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Geoscientific Model Development Discussions
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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.

Anonymous Referee #2

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The manuscript "gmd-2016-273" should present "an improved estimates for the solubilities and fluxes of greenhouse gases and aerosols" (see the title). However, the manuscript does not present any novel way of estimating air-sea fluxes, neither improves any of them, but rather tries to summarize what is available in the literature and to present an algorithm where many different approximation can be used. In general I am largely in favor of such manuscripts, as these can really show the state-of-the-art and our comprehension of the process described.

Nevertheless, I must admit that I found the manuscript very approximative, inconsistent

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and full of mistakes (hopefully only typos). Some equations are wrong and, although this could be due to conversion of the text to pdf format, it show the lack of attention of the authors in checking the quality of the manuscript. Further, numerous acronyms are used without any explanation. As the authors seem to consistently ignoring acronym explanations, maybe the easiest solution is to add a table listing all of them at the end of the manuscript. Further, a very deep language editing is necessary before any publication.

Importantly, I would suggest to reformulate the title of the manuscript as this does not correspond to the real work presented in the manuscript. Aerosols are not included in the text, and I am puzzled to understand how a calculation of gas solubilities and piston velocities can be used to estimate fluxes of aerosols. As mentioned before, the manuscript does not present any new parametrization, but rather uses what present in the literature, showing the differences in estimating solubility and piston velocity between different formulations. Maybe a more stringent and precise title could help the reader.

Despite that, I think that the science contained in the second section (i.e. Section 3) is still interesting and valid, and would be nice to see this analysis in a well written manuscript.

As it is difficult for me to see what could be improved to make the manuscript acceptable, I will list here below some of the issue I have been finding in the manuscript.

title FuGas2.1 is mentioned in the title, but the acronym is NEVER explained in the entire manuscript.

line 39 Not all Regional models have land, ocean, atmosphere and cryosphere components

Introduction Probably few more citation would help the reader.

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line 51 In all the text there is a consistent usage of acronyms that were never explained before. Example: IPCC, MPI, CMCC.

line 55 Here's a good example of acronym explanation missing: What is MOHID? Why is that important in this text/section? I really appreciate that MOHID allows to use different formulations, but, is this really important?

line 58 As you mentioned that "there are many other simpler formulation" it would be probably good to list some of them.

line 60 "...adjustment to their specific data": which data? What do you mean with such sentence?

line 81 Well, also the 3-layer model is present (see Cen-Lin and Tzung-May. (2013)). Which model are you using in all the flux calculations afterwards?

line 84 Actually the transfer velocity is NOT averaged over both layer. The formulation follows the Fick's law of diffusion, i.e. assuming that the transport across the thin layers is in a steady state.

section 2.1 Maybe it would be good to make two subsection for the two solubility formulation, so that the reader immediately understand which parametrization will be compared afterward.

line 106 What do you mean with "alternative chemistry background"?

line 116 There are typos in the equation (6). Some 1 are present making 298.15 equal to 1298.15

line 144 As you mention wind and bubble (white caps), what about precipitation (i.e. rain)? See Ho et al. (1997).

line 164 Here the Schmidt number is to the power of 12. Should be $1/2$.

line 181 As in many equations in the manuscript, here as well all the terms of the equation are not fully explained. The meaning of L_p comes only on page 7, line 236.

line 186 What do you mean with "in its turn" ?

line 188 . I appreciated that you are now listing terms of equation (17). However, you also list terms which do not exist in the equation, such as T_z, P_z and q_z. This is very hard for the reader, as most of the equations are not well explained and other have additional explanations which should not be present...

line 203 "..we compared between..." . Where are the results presented?

line 207 Again here you have typos with the power. I expect these to be $1/2$ and not 12.

line 224 I do not think that this title is appropriate. You do not present any coupler, but rather you are describing the simulated data you will be using for your algorithm.

line 261 Nice that you explain the metric. However these variable (i.e. $K_{HJohn10}$ and K_{HSar13} are nowhere explained before. The only explanation is in the figure labels. Although one could guess where they come from, this should be better explained.

line 269 I did not understand where this equation comes from. Do you need a piston velocity to calculate the differences? If so, how do you calculate that? What about the concentration in the water? Do you assume that equal to zero? Could you please formulate better this calculation?

line 275 "E-C" has never described before. I could guess it refers to E(ddy)-C(ovariance) method but it is impossible to know for sure.

line 296 As before, the "ZRb03 iWLP" formulation is based on a mysterious parametrization, that the reader can only guess from the sequence of symbol and letter. Probably you should list them and explain exactly on what they are based. A table could also help.

line 355 I disagree that ESM use simple approach. Please see Pozzer et al. (2006) and the model AIRSEA.

line 359 This line does not make any sense to me: what do you mean with "both formulations matched their estimates"???

line 360 Would be nice to put this number in contest of numerical error. Does this difference in solubility really play a role?

line 368 This can be easily tested, using he Takahashi et al. (2009) compilation and calculating the effect for different formulation (for CO_2). However, here the discussion must be taken cautiously: in fact, due to their coarse resolution, Earth System Models do not represent coastal area very well. Is that important at all in the overall, for example, CO_2 budget? How much is "coastal area" compared to open ocean. Can this difference really influence the calculation of carbon cycle in global model?

line 394 I do not think you can make such general statement... "do loops" exists also in vectorised and/or parallel processed algorithm.

line 499 Maybe the reference is wrong as I though that the book of Sarmiento was published in 2006 and not 2013. Please check.

Figure 4 It is not explained what the bars represent. How was the "elasticity" range calculated? Maybe additional explanation in the text may help the reader.

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