

Review of Ulses et al. (2022): “Seasonal dynamics and annual budget of dissolved inorganic carbon in the northwestern Mediterranean deep convection region”

Summary

This study by Ulses and coauthors presents a detailed carbon budget in the deep-convection area of the NW Mediterranean Sea for the period between September 2012 and September 2013. Using an ocean biogeochemistry model forced with daily output from a physical model, the authors find that their focus region is a moderate sink of atmospheric CO₂ over the study period. In addition, by dividing the study area into the upper 150m and the deep ocean below that, they find that both physical and biological fluxes play an important role in controlling carbon fluxes across seasons.

Overall, the authors did a great job comparing their model results to existing observations and presenting the carbon budget of their study region in great detail. Therefore, the study is generally suitable for publication in Biogeosciences. However, I would like to raise several points, mostly regarding the presentation of the study, which should be addressed before the publication of this manuscript.

Please see the detailed explanation of these major points and all my detailed comments below.

Main comments

1. Introduction: Acknowledging that I am not 100% familiar with the literature concerning the Mediterranean Sea, the introduction appears to give a good summary of previous work. However, it fails to make the knowledge gap clear enough in my view. In its current form, it still reads too much as a collection of results from individual studies, making it hard for the reader to figure out what has not been addressed or what the short-comings in each of these previous studies are. As a result, I am a bit lost guessing what exactly the focus of the study by Ulses et al. is until L. 108. I suggest revising the introduction to more clearly state where the knowledge gaps are that are to be addressed in this new study.
2. Description of the model setup: While being methodologically sound from what I understood, the description of the model setup in section 2.1.2 is currently hard to follow. I suggest including a sketch in the revised version of the manuscript illustrating the downscaling approach and providing information on the initialization and run time of the simulations in each of the steps of the setup.
3. Result section 4.1: I admittedly found it quite difficult to keep up with all the provided details in this section. In general, I appreciate the detailed description of the figures, and I generally think the clear division into the different seasons is

good. However, this division means that the reader must constantly jump back and forth between Fig. 6-11, making it very important to have consistent structure and summarizing sentences throughout this section. While such summarizing sentences already exist for some of the seasons (see e.g., winter sub-period 2), they do not for others. I thus suggest that the authors carefully screen the result section again to structure the description of each season as consistently as possible and that they add clear summarizing sentences to each of the seasons. I encourage the authors to work on the paragraph structure (including topic sentences), as this will greatly improve the readability of this part of the manuscript. Lastly, since I really appreciated Fig. 12 as a summary for the annual mean budget, a similar figure for the seasonal budgets (=1 figure, 4 panels) would be a valuable addition to the paper and would serve as guidance for the reader throughout section 4.1.

4. For the sensitivity experiment regarding calcification: I was surprised to see an enhanced oceanic CO₂ uptake relative to the reference case in the experiment accounting for calcification. For such an experiment, I would expect less oceanic CO₂ uptake, given that the impact of calcification on alkalinity is twice that on DIC (thus increasing seawater pCO₂ at the surface). Going back to your method section 2.1.4, I noticed that you only specified the impact of calcification on DIC – did you also include its impact on alkalinity in your sensitivity test? How did you parametrize dissolution at depth? I note that I realize that either way, this will not impact the outcomes of the main findings of this study, but if this was indeed a mistake, I suggest that the authors correct it.
5. Language: I spotted numerous (minor) grammar mistakes, e.g., related to prepositions (see detailed comments below). While this did not impact the readability much, I encourage the authors to carefully check the text again during the revisions.

Detailed comments:

L. 22: Maybe better: “seasonal and annual budget”?

L. 26: “reduction of *oceanic* CO₂ uptake”

L. 27: I suggest rephrasing this sentence by being more specific: Aren’t the physical fluxes (of DIC) always larger than the biological ones? How are both dominant?

L. 28: define “upper”

L. 29: I suggest replacing “air-sea flux” by “oceanic CO₂ uptake”

L. 37: “comparable role *to...*” and “processes *for* carbon”; carbon transfer from where to where? Please specify.

L. 42: I suggest rephrasing to “taken up at the ocean surface”

L. 43: If there is an “on the other hand”, I am immediately looking for “one the one hand”. Maybe better: “at the same time” or “simultaneously”?

L. 56: moderate phytoplankton bloom

L. 55-56: Is it typical in the Mediterranean science community to refer to the fall bloom as the “first” bloom? I realize this is a matter of defining the start of the growing season, but from all other regions globally, I am used to describing the bloom phenology starting with the strong first bloom in spring after nutrients were replenished in winter and a secondary typically weaker bloom in the fall.

L. 57: “nutrients *to* the euphotic layer”

L. 68: Please add a reference to Fig. 1.

L. 71: “which bring DIC-rich water to the surface”

L. 78: Maybe “complemented” instead of “enriched”?

L. 79: delete “fixed”

L. 81: “drives *an increase in surface pCO₂*”

L. 83: model instead of modelling

L. 83-84: I am not sure what this approach means. Can you rephrase this part?

L. 86: biological instead of biology

L. 93: “limited *to*”

L. 92-94: This sentence was very confusing to read due to all the “or”. Can you rephrase or split it into two?

L. 94-96: To me, this knowledge does not yet become clear enough from what is written up to this point. I suggest revising the introduction to more clearly highlight the knowledge gaps and why these matter.

L. 104: “by a *positive* net community production”

L. 108: Maybe better: “take advantage of” instead of “benefit from”

L. 112: Throughout the paper, you sometimes say “biological” and sometimes “biogeochemical”. I suggest to consistently use one because from what I can see (please correct me if I am wrong), you are always referring to the same processes.

L. 120 & L. 125: Have the different models been evaluated in detail over the bigger study regions in any of these studies? It might help to explicitly state that for the interested reader.

L. 124: How are particle dynamics parametrized in the model? Given that sinking fluxes of biologically-derived particles are an important part of your study, some information on that will be helpful.

L. 131: Before looking up the cited references, it was unclear to me how the version before can resolve the cycling of carbon without including DIC. I suggest clarifying that only particulate organic carbon was included before.

L. 136: "is *the* respiration"

L. 142: "not the case *for* total alkalinity"

L. 146: Maybe add "throughout the water column" if that is what it is.

L. 147-149: Personally, I wouldn't call a paper from 2005 "present knowledge". There are several studies that, albeit of course not perfect, have parametrized it. Thus, I suggest rephrasing this part.

L. 149: "tests *on* this"

L. 165: Please add a reference to Fig. 1.

L. 167: I suggest adding "have been described in detail in X and Y and will be summarized here."

L. 169: It is unclear to me what "hydrodynamic analyses" are. Please clarify and possibly rephrase.

L. 167-175: I found the description of the steps rather difficult to follow. I think adding a flow chart detailing the different steps could help a lot.

L. 179: Given that the model simulates the negative charge and not alkalinity, did you correct the measured alkalinity to correspond to the model tracer? Please clarify.

L. 183: What is a "rigorous mathematical approach"? Please clarify or delete.

L. 189: You only specify what was used for winds here. What about other atmospheric forcing variables (e.g., radiation, humidity, precipitation etc.)? Please be complete.

L. 191: What I am missing here is a description on the model run time in each step. Also, in L. 179 you mention an initialization in summer 2011, while I think (if I understood correctly), the final model was run from September 2012 onwards. Could you clarify? My confusion on this point convinces me even more that a flow chart detailing the model setup procedure would help.

L. 193: I find "DIC flows" and "inventory variations" rather confusing. Maybe "DIC fluxes" and "inventory tendencies"? Please check throughout the text.

L. 194: "for at least 1 day"

L. 201: Given the title of this section, I wonder if Eq. 1 is better to be placed here. Additionally, I think at least the general budget equation (Eq. S1) should be moved to the main text.

- L. 203: What do you mean by “internal variation”? Please clarify.
- L. 215: Please add a reference to the respective Equation.
- L. 216: “as 0.5”
- L. 220: Please be precise: NCP does not appear as such in Eq. 1.
- L. 221: Please state here what the parametrization by Lajaunie-Salla et al. (2021) is. Ideally, the reader should not have to look up other papers to understand what you’re doing.
- L. 225: sea surface
- L. 284: I suggest adding “reflecting a” in front of “period”
- L. 298: Does the southern zone include everything south of 41°N or is there a southern limit?
- L. 299: I assume the depth profiles have been subsampled to only include the cruise locations shown in Fig. 3. Please clarify.
- L. 324: Do you mean “alternating” instead of “alternative”?
- L. 325: Where can the direction of the wind be seen? If this is previous knowledge for the region of interest and you therefore decided not to show this explicitly, please make sure it is introduced in the introduction for clarity.
- L. 337: Unless I misread something, I think the minus sign should be omitted (the cumulative flux is positive according to Fig. 7).
- L. 344: Do you mean “DIC concentration in the ML” or “the DIC flux into the ML”? Please clarify.
- L. 441: “episodes of heat *gain*”
- L. 466: To me, it is odd to call this flux biological production, when this is in fact remineralization/respiration. I understand why you do it and it is technically correct, but I still suggest rephrasing to avoid confusion.
- L. 469: For consistency with how you described the biological component, it would be easier to read if you also reflected the sign convention in your wording here.
- L. 474: I suggest deleting “an amount”.
- L. 486: Please see my comment on the abstract regarding “both dominate”. I suggest to also rephrase here.
- L. 489: Here and throughout the discussion section: Can you find more descriptive/informative section titles? It is incredibly useful to the reader if the title of each section already conveys information, i.e., ideally the main take-away message.
- L. 490-502: As far as I can see, these are results. I am not convinced this part is necessary.

- L. 508-509: This sentence is unclear to me. Can you rephrase?
- L. 528-530: Here and throughout the text: Try to avoid 1-2 sentence paragraphs.
- L. 552: Please see my major comment on these sensitivity experiments.
- L. 566: Is there a “yr-1” missing? Additionally, it would help to provide the range based on your model here again to compare to the cited paper more easily.
- L. 576: It might be more appropriate to say “physical transport”.
- L. 577: “the vertical DIC distribution”
- L. 581: “greater magnitude” – Please specify the sign.
- L. 582: “sea heat loss” Do you mean “ocean heat loss”? Please clarify.
- L. 589: Please rephrase “DIC exchange flows”.
- L. 595: “as illustrated *in*”
- L. 608: “slowed down” instead of “braked”
- L. 617: “convection” instead of “convention”?
- L. 633: “from” instead of “into”?
- L. 634: “a lateral outflow”
- L. 640-646: It would be a lot easier to compare to the findings of your studies, if you reported these numbers as flux densities instead of as integrated fluxes (or to here report your findings in the same integrated unit).
- L. 648: “into” instead of “in”
- L. 666: I suggest adding “...and rising atmospheric CO₂ levels”.
- L. 680: budgets
- L. 691: What exactly are the first and second part here? Please clarify.
- L. 701: “subject to”

Figures:

Fig. 3: Please specify for what depth(s) the model output is shown here.

Fig. 4: I suggest adding a legend/title above each column.

Fig. 7: I suggest using the same colors for the same components in all panels, not only in a & b, but also in panel c. Additionally, it is unclear to me why you decided to show the seasonal averages only for the upper layer and not for the deeper layer. Please consider adding the extra panel for completeness.

Fig. 14: Please link the caption more clearly to the figure: which bar is which experiment? Only giving the reference requires the reader to be familiar with every single paper, which will not necessarily be the case (it certainly isn't the case for me).

All figures: Please double-check that the sign convention of all fluxes is defined in the respective caption.

Supplementary material:

Eq. S2: "DCA" is not defined in the text.

L. 29: How are sediment fluxes treated in the model? How large are they compared to the other components? Without any further information, it is difficult to judge for the reader to what extent this assumption impacts the role of vertical fluxes (which are treated as the residual and will therefore include any sedimentary contribution).