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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 #2

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This manuscript, using the FluxEngine software tool, aims at identifying the influence of using different gas transfer parameterizations in calculating CO2 fluxes in the N. Atlantic, Arctic, and globally. They find that the k values do not influence the results in their focus regions as much as they influence global values, largely because the average wind speeds in the N. Atlantic and Arctic are above average. These above average values are close to the point where the different parameterizations converge, therefore reducing the range in calculated flux values. In addition, the authors examined the seasonality of the CO2 flux and the influence of using the SOCAT database or Takahashi

climatology on the calculated fluxes.

My biggest concern about this manuscript is that I am not sure if it makes a very substantial contribution to our knowledge. While I agree it is important to understand how we make our flux calculations (e.g. limitation of the gas transfer coefficient) and to use large datasets with up-to-date information, I do not think this stage of the paper offers any deep insight. We have known for a long time that k parameterizations do not reflect the physical processes behind gas exchange and exhibit large ranges over a variety of wind speed regimes. With this information, anyone can plot average wind speeds over the globe and determine where the quadratic and cubic wind speed parameterizations will diverge the most. The global SOCAT or Takahashi data is not needed. In addition, the authors themselves say that other scientists have determined the main conclusion of this paper, but simply have not written it down in equation form in published manuscript (Pg. 2600, line 21). Finally, the idea of uncertainty here is not exactly in relation to obtaining more accurate fluxes, since the measure of uncertainty is comparison of calculated fluxes using one or the other potentially flawed parameterization. Even if the parameterizations give the same value, we are still not sure if the calculated fluxes are accurate (both because of the parameterizations and the concentration gradients that go into the calculations).

In general, I support the idea of using a tool like FluxEngine and mining the very substantial SOCAT database. If the paper can undergo a major revision, especially to its scope, than it is a worthwhile exercise. Perhaps a comparison of other important sink regions would be interesting? Also, see comment about Figure 8 below.

Specific comments: General English mistakes happen throughout (for example, line 8 in abstract should read for example instead of or example, line 11 on pg. 2594 should be suite instead of suit) Pg. 2592, line 25 – refers to Talley, 2013 for NADW formation, but this phenomenon has been known for much longer. Is this the best reference to use? Pg. 2599, lines 10-13 – I am not sure this info about the discussion session at SOLAS adds anything to the manuscript. I think it should be taken out. Figure 7 – is this figure really necessary? I am not sure why it adds something more than Figure 6. Figure 8 – I am missing a more detailed discussion about why there is this inverse in the seasonality. This could lend substance to this paper.

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