

Interactive comment on “Net sea-air CO₂ flux uncertainties in the Bay of Biscay based on the choice of wind speed products and gas transfer parameterizations” by P. Otero et al.

P. Otero et al.

pablo.otero@co.ieo.es

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We are grateful for your review, and fully agree that the present analysis could form a good accompaniment to previous studies examining CO₂ fluxes on a global scale. However, you have some disappointments that we will try to reply following the same scheme presented in your review:

1) Wind speed: choice of products.

As you mention, there are significant known problems related to NCEP 1 (R1) and NCEP 2 (R2). However, these first generation reanalysis are still been widely used in CO₂ related studies in both ocean regions (Lourantou and Metzl, 2011), continental
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margins (Bates et al., 2006; Fransson et al., 2006; Arrigo and Van Dijken, 2007; Borges et al., 2008; Årthun et al., 2012; Walström et al., 2012) and near shore ecosystem close to the ECO route (Bozec et al., 2012) which encourages to include them in this study. Moreover, it is suppose that R2 corrects many errors and updates some components of R1 (Kanamitsu et al., 2002), but the large differences between both products at our latitudes demand the inclusion of both products.

Following your suggestion, we have added CCMP dataset (Atlas et al., 2011) to the wind speed typologies of our study. The inclusion of this product makes the paper more complete attending to the recent papers using the CCMP dataset as an accurate and realistic measurements of ocean winds (Park and Wanninkhof, 2012; Schuster et al., 2012). As example of this inclusion, we include the new Figure 2 as supplementary material to this letter (statistics with this wind product and the corresponding FCO₂ estimations are ready to be included in the revision; see attached pdf).

2) Wind speed: methodology

Preprocessing of QuikSCAT Level 2B are done by NASA JPL. In the manuscript, we comment some of these preprocessing steps: removal of cells with poor azimuth diversity, land contamination, heavy rain or when the derived winds are outside the optimum modulus range. The characteristic of the cell can be known by the bit flag of the data. If the Level 2B Processor detects an anomalous condition which halts the processing for a particular wind vector cell, the appropriate bit flag will indicate it. It is important to note that the preprocessing is a matter that has nothing to do with a potential end-user. More information is available in the QuikSCAT Science Data Product User's Manual: http://rda.ucar.edu/datasets/ds744.2/docs/QSUG_v3.pdf

The use of swath (Level 2B) and not gridded data (Level 3) implies that, close to the coast, some portion of the wind vector cell is over land. In this case, the cell will be removed due to land contamination. During the preprocessing, the algorithm checks the center of the wind vector cell against the sea-land map. Following the Manual, starting

with the reprocessed QuikSCAT data in July 2006 the land flags will be determined using a land-sea mask at higher spatial resolution (2 arc-min in lat/lon) than previously was used (CIA land map used for NSCAT). Thus, locations closer than 12.5 km from the coast will be permanently affected by land. However, cells located at distances slightly further offshore, will be or not land contaminated depending on the position of the cell and the resolution of the sea-land map in that region. This is the reason why our closer buoys to the coast were permanently land-contaminated, whereas other like the Silleiro buoy (37 km offshore) were occasionally affected. We agree with you that the meaning of the sentence on page 9997, lines 3-5, is unclear. We must correct it to clearly specify that “records below the accuracy of the instrument ($\pm 0.3 \text{ ms}^{-1}$) and anomalies higher than 3sigma have been removed” and that “the remaining data have been height-adjusted to 10m”.

In the manuscript we also comment that stability is expected to contribute wind speed differences of less than 0.2 m/s. And, as you mention, accuracies greater than 0.2 m/s are presented in Table 1. We added this comment to enforce the idea that the correction by the stability is not critical in our study. Nonetheless, maybe the comment could be removed.

3) Gas transfer parameterizations: choice of products.

After reading your comments, it's obvious that the description of the various U-k relationships should be improved.

You suggest “to select a more representative range of parameterizations”. We agree that some of them are very similar. Thus, we will exclude Ho et al. (2006) because this parameterization was estimated in the South Atlantic under the action of strong winds. In addition, we will include the cubic relationship of McGillis et al. (2001) in the revision. We will keep the rest of them.

4) Other issues.

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We really apologize for the use of non-standard English in some sentences. The manuscript will be carefully checked.

We accept the suggestion of changing Figure 3 by a Table. The figure could be added as supplementary material, because to present data in a circular layout is ideal for exploring relationships among the dataset. The plot classifies winds and algorithms, but also allows to explore the combination of both data.

We fully agree with you that gas bulk flux equation should be included in the introduction.

Finally, in atmospheric and ocean modeling, the time 00Z, 12Z, etc., are forecast hours after initialization. The time origin of the initialization is the start of the day, 00 UTC.

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Please also note the supplement to this comment:

<http://www.biogeosciences-discuss.net/9/C5197/2012/bgd-9-C5197-2012-supplement.pdf>

Interactive comment on *Biogeosciences Discuss.*, 9, 9993, 2012.

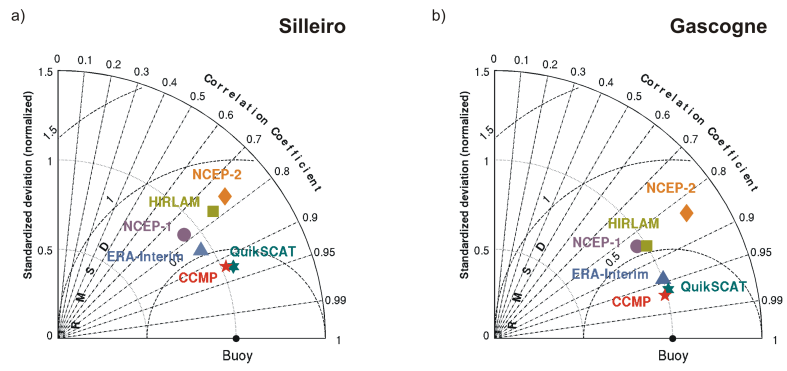


Fig. 1. New Figure 2