant text accordingly and replaced it with the proposed terminology (e.g., see lines 328, 517-563).

Comment 3: According to ll.146-147, missing values have been imputed by the median – of the whole time series (segment)? It needs to be noted that this strategy may have quite different effects on the different time series analysis methods used in this work. For SSA, it might actually be better to just ignore missing values. What is more, missing value imputation based on the results of the singular value decomposition of the lagged trajectory matrix (e.g.

Kondrashov & Ghill, Nonlin. Proc. Geophys., 2006) would allow for a more reasonable (e.g., consistent with the records' power spectra) gap filling and, hence, likely more reliable results of the other analysis methods.

Response 3: Following this very useful suggestion, the missing values were imputed using SSA.

Comment 4: I appreciate that the authors use nonparametric statistical tests for the homogeneity of distributional (location and variability, respectively) properties of their three proxies between the two study periods (e.g., using the MWW test instead of classical parametric ANOVA). However, when reporting the corresponding results (Tab. 2), what is presented turns awkward. Notably, according to its contents Tab. 2 apparently relates the results of MWW and AB tests to either the fall (MWW) or the rise (AB) period (which would be meaningless), while both tests actually compare both periods. I suppose the authors accidentally copied the first column of the Table from Tab. 1, yet it must be removed from Tab. 2 to make any sense. In Figs. S2 and S3, I don't quite get the statistical reasoning beyond multiplying the p-values of both tests (which are not independent of each other); this should be better motivated.

Response 4: Thank you! Yes, this was indeed an unfortunate mistake and column 1 was removed. The table compares the properties of the two periods. The mentioned supplementary figures have been removed. As for the multiplication of the two tests (Figs. S2 and S3), the multiplication of the two tests was indeed redundant and was therefore removed from the revised version (Figs. S7 and S8).

Comment 5: For detecting regime shifts within the study period, the authors use running MWW and AB tests for 51-year sliding windows in time. Here, I am wondering about several things:

(i) What are the two samples which are compared by the two tests? The 25-year sub-periods before and after the reference point? I don't find this clearly explained in the text.

(ii) What is the reliability (power) of MWW/AB tests for such small samples of 51 (25?) values only?

(iii) Sliding windows mean that the samples considered in subsequent tests largely overlap, potentially causing multiple testing problems when assessing the significance of pointwise MWW or AB tests. The same applies to performing the same tests for different window sizes. I don't see this aspect being addressed, so it would be good if the authors could comment on this and why they might think it could be ignored in the context of the present work (which I personally believe could, but has to be justified). In any case, it can be expected that results for neighboring windows and different window widths will mutually depend on each other.

(iv) For assessing statistical significance, the authors use resampling of the full time series. If I understand correctly, this is being done by permuting individual values. However, this procedure does not only destroy any differences between sub-periods, but also any serial dependencies of proxy values within such periods, thereby making the obtained confidence bounds over-confident (i.e., potentially too narrow). Figure S4 indicates the absence of such serial dependencies (to a great extent) and thereby could justify the employed procedure (i.e., not using block bootstrapping instead of point-wise resampling), but this should be mentioned explicitly in the text.

Response 5:

(i) Yes, this is now explained in the revised manuscript (see lines 253-255)

(ii) We chose a window size of 51 years after trying several window sizes, and examining their performance. On one hand, we wanted to avoid over-detection of "noise" by choosing a too narrow window, and on the other hand we wanted to avoid looking into too-long intervals, which would depreciate the implications of the analyses. Because each series spans ~700 years, we consider the semi-centennial timescale as a reasonable window length that serves as a compromise between the two abovementioned aspects. We did not conduct a test to determine the power of this approach, because we think this is not strictly necessary for the discussion and is beyond the scope of this paper. In our view, this analysis is only carried out as a complementary method to refine the results of cluster detection based on the Monte-Carlo approach elaborated later in the manuscript (Figs 3 and S1-S5). The implications of different window widths in depicted in Figs. S7 and S8.

(iii) This is again a very good comment. Because we don't rely solely on this approach and consider it together with the other approaches elaborated throughout the manuscript, we did not account for the multiple-tests issue related to these overlaps. However, because the p-values of the sliding windows are dependent, we do not consider the p-values of the tests themselves (e.g., vs. a specific alpha level etc.), but instead it is the patterns they form along the series that is utilized, and more specifically whether substantial minima can be identified. This is why we think this issue can be ignored in this context, and we can assume that this is what Prof. Donner is referring to.

(iv) This is explicitly mentioned in the revised manuscript (lines 233-255).

Comment 6: Still in Section 3.2, the authors describe their rationale for using recurrence analysis, which is probably rather unfamiliar to the vast part of the readership of Climate of the Past. Yet, also the authors appear not to be specialists in employing this technique, which is suggested by a couple of observations.

(i) The authors claim that they have also performed cross-recurrence analyses (l.176) but do not report any such results (which might also not be very useful since they would compare two proxies with different meanings, physical units, etc.).

(ii) Recurrence analysis can indeed be used to infer short-term periodic and quasi-periodic dynamics (in the proper meaning of both terms), as claimed in l.177, but neither of the corresponding approaches is used in this manuscript (e.g., studying the properties of the tau-recurrence rate a.k.a. generalized auto-correlation function; cf. Zou et al., Phys. Rev. E, 2007).

(iii) Comparing the use of recurrence analysis with that of harmonic and wavelet functions (l. 182) and their respective "robustness" is somewhat odd since those methods serve completely different purposes.

(iv) A vast body of recent work has detailed the problems of using a fixed recurrence threshold and taking the recurrence rate RR as a parameter, as other quantitative characteristics of recurrence plots intimately depend on the value of RR. Fixing the threshold at a multiple of the standard deviation of the data only partially solves the problem, since the distribution of distances between state vectors (values) evaluated is commonly non-Gaussian and may crucially differ between different settings studied. Most notably, the values of epsilon (sigma or even 1.5sigma) are far larger than those commonly recommended in the literature (e.g. Schinkel et al., Eur. Phys. J. Special Topics, 2008). The resulting RR values (Figs. 4 and 5) approach values between 0.1 up to even 1.0, which are far too large to allow for any meaningful interpretation of the transitivity values (Zou et al., Phys. Rep., 2017). This also explains why RR and transitivity show the same type of time dependence, while the transitivity should actually be independent of RR for a reasonable range of epsilon values.

(v) Using time delay embedding and reporting/justifying the corresponding embedding parameters is key for interpreting the results of recurrence analyses and making them reproducible. This is poorly described in this manuscript, although all necessary results are found in the supplement. It is particularly interesting to observe that both proxies more or less instantaneously de-correlate (Fig. S4c,d), which is a behavior common for white noise. On the other hand, using embedding dimensions of 4 or 5 for 700 data points might already exceed what might be required for a reliable statistical inference of the key recurrence structures. Here, m=3 might be a more pragmatic choice. In general, using the same embedding parameters for all recurrence (and joint recurrence) plots would help making the obtained structures, as well as their quantitative characteristics, better comparable.

Response 6:

(i) We acknowledge the fact that we are not experts in employing recurrence analyses. However, we have made our best efforts to apply these methods based on the available literature and the software package distributed by PIK (Marwan et al., 2007), and we would gladly adjust the employed parameters in accordance with Prof. Donner's suggestions. The mentioning of cross-recurrence is indeed redundant, as we eventually report only the results of recurrence and joint-recurrence, rather than the cross-recurrence. This was fixed in the revised manuscript.

(ii) Several RQA (JRQA) metrics were calculated and presented in the revised manuscript.

(iii) This was rephrased and clarified in the revised manuscript.

(iv) The ε of the updated RPs was selected by adjusting the RR to 0.1 following the recommended reference (Schinkel et al., 2008; Figs. 6 and 7).

(v) These parameters were adjusted in accordance with those suggestions (Figs. 6 and 7).

Comment 7: Regarding the wavelet analysis, I again appreciate that the authors provide the results of significance testing with an AR(1) red noise null model. However, what is crucial to remark is that they perform this test in a point-wise manner (which is unfortunately still the standard in the applied geosciences literature), thereby overemphasizing possible false positive results. Due to the serial dependence of point-wise values of the wavelet coefficients at neighboring times and scales, false positives can only be ruled out using areawise tests (Maraun & Kurths, Nonlin. Proc. Geophys., 2004; Maraun et al., Phys. Rev. E, 2007). I don't argue here that it is necessary to employ such tests as part of the present study, but recommend to evaluate and interpret the results of point-wise tests with more caution. Quite a few of the high-frequency episodic significant patches in the wavelet spectrograms shown in this manuscript (e.g. Figs. 6 and 7) could potentially be associated with such false positives. (Referring to "non-persistent periodic[al] components of 2-6 years" in ll.371-372 appears more like wishful thinking than proper interpretation of the obtained results.) Along with my former comments on the use of the terms "periodic" and "quasi-periodic", I recommend to focus on spectral power in different frequency bands instead of seeking for true periodicities which are unlikely to exist at the timescales of interest (due to an absence of obvious mechanisms except for maybe solar activity variations).

Response 7: The wavelet analyses was recalculate using an area-wise false-positive test (e.g., Figs. 8 and 9). We agree that no clear significant periodic components could be identified in the records. We have further adjusted the text according to the comments related to "different frequency bands" (e.g., see lines 445-455, 544-546).

Comment 8: Singular spectrum analysis (SSA) appears to be primarily used here for detrending and "denoising" (i.e., reconstructing the underlying signal based on a few modes, which is probably related to what the authors refer to as "overfitting" in l.209 – this should be clarified for non-specialist readers). As already outlined above, I would recommend using this method also for gap filling and, hence, as a first analysis step. It might also be worth mentioning that SSA is more flexible than wavelet analysis (or at least classical spectral analysis based on Fourier transform or harmonic regression models) in that it allows for an arbitrary shape of possible oscillatory components along with time-dependent amplitudes (like in wavelet of classical EOF analysis/PCA), but not for time-dependent frequencies. The latter restriction might be alleviated by using other even more data-adaptive time scale decomposition techniques like empirical mode decomposition, which the authors decided not to consider in their present work (which is fine, since possible advantages of other methods also come along with additional caveats). Notably, the authors also use the multivariate extension of SSA, yet only show the corresponding results as plots in the supplement without further discussion and interpretation, so one may argue that this material might not be relevant. (If relevant, it should also be discussed in the text.) In a similar spirit, it is notable that Figs. S6 and S7 are currently not (respectively, wrongly) referenced in the text, but should be referred to in l.212.

Response 8: This is now elaborated in the revised manuscript (see lines 339-350). As for the application of additional methods, such as empirical mode decomposition, we tried to limit the amount of applied methods to a reasonable extent, which would suffice for addressing the goals of the research. We agree that more analyses can be done, but as Prof. Donner suggests, every method has its advantages and disadvantages, and our general notion is that enough analyses are presented in the current version, so we would refrain from conducting additional analyses. The multivariate SSA was removed from the supplementary, as it became clear during the revision process that it does not provide additional insights. The number of supplementary was substantially reduced and all the figure references and captions were corrected.

Comment 9: A bit worrying is the application of cross-wavelet analysis, which is not well described in the manuscript (ll.212-214). My understanding is that the authors first use SSA for detrending and denoising the time series under study and then estimate the cross-wavelet spectrograms for pairs of time series. It is notable that this type of analysis is commonly not recommended, since it can provide large spectral power even if only one of the two signals actually exhibits a "periodic" component. For the purpose of seeking for joint oscillatory components, the normalized wavelet coherency should be the method of choice instead (Maraun & Kurths, Nonlin. Proc. Geophys., 2004).

Response 9: Following the provided reference, we agree that the cross-wavelet analysis is not robust enough on its own to support the identification of mutual periodicities, so this was replaced with the wavelet coherence analyses.

Comment 10: Section 4.3, 2nd paragraph: It should be clarified that substantial spectral power in the low-frequency part is a common feature of climate time series. Hence, the fact that the wavelet spectrogram does not indicate statistical significance in this range of frequencies indicates that any low-frequency (inter-decadal) oscillations embedded in the signals do not follow a strictly periodic pattern. Note that at the mentioned time scales, the cone of influence becomes so narrow here that the number of oscillations may not be sufficient to identify properly any periodic structure.

Response 10: This was clarified in the revised manuscript (see lines: 450-455).

Response to minor comments:

Comment 11: The second paragraph of the introduction briefly discusses key drivers of hydroclimate variability in the Levant. In this regard, I am somewhat missing any brief statements on possible teleconnections from the Indian Ocean. In modern times, there exist anomalous circulation patterns linking the Arabian Sea branch of the Indian summer monsoon with the climate of the Eastern Mediterranean region. The active Red Sea troughs (ARST) are a manifestation of associated episodic events providing heavy precipitation to the study area, as also mentioned by the authors in the last paragraph of Section 5.3. It might be interesting to explore, or at least speculate about a possible link between elevated flood frequency and Indian monsoon failures as documented in historical heavy precipitation events of the recent past. More specifically, I am wondering if there is a way to (indirectly) link the inferred flood frequency to late Pleistocene Indian monsoon variability. Or can we expect the corresponding teleconnection not to play an important role during that period (e.g., due to a suppressed monsoon-desert mechanism)?

Response 11: This is an interesting suggestion that we have considered. To the best of our knowledge, the Indian summer monsoon currently does not trigger precipitation at the eastern Mediterranean. In contrast, the monsoon-desert connection indicates that the monsoon causes air subsidence in the Levant and does not deliver precipitation (Rodwell and Hoskins, 1996; Dayan et al., 2017). Furthermore, the ARST and the RST are seasonally unrelated to the summer Indian monsoon, as they are late fall and winter phenomena, when the monsoon is long gone from the Arabian Sea.

Comment 12: L.386: I don't quite get what an "NAO-like periodic component" should be, since the NAO does not have any clear periodicity. In a similar spirit, ll.467-468 claim "quasi-periodic ~3-4 years components, possibly related to the North Atlantic Oscillation", which I am not aware of to exist.

Response 12: It is true that NAO does not have a strong periodic component, however, there are different opinions on that aspect. These aspects were adjusted and are now addressed more cautiously in the revised text (see lines: 549-563).

Comment 13: L.409: Please check if the reduced recurrence rate is not just due to an increased variance within the considered time window.

Response 13: The entire calculation of the RPs was modified and the RQA (and JRQA) were adjusted accordingly (see Figs. 6 and 7, and lines: 266—327).

Response to technical corrections:

All technical corrections were corrected accordingly.

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