

Clim. Past Discuss., author comment AC3
<https://doi.org/10.5194/cp-2020-161-AC3>, 2021
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Reply on RC2

Yoav Ben Dor et al.

Author comment on "Hydroclimatic variability of opposing Late Pleistocene climates in the Levant revealed by deep Dead Sea sediments" by Yoav Ben Dor et al., *Clim. Past Discuss.*, <https://doi.org/10.5194/cp-2020-161-AC3>, 2021

Response to Report #2

By Anonymous Referee #2

Response to general comments:

We appreciate the reviewer's positive impression of the quality 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 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 determined for the studied interval Lake Lisan (e.g., Prasad et al., 2004; Ben Dor et al., 2019). We will make sure this is clear in the introduction of the revised version.

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 floods contribution to the overall hydrological (and alkalinity) balance 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). We will make sure these principles are explicitly described in the revised manuscript.

Comment: (iii) Moreover, it was recently shown that calcite dust is an important bicarbonate source in the region, and exerts 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: We will directly address this aspect in the revised manuscript and we will also refer the readers to our recently accepted paper in *Chemical Geology* (Ben Dor et al., 2021), where this topic is discussed in detail. The potential contribution of dust directly settling on the lake is not sufficient 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.

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: We agree that this is not properly explained in the current manuscript. We will elaborate on carbonate sources and the carbonate budget of the lake and the composition of the detrital layers. Because the total contribution of carbonate from the floods that deliver detrital sediments to the core from the streams that directly face the coring site is negligible, we addressed the two series as independent, and then we examined their relationship. This is also 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 independent records), about the encountered periodicities.

Response: The interpretation of these sediments as varves is mostly based on the monitoring of modern lakes and previous investigations of available exposures, where laminae counting consistently matched radiometric dating (Ben Dor et al., 2019 and references therein). We will make sure this is made clearer in the revised version.

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: We will revise this part and move it to the discussion.

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 raises a good question that will be addressed. As written above, we will elaborate on carbonate sources and why this notion is reasonable in that case.

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

Response: We will revise this section to make sure that it fits within the manuscript and additionally elaborate to make sure its logic is clear.

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: We agree that this part of the discussion requires additional clarification. 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 determines annual precipitation over the watershed (Saaroni et al., 2010) and flood frequency in the relevant ephemeral streams (Goldreich et al., 2004), whereas the contribution of the other synoptic systems to annual precipitation by far, is far less substantial (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 means to decipher the sedimentary record (e.g., Enzel et al., 2008; Goldsmith et al., 2017). We will clarify that in the revised manuscript to make sure the distinction between observations and interpretation is clear.

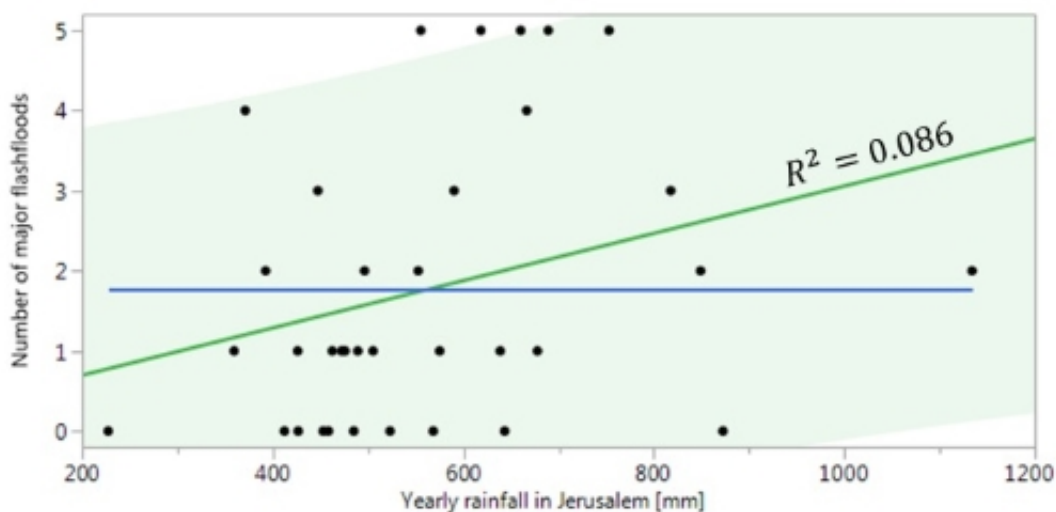


Figure R1 – Frequency of major floods in the Negev Desert (Kahana et al., 2002) and annual precipitation in Jerusalem. The low correlation indicates the decoupling of annual inflow into the lake and flood frequency, demonstrating the importance of different synoptic systems and their characteristics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

Response to technical corrections:

All technical corrections will be corrected accordingly.

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