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Comment on egusphere-2022-564

Siv K Lauvset

Community comment on "Long-term and short-term inorganic carbon reservoirs in Aegean seawater – an experimental study" by Fabian Matthias Gäb et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-564-CC1, 2022

I stumbled upon this manuscript after a discussion with a colleague and was intrigued. I quite like the concept, but I was disappointed at the authors failure to relate their results properly to the real world. It would have been great to see a proper motivation for the study, and a discussion about the applicability of these equilibrium experiments to a world that is currently far from being in equilibrium. The manuscript is reasonably well-written, but throughout the authors' unfamiliarity with this scientific discipline is apparent. There are several cases where previous work is misunderstood, and these misunderstandings used to support conclusions. Unlike another reviewer I do not find the conclusions to be defendable. This work does merit publication, but only after major revisions. The authors also need to do a proper literature review about anthropogenic carbon and the methods used to estimate this (there are several with varying assumptions associated). Below I've listed the most serious issues I've identified.

Section 3.2

The authors claim that previous work and textbooks state that TA is independent of pCO2. This is incorrect. TA is in textbooks claimed to be independent of air-sea gas exchange. Meaning that increasing pCO2 in the ocean as a result of increasing pCO2 in the atmosphere does not change TA. The authors show that when pCO2 becomes very low aragonite precipitates and this changes TA. But what changes TA in this case is the precipitation of calcium carbonate, not gas exchange. The authors need to change their wording in this section.

Section 3.3

Here the authors make some quite wild speculation regarding the formation of aragonite ooides observed at the Bahama banks. It could very well be correct, but some more context would be useful here.

Section 4.1

The authors quantify the anthropogenic content by equilibrating their sample with atmospheres of 280 uatm (pre-industrial) and 410 uatm (present day) respectively. However, the present day atmospheric level is a result of the ocean and land sinks already removing ~50% of the emissions. So it is inappropriate to equilibrate with the current atmospheric level to quantify anthropogenic carbon in the ocean given that the current atmospheric level is a result of the ocean absorbing large amounts of emitted carbon. In addition, the authors are treating this as an equilibration problem while the real world is not in equilibrium. The thinking behind the method is flawed, and I am uncertain how relevant the results are to the real world. The authors offer no discussion for context here. A better motivation and justification on how these results relate to the real world is necessary.

Section 4.2 (and parts of the introduction)

Here the authors have fundamentally misunderstood the studies they cite, and the methods used by those studies. The authors, unfortunately, seem to be very unfamiliar with this field (ocean anthropogenic carbon) of research. This is a major flaw and the manuscript should not be published before it has been fixed. Gruber et al (2019) does not use mass balance to calculate the anthropogenic DIC changes in the ocean. Friedlingstein et al (2020) does not estimate anthropogenic DIC in the ocean at all, but rather the net flux of CO2 into the ocean, and does not use mass balance to do this. Equation 1 in Friedlingstein et al (2020) shows how the budget imbalance is estimated as a residual when all terms on the right-hand side of the equation are estimated/calculated independently. This is clearly explained in the Friedlingstein et al. (2020) paper. In the introduction the authors also claim that anthropogenic DIC is usually calculated using mass balance. This is incorrect. None of the studies they cite, nor any other as far as I am aware, use mass balance for this. Neither do anyone use "comparative analysis of DIC of deep ocean water that has not yet seen ingression of anthropogenic CO2 and other anthropogenic gas species". Sabine et al (2004) and Gruber et al (1996) both use a method called DC*, but the authors seem to think these studies use different methods (mass balance in the former and comparative analysis of deep ocean and surface in the latter). Friedlingstein et al (2020) and Na et al (2022) do not estimate anthropogenic DIC at all, so it is very strange to include these studies here. Since the authors in section 4.2 goes on to draw conclusions about their own work based on their flawed understanding of previous work this is a major flaw in the paper.