Biogeosciences Discuss., 11, C7912–C7915, 2015
www.biogeosciences-discuss.net/11/C7912/2015/
© Author(s) 2015. This work is distributed under
the Creative Commons Attribute 3.0 License.

Biogeosciences
Discussions
Open Access

Interactive comment on "Trends and drivers in global surface ocean pH over the past three decades" *by* S. K. Lauvset et al.

Anonymous Referee #2

Received and published: 7 January 2015

General comments:

In this manuscript, authors give an overview of the global trends in pH change in surface seawater in the last two or three decades based on the data of measurement. They combined fCO2 data collected in SOCAT database (Pfeil et al., 2013) and total alkalinity evaluated from algorithms expressed as a function of temperature and salinity (Lee et al., 2006; Nondal et al., 2009), and calculated pH at in-situ temperature and the mean rate of pH change in each of 17 biomes in the open oceans defined by Fay and McKinley (2014). In many of these biomes, the uncertainty in the mean rate of pH change is still not small enough to document and mechanistically understand the differences among the biomes. I think that the method of analyzing pH and its temporal change authors have used is sound. The large uncertainty in the rate of pH change

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

is mainly ascribed to the paucity of data and a small signal (long-term pH change)-to-noise (shorter-term natural variability in space and time) ratio. It appears that the uncertainty is also ascribed partly to the imperfectness of the algorithms of TA. More robust analyses for the trends toward pH decrease in the open ocean have previously been made in several regions, mainly at time-series stations (e.g., references in IPCC AR5 and Bates et al. 2014). However, spatial coverage and robustness of the trend analyses have a relation of the trade-off because pH is spatially variable. In spite of these shortcomings, I think this global-scale overview of pH change that the authors have given in this manuscript is the first one that describes the long-term pH change in a global scale based on the measurements and is complementary to more robust regional analyses. In summary, I think this paper is a significant contribution to the effort of documenting the status of ocean acidification. However, several points specified below need to be addressed before this manuscript is accepted for publication in Biogeosciences. In particular, I concern with the interpretations of carbonate system by the authors.

Specific comments:

(1) P15553, L1-3; "This causes a decrease . . .": This will be wrong. First, the increase in surface fCO2 leads to the decrease in surface ocean pH. Second, the increase in Revelle factor causes a slower decrease in surface ocean pH for a given increase in surface fCO2 from the addition of CO2 while it causes a faster decrease in surface ocean pH for a given increase in surface DIC from the addition of CO2. Please check.

(2) P15553, L11-14; "Thus local and . . .pH trends. The complex spatial variability . . . this hypothesis.": The hypothesis authors propose is unclear and needs more explanations. Do authors suggest that biological production and calcification cause the decoupling of fCO2 and pH variations? In carbonate system, it is evident that the increase (decrease) in fCO2 and decrease (increase) in pH at in-situ temperature nearly couples to each other, as the changes in pH and aqueous CO2 concentration are thermodynamically coupled. The relationship between pH and aqueous CO2 concentration (or

pCO2) changes with pH (or Revelle factor) but can be approximated by a linear function within the range of pH change of about 0.1, which is the range of pH variation in each biome described in this manuscript, under the contemporary oceanic surface water condition.

(3) P15554, L22-25 and Figures 2-4: In Figures 2-4, the number of data points used to analyze the pH trend is significantly smaller than those used to analyze the fCO2 trend. For example, in NPSTPS, the number of data point is N=299 for fCO2 and N=246 for pH. Are these differences attributable to the "data fall outside the valid range for input data for the Lee et al (2006)"? These large differences might cause a significant inconsistency between the trends in pH and fCO2.

(4) P15556, L15-17 and L20-21; "The pH change expected from . . .": In principle, it is impossible to change pH and fCO2 without changing either of alkalinity, DIC, SST, or salinity. I think DIC and Revelle factor are allowed to change here.

(5) P15561, L16-18; "In the biomes . . .": I am not clear what is meant in this sentence. Discussion here should be improved.

(6) P15563, L8-9: Decadal variation is not visible in Table 1.

(7) Table 2 and 3: Unit should be specified.

(8) Both "carbon chemistry" and "carbonate chemistry" have been used in the text to refer to the chemical equilibria of carbonate species in seawater. I would suggest using "chemistry of carbonate system" as used in P15550, L24 or just "carbonate system", because "carbon chemistry" sounds to designate the chemistry of carbon compounds in general.

Technical corrections:

(1) P15558, L12; Typo: Delete "was" after "corroborated".

(2) P15559, L14: "CARIACO" instead of "CARIOCA".

BGD

11, C7912–C7915, 2015

Interactive comment on Biogeosciences Discuss., 11, 15549, 2014.

BGD

11, C7912–C7915, 2015

Interactive Comment