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Interactive comment on “Potential impact of DOC accumulation on $f\text{CO}_2$ and carbonate ion computations in ocean acidification experiments” by W. Koeve et al.

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General Comments: Koeve et al investigated the inconsistencies between measured and calculated $f\text{CO}_2$ as evidenced in manipulated phytoplankton culture experiments. Consistent with prior reports for the role of DOM in affecting the total alkalinity of a system, and thus variables calculated from TA, they found that the high DOM concentrations occurring during ‘ocean acidification experiments’ introduces significant error in calculations of carbon system variables. They point out that software tools such as CO2SYS do not account for the proton acceptors in DOM, so calculations of $f\text{CO}_2$ will be and are incorrect in high DOM systems. They warn that global ocean biogeo-

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chemical models need to address this problem in coastal zones, where DOM may be elevated enough to make a difference. Overall the paper is well thought out and presented. The science presented was cleverly done; the authors used existing data from a prior publication to make their case. The effort is a timely one given the high interest and use of mesocosm studies at present.

Specific Comments: Another paper from which delta DOC/delta NO₃ data can be retrieved for inclusion in Fig. 7 is that by Goldman et al., 1992 (MEPS).

Where is “California Bay”? I have not heard of this place. In looking at the reference, I see that the authors are referring to the “Gulf of California”, also known as the “Sea of Cortez”, but not known as “California Bay”.

The first paragraph of the “Conclusions” is written as a summary instead. A summary is not necessary for this short paper. Revise so that conclusions and recommendations alone are found in this section.

As an aside, from someone who is not expert in the science of DIC: Koeve et al used as their motivation the ‘discussion paper’ by Hoppe et al. 2010, a paper which apparently has not advanced to ‘full publication and acceptance’ by the journal. I wonder if, given the results of Koeve, Hoppe et al should be reevaluated. Hoppe’s main finding was “Calculated pCO₂ matched measured pCO₂ if pH and TA or pH and DIC were chosen as input parameters, whereas pCO₂ calculated from TA and DIC was considerably lower than measured values.” It appears from the work of Koeve et al that the Hoppe finding is what should be expected given the experimental setup (i.e., Hoppe mesocosms started with 111 $\mu\text{mol/L}$ of NO₃, so high DOC would result). Hoppe et al may not have realized why the discrepancy existed, but they certainly observed it. The Hoppe et al reviewers did not trust the discrepancy reported. According to Dickson, “there are too many ways in which the quality of the experiment is unclear, particularly given the potential significance were the reported observations to be correct.” Wanninkhof wrote that “the magnitude of the deviation...rais(es) questions of exper-

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imental execution". The reviewers have many valid criticisms that Hoppe et al should take to heart, but it may be that on the most fundamental level Hoppe et al reported a valid finding; and their execution, though perhaps imperfect, resulted in findings that are nonetheless meaningful and important. Having apparently been rationalized and validated by Koeve et al., should Hoppe et al now rise to the level required for 'acceptance' by the journal? This outcome could be considered by the editor given Dickson's statement, regarding Hoppe et al, of "the potential significance were the reported observations to be correct." The observations of Hoppe appear correct based on Koeve, so the Hoppe paper emerges as significant, right? If Hoppe's main finding is still problematic, then we might conclude that Koeve is problematic. But the reviewers have not yet identified a major problem with Koeve, so. . . round and round we go.

Technical Corrections:

Page 3799/Line 10: should be "An obvious approach . . . is.." 3799/16: delete "to perform" 3800/8: "were found" 3800/17: "Such software applies established models. . ." 3800/26: "and how neglect of DOC" 3809/14: When starting a sentence with "this", "this" must be defined. "This" what? The authors may mean "This finding", but the reader should not be required to guess. 3810/10: should be "decrease" 3810/23: I don't know what is meant in the sentence starting with "So far. . ." because the word "comprising" is not used correctly. I can't guess what is intended, but it needs to be fixed. 3811/20: "from", not "form" Remove comma's: at end of 3800/15, final comma in 3810/28, first comma at 3811/15, at 3814/15.

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