

April 1, 2014

This manuscript presents results based on a set of repeat hydrographic measurements spanning nearly 50 years in the western Mediterranean Sea. It provides evidence of increasing temperatures and salinities in the intermediate to deep waters at a particular location. Through comparison to earlier studies that use data from same repeat transects and as well as observations from elsewhere in the Mediterranean the analysis weighs the importance to changes in deep water mass properties of persistent salt-fingering versus intermittent wintertime convective events, and occasional (decadal-scale) extreme winters. I think there are results here that are worthy of publication, and although the manuscript is well organized and reasonably well written, I have some major concerns.

- 1) Results asserting differences and arguments testifying to the importance of one difference compared another are meaningless unless associated uncertainties are provided.
- 2) Why with entire repeat sections available are the results from only a single station shown and discussed? If data from other positions support these results they should be included in the analysis, and if they don't, this is also of interest and should be discussed.
- 3) It is difficult in the latter part of the manuscript to tell what part of the discussion represents conclusions drawn from the authors' data set and analysis, and what parts are based on the conclusions of others. Along with this confusion, I get the impression that conclusions are being based on a steady state ideal, but I don't understand why these days one would expect a steady state on decadal time scales.
- 4) The manuscript needs a table of estimated changes with uncertainties - all in the same units.

In the lists below I have attempted to point out where these concerns arose. Please note, it appears the manuscript changed from the time I first obtained it to when I went back to the website to check on some details. Therefore, it is possible, particularly in the early part of the manuscript, that comments are no longer relevant. (P - page; L - line)

Particular concerns, suggestions and critique:

P736 L2: Why 40 years? Isn't it a 47-year record? Why are only the Location A results presented in the abstract. Neither of the two processes is actually illustrated with the repeat data in western Mediterranean. The changes seen in these data sets are discussed in terms of the expected affects of the two processes.

P737 L16: If this is the reasoning used by 'most', other references should be included.

P737 L20: How is this 0.04 related to the 0.13 change in outflowing Mediterranean waters?

P738 L1: When did this LIW transit the Sicily Channel and what year(s) are the 'large deep water formation events'?

P738 L23: Around here I started wondering what definitions were being used for LIW and WMDW. It would also be useful to have a table for these two water masses with theta, S and perhaps depth or pressure averages & standard deviations for location A, station 9 and Dyfamed in each year of interest.

P739 L2 and through out: Please include estimates of uncertainty on all property values. Also it would help the reader to understand the comparisons if all numbers could also be quoted in some standard units, perhaps change per decade. E.g. It would answer the question in P736 L2 above.

P739 L5: The 12 cm yr^{-1} is similar to (consistent with?) the Nof (1979) estimate for the effect of Nile damming. Can the 1.6 W m^{-2} also be compared to previous estimates?

P739 L9-13: I'm a little confused by this explanation as every profile is warmer and saltier than its predecessor at depth. Perhaps labeling the water masses would help.

P739 L23-24: And indicating that this process may account for all the estimated salinification at Location A. The salt fingering effect is the same magnitude as the salinity change suggested by Rohling & Bryden for Nile damming compensation and it appears larger than the temperature change seen at Location A. I'm writing this here because this is where I am thinking about it. Perhaps a pointer to later discussion is needed.

P739 L25: As far as I can tell the repeated surveys are not being used. Only station 9 is considered. If nothing else changes then this sentence and the abstract should be reworded, and the perhaps the rest of the survey stations should be in grey or some other color in Fig. 1 to indicate that they are not used. But why use only one station?

P740 L7 onward: Please include uncertainties and use the terms significant and consistent where they are appropriate.

P740 10-13: Is this the conclusion of the authors or Schroeder et al.?

P740 L14-16: Are these Schroeder et al.'s numbers? Are the referenced Figs. 3a&b Schroeder et al.'s figures? Because here, Figs 3a&b are for station 9, which is in not the northwestern Med. However, the sentences that follow appear to be referencing this manuscript's Figs. 3a&b. Please clarify and if the station 9 figure is intended, the new layer should be indicated on the figure.

P740 L16: So is there a one-year (less?) time lag assumed from formation region to station 9? Were all the cruises done at the same time of year?

P740 L26 – P741 L4: If I write down the numbers as I find them converting to the same units, this is what I get: (A table such as this is needed and should include uncertainties)

A) Nof (1979, estimate based on Nile damming)	$\Delta S = 0.13$
B) Skliris & Lascaratou (2004, Nile modeling)	$\Delta S = 0.01/\text{decade}$
C) Bryden et al. (2013, 1000 m of deep water)	$\Delta S = 0.017/\text{decade}, \Delta\theta = 0.19^\circ\text{C}/\text{decade}$
D) Here Location A (> 200 m, 1961 - 2008)	$\Delta S = 0.015/\text{decade}, \Delta\theta = 0.04^\circ\text{C}/\text{decade}$
E) Here Station 9 (> 1900 m, 2004 - 2013)	$\Delta S = 0.035/\text{decade}, \Delta\theta = 0.08^\circ\text{C}/\text{decade}$
F) Here Station 9 (1500-1900 m, 2004 - 2013)	$\Delta S = 0.016/\text{decade}, \Delta\theta = 0.04^\circ\text{C}/\text{decade}$
G) Here Station 9 (>1800 m, 2004 - 2010)	$\Delta S = 0.042/\text{decade}, \Delta\theta = 0.10^\circ\text{C}/\text{decade}$
H) Dyfamed (1800 - 2000 m, 1995 - 2005)	$\Delta S = 0.027/\text{decade}, \Delta\theta = 0.05^\circ\text{C}/\text{decade}$
I) Dyfamed (1600 – 1800 OR 1800 – 2000? 2005-07 Jump)	$\Delta S = 0.04/2\text{yr}, \Delta\theta = 0.08^\circ\text{C}/2\text{yr}$

So a few questions:

- Why is the change at station 9 at depths greater than 1800 m (? or is it 1900 ?) compared to the change at Location A averaged below 200 m.
- If I believe that these two estimates can be compared, I can see that if one looks at the maximum possible estimate of ΔS at Station 9 that this is nearly 3x the long term average taken at Location A, but the long-term estimate is based on only 5 observations – so is it really a long term estimate?
- P741 L2-4: Lastly, a little explanation is needed to explain the importance of estimate F (above) given that the estimates for change in the indirectly affected depth range are greater. Is this the intention of the next paragraph? If so, it should be indicated.

P741 L30: Reference the early range in Fig 4.

P742 L1-4: Are they slightly smaller? Can't tell without error bars.

P742 L15-18: This seems like a complicated way of saying that the depth to which water sinks is a function of T and S.

P742 L19: This sentence is quite confusing. The term 'mostly' is relative to what? Do the authors refer to the relative effect of temperature & salinity on density in this temperature range? Is this what is intended? Wintertime heat loss (mainly through evaporation) has an accumulated effect so that during a severe winter, each episode of new deep-water formation produces denser waters that are both colder and saltier than their earlier counterparts. At these temperatures, decreasing temperature dominates the density increase.

P742 L20-26: Perhaps because I am not an expert on the Mediterranean, but I don't get it. What is meant by 'above' (L22) and 'lower' (L25)? I have missed what this argument is based on. I agree: a) salt fingering makes deep water saltier; b) deep-water formed through convection may be saltier than the water column average (but is this necessarily true?); and c) mixing with saltier waters above or below makes any water mass saltier. Why do we expect steady state salinity in the deep waters when it's just been shown that salinities are changing over time? Even if I do believe this, why do new deep waters have to be fresher than old deep waters? Isn't this a function of temperature as well and doesn't the change in temperature dominate the density change? The two processes do not necessarily occur in the same location? Are deep Mediterranean waters fresher than the waters above them? Everywhere?

P743 L6-10: What does 'appreciably' mean? There are no error bars provided so I can't tell. Further confusing the issue the density quoted in the text is presumably σ_θ , but the curves in the figure are σ_2 . Why would one use σ_θ to compare changes at 2000 m?

P743 L10-14: But don't these statements assume that the deep water is static? We know there is a fresh cold source in Atlantic waters – so I don't see the 'conundrum.'

P743 L15: Having taken a quick look at Bryden et al. (2013), I find the following:
(Bryden et al., 2013 depth?, 1000 m layer 2006-2010- not quoted here)

$$\Delta S = 0.0017/\text{yr}, \Delta \theta = 0.0039^\circ\text{C}/\text{yr}$$

$$\Delta S = 0.017/\text{decade}, \Delta \theta = 0.039^\circ\text{C}/\text{decade}$$

These changes are similar in magnitude to others quoted in the manuscript, so why is the density change considered 'small'? Yes over the course of a single year it is, but the big

jumps such as 2005 happen only once or so in a decade and the salt fingering process occurs continuously. And if these T & S changes are of similar magnitude, why aren't the calculated density changes also of similar magnitude?

P743 L20: same comment as P743 L6-10

P744 L4: And therefore what? It seems as if this mechanism is still feasible.

P744 L9-14: These questions seem a little out of place. A better conclusion would be some discussion of what would be necessary in terms of observations or modeling to really get a handle on what is happening in terms of the balances discussed in the manuscript.

Triva: (here, my comments are in italics, suggestions for changes to text are not.)

P736 L2: *replace - yr with years.*

P736 L12-13: *reword - ...basin with an estimated freshwater (Evaporation – Precipitation - River Runoff) loss of 50 to 100 cm yr⁻¹ ... because evaporation cannot be defined as E-P-R.*

P736 L15-21: *There's a lot of unnecessary repetition here confusing what should be a reasonably simple statement. Try: ... loses buoyancy due to both freshwater and heat loss. For steady ..., the inflow of warmer, fresher Atlantic waters and outflow of colder, saltier Mediterranean waters through the Strait of Gibraltar balance the evaporative loss of freshwater and heat.*

P736 L24: *reword – Nof did not limit the Nile inflow.*

P737 L9-10: *...rates of increase in salinity and temperature...*

P737 L14: *.. some (need a verb here) to changes in air-sea fluxes...*

P737 L28: *repetitive, remove indeed.*

P738 L1: *reword - This saltier LIW was observed ...*

P738 L2: *reword - ... for the large ... that resulted in extremely salty western ... Because there is nothing to which to compare 'saltier'.*

P738 L4: *Western Mediterranean Deep Water (WMDW)*

P738 L6: *Define CNR.*

P738 L7: *... 2010, and 2013 ..*

P738 L10-13: *All the information after the colon is unnecessary. It is already repeated in caption for Table 1. I would suggest that it could be removed both from the text and the caption and be placed in Table 1 with columns for Ship, Station# and Reference. But, regardless, it should be stated only once.*

P738 L14: *Please include reference for Dyfamed and at least a minimal amount of description. e.g. Is it a mooring site or repeat hydrography?*

P738 L17: *What is this statement based on – the previous discussion? Table 1?*

P739 L21 and elsewhere: *remove psu unless used everywhere*

P739 L28: *... deep waters on long-term ... OR ... deep waters as the cause of the long-term...*

P&40 L12: *northwestern*

P740 L14: *...that was ...*

P741 L6-8: *Reference Bryden et al 2013.*

P741 L11-14: *Are these results of this analysis or is a reference required. If the result of this analysis, what data were used?*

P741 L16-17: *odd phrasing, please reword, perhaps something like: ... because the salinity excess and potential temperature difference in the new deep water were not as large as in 2005.*

P741 L20: *replace salting with salinification*

P742 L1: ... comparable to the long-term trends [seen at Location A \(Table 1\)](#).

P742 L9: ... of LIW, so

P742 L20: Remove 'In terms of deep salinity,'

P743 L20: Remove 'In terms of deep density,'

Table 1: See note above (P738 L10-13). Uncertainty estimates should be included. These could perhaps just be standard deviations over the water column below 200m. Also, please indicate the scales used for temperature and salinity either here or in the text. This statement ought apply to all values used in the manuscript.

Figure 2: Nice figure. Please indicate LIW, WMDW and the halocline-thermoclines discussed in the text. Note in the printed .pdf version of the figure text is too small to read.

Figure 3: Please indicate LIW and WMDW. Again, in the printed .pdf version, the figure text is too small to read. The 2004 and 2013 curves are the same color. The figure looks like it was created in Matlab. There are other colors besides the standard 6 available. The authors' might even consider using a colormap scale that would say go from blue to red with increasing year (and incidentally property value).

Figure 4: Please indicate the difference between the red and blue curves. Why is the 1600-1800 m layer shown as it is not discussed and appears so similar to the deeper curves that it cannot easily be distinguished. If there is a reason for including these curves, comparison would be easier if they were provide in the same panel as the deeper layer curves.