We thank both reviewers for so many very helpful comments and appreciate for constructive feedback on the paper, which has helped to improve the manuscript. All their comments have been addressed and changes have been included the revised version the manuscript (see below).

Because some of the comments overlap, we would like to start with responding to the two most important topics rose in **both reviews**: the purpose of the paper and the toolset used.

The purpose of the paper is to constrain the uncertainty resulting from the choice of the gas transfer velocity (*k*) parameterization in the case of the North Atlantic. This is a region best covered with measurements and one with stronger winds than average over the world ocean. We started the study convinced the relative uncertainty will be larger than elsewhere. However it turned out it is smaller. This is a previously unpublished finding, important not only for the ocean carbon budget but especially to the gas transfer velocity community. The North Atlantic is a place where multiple experiments aiming at containing the *k* parameterization were performed. Knowing (thanks to our results) that typical North Atlantic winds are the environment least suited for choosing between parameterizations of different wind speed power should be taken into account when planning future experiments of this kind. The feedback we had presenting early results at several meetings (EGU and SOLAS conferences, a SOCAT workshop and a grant meeting involving most of the key European researchers in the field). The feedback was very encouraging. It is the feedback which made us include some of the text the reviewers had comments on (like the Arctic seasonality which was treated by us as a curiosity until we heard feedback at the PICO session at EGU). The discussion we had at the meetings, in one case so much we thanked its author with a citation (see also below). Also the manuscript revision showed to us that the gap in literature (lack of papers which show comparisons of resulting fluxes for multiple parameterizations, especially the recent ones) makes the manuscript even more relevant.

The other subject raised in many comments is the FluxEngine toolset. It has been developed by researchers we cooperate with but during a previous project we were not a part of. A paper describing the toolset has been recently published in peer reviewed literature (Shutler et al. 2016, doi:10.1175/JTECH-D-14-00204.1, available also free of charge on ResearchGate). The manuscript has been available to us when we were working on this study but otherwise we worked as end-users. Therefore we reply (below) to questions about FluxEngine according to our best knowledge. However, because we have not yet seen the actual source code (it will be open sourced in near future), we know only as much as stated in the documentation (online in the tool and in the paper). In fact, the decision not to bombard the toolset authors with email questions was one of the additional aims of the study, the first one performed by persons other than its authors. We wanted to check whether the toolset is “user ready”. We have to add that it did work and therefore we plan to use FluxEngine in our next projects, including ones involving fluxes that are not yet included (this should be easier after it becomes open sourced).

Comments from referee#2

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.

A: This is a very general remark. We answer it at the top of this document and answering several detailed comments by Reviewer #1. We believe we gave several arguments for the importance of this kind of study both for end-users of k parametrization and to the researchers working on improving them (we belong to the latter).

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).

A: Not exactly. When starting the work, we had no idea that North Atlantic, a region with winds higher than average, will have smaller differences of the net fluxes than the global ocean. We actually expected the inverse. We were surprised with the result and discussed that with several experts, when presenting the results at three different meetings. Only one of them (Andy Watson) was not surprised and offered the constant direction of the flux in all seasons as a possible explanation stating he has seen it in the data. However, he could not give any citation on that (we asked). We confirmed his intuition with calculations (and cite his input as a "private communication") and we also found a second, possibly more important, reason for the closeness of quadratic and cubic parameterizations in the North Atlantic (the fact that they intersect at wind levels typical for the region). We are highly confident that neither of fact that the parameterizations cause less spread in flux results in the North Atlantic not any reasons for that were never published (we really did search and ask around). We cannot agree that something is unpublishable because someone had an explanation for part of our unpublished results after seeing them at a meeting where the results were presented (Andy Watson was only one of many persons we presented the results to at different meetings and the only one who offered an explanation, even if a partial one). That would mean that the results for this phenomenon would be, at the same time, unpublished and unpublishable, which does not seem right.

We changed the wording to make it clearer that the phenomenon has been neither known within the community (the one known exception is explicitly mentioned and thanked for with the citation) nor previously published to the best knowledge of multiple experts we talked to.

Finally, the idea of uncertainty here is not exactly in relation to obtainin 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).

A: The purpose of the paper is not to tell the people who use *k* formulas which parameterization is the best. One can need experimental data for it (dual-tracer experiments etc.), not comparison to resultant net fluxes. By the way, this is the reason why the SOLAS conference discussion session conclusions mentioned in the reply to Reviewer 1 (and also below) are important as they anchor our study with recent results of dual-tracer experiments. No, the purpose of the paper is to determine how much the flux results will differ, depending on which formula is chosen. It is an important part of uncertainly which previously has been rarely discussed explicitly (usually it has been lumped with other sources and only a total uncertainty was given).

On the other hand, the results we provide should also be useful to the authors of the very parameterizations as a "sanity check". For them, it is an additional tool to determine how the new formula fares when faced with global data. We can attest to that by relating how much interest we got from some of them present at the meetings where we presented the data. We are also aware of at least one new *k* parameterization, created basing solely on theoretical arguments (already submitted) which would be easily shown by analysis such as ours to be a massive outlier in global and regional net fluxes (assuming the formula survives the peer review process – we have no rights to publish it before its authors and therefore we omitted it). We believe it is an additional argument for the need of studies such as the present one.

line 8 in abstract should read for example instead of or example

A: Thanks (it was actually "or comparison"). We changed this and added some additional corrections: We used a recently developed software tool, FluxEngine, in order to estimate monthly net carbon air-sea flux for the extratropical North Atlantic, the European Arctic, as well as global values (**for** comparison) using several available parameterizations **of the** gas transfer velocity **for** different dependence on wind speed, both quadratic and cubic.

line 11 on pg. 2594 should be suite instead of suit

A: Thanks for noticing it. It has been corrected.

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?

A: The purpose of this quote (supporting the statement "a region where a large part of ocean deep waters are formed") was to show a recent review paper which gives the newest estimates of the volumes of deep water formation, not to place a claim of who first recognized the mechanism (which would be Wally Broecker if one person were to be named but actually it was a long process). We believe that neither going into more details on overturning circulation nor relating the history of its discovery is within the scope of the manuscript.

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.

A: Actually, two comments by Reviewer #1 made it necessary to add more text about it (and a link to the session report). This session, convened by the leading authors of the very parameterizations used in this study, gives a strong support to one of the statements in the manuscript. The very fact that it was convened, in our opinion, is also a strong sign that the subject of this manuscript is a topic interesting for many researchers working in the field.

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.

A: We understand the question as one about Figure 8, not 7, as the one where inverse of seasonality in shown. This issue was also raised by Reviewer #1 (see the detailed answer discussing the question above). In short, the feedback from presenting this figure at two major conferences and a special SOCAT workshop were very encouraging.