**Biogeosciences
Discussions**

Interactive comment on "Net sea-air CO_2 flux uncertainties in the Bay of Biscay based on the choice of wind speed products and gas transfer parameterizations" *by* P. Otero et al.

Anonymous Referee #1

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This paper addresses an interesting and important area, examining the variation of calculated CO2 flux with choice of wind speed product and gas transfer parameterisation on a regional scale. An analysis of this sort could form a good accompaniment to previous studies examining the problem on a global scale. However, there are substantial problems with the analysis as presented here which should be addressed before publication.

1) Wind speed: choice of products.

The analysis in this paper uses the NCEP 1 and NCEP 2 reanalysis products. There are significant, well known problems with both of these datasets, particularly for studies

exploring variability (e.g., Trenberth et al., 2009), and they are no longer suitable for an analysis such as this. Use of a more recent, higher spatial resolution reanalysis such as NASA's MERRA would be more appropriate for this study. Other up to date products such as CCMP (Atlas et al., 2011) would also be more appropriate.

2) Wind speed: methodology.

Adequate detail on processing steps and quality control is not provided. This should be given to a level of detail that the analysis presented in the paper could be recreated. For example, when describing the QuikSCAT data on page 9998, lines 6-11, the data removal criteria need to be more clearly explained and quantified. It is also not apparent to me how the QuikSCAT accuracy, stated on line 14, is obtained.

There are further issues with the description of QuikSCAT winds on page 10001, lines 4-8. The exact meaning of these sentences is unclear, but the author's seem to imply that land contamination only sometimes affects the various fixed buoy moorings. I do not understand how this effect can vary with time.

On page 9997, lines 3-5, equation 8 from Large et al., (1995) is a relationship of u* to U10 derived from published observations. More information is required to understand how the author's height adjusted the wind speed measurements. I also note that the meaning of the sentence starting on line 3 as 'Records ...' is unclear. It implies that data was both removed from the analysis AND height adjusted?

Page 9997, lines 5-7. The authors state that stability is expected to contribute wind speed differences of less than 0.2 m/s. However, this is a global value and close to the coast I would not expect it to be relevant. From ERA-Interim at native resolution, the monthly mean stability adjustment in the region, averaged over 10W-0W and 43N-49N, has an annual cycle of $\sim$ 0.3 m/s. In any case, in Table 1, wind speed differences with accuracies greater than 0.2 m/s are presented.

3) Gas transfer parameterisations: choice of products.

The authors select a range of published k-U relationships. Several of these relationships (Ho et al., 2006; Sweeney et al., 2007 and Nightingale et al., 2000) are very similar. It would be better if the authors had selected a more representative range of parameterisations, including a cubic relationship, and perhaps demonstrated the range of relationships used in a graph. There have been several recent field experiments (e.g., Edson et al., 2011; McGillis et al., 2001; Prytherch et al., 2010) that have observed a cubic dependence of k on U.

On page 9996, lines 18-20, the authors state that a cubic relationship was not included in the analysis due to having a greater sensitivity to wind speed. But it is the sensitivity to wind speed variation of the relationships that is being investigated here?

The description of the various U-k relationships (page 9996, lines 13-18) is vague. The methodologies used to obtain the relationships are given in some cases but not others. For example, Wanninkhof 1992 is also based on a quadratic fit through C14 data, and has been superseded by Sweeney et al., 2007 (though it is still widely used). Nightingale et al. 2000 presented results from a number of experiments using dual tracer techniques.

4) Other issues.

The standard of English in the paper is not always adequate, and in places the intended meaning is lost. Several examples are given in the comments above.

The information presented in Figure 3 would be more clearly presented in a table.

The introduction would be made clearer through inclusion of the gas bulk flux equation (e.g., F = K0 k deltapCO2).

Page 9999, line 4. I do not know what is meant by 0Z, 6 Z etc. Local time? UTC? This needs to be explained.

References:

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