Geoscientific

Model Development

Discussions

GMDD

Interactive

comment

Interactive comment on "The FuGas 2.1 framework for atmosphere-ocean coupling in geoscientific models: improving estimates of the solubilities and fluxes of greenhouse gases and aerosols" *by* Vasco M. N. C. S. Vieira et al.

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Referee Comment: The manuscript "The FuGas 2.1 framework..." by Vasco M. N. C. S. Vieira and colleagues is an interesting study presenting a model which aim is to improve parameterization of air-sea gas exchange. The model is based on previous work by the same first author but on top of that, it presents the differences in gas fluxes in European coastal seas between the model and previous parameterizations. The manuscript looks promising but still has at least one large problem (and some minor ones) which needs to be addressed before it is published. This is the very reason I

suggest it needs a major revision. Authors' reply: We kindly acknowledge the revision with constructive comments that were a significant help improving the manuscript.

Referee Comment: Namely it does not convincingly state why the new parameterization is supposed to fit better experimental data. Lines 295-6 contain the following statement: "The red markers [in Fig. 3] representing the ZRb03 iWLP give the best example." I believe the "example" is actually "fit" but I do not see why the red triangles (representing ZRb03 iWLP) are supposed to be best fit. That is unless the authors imply that the obsolete Wannninkhof 1992 is the one they are fitting. That would be wrong because even its author suggests using his newest formula from Wanninkhof, R. (2009) [1]. This new function is closer to Nightingale et a;. 2000, also shown in Fig 3. And "Rb03 WLLP" seems closer to it. Maybe I have misunderstood the authors' intentions (for example I have no idea what the following is supposed to mean: "the comprehensive algorithms split the data points into two distinct scatter lines, the upper line for kw obtained under rougher sea-surfaces and the lower line for kw obtained under smother ones") but I have not been convinced why "ZRb03 iWLP" is supposed to be better. This certainly needs some additional arguments in any revised version.

Authors' reply: The reviewer is quite right. Besides fair, his demand was quite fortunate as it drove us into significant improvements on the manuscript, and particularly in this section. From these improvements resulted a much stronger manuscript, more interesting to the scientific community, and better able to answer questions raised by reviewer #2. Our kind regards to reviewer #1. The "example" was not to be interpreted as "fit".

Referee Comment: The "renowned functions" in Fig. 3, as the authors call them, miss some other important ones like Ho et al. 2006 [2] and Sweeney et al. 2009 [3]. In fact the recommendations of a special discussion session "Relationship between wind speed and gas exchange over the ocean: Which parameterisation should I use?" on the latest SOLAS Conference (Kiel, 2015) are: "For gas transfer of CO2 over the oceans the relationships proposed in Nightingale et al. (2000), Sweeney et al. (2007),

Ho et al. (2006), and Wanninkhof et al. (2009) are recommended. They are very similar and fall within the overall uncertainty of DT measurements."

Authors' reply: Done. The formulations by Nightingale et al. (2000) and Sweeney et al. (2007) were already included in the software. The formulations by Ho et al. (2006) and Wanninkhof et al. (2009) were presently included. They are all in the new Fig.3.

Referee Comment: The manuscript has also some minor problems, easy to address: - equations (13) based on Zhang et al. 2006 (eq. 3) and (18) based on Jeffrey et al. 2010 (eq. 5 and 6) are both badly mangled, most probably by the Copernicus editing software (I had the same problem with a manuscript I was a co-author of) with all "1/2" values changed to "1.2" and other errors.

Authors' reply: Done. Actually, we realize this problem occurred prior to submission due to change of Windows based Equation Editors (Microsoft Office 2007 is terrible!). Luckily, the reviewer detected it. We found more similar typos and corrected them all.

Referee Comment: - Ks is dependent on theta (equation 7) which should be explicitly shown

Authors' reply: But it was (line 113 of old manuscript).

Referee Comment: - the Wind Log-Linear Profile (equation 14) is actually the MoninObukhov similarity theory which could be directly named and cited (either the original 1954 Russian language paper or its English translation http://mcnaughty.com/keith/papers/Monin_and_Obukhov_1954.pdf)

Authors' reply: Done. We believe Stull (1988) is another famous and adequate citation. We included both. Without any disregard for Monin and Obukhov, we had not done it before as the common policy by publications is no need for citations on well-known, unanimously accepted theories, laws, methods, etc. And how do we cite it? Is it OK how we did?

Referee Comment: - "the alternative model" of Liss and Slater (1974) [line 81] is not

really alternative but rather needed for gases which airside resistance (1/ka) is not negligible)

Authors' reply: Done. Changed accordingly.

Referee Comment: - The "E-C" acronym, introduced in line 275, is never explained and has to be guessed as (non-obvious) eddy covariance / correlation

Authors' reply: Done. Changed accordingly.

Referee Comment: - "calculus" is used multiple times in the manuscript in the meaning of "calculation" (the Enlish language meaning is narrow and covers only derivatives and integrals)

Authors' reply: Done. Changed accordingly.

Referee Comment: - ocean deep waters are not formed in "pole regions" [line 46] but rather sub-polar ones, mainly Nordic Seas (Norway and Labrador Seas) of the North Atlantic.

Authors' reply: Done. Changed accordingly.

Referee Comment: - it is not clear from the caption of Fig. 1 what is subtracted from what.

Authors' reply: Nothing is being subtracted. The colour scale is a quotient comparing the kH predicted by either formulations. Done. Changed accordingly.

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