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Ocean Science
Discussions

Interactive comment on "Simulation of the mantle and crustal helium isotope signature in the Mediterranean Sea using a high-resolution regional circulation model" *by* M. Ayache et al.

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This manuscript deals with an interesting modeling topic, worth of publishing in Ocean Science. A high-resolution ocean GCM (NEMO-MED12) is used to simulate the helium isotopes 3He and 4He in the Mediterranean, distinguishing between the components atmospheric, mantle and crust-derived and tritiugenic 3He, comparing the results with observations. The model-data comparison serves to check model performance. The tritiugenic 3He is taken from Ayache et. al. (2015), which used the same model. Mantle He is a small contribution, which the authors take from information in the literature. Rightly, they point out that mantle He in the Mediterranean deep waters is low, because

the sources are located at rather shallow depths. The authors conclude that the model simulations are generally realistic, but that the Adriatic source rate and its density are low. The upper boundary condition is He fluxes calculated using established functions of air-sea gas transfer. Their derived crustal He-flux is a factor of 10 lower than obtained in my work Roether et al. (1998). Abstract and Introduction are very good, the English is fine and the list of references is comprehensive. However, I note deficiencies that the authors must consider before submitting their final manuscript

Major items 1. A chapter on the observations used for the comparison, their uncertainties and their treatment is missing. As for differentiating between the components, the text simply mentions (Caption to Fig. 4) that they used the procedures of Roether et al. (1998 and 2013 of which the latter is more advisable; note that in the former paper the equations are corrupted, the correction paper J. Geophys. Res., 106 (C3), 4679 (2001) needs to be consulted). But that procedure makes use of Ne data, which the authors did not model. It must be clearly specified what procedures were used. Fig. 2 shows four Meteor del3He sections, but apparently only the first of them (1987) was used, which must also be stated. The choice is natural because the treatment assumes a quasi-steady state circulation. Furthermore, the atmospheric component, which is by far the largest, must be clearly defined, also considering that He solubilities are uncertain by up to 1%.

2. Apparently the tritiugenic 3He results from Ayaches paper earlier this year are used to correct for tritiugenic 3He, but In view of the fact that tritiugenic 3He dwarfs the terrigenic components (Table 3), I am convinced that the correction lacks the necessary precision. A case in point is Figure 4, which presents simulated and observed data on del3He for the sum of crustal and atmospheric components. The needed correction for tritiugenic 3He makes determination of the crustal component rather uncertain.

3. Judging from Figure 6, I have the impression that mantle He for the Tyrrhenian is overestimated, although the authors state that they were aware of my paper with John Lupton (OS, 2011) in which we demonstrated that most of the del3He effect is

tritiugenic.

4. I do not understand how the mantle 3He fluxes in Table 1 come about (Section 3.4). For the Tyrrhenian, various authors are cited, but I wonder what their basis was prior to Lupton's 3He observations, and to which degree their values are consistent. I also have doubts about the Sicily Channel values. The text states enhanced 3He between 600 and 1000 m depth, which apparently is in the depression in the Sicily Channel. That depression certainly received input by overflow across the eastern ridge by high-del3He waters (mostly tritiugenic) during the early EMT when density was distinctly enhanced (see 1987 section in Fig. 2). In the last paragraph it is argued on the basis of average release rates of 3He as a function of ridge length, for which an uncertainty of a factor of 2 is expected. Might the error not be even higher?

5. The discrepancy in the derived crustal He flux density from that in my 1998 paper is tentatively assigned to a possible overestimate in my work. I am convinced, however, that my flux stands on firm ground. The box model that I used was calibrated using observations of CFC-12 and tritium from my 1987 cruise assuming a quasi-steady state situation (Roether and Schlitzer, Dyn Atmosph. Oceans 15, 333-354, 1991). That work gave a renewal time of the Eastern Mediterranean deep waters of about 150 years (a value that never was challenged). This value is the basis on which my 1998 paper converted the 1987 He observations into flux densities of crustal and mantle He (about 5 % mantle He) using literature values for their isotopic composition and assuming steady state (just as assumed in the present work) and an arealy homogeneous mixing. A correction for tritiugenic 3He was made in the deep waters where that correction is small. A 30% uncertainty was reported. With respect to the authors' rate, note that the flux rate naturally adjusts to the vertical transport in the model. The authors admit that the model underestimates the strength and density of the Adriatic source, which after all is the principal deep water source in the eastern Mediterranean. Clearly, thus, the author's value is an underestimate. Because of the mentioned adjustment and considering that the atmospheric component is independent of water turn-over, model-

data agreement (Fig. 4) does not prove that the terrigenic flux rate is correct.

6. The authors state that the ocean surface He is essentially in solubility equilibrium with the atmosphere (p. 2009, line 10 f.), which means that the limiting step is the net upward transfer of He into the mixed layer from below. I therefore wonder why the authors chose a surface boundary condition in the form of water to air gas exchange (Section 3.2). Having instead assumed quasi-equilibrium at the surface, the vertical tracer gradients in the water column would hardly be different.

7. p. 2011, line 4 f.: I wonder whether the bottom layer extensions in the model as large as 450 m (p. 2011, line 3 f.) are really suitable (but I am not an expert in this), even if special adjustment to the bottom topography is applied. Especially in the Eastern Mediterranean there are ridges and deep passages that control the deep circulation on vertical scales of less than 100 m. To deal with that is a big challenge for modelers. A further example of such problem is that the EMT-related outflow from the Aegean and its densities obtained by Beuvier et al. (JGR 2010) were low compared with our own assessment in Chapter 6 of

8. I note in passing that, had simulated Ne been available, the authors could have obtained a clear separation of the atmospheric component and data on terrigenic 4He with no correction for the other He components being needed. Also scale problems in the He data (from measurement, solubility, incomplete equilibration at the surface) could have been avoided.

Technical items

1. P 2009, line 5; A citation for the atmospheric residence time of He is needed.

2. p. 2009, line 2 f.: It is stated that the low del3He in the deep layers is erased by the addition of tritiugenic 3He. In my view an even larger effect is due to EMT induced upwelling (the T-S correlation was totally changed).

3. p. 2013, line 18: Replace Weiss and Roether (1980) by correct citation (Weiss,

1971?).

4. Figure 3: the colors are hard to identify, in an inset showing just the colored lines at higher areal resolution might help.

5. Figure 4, caption: It is stated that data in Western Med to compare with the graph B are missing. Our book chapter mentioned above states, on the basis of the 1997 Poseidon cruise observations, that qualitatively that the crustal component was rather small, with the faster deep water renewal being one possible cause.

Interactive comment on Ocean Sci. Discuss., 12, 2007, 2015.