Ocean Sci. Discuss., 11, C820–C823, 2014
www.ocean-sci-discuss.net/11/C820/2014/

Ocean Science
Discussions

Interactive comment on "Deriving a sea surface climatology of CO_2 fugacity in support of air–sea gas flux studies" *by* L. M. Goddijn-Murphy et al.

Anonymous Referee #3

Received and published: 17 September 2014

Goddijn-Murphy and colleagues present a gridded monthly climatology of sea surface CO2 fugacity based on SOCAT measurements. Prior to interpolation, the data are adjusted from their in-situ temperature to climatological temperature derived from satellite observations. While the interpolation method seems less advanced than pre-existing approaches like Takahashi et al (2009), the consideration of the appropriate temperature is novel and interesting, and in my opinion worth publishing. Nevertheless, I have several concerns about this temperature adjustment, both regarding the way it is presented in the manuscript and the steps actually taken, as follows.

The mentioning of a bias in Takahashi et al (2009) on page 1898 line 10-11 seemed to suggest that it is the fCO2 change along this vertical temperature gradient you aim to correct for (beginning of Sect 1.2 seems to be in this line as well). Later however

(page 1902 line 11-13 and methods) you actually seem to adjust from subskin in-situ temperature to subskin monthly mean temperature, ie. along temporal variations within the months and between years. These are very different targets. It should be clarified what your target actually is.

If I understand correctly that your target is adjusting for the temporal variations (one of the other reviewers understood it differently), you should clarify that this temperature adjustment is only relevant because you use the data in the chosen climatological year rather than in their actual year. In contrast to what some formulations (e.g. beginning of Sect 1.2) may suggest, the instantaneous sea-air CO2 flux (mentioned as primary target of the work) is actually determined by the CO2 fugacity at its in-situ temperature (not at some "true climatological mean" [page 1898 line 9] which is actually an oxymoron as a mean is a conceptual quantity only). So far it remains open how the actual sea-air CO2 flux would be re-calculated from the presented mean monthly fugacity climatology. The authors should address how they envisage to do that, and spend a few more words on their motivation to create a mean climatology only.

Concerning the vertical temperature gradient, a constant cool bias of 0.14K is assumed at all times and locations. This is of similar order of magnitude than the dT values reported in Fig 1, such that any deviations of the actual vertical temperature gradient from this assumed number will be a substantial error of fCO2 introduced through the temperature adjustment. I did not find this error being discussed in Sect 5, but it should certainly be considered and quantified.

There are various other fugacity (partial pressure) gridded products in the literature (global products, e.g., by Takahashi et al. (2009), Park et al (2010), Rödenbeck et al (2013), as well as regional studies, e.g. Telszewski et al. (2009), Landschützer et al (2013), Ishii et al (2014) and various others). How do the presented results compare to these previous results? I feel it would be appropriate to at least mention this body of work, and to explicitly compare to at least one of them, to allow the reader to see how this study relates to the work of the community.

Minor comments:

p 1896 l 10: The term "climate quality" may need a short explanation (is it synonymous to "climatological"?)

Sect. 1.4: Please give the definition of "mass boundary layer" and "thermal boundary layer". Are they used synonymously?

p 1902 l 11: Does index i refer to the SOCAT point? Please clarify in the text.

p 1902 l 12-17: Give the units of the numbers (here and throughout).

p 1902 l 18: The judgement of overestimation is only true if the 0.14K adjustement is correct.

Sect 1.5: The text is a bit out of context. If this is meant as an announcement of later sections, explicitly refer to there.

Sect 2: The description of SOCAT (Sect. 2 and elsewhere) is formulated like a method's description and, as I feel, can be misunderstood as if the authors were part of that work. Please use formulations that clarify that SOCAT is not part of the present study.

p 1906 l 14: The difference between Eqns (1) and (8) can be explicitly calculated, so why do you just "expect"?

p 1909 l 3-5: I don't think this expection is justified. Please show representative data records to convince the reader.

p 1909 l 18: As atmospheric growth has accelerated, I don't think the 1.5uatm/yr slope is still justified for 2010.

Sect 3.3, item 3: Aren't you concerned by that? How many values are affected?

Sect 4: I feel reference to a piece of software is not a valid method description, because this software may not be available in the future any more. Please give a more mathematical description of what you did.

p 1911 l 6: Please give units of these parameters.

p 1911 l 20-22: I feel it is neither fair nor meaningful to compare the actual numerical bias of Takahashi et al (2009) to the theoretically expected zero bias of Kriging. Please give the actual numerical bias of your results. If you want to rate the two methods, the bias would need to be calculated in an identical way.

p 1913 l 1: Does min and max refer to space or between months?

Sect 5.7: Clarify (also in the headline) that you are talking about the auxiliary values here, not about fCO2.

p 1916 l 22: It is not transparent to me where the numbers in the square root come from. It would be easier if you used symbols (rather than numbers). (here and at several other places)

Fig 11: Is this the fraction per cruise?

Interactive comment on Ocean Sci. Discuss., 11, 1895, 2014.