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Geoscientific Model Development Discussions

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 "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. Authors' reply:

1. The objective of the FuGas framework is allowing the researchers to test and com-

pare alternative algorithms for the estimation of the solubilities and transfer velocities, which together set the atmosphere-ocean fluxes of greenhouse gases and DMS, with an impact on global warming.

2. The FuGas is novel when it "tries to summarize what is available in the literature and to present an algorithm where many different approximation can be used".

3. The closest to the FuGas is the EngineFlux, which is not even close to the amount of possibilities provided by the FuGas for transfer velocities, is not able to fill in the gaps by using the formulations possible given the data constrains at particular locations, and does not consider alternative formulations for solubilities. The FuGas is novel in all these aspects.

4. The code relative to Equation 10 considers simultaneously kwind, kbubble and kcurrent, which is novel. The reviewer #2 will not find it anywhere else. The kcurrent was not explicitly included in the manuscript due to previous negative feedback about presenting a term that was not being tested. In reply to reviewer #2, we got it back in.

5. Equations 10-16, with focus on equation 14, represent a novel way to integrate the effects of wind speed, atmospheric stability and sea-state on the estimation of kw. The reviewer #2 will not find this solution anywhere else. This was clearly recognized by reviewer #1 and in previous reviews by Dr Wanninkhof and Dr Johnson.

6. The results summarized in Fig.3 were significantly improved in reply to reviewers #1 and #2. Consequently, it is demonstrated that our solution represents a major improvement.

Referee Comment: 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 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.

Authors' reply: The mistakes were typos due to format conversion. The equations are all correct in the docx file that we attach. They have always been correct in the software. Nevertheless, the software has been upgraded with new formulations suggested by reviewer #1. The software upgrades (FuGas 2.2) are presented in a separate comment.

Referee Comment: 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.

Authors' reply: Done.

Referee Comment: Importantly, I would suggest to reformulate the title of the manuscript as this does not correspond to the real work presented in the manuscript.

Authors' reply: Partially done as we partially disagree with the reviewer.

Referee Comment: 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.

Authors' reply: We meant DMS, which is released from the ocean to the atmosphere and quickly degenerates to sulphide aerosols. These are the only aerosols with a cooling effect on the atmosphere. The FuGas allows for the estimation of the DMS fluxes applying the DMS solubility constants provided by Sander (1999, 2015). The transfer velocities and fluxes of DMS have been studied by Hubert et al. (2004,2010) Blomquist et al. (2006), Fairall et al (2006), Vlahos and Mohonan (2009), Vlahos et al. (2011), Bell (2013, 2015), among others. Done. Changed accordingly in the title and text.

Referee Comment: 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.

Authors' reply: The manuscript does present a new parameterization, as replied above. Nevertheless, the title was changed as replied above.

Referee Comment: 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.

Authors' reply: We acknowledge the help to improve the manuscript. Consequently, we changed significantly all sections. The manuscript is much sounder and reads much better. Our kind regards to reviewer #2.

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

Authors' reply: Done. In part, because it is also an analogy with the Portuguese word spelled equally and meaning "leaks" or "escapes". However, only Portuguese speaking readers would understand.

Referee Comment: line 39 Not all Regional models have land, ocean, atmosphere and cryosphere components.

Authors' reply: The sentence identifies the model components but does not state it is mandatory to have them all.

Referee Comment: Introduction Probably few more citation would help the reader.

Authors' reply: A few more citations were added to the work

Referee Comment: line 51 In all the text there is a consistent usage of acronyms that

were never explained before. Example: IPCC, MPI, CMCC.

Authors' reply: Done

Referee Comment: 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?

Authors' reply: MOHID has no acronym explanation. But what it is was well explained in the same sentence. We also upgraded MOHID with the COARE algorithm and this is debated in the discussion.

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

Authors' reply: Done

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

Authors' reply: The data used in their calibration. Explanation inserted in the text.

Referee Comment: 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?

Authors' reply: We used the single layer except were explicitly mentioned the double layer. That occurred in the comparison between both models and is presented in the last paragraph of the results. It was shown in Fig.6 as well as in video 5. Differences were negligible when using greenhouse gases. We changed fig.6 to be more focused on the essential. Nevertheless, the comparison between both models is still presented in the results and shown in Video 5.

Referee Comment: 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.

Authors' reply: Done. Changed.

Referee Comment: 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.

Authors' reply: Each solubility formulation is already well separated in its own paragraph. Each specific paragraph starts by identifying the formulation:

1. "Sarmiento and Gruber (2013) compiled the algorithm . . . "

2. "Johnson (2010) developed an algorithm . . ."

We think this is enough

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

Authors' reply: As explained in the text, the algorithm compiled by Sarmiento and Gruber (2013) is based on the ideal gas law while the algorithm compiled by Sander (1999, 2015) and Johnson (2010) is based on the molecular and thermodynamic properties of the water, its solutes and the specified gas.

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

Authors' reply: Done.

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

Authors' reply: Rain was mentioned in the introduction and in the second line of this section (2.2). But we cannot include it in the sentence (formerly in line 144) as we are presenting the framework, which still does not account for rain. The importance of upgrading the framework with the effect of rain in kw was already in the Discussion.

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

Authors' reply: Another typo that was rectified.

Referee Comment: 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 Lp comes only on page 7, line 236.

Authors' reply: Done. Changed.

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

Authors' reply: Deleted.

Referee Comment: 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 Tz, Pz and qz. 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...

Authors' reply: Done.

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

Authors' reply: The results were presented in the last paragraph of the results, in Fig.6 and in Video 5.

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

Authors' reply: Another typo that was rectified.

Referee Comment: 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.

Authors' reply: Done.

Referee Comment: line 261 Nice that you explain the metric. However these variable (i.e. KHJohn10 and KH Sar13 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.

Authors' reply: Done.

Referee Comment: 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?

Authors' reply: Done. Explained in the methods, Section 2.1 (Solubility) after presenting both formulations.

Referee Comment: 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.

Authors' reply: Done.

Referee Comment: 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.

Authors' reply: Fig.3 was changed substantially and this is no longer an issue.

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

Authors' reply: Done. In fact, the AIRSEA is more elaborated than other couplers in ESM and regional models, and we were unfair with its developers. In the meanwhile, this whole section had already been changed in reply to reviewer #1 and this sentenced

was deleted.

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

Authors' reply: Done. It means that both solubility formulations generally estimated similar solubilities. Changed accordingly in the text.

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

Authors' reply: We absolutely agree. But that is a whole new study and publication; which, by the way, we intend to do.

Referee Comment: line 368 This can be easily tested, using the Takahashi et al. (2009) compilation and calculating the effect for different formulation (for CO2). 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, CO2 budget? How much is "coastal area" compared to open ocean. Can this difference really influence the calculation of carbon cycle in global model?

Authors' reply: 1. As above, it is a whole new work that makes part of our future plans. 2. Besides the Takahashi (2009) compilation, there are other alternatives as the SOCAT or applying algorithms to the sea-surface data soon to be provided by Copernicus Marine for the whole world at fine resolution. 3. The importance of the coastal ocean was better debated in a previous version of this manuscript in Biogeoscience Discussions. That presentation was a bit off from the objective of this work and thus was significantly shortened. Nevertheless, (i) according to Smith and Hollibaugh (1993), although the coastal ocean is ≈5% of the global oceans, ≈50% of the CO2 exchange occurs there; (ii) A large proportion of the atmosphere-ocean exchange of CH4 and N2O occurs in the coastal ocean and represents release to the atmosphere.

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

Authors' reply: Yes, they do, but they are very conflictive. It is not hard to vectorized and parallelize a simple Monte Carlo simulation applied to a simpler numerical or statistical test using a data set that has a simple structure (irrespective of its size). But it is a whole different thing to vectorized and parallelize when you have an intricate algorithm with parent scripts and child scripts, nested loops and functions, etc. Parallelization becomes unstable and crashes with apparently no reason. As an example, it is virtually impossible to vectorized and parallelize nested for-loops. This was better explained in the text.

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

Authors' reply: The book has 4 editions, including one from 2006 and another from 2013. Both are available in amazon.com. I read the one from 2013.

Referee Comment: 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.

Authors' reply: Done in the figure legend.

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