

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

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In answer to the reviewer’s main point: we will include a more careful and detailed description of the sampling season and location with a concession that these differences may produce subtle differences in the data and therefore highlight the need for more comprehensive sampling of the Mediterranean basin to fully characterize it in terms of seasonal and spatial variations in the stable isotopes as the reviewer mentions below. We will also include a discussion regarding the Baltic Sea. Also a more careful discussion of the G96 interpretations will be added.

In answer to the reviewer’s other points:

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1. This was not the meaning of that sentence; the world surface ocean mixing relationship (7.37) is slightly shallower than the GMWL (8) as a result of there being greater evaporation at lower latitudes.
2. Figure 1 will be modified with this in mind.
3. A more detailed discussion of the differences in sampling locations will be included.
4. Discussion of the need for a more complete sampling regime will be included. Our data however is consistent with that of Gat et al 2003, stated in the manuscript, however we do not have access to this data for the purposes of including it in Fig 4. In summary, there are 2 datasets before and after the EMT, and so we can verify the $\delta^{18}\text{O}$ measurements on either side of this event and we have concluded that the $\delta^{18}\text{O}$ data highlight a change in the oxygen isotopes potentially caused by the EMT. Finally the argument that the EMT could have caused the disparity between the datasets will be included in the discussion of the results and conclusions.

Thank you for your comments.

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