Ocean Sci. Discuss., 8, C407–C410, 2011
www.ocean-sci-discuss.net/8/C407/2011/

Ocean Science
Discussions

Interactive comment on "Calculating the water and heat balances of the Eastern Mediterranean basin using ocean modelling and available meteorological, hydrological, and ocean data" *by* M. Shaltout and A. Omstedt

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Received and published: 13 July 2011

The manuscript deals with the water and heat balances of the Eastern Mediterranean (EMed). The balances are assessed by interpreting observational data within a process-oriented modeling approach that was previously applied to the Baltic Sea. This appears to be a complex project, which requires taking into account a wealth of observational data from a variety of sources. Output is (monthly and yearly average) time series of in- and outflow through the Sicily Channel, surface and 500 m temperatures and salinities, and evaporation, furthermore a heat balance for the EMed. However, I find in the manuscript much to criticize, from inadequate information to inconsisten-

cies, too many items to address them all. Thus, the value of the mentioned time series, and of the items addressed in the Discussion Section, becomes partly questionable. In summary, the manuscript needs a major revision before it could be accepted for publication. My major items 2, 3, and 6 will require particular attention.

Major items: 1. Apparently, the approach deals with depth-resolved horizontal averages. The authors make reference to a previous paper (Omstedt and Axell, 2003) dealing with the Baltic Sea, which uses a similar formulation. I do not understand why such an approach needs horizontal momentum equations (Eq. 1 and 2). The relevant equation is Eq. 3, which defines the downward vertical transport W in terms of the overall water balance. But this equation is also funny, containing an obscure z-dependence: outflow Qout(Z), which is entirely undefined, Area(Z), but inflow Qin(z); W(Z) or W(z)?? What does all that mean? 2. The authors complement Eq. 3 with turbulent vertical mixing. The horizontal homogeneity implies that the T-S distribution is governed by the surface values and should thus tend toward homogeneity also vertically. In the real EMed the dominating vertical transports are winter convection and intermediate and deep water formation. The latter two are naturally rather localized, and they induce, among other features, T-S extremes and also induce upwelling. Winter convection dominates the upper few 100 m, leading to seasonally varying distributions. Vertical mixing of course has a role and depends on many parameters (e.g., topography), but I doubt that the authors' rather more theoretical formulation (Eqs. 9 to 12) is very realistic. 3. A case in point is Fig. 10, which shows that the deep-water model values decrease rather monotonically, missing the T-S decrease below the mid-depth maximum in the observations. The fact that the deep model values do not deviate too much from the observations must mean that the model was started using observations and over the model period did not deviate too much from these in the deeper layers. The claim in the Abstract (lines 10 and 11) that the water mass structure is modeled realistically and mixing modeled adequately is thus absurd. 4. With deep-water renewal times being appreciably higher (Roether and Schlitzer, 1991) than the model period, the initial values will naturally be of distinct influence for the waters of sufficient depth.

However, I do not find in the manuscript any information on initial values. 5. The water and heat balances are largely a matter of in- and outflow through the Sicily Channel (Eqs. 17 and 18). The authors deduce the inflow from absolute dynamic topography (ADT) based on satellite altimeter data. ADT data need a geoid, which the authors take for granted. But at least the geoid uncertainty and how that translates into a current uncertainty has to be given. Moreover, the authors assume that the surface velocity decreases linearly with depth, vanishing at a constant depth of 150 m. This is certainly an unrealistic assumption, if only because the interface must have (on average) a north-south slope in geostrophic response to the stacked in- and outflow. In my view the inflow, and thus the balances, become highly uncertain. The authors defend their approach by showing in Fig. 10 model T-S distributions with their normal Qin, and values changed by +-15% and +-50% compared with observations. In the upper waters, the spread of the model data is always less than in the observations, the normal value and +-15% are on the low side, while the +-50% model T-S values bracket the observations. So +-50% can be excluded, but Qin is known much better a priori than this. 6. A further problem is the fact that the Eastern Mediterranean Transient (EMT) since about 1990 induced highly transient conditions in the EMed, both in time and regionally, questioning the usefulness of horizontal averages. It is also a fact that the data bases quoted have to rely on averaging over several years, and thus are hardly capable to represent the transient situation during the EMT, in particular since the data base during that period is rather meager, extremely so for the peak year 1993. Even the T-S relationship was changed from that shown in Fig. 10 and the deep-water salinities were raised significantly. For detail, the authors should consult the work of Roether et al., Prog. Oceanogr. 74 (2007) 540. This problem adds uncertainty to the authors' model output.

Technical items: 1. p. 1304, line7: Levantine forms deep water only occasionally, limited in amounts and downward penetration, the players are the Adriatic and, certainly recently, the Aegean. 2. 1305/4-5: The discharge of the Nile after 1964 decreased by more than a factor of two that the authors mention, having mixed up information

in Ludwig et al. (2009). 3. 1306, item 6: Vertical spacing of the data must also be mentioned. 4. Numerical values for various parameters are missing, with only a very implicit reference to Omstedt an Axell (2003), an example is sigmaeff in Eq. 4. 5. ΔV in Eq. 6 is undefined. 6. I miss net evaporation in the conservation equation for salinity (Eq-. 7). 7. Qf in Eq. 8 is only defined in the context of Eq. 17. 8. Eq. 18 is wrong, in that Fin and Fout are in Watts, whereas Floss is in W/m2. 9. 1312/7: Increasing Mediterranean salinity has been demonstrated by several authors. 10. 1313/20 ff.: Fig. 3 overinterpreted? Relevant currents are only those that have crossed the Sicily Channel, in Figs. 3a and 3c I regard the southern current rather as (semi-) closed gyres. 11. References: Ludwig et al. is Vol. 80, not 8. 12. Fig. 2b suggests that the 800 m channel depth as shown represents the sill depths, which rather is at about 500 m depth as stated elsewhere (1304/3). The caption should clarify this. 13. Figs. 4, 5a, 6a, 7, and 11a show two graphs superimposed, but to distinguish between them is rather difficult. 14. The text has repeatedly language problems for which help must be sought.

Interactive comment on Ocean Sci. Discuss., 8, 1301, 2011.