

Interactive comment on “New constraints on the Eastern Mediterranean $\delta^{18}\text{O}:\delta\text{D}$ relationship” by K. A. Cox et al.

Anonymous Referee #1

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Cox et al. measured surface seawater samples from the Eastern Mediterranean and the East Greenland Current System for salinity, $\delta^{18}\text{O}$ and δD . They show that the new data fit to the linear relation of the modern world surface $\delta^{18}\text{O}$ - δD slope and are very close to the GISS model results. They conclude that the data of Gat et., 1996, do not represent the Eastern Mediterranean basin.

Without commenting on the general applicability of their model for open ocean waters their denial of the veracity of the G96 data because it does not fit their model or their own data set is not acceptable. It should be noted that while their data set refers to the end of summer, the data of G96 were mostly end of winter and spring data, a time

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when a larger influence of depleted freshwater influxes are to be expected. Further, the geographic location of the data and their proximity to influx of continental waters is a factor to be considered. It should also be noted that other so-called marginal inland sea areas such as the Baltic show similar attributes.

Obviously a much more detailed and year-over sampling program is called forth. I would also point out that their description of G96 interpretation of the data in lines 15-17 of section 3, namely: “G96 postulate that such a relationship may have developed as a result of continuous evaporation and precipitation cycles in a warm atmosphere with low relative humidity” completely misses the point. The effect they describe is related to the appearance of precipitation in the region with a high d-excess (which has little to do with the effect under discussion), whereas G96 invoke the addition of very depleted continental waters by influx of river discharge through the continental tributaries.

Several points should be addressed in the revised manuscript: 1. Page 42, lines 22-28: slopes of 4.3 are not “slightly shallower” than the slope of 8. 2. I suggest separating between data (real world measurements) and model results. It is difficult to differentiate between the two in Figure 1. 3. The distribution of the sampling stations in the Eastern Mediterranean must be discussed. Figure 2 clearly shows that the present study does not cover the area (South, South West basins) that was sampled by G96. 4. It is difficult to accept the explanation in Appendix A (which should be incorporated to the main text) why the present d18O values are lower than G96 and Pierre 1986 and 1999. It is very clear from Figure 4 that the present study did not capture the full variability of the Eastern Mediterranean.

Personally, I would like to suggest to the authors to consider the possibility that variability in time (over two decades between the two sampling) and the EMT as a major cause for the differences between the new and the old data, both of G96 and Pierre. The EMT was a major event, and strong evaporative conditions might have prevailed prior to 1987.

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