

# Simulation of the mantle and crustal helium isotope signature in the Mediterranean Sea using a high-resolution regional circulation model

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We thank Prof. W. Roether for their constructive comments and suggestions, which have helped to improve the manuscript. We have carefully considered all questions and concerns raised.

## Reply to Referee#1

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  $^3\text{He}$  and  $^4\text{He}$  in the Mediterranean, distinguishing between the components atmospheric, mantle and crust-derived and tritiogenic  $^3\text{He}$ , comparing the results with observations. The model-data comparison serves to check model performance. The tritiogenic  $^3\text{He}$  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.

**Response:** Done. A specific paragraph has been added to the text (see § 4)

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).

**Response:** We used Roether and al.'s methodology but we programmed our own formulas in an Excel spreadsheet. Therefore, our calculations are not affected by the "corrupted" equations.

But that procedure makes use of Ne data, which the authors did not model. It must be clearly specified what procedures were used.

Response: Done. See §4

Fig. 2 shows four Meteor  $\delta^3\text{He}$  sections, but apparently only the first of them (1987) was used, which must also be stated.

Response: Done. See §4

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%.

Response: With respect to data, the atmospheric component is that deduced from measured neon concentrations (see §4). In the model, the atmospheric component is the helium distribution in equilibrium with the atmosphere (= in the absence of any helium flux at sea bottom). This definition has been added at the end of §3.1.1 for the sake of clarity.

2. Apparently the tritiogenic  $^3\text{He}$  results from Ayaches paper earlier this year are used to correct for tritiogenic  $^3\text{He}$ , but In view of the fact that tritiogenic  $^3\text{He}$  dwarfs the terrigenous 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  $\delta^3\text{He}$  for the sum of crustal and atmospheric components. The needed correction for tritiogenic  $^3\text{He}$  makes determination of the crustal component rather uncertain.

Response: Tritiogenic  $^3\text{He}$  results from Ayache et al. paper 2013 intervene only in Figure 7 to sum up of all modelled helium components (including tritiogenic  $^3\text{He}$ ) for comparison with the  $\delta^3\text{He}$  measurements on the 1987 Meteor section. Figure 4, which compares modelled crustal+atmospheric  $\delta^3\text{He}$  with the corresponding data, do not require to consider any tritiogenic  $^3\text{He}$ , neither modelled nor deduced from data.

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  $\delta^3\text{He}$  effect is tritiogenic.

Response: As stated in the new §4, mantle  $\delta^3\text{He}$  for the Tyrrhenian is calculated by subtracting the background  $\delta^3\text{He}$  profile of station V01 in Lupton et al (2011) from all measured  $\delta^3\text{He}$  vertical profiles. Potential overestimations might have been possible if the stations located right above observed hydrothermal plumes were included in the dataset, but as explained in Fig.5 caption, those stations were discarded.

4. I do not understand how the mantle  $^3\text{He}$  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  $^3\text{He}$  observations, and to which degree their values are consistent.

Response: As explained in §3.1.3, mantle  $^3\text{He}$  fluxes for the Tyrrhenian (and the Aegean) were determined by simple scaling to the global  $^3\text{He}$  flux from arc volcanism, which can be estimated (to within a factor of two) to be  $\sim 4 \times 10^{-3}$   $^3\text{He}$  mol per km of arc based on the assumption that the magma production rate of arcs is  $\sim 20\%$  of that of Mid-Ocean-Ridges (Torgersen, 1989; Hilton et al., 2002) and the total length of subduction zones. In the absence of data concerning local/regional  $^3\text{He}$  fluxes, this method is the only one at our disposal. Although the method has a large uncertainty (a factor of 2), the reasonable agreement between model and data suggests that this estimate is correct.

I also have doubts about the Sicily Channel values. The text states enhanced  $^3\text{He}$  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-  $\delta^3\text{He}$

waters (mostly tritiogenic) during the early EMT when density was distinctly enhanced (see 1987 section in Fig. 2).

**Response:** As explained in the new §4, the mantle delta  $^3\text{He}$  in the Sicily channel was calculated in the same way as for the Tyrrhenian (i.e., by subtracting the local background delta  $^3\text{He}$  obtained from a station showing no plume-shape  $^3\text{He}$  anomaly). Therefore, any tritiogenic  $^3\text{He}$  contribution might have been removed.

In the last paragraph it is argued on the basis of average release rates of  $^3\text{He}$  as a function of ridge length, for which an uncertainty of a factor of 2 is expected. Might the error not be even higher?

**Response:** the exact error is difficult to assess but considering that the 20% contribution of arc magmatism to global magmatism is known with an uncertainty of +/- 50% and that the uncertainty on global  $^3\text{He}$  flux at Mid-Ocean ridges is about the same (=50%), the 100% uncertainty on our flux estimate seems reasonable.

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 areally homogeneous mixing. A correction for tritiogenic  $^3\text{He}$  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 terrigenous flux rate is correct.

**Response:** Our terrigenous flux is clearly a lower limit because of the weaknesses of the model regarding deep Mediterranean water ventilation rate. We agree that Roether's estimate is much closer to the reality. We corrected the text accordingly to make this clear (removed part in §6).

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.

**Response:** we agree that the vertical tracer gradients in the water column would hardly be different. We chose a surface boundary condition in the form of water to air gas exchange to be consistent with the standard protocol used by the model for other trace gases (CFC, SF6 ...).

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 modellers. 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

**Response:** We agree that this is a serious limitation of the model to describe in a fully realistic way the bottom and deep circulation. A new version with higher horizontal and vertical resolutions is being developed, which hopefully will overcome some of the shortcomings in model physics.

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 terrigenous  $^4\text{He}$  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.

**Response:** In contrast with the data, for which neon is essential to separate the various helium components, neon simulations are not necessary to separate the various helium components in the model, since each component is modelled separately.

#### Technical items

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

**Done.** See §1 line 40

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

**We agree with this remark. A sentence has been added to the text to make this perfectly clear in the revised manuscript. See §1 line 64.**

3. p. 2013, line 18: Replace Weiss and Roether (1980) by correct citation (Weiss, 1971?).

**Done.** See line 163

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

**Done,** the revised figure is enlarged.

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

**We thank the referee for this reference. We chose not to use the 1997 Poseidon cruise because, as acknowledge by the referee, the crustal component can only be estimated in a qualitative way due to the lack of a good estimate for the  $^3\text{He}/^4\text{He}$  ratio,  $R_{\text{ter}}$ , of the terrigenous component. However, a lower limit of  $\delta^3\text{He}_{\text{crust+atm}}$  can be estimated (taking  $R_{\text{ter}}$  equal to zero), which shows that the crustal component is indeed smaller than in the eastern basin, in agreement with Roether's findings. We added a sentence at the end of §5.1 to mention this.**

## References

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