

## ***Interactive comment on “Numerical tools to estimate the flux of a gas across the air-water interface and assess the heterogeneity of its forcing functions” by V. M. N. de C. da S. Vieira***

**Anonymous Referee #1**

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Review of “Numerical tools to estimate the flux of a gas across the air-water interface and assess the heterogeneity of its forcing functions” Vieira

General comments The manuscript presents a numerical tool to estimate the gas flux across the air-water interface, primarily CO<sub>2</sub>. The flux is calculated using previously published parameterizations by other research groups. The tool also allows for comparing two gas flux estimates from different sites/times and evaluate what parameter in the parameterization that is responsible for the difference in the flux estimate (if any).

This manuscript is technical in its nature. For gas flux estimation, it doesn't provide any new scientific information, rather it is more of review of previous findings and ap-

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plying these in a software for estimating the fluxes. The software is an upgrade from a previous software by Johnson, (2010), allowing for more choices of parameterization, including stability and turbulence generate by current drag. The main new thing contributed by this paper is the “comparison function” which allows for an analysis of which forcing is causing difference between two flux estimation.

The software is tested on an observational data set consisting of 9 observations using floating chambers during three days. However, this dataset is not properly described, this must be included in a revised version.

The paper is rather long and a bit unstructured. I think it could be shortened for instance by removing the text that the author describes the process in which the algorithm was developed, this is not interesting information for the reader. It must also contain a clear description of the software structure and what choices the user can make. The paper should also contain a section where the observations are described in detail (prior to the result section). See also the specific comments of this review

18 figures is a bit steep, please consider removing figures that are not essential to the paper.

Specific comments

Page 911, line 2: important reference missing regarding transfer velocity from lake observations: Cole and Caraco 1998: “Atmospheric Exchange of Carbon Dioxide in a Low-Wind Oligotrophic Lake Measured by the Addition of SF<sub>6</sub>”

Page 912, lines 11-24. I think this text (describing the examples) could be removed from the introduction, in order to shorten the manuscript. It does not add any new information.

Row 25: “During the course of work” etc. Here the author is describing how the work has progressed, which isn't important for the reader, please reformulate.

Page 913, equation 1, The convention is to write the equation first and then describe

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the terms in the equation on the following rows. The author has shifted this order and describes the terms first and then presents the equation, I think this need to be reformulated (it goes for all equations in the manuscript).

Page 913-914, lines 11- 4(914): I think these two paragraphs are a little bit too detailed, in order to shorten the manuscript please consider revising this text. For instance, it isn't necessary to keep the sentence on what letter Zhao et al use for the Bunsen's solubility coefficient or why he is making this choice.

Page 914, line 17:  $U_{10}$  doesn't have to be the measured wind at 10m, it can also be the estimated wind at 10m by using a measured wind at some other height. It is sufficient to say: "10m wind speed" or similar. The many references listed after this statement ( 8 of them) are not motivated in my opinion.

Page 915, line 4: The first paragraph should start with equation 2 (see previous comment) Row 13-24: This is a lot of detail (too much) on the Schmidt number. This could be shortened considerably.

Page 917, line 18: Please specify how air temperature and pressure affects the air transfer velocity "in a mild manner".

Page 918: lines 9→ Here you describe how the flux across the air water interface can be estimated. This has in principle already been described in the previous section. If the intention is to describe the actual software please make this clearer. The text here can also be shortened, for instance it is not necessary to in detail describe the concept of concentration difference across an interface and how it affect the direction and strength of the flux (as is done in the list on this page).

In addition, I think the language here needs to be checked e.g. on row 10 "the gas concentration in the water and in the air gave the direction..". What is meant here by "gave"? Is this what the user needs to give the software (input)? If so, please make this clearer.

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My understanding is that the author intends to describe the details of the model in Section 3 and how a user should use it. However, the author consequently uses the past tense here (preterit) which make it hard to clearly understand the intention/meaning of this text in this section (what has been made or what the user should be doing)

Page 919: line 3 (as an example) Again, it is not clear the users has the possibility to choose between several different chemical enhancement parameterizations or if the author is describing how the software was developed. If the latter is true, then this entire paragraph could be removed.

Lines 14 → Please remove the text which describes how the author developed the algorithm. Row 23: Surface roughness is primarily dependent on the waves, please reformulate.

Page 920. It is clear that the author has limited knowledge in micrometeorology. Atmospheric stability is not related to advection of warmer air, please see any meteorological text book. This is also basic knowledge and doesn't need to be described in detail in a research paper. Eq(6): The Psi function should be specified in direct connection to the equation. The Psi-function should also have an index "m" to specify which function it is. It should also be stated that the Psi function is the integrated non-dimensional gradient (fi-function).

The author presents a psi-function which only applies for stable conditions (see e.g. Stull 1988, page 385). This is a serious flaw, a function dealing with unstable stratification must also be included (also the reference for the Psi-function is unusual).

The unit of H is usually written as  $W m^{-2}$  (not  $J m^{-2} s^{-1}$ )

Page 921, lines 1-21. This is text book knowledge and shouldn't be included in a research paper. Rows 24: what are you missing to determine L?

Page 924, lines 2-8: This part describes results and should be removed from the "Methods" section.

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Page 924: I think it should be made clearer that the difference is calculated for only the actual parameters implemented in the model. What if the difference is due to some unknown process, not included? How does the algorithm deal with measurement errors and measurement uncertainties?

Eq 12: Why not use the index ref and env to avoid confusion?

Page 926-928. It shows that this is the author's expertise. However, I think this text described too many details and should be shortened to make it easier to read. Also, again, remove the text which describes how the work progressed (e.g. in paragraphs 2-3 on page 926 and paragraph 1 on page 928). It also not necessary to know all the details in the implementation (e.g. the comment on Matlab indexing on line 9 page 925).

Page 928. A Measurement section should be included prior to the Result section.

Page 930, lines 17-20, lines 27-28: You cannot say anything about under/overestimation since you don't compare with measurements

Row 28: "The Raymond and Cole (2001) was an exponential function" was? I guess it still is an exponential function, please revise the text. The problem with writing in the past tense is appearing at several occasions in the text (as stated earlier in this review).

Page 932: Which wind speed is used in Fig 10?

Page 938, lines 8-9: How can you say that the linear functions underestimate the transfer velocities at high wind speeds due to lack of such data? If you don't have any data to compare with you cannot evaluate the parameterization.

Page 940, lines 2-4: Stability is physical concept, not something "developed for air over land", please rewrite this or remove.

Line 8, A drag coefficient of 3.91 is at least 1000 times to large, it is normally of the order 0.001. If you have used this value in the algorithm then something is wrong.

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Please comment.

Line 12: If you cannot use L, use the bulk Richardson's number to evaluate stability.

Page 942, line 26: It is not as simple as "solely" evaluate  $u^*$  from  $z_0$ , wind speed must be included somewhere.

Figures

Figure 1. It should be made clear that no algorithm has been compared with measurements at high wind speeds (e.g. up to 30 m/s). Rephrase the figure text, (a) shows  $k$  for wind speeds in the range 0-30, (b) is a close up showing  $k$  in the wind speed range 0-8 (not 5 as is stated in the figure caption).

Fig 4, Formatting of y-axis label: superscript on  $cm^2$

Technical corrections

Firstly, all subfigures should clearly be marked with (a), (b) etc. not with (up) (down) (left) etc. Some figures use a figure heading, some don't, please be consistent. Also be consistent with the formatting of the figure legend (use box or not). Express ratios using exponents e.g. meters per second as  $m s^{-1}$  not m/s (in the current versions both formats are used).

Page 909 Title: "Heterogeny" change to heterogeneity (?)

Page 934: DDF is a new abbreviation and should be written out.

Page 944, lines 9-10: "take forever" please rephrase. Second paragraph, this repeats what is described earlier, could be shortened.

Fig2-3 Please be consistent with the formatting of the figure Legend (sometimes it has a box around it, sometimes not).

Fig 5. Please use the Greek letter and not "alfa".

Fig 7: Figure caption, what is the capital letter referring to and why use a semi-colon?

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Fig 9, 14: y-axis label, please correct the dot between mmol and m-2.

Fig 10b and 11b Demand is introduced on the y-axis label, this should be reformulated as  $\Delta\text{CO}_2$

Fig 10a: y-axis label, use capital letter on the F in "flux".

Fig 12.13 Different formats are used for the dates, be consistent.

Fig 13: Please be more specific in the figure caption. In what sense does the figure display "the transfer limiting phase"?

Fig 18. Caption: "Difference" don't use capital letter. Write out the meaning of the abbreviations used on the x-axis. Y-axis label, correct the "dot" between m-2 and d-1.

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Interactive comment on Ocean Sci. Discuss., 9, 909, 2012.