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Biogeosciences Discussions

Interactive comment on "CO_2 exchange in a temperate marginal sea of the Mediterranean Sea: processes and carbon budget" *by* G. Cossarini et al.

Anonymous Referee #2

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##General comments

Manuscript "CO2 exchange in a temperate marginal sea of the Mediterranean Sea: processes and carbon budget" by Cossarini et al. presents a study on CO2 air-sea exchange and carbon budget in the Adriatic Sea during two years, 2007 and 2008, using a 3D carbonate-biogeochemical-physical model. The authors conclude that the Adriatic Sea acted as a sink of CO2 for the atmosphere during the 2 studied years characterised by different winter and summer conditions. According to the authors, the formation of dense water in the northern region in winter and its southward flow are

the dominant processes explaining the uptake of atmospheric CO2 by the basin. The model results presented here could be a great contribution to our understanding on the coastal region in CO2 exchange and carbon cycle, especially given the few modelling efforts undertaken so far in air-sea CO2 exchange. However, I will not recommend this manuscript for publication until major and minor points of concern are addressed in a revision.

A major issue of critiques concerns the validation of the coupled model. The comparison of model results with data is not sufficiently extended to allow a confidence in modelling results for the computation of air-sea CO2 exchange and carbon budget. The reviewers have yet no access to the results of the physical modelling of 2007 and 2008 (described in Querin et al., 2012) and their validation with observations (formation and spreading of dense water, temperature, water column structure). The variables and fluxes of the biogeochemical and carbonate models are compared with observations on different periods. A comparison with 2007 and 2008 observations when available (surface Chl, sea surface CO2 in the Gulf of Trieste) would be useful to validate the model. Additional comparisons with data, if available, relevant for the air-sea CO2 exchanges (respiration rates or net community production, DIC profiles) would also be useful to make the model estimates more robust.

Below is a list of some general points, on which I will come back in following specific and technical comment sections:

- The description of the coupled model should be more detailed.

- Estimates of fluxes at the Otranto Strait are questionable considering the proximity of the Strait to the boundary and the forcing method at this boundary.

- The use of two different atmospheric models for wind and heat and water fluxes forcing reduces the reliability of the estimates of air-sea CO2 exchanges.

- The comparison of atmospheric conditions 2007 and 2008 with climatology seems

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questionable. This has implications on the discussion and conclusion of the study.

- The authors should discuss the different sources of uncertainty in their estimates based on modelling.

- The manuscript generally suffers from poor readability: some sentences are awkward; it seems that some Figures and Table are poorly referred.

##Specific comments

#Section 2 - Move the site description from section 2 to the introduction section where the Adriatic Sea is partly described.

- A description of the coupled model in an appendix section would considerably help the reading and understanding of the manuscript.

- Could you give the value of the fraction of sinking material reaching the bottom that is remineralised in the sediment? Are both inorganic nutrients and DIC released in the water column?

- Was the calibration of the biogeochemical model done using observations in the Adriatic Sea?

- The authors estimate the flux of DIC at the Otranto Strait in section 4.4. However the forcing at this domain limit appears questionable considering the use of two different hydrodynamic models. The use of 1/16° INGV model for the physical simulation on one side and the use of OPATM for the physical/biogeochemical simulation on the other side could be a source of inconsistency in the fluxes at the boundary.

- The use of two different atmospheric models (ALADIN for wind data and MFS for heat fluxes and water fluxes) could generate inconsistency in air-sea CO2 exchanges.

- The authors should specify that no atmospheric input (except for CO2) is prescribed and how this could change the results of the study.

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- Could denitrification process, not taken into account in this study, modify the organic carbon remineralisation and air-sea CO2 exchange?

#Section 3.1 - Could the authors explain how they obtain the last decadal climatology?

- The authors described winter 2008 as similar to a “mean winter”. I don’t see this similarity: according to Figure 3, 2008 is warmer than the climatology between mid-December and March. The net positive heat flux anomaly in winter 2008 with respect to the climatology appears even more important than the one obtained in summer 2008 and mentioned by the authors L7-9 p10339.

- The authors should describe the results of comparison between model outputs and observations regarding the formation and spreading of dense water. Meanwhile the manuscript by Querin et al (2012) is published, could they send the last version or relevant extracts to the reviewers?

- The authors describe weather conditions and physical dynamics only for the Northern Adriatic while they study, in the following sections, biogeochemical and carbon exchange in the whole basin. Variability in winter heat fluxes and open ocean convection intensity that would occur in the Southern Adriatic Sea between 2007 and 2008, could induce differences in biogeochemical productivity during winter and spring in this region. Besides, the difference in summer heat fluxes between 2007 and 2008 for CA and SA is also discussed in section 4.4.

- A comparison with observed temperature at different points of the modelling domain would be relevant to validate the modelled solubility of CO2.

#Section 3.2 - A comparison of model results with satellite maps for 2007 and 2008 years at different seasons, instead of the comparison with those presented by Barale et al. 2005, would give more confidence in modelling results.

- The CO2 exchange due to biological processes depends not only on primary production rate but also on respiration rates and on the structure of the water column

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(stratified or well mixed). However, no comparison with data on the stratification of the water column and on the respiration rates is presented.

#Section 3.3 - A comparison of model results with observations of pCO2 and CO2 exchange at the VIDA point (Turk et al, 2012) would contribute to a validation of the carbonate model results.

- Does pCO2 decrease to 200-250 ppm in winter in the whole Adriatic Sea? The pCO2 in winter in the SA presented in D'ortenzio et al. (2008) is around 350 ppm. Could you compare simulated SA pCO2 with results of D'ortenzio et al. (2008, Fig10) for the different seasons? It would be nice to see maps of annual or seasonal pCO2 and CO2 exchange.

#Section 4.2 - The definition of Adriatic dense water (referenced as 'AdDW'), being rho>1029.5 kg/m3, is confusing and seems inappropriate with the following description of the spreading of dense water: the isoline 1029.5 and thus the associated AdDW water mass are only visible for March 2008 on Fig 8 (i.e. Fig 8.b), whereas the authors describe the flow of AdDW mass in the whole basin.

- It would also be nice to see vertical sections of DIC.

- A core of dense C-rich water masses (less dense and C-rich than in 2008) is also visible on the bottom of the slope for 2007 in CA region (Fig. 8c), contrary to what is suggested by the authors L25-26 p10345.

#Section 4.3 - Are the DOC fluxes included in the organic carbon fluxes described in section 4.3 and presented on Fig 9?

- L 11-12 p10346: Is sinking taken into account in the vertical fluxes of organic carbon plotted on Fig 9?

- L 24 p10347: Could you give the value of the fraction of organic carbon reaching the bottom?

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- The comparison of simulated and observed (Boldrin et al., 2002) organic carbon vertical fluxes would be more relevant if done at the same depths (150 m and 1050 m), same period (post-open-convection) and same location (L26 p10347-L2 p10348).

- The sentence L9-10 p10348 is partly redundant with the sentence L24-25p10347.

- L19 p10348: Use of “Thus”: I don’t find logical the linkage between the previous sentence and this sentence.

#Section 4.4 - “Our simulation shows net outward POC ïňĆuxes of 0.2 and 0.4 Tg C yr−1 and net outward DIC ïňĆuxes of 3.2 and 4.8 Tg C yr−1 in the two simulated years.” What about DOC fluxes?

- As mentioned before, the estimation of fluxes at Otranto Strait is questionable considering the forcing at the southern boundary.

#Conclusion - “The winter dense water formation in the northern continental shelf, as well as its spreading and sinking along the slope and into the deeper layers of the central and southern basins, are the key processes for [. . .] preventing carbon re-exposure at the surface during the subsequent mixing period.” This is not shown by the results of this study.

- Observed and simulated fluxes (sink of organic carbon and burial) indicated in Table 1 seem to be not commented in the text.

##Technical corrections

- Replace “De Madron” by its entire name “Durrieu de Madron”

- L17 p10333: Replace “equal” by “estimated”.

- Fig 4: Maps with labelled isolines would help the reading.

- L19 p10339: “DIP concentrations are in the range 0.1-0.15 mmol/m3”. Do the authors refer to modelled or observed DIP concentrations? In Table 1, simulated DIP

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concentrations are not in the range 0.1-0.15 mmol/m3 except for spring-summer 2007. In general, it is often not clear if the authors refer to simulated or observed results.

- DIP concentrations in CA deep water are not indicated in Table 1 as suggested L5-6 p10340.

- L14 p10340: the “nearly” term is exaggerated regarding the model results.

- L16 p10340: Do “The maps” refer to Figure 4?

- L10 and L22 p10342: It seems that the authors refer to Fig6 instead of Fig7.

- Add “simulated” before rate of CO2 exchange in legend of Figure 6.

- Fig. 7: The PP –PR curve is cut by the graphic box’ edge.

- L24 p10344: It seems that the authors refer to Fig6 instead of Fig4.

- L6 p10345: “covers” instead of “cover”?

- L11 p10346: Replace 190m by 180m depth as it is indicated on Fig9?

- L11-12 p10346: Remove one “spatial” in the sentence.

- Fig.9a: The “Corg flux” curve is cut by the graphic box’ edge.

- L 28 p10346-L1 p10347: I don’t understand the sentence “It was later determined that the largest fraction of carbon entering the CA is transported southward to the SA.”

- L 2 p10347: Could the authors indicate the Bari canyon on Fig1?

- L11 p10348: Remove “(winter 2008)” for a better consistency with the next sentence.

- L18 p10348: I don’t understand the reference to Fig6. Do the authors want to refer to Fig 5 after “phytoplankton bloom”?

- L7 p10348: The authors refer to Fig6 instead of Fig4?

- L8 p10349: Remove “unexpected”.

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- L17 p10349: Do the authors refer to Table 2 instead of Table1?

- L 27 p10351: The linkage “In contrast” seems inappropriate since negative fluxes are also simulated in the eastern NA (Fig 6).

- L11-13 p 10352: I don’t understand the sentence “Because the C-rich, dense water of the deep layers fuels the deep Mediterranean Sea circulation, climate change would potentially decrease the capacity of the Adriatic Sea to absorb atmospheric CO2.”

End of review

Interactive comment on Biogeosciences Discuss., 9, 10331, 2012.