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Ocean Science
Discussions

Interactive comment on "Effect of gas-transfer-velocity parameterization choice on CO₂ air–sea fluxes in the North Atlantic and European Arctic" *by* I. Wróbel and J. Piskozub

Anonymous Referee #1

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Wróbel and Piskozub explore the effect of several different gas transfer formulations on the air sea flux of CO2 in the North Atlantic and European Arctic region using the Flux Engine network. To calculate the air-sea gas fluxes, the authors use observational data from the Takahashi and SOCAT databases in combination with satellite derrived wind speeds. The authors conclude that the largest discrepancies occur between quadratic and cubic gas transfer formulations, both in the Atlantic/Arctic basin and the global ocean and that notable differences exist between the use SOCAT and Takahashi data based climatologies.

While I do appreciate the effort to quantify the uncertainty between different gas transfer formulations, I am not convinced that this paper tackles this task in the right way. Unfortunately, I do believe that the study in its present form has some major flaws which corrupt the conclusions drawn by the authors. Furthermore, the manuscript is lacking clarity in many places. Please find a detailed list of comments below:

Major comments:

(a) Many of the mentioned gas transfer formulations have been developed for different wind products (e.g. NCEP, CCMP), whereas the authors only use one wind product. There are some major differences between wind products. I am not convinced that, If you would consider using a certain transfer formulations in combination with the wind product it was initially calculated for, that you would still get the same difference in the results. I believe this aspect has to be thoroughly discussed.

(b) The authors mention the use of both Takahashi and SOCAT climatology. While the Takahashi et al 2009 climatology fills data gaps using an advection based algorithm, the SOCAT climatology to the extend of my knowledge does not use any gap filling methods at all. The authors report a difference between the climatologies of 8% (NA) and 19% (Arctic), whereas it is not clear if this number truly stems from the difference in the climatologies or simply the difference between gap-filled and not gap-filled estimate.

(c) I am struggling a bit to find the importance of this work – i.e. what do you add to our scientific understanding of the topic that has not been known before. It is well known that there are differences in the formulations, but if the intention of this paper is to quantify this difference, then I believe you need to quantify point (a) above. Furthermore, many of the gas transfer formulations are developed using data collected over a somewhat narrow wind range (mainly between 5-12m/s), which explains large differences at the edge or outside the sampled wind range. This aspect also needs to be discussed.

(d) In the introduction page 2593 lines 22-24, the authors mention that the uncertainty of the flux has been recently discussed in Woolf et al 2015 a and b. But there is no discussion of the results of Woolf in comparison to this study. In general I am missing a proper comparison to prior studies, e.g. Sweeney et al 2007, who found that the gas transfer parametrization leads to a 30% uncertainty in the flux, whereas Landschützer et al 2014 find 37% (also including measurement uncertainty and gridding error), or any other recent study. How do previous studies compare to this one? Does this new estimate fundamentally change our current estimates?

(e) There are many minor comments, which overall are of major concern. More details are in the minor comments section below.

Minor comments:

Throughout the manuscript, Flux engine is sometimes spelled “Flux Engine” and sometimes “FluxEngine”. In this review I will spell it the way of its first appearance, i.e. Flux Engine.

Abstract line 2: The authors mention the importance for the anthropogenic budget, but there are some issues with this term. Surface observation based flux estimates, like those calculated in this work do not provide an anthropogenic sink estimate, but a contemporary sink estimate. The anthropogenic sink can only be determined by the pre-industrial state of the ocean, which is estimated to be a source of natural CO2 to the atmosphere due to river input of carbon.

Abstract line 3: “uncertainties in”

Abstract line 4: remove “sink”.

Abstract line 5: “parameterization of THE CO2 gas transfer velocity”

Introduction, page 2593 line 1: There is a spurious “Le” in the reference list before “Landschützer et al 2014”. Presumably this belongs to “Quéré et al 2015

Introduction page 2593 lines 5-8: The word “interdecadal” might be not appropriate here, as Schuster and Watson 2007 report results from the mid 1990s to the early 2000s, i.e. only 1 decade. More appropriate would be interannual or intra-decadal.

Introduction page 2593 lines 16-19: The authors list a number of potential sources for flux uncertainty, yet later in the manuscript, only one is considered, namely the transfer parametrization. As a reader I would like to know what is the most important of these uncertainties? Is there any literature regarding this topic besides Takahashi 2009?

Methods page 2594: I am not familiar with the Flux Engine software, so a bit more detail would be appreciated (e.g. what reanalysis and model data are included? Are there other wind products available to test? Is it publicly available, and if yes, is there a URL?)

Methods page 2594 line 21-22: The authors mention that both SOCAT and Takahashi climatology are calculated for 2010. Takahashi et al 2009 is calculated for a reference year 2000 and to the extend of my knowledge, the SOCAT climatology does not have a reference year. Have they been recalculated, and if yes how?

Methods page 2595 lines 1-3: I assume the wind speed data are at a height of 10 meters above surface. To the extend of my knowledge, all parametrizations used use the 10 meter above surface wind speed. In general, how has the second and third moment of the wind speed been calculated. There is an interesting discussion in Wanninkhof et al. 2013 where the authors caution that it is essential to use $<u^2>$ not $<u>^2$. Hence some information how (if so) the wind product has been averaged.

Methods page 2595 lines 8-10: I was wondering what the motivation was to separate North Atlantic and Arctic at 64N? Furthermore, please state how far north the Arctic estimate extents, and how you have dealt with ice covered areas. From Figure 1 it seems like the surface area changes from season to season - this is relevant information for your final flux estimate that is currently missing in the text.

Methods page 2596 line 1: “ignore the difference” - please provide a reference

Methods page 2596 line 4: change “taken” to “referred to as”

Methods page 2596 lines 25-26: Please explain in more detail what “wind driven and radar backscatter driven” mean.

Results page 2597 and Figure 1 and 2: I do not understand why there are gaps (white areas) in the Takahashi et al based pCO2 and flux estimate in the North Atlantic e.g. in the center of the basin between 40-50N? I could not identify such gaps in the Takahashi et al 2009 publication. Do they result from k and if yes, then why? Please explain.

Results/Discussion/conclusions: Many concerns regarding these sections have been raised in the major comments section above

Discussion page 2599 lines 9-10: please quantify what “within the experimental uncertainty” means.

Discussion page 2601 line 4: “Takahashi and SOCAT pCO2 climatologies”. SOCAT reports fCO2 How has this been converted to pCO2? Via the Flux Engine software?

Discussion page 2601 line 11: SOCATv3 has been released in September 2015.

Figure comments:

Figure 1,2,3 and 4: I am wondering where the data gaps come from? Also, please increase the font of the plot, as numbers, e.g. from the colorbar are difficult to read.

Figure 5,6,7 and 8: There is contradicting information in the caption compared to the y-axis or the figure title. In the caption, the authors report units of g/m^2/day whereas on the y-axis/title they report Tg. Which one is correct? In case Tg is correct, should it not be Tg/yr?

Figure 6: please increase the font to make it better readable.

Figure 6 and 7: It is remarkable that without a few exceptions, the majority of the

parametrizations are within the standard deviation of all parametrizations. Using the standard deviation as an uncertainty criterion, this would suggest that you based your statement page 2599 lines 9-10 on this figure. Is that correct? If so, please state this more explicitly.

Figure 8: Again, it is important to understand for the reader how the SOCAT climatology has been created. If it is a climatology from the cruise tracks only as provided on the SOCAT website, than it is not directly comparable to the Takahashi climatology. If it is a climatology created by a gap filling method, then please explicitly explain how it has been created. Otherwise figure 8 is more misleading than helpful.

Interactive comment on Ocean Sci. Discuss., 12, 2591, 2015.