Biogeosciences

Discussions

Open Access

EGU

# Interactive comment on "A probabilistic assessment of calcium carbonate export and dissolution in the modern ocean" by G.Battaglia et al.

W. Koeve (Referee)

wkoeve@geomar.de

Received and published: 20 January 2016

The CaCO3 cycle is an important component of the oceanic carbon cycle. Modelling and model evaluation of this component, however, appear to be less well developed, compared to that of the so called organic tissue pump. The manuscript of Battaglia and co-authors applies a probabilistic approach using the Bern3D ocean model to assess export and dissolution of calcium carbonate on global and basin scales and in the modern ocean. It is one of the most detailed studies on this subject, that the reviewer has seen. I regard it an important contribution to our quantitative understanding of the oceanic carbon cycle, its modelling and hence forecast.

The study provides new estimates of CaCO3 export and the large scale distribution of its dissolution, both with respect to the three major oceans, as well as to the three principal domains where dissolution might occur (in/on top of the sediment, below the saturation horizon, and above it).

A major result of this study is that the kind of observations (alkalinity, CaCO3 deep ocean flux data) and tracers constructed from them (TA*) that are currently available do not allow to constrain at the same time CaCO3 export and the functional form of its dissolution in the ocean. In particular, significant dissolution above the saturation horizon can not be ruled out based on the specific combination of data and the approach and model chosen. "Dissolution schemes, with and without dissolution above the saturation horizon, achive realistic (i.e. observed) TA* distributions". Ocean circulation, including mixing, affect concentration gradients such that different dissolution schemes can't be distinguished statistically.

I suggest publication of this work in BG, however, after moderate revisions of the ms.

Important issues:

Structure: In particular, the organisation of the paper and of the arguments put forward lacks clarity. In fact, I got the strong impression that the authors have reorganised their paper very recently, but without making sure that the order of arguments supports reading and understanding of the presented material as good as it could. This reorganisation is very obvious from the order of references to several figures which are misplaced to a degree I have not seen before. For example, Fig 2 is referred to three text pages after Fig. 3, Fig. 4 is referred to 2 pages after first referral to Fig. 5. Finally Fig. 6 is referred to on p20245 for the first time, long after Figs 7, 8, and 9 have been discussed on the four pages before. This is clearly not a good practice and very confusing for the reader. Please improve this.

TA*-CFC method: In the abstract, introduction and discussion you make reference to 'the TA*-CFC-age' method, which has been used by others (e.g. Feely et al.) in sup-

port of shallow (above saturation horizon) dissolution of CaCO3. The way you present this material is not really appropriate. Since the paper of Friis et al. 2006 it is well acknowledged that TA* as a tracer being produced in the interior of the ocean and being destroyed only at the very surface (either explicitly as in Friis et al. or Koeve et al. or implicitly when TA* is diagnosed or 'constructed'). Such a tracer has to show gradients towards the surface, and significant concentrations far away from the site of TA* generation. These gradients, if not properly corrected for the effect of mixing will give rise to artificial rate estimates when combined with age tracers. The Friis paper is well received by the community, hence the TA*-CFC-age based estimates are more a historical note, and not a state-of-the-art rate estimate. I would hence not consider the difference between rates estimates from this method and yours to be a surprising/important result worth to be mentioned in the abstract or the introduction. This avoids the problem of introducing this method in more detail there (you can't assume that everybody knows the TA*-CFC-method!). I therefore suggest to discuss the older TA*-CFC-age based estimates briefly in the discussion only. By the way, TA* tracer mixing is only one of several issues with this approach.

CO3-ions: In the introduction you speak about carbonate ions in a sometimes strange, awk or wrong way. Eg. 20225, 11 you say that CaCO3 is formed from Ca2+ and CO32-. To the extend that the actual carbon species used in the formation of CaCO3 is know it appears to be different for different CaCO3 producers. Even if 1 mol of CO32- would be taken up per mol of CaCO3 formed (and exported), the net effect would not be a decrease of 1mol of CO32- in the surface ocean. This is simply due to rapid re-adjustments within the carbonate system. The actual rate of change per mol of CaCO3 depending on conditions. Also, 20225, 14-15, the 'uptake and release of CO32- introduces vertical gradients in alkalinity and DIC' is misleading, at least. 20226, 27: 'mixing of CO32-' is awk. CO32- is NOT a tracer, hence it is not mixed conservatively. Please consult e.g. Wolf-Gladrow et al.-, 2007, Mar. Chem., 106:287ff for details. Or use a tool like co2sys to take a look at the effect of mixing of e.g. two water masses with distinct TA and DIC for the effect on CO32- concentrations along

the mixing line. Please carefully revise the text accordingly.

Methodology: Latin-hypercube, the specific way in which skill scores are assigned and used when computing e.g. medians could at times be more specificly presented and/or supported by references. Having not done this myself (like most of the readers of BG), I sometimes had to guess what you actually did. From the list of possible journals within the special issue you chose BG instead of GMD. Hence I think you should consider that the audience of BG consists of many non-modellers. Also your work should be understandable to people that address the CaCO3 cycle from the observational side in order to make your work most influencial. Following to my argument about the organisation of the paper, I sometimes had the impression that some details of your approach become clear late in the paper while the respective Fig. numbers indicate that that section may have been moved towards the back of the paper recently. In practice, section 3.3.2 could be extended providing more details on how eg. medians shown in Fig. 5 are computed. It is particularly in section 4.1 where I sometimes missed details of the approach, e.g. when you speak about ' the optimisation procedure with the application of regional skill scores' (20239, 19) or the 'median fields' (20240, 3) shown in Fig. 4. How are the latter actually computed. Potentially a reader (me) would better understand all this if s(he) reads Steinacher et al. carefully first, but then (s)he may not return to continue reading your paper. Which I would regard a pitty.

Prescribing the saturation state from observations: This is potentially a (severe?) limitation of this study. The actual saturation state is due to the pressure effect, the preformed DIC and TA, the remineralised DIC and TA and the imprint of CaCO3 dissolution (in terms of DIC and TA). To the extend that CaCO3 dissolution is saturation dependent, the former feeds back on its conditions in the real ocean, but not in your modelling. I suggest to discuss, and potentially quantify in sensitivity runs, how significant that feedback is for TA* profiles and inventories in the non-constant cases.

Units: In 20229, 22-25 you say that you will present TA* in TA-units, but later you give TA* inventories in Pmol C. I suggest that, for the purpose of your paper, it would be

least confusing for the reader if you always use mol C based units (for local TA*, its inventories, and ALSO the CaCO3 flux values, which are currently in Gt PIC; Gt PIC-C, or PIC-CaCO3 is not specified). Currently you use three different sets of units, which is not helpful for the reader.

Below I provide some more specific comments and suggestions, ordered largely by appearance in the ms;

20225, 10: name other CaCO3 producers, please

20225, 17: 'This redistribution' refers to mixing and transport in the sentence before, right? But isn't the CaCO3 counter pump, which makes up the gradients (l 15) what in particularly affects the ocean-atmosphere partitioning of CO2? The redistribution tends to reduce that effect. Rephrase please.

20225, 20: 50ppm on century times scales appears to be a lot. I think, that that is not in agreement with e.g. Hofman and Schellnhuber, 2009 (PNAS, I think).

20226, 15: 'the TA*-CFC age method'; you can't assume everybody to know what that is; either you introduce it here, or (better) leave it out; much of the issues raised by the TA*-CFC-age approach disappears when understand the general nature of the TA* tracer; however, that one (TA*) has not even been introduced at this point to the reader; please do so at least in a generic sense (e.g. TA* to reflect the imprint of CaCO3 dissolution on TA) and rewrite the paragraph accordingly; perhaps point to section 2 for details)

20226, 20-21: perhaps add a reference here

20227, 27 & 20228, 3 & 20228, 25: at this point it is not clear what the mean values are referring to, i.e. which dataset (GLODAP, WOA)?

20228, 12: 'observations of TA', it is not clear whether you did this with observed TA (bottle data e.g. from WAVES) or gridded GLODAPv1 or regridded GLODAP to your Bern3D grid. Please specify.

20229, 12: refer to Fig. 1a, not just Fig. 1

20229, 20: Is the fraction of TA* above the saturation horizon similar to that reported in Koeve et al., 2014?, Compare, please.

20229, 22-25: TA-units. Here, like in much of section 2 your description follows that of Koeve et al. 2014 to a certain extend. However, later in the text you don't follow your own advise. TA* inventories are given in Pmol C, not Pmol TA. See my general comment above for consolidating the units.

20230, 6: Latin-Hypercube. Since you don't introduce this method in too much detail, perhaps give at least some references pointing to papers that present more details, if possible from ocean studies, helpful to readers from that domain.

20231, 2 : 'scaling-factor', as used here is lab slang. Please be more specific.

20231, 3: 'describes'? 'represents'

20231, 16: 'six regions (Fig. 3).,': The regions are not really visible from that figure. This confusion is amplified by the fact that the text speaks about different values of the rain ratio while the caption of Fig. 3 talks about a constant value for the explicit experiment shown in the figure. I suggest to add a map in which the six regions are colour coded. This figure can be presented in the appendix. Fig. 3 should not be referred to here.

20231, 23-29: please give Friis et al. and Koeve et al. as references for the explicit TA* tracer

20232, 5-10: perhaps mention explicitly that TA* of the deepest wet box is affected/modified accordingly (from sediment fluxes)

20232, 16, 'thereafter' ? is there any thereafter? I though all your runs are spin-up runs. Delete the phrase 'and kept constant thereafter'.

20233, 3: (here and elsewhere) 'Gangstoeet' al misses a blank before 'et'

20234, 22: Ref to Tab. 4. Looking at the table I don't understand kdia since that is introduced much later in the text. I suggest to improve the table caption accordingly.

20234, 23 (and elsewhere) a ratio is not to be given in % (but here in mol/mol) Also: I suggest to always be explicit 'molar CaCO3/POC export rain ratio' instead of just 'rain ratio'. Rain ratio may be easily mixed up with rain rate, which is POC flux to the sediment. It could also be the POC/PON rain ratio, or whatever. Avoid ambigiouties.

20234, 24-25: Units, see general remark above

20235, 11: fcalc is a fraction, but is given in %, correct please (see above)

20237, 15: change title to 'A first order correction to CaCO3 burial' since that is the important aspect

20237, 20-21: isn't this in conflict with 20238, 12-13? '25%' is not equivalent to 'largely unchanged'; also the phrase on 20237 is presenting some results in the mid of the methods presentation. Improve please.

20238, 8: 'sediment BURIAL correction'

20239, 8, 'by definition' this is explicitly true for your model TA* tracer, but not 'by definition' for the reconstructed (or diagnosed) TA*; you mentioned some non-zero (negative) values yourself, likely in waters close to outcrops or at the surface

20239, 11, 'age of water' this is very implicitly known here only; you could refer to A2, though that is del14C which is not age (see e.g. Koeve et al., 2015, GMD, and refs therein)

20241, 3: Fig. 7: I don't understand how the black line in Fig. 7 reflects the sums of the coloured lines ('the sums of regional PDFs', line 4).

20141, Fig. 8: SO export fluxes show strong discontinuities when moving from PAC to ATL and from ATL to IND. That points to some limitation of your realisation of a regional approach (e.g. not considering the SO as an explicit region). This should be discussed.

20241, 18-20: treatment of deepest wet box in no-sediment runs needs to be presented earlier, i.e. section 3

20242, 6-14: I wonder how sensitive that is to the aspect of having no feedback from CaCO3 dissolution on the imposed omega field

20244, 3, kdia. This is one of the details I missed earlier in the paper.

20244: are global mean del14C values meaningful? Koeve et al., 2015 GMD showed that different ocean models which applied the same 'scaling factor' may have quite different preformed del14C; hence the global mean del14C is not a good measure of the mean age; this is likely the case also for one model with different kdia realisations, I suspect; please discuss

20245, 26-28: This is implicit only in the comparison of upper and lower panels of Fig. 6. Could you prepare a figure to make that explicit, please? Also, I think Fig. 6 should be shown much earlier in the paper. It could help to understand Figs. 7-9 which you discuss before Fig. 6.

20246, 5-9: One of the CaCO3 related papers by Andy Ridgwell should be discussed here. Didn't that show also that parameter values could not be constrained well/independently from TA data, if I recall correctly.

20246, Section 4.5. You use flux data instead of TA* as a constraint. Is it possible to combine both? Would that be a better constraint?

20247: You discuss the TA*-CFC-age method before introducing it. I suggest to reorganise the text accordingly.

20249, Fig. 10 very nicely reflects the current status of our quantitative understanding of CaCO3 export. This is very helpful.

20249, 26, lab slang again: 'the TA*-CFC age method' needs to be introduced before being referred to that often;

20249-20250 (Section 5.2) Please rewrite this paragraph. First explain how the TA*-CFC method is supposed to work, thereafter its by now well know caveats. Overall there is too much talking about estimates that have been disqualified 10 years ago by Friis et al.. But what about the work of Barrett, you cite that paper earlier, but do not refer much to the non-TA* related details presented there. Do the same arguments apply?

Section 5.3: For the relative importance of shelf vs. open ocean CaCO3 production, perhaps refer to Milliman or some other global CaCO3 production review that considers non-open ocean realms.

Section 5.4: I see the advantage applying the the 'constant' dissolution formulation to e.g. climate models, but are there potential downsides of your suggestion? Discuss please.

Section 6: Perhaps you should at least mention the limitation of your study, which I argued about above: i.e. that you prescribed the omega distribution from observations and CaCO3 dissolution can not feed back on its conditions.

Overall, a very interesting work, which I was very happy to read and review - though it was not always easy. Looking forward to see an improved version being published.