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Ocean Science Discussions

Interactive comment on "A numerical scheme to calculate temperature and salinity dependent air-water transfer velocities for any gas" *by* M. T. Johnson

Anonymous Referee #2

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General Comments

This paper presents a "scheme" for calculating air-sea gas exchange fluxes that can be applied to any gas, where scheme refers to a choice of parameterizations for the various terms needed for the flux calculation. Most of the parameterizations chosen already exist in the literature and the contribution of this paper is to highlight the ones the author believes to be most suitable. The paper includes a new parameterization for the Henry's law constant as a function of solubility and a modification of the parameterization for calculating the air-side of gas transfer velocity. Although the author calls these both new parameterizations, they seem to me (especially the gas transfer ve-

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locity one) to really be modifications of existing parameterizations. Nonetheless, they seem to offer improvement and therefore are a worthwhile contribution. Additionally, the author supplies code in the language R that can be used to calculate the fluxes of any gas. I think the two new/modifications to the parameterizations are a useful contribution. Additionally, I think the idea of clearly assembling the different parameterizations needed to calculate air-sea gas exchange fluxes could be helpful. However, I am worried that some readers may leap upon this scheme as the "answer" for calculating air-sea gas exchange fluxes without looking in detail at the different choices. It is not always obvious which parameterization is the best one to choose and I am concerned that this scheme might actually be a detriment to the field because it might prevent people from using more appropriate parameterizations. This latter fear could be addressed if the author made it clear that other parameterizations (especially ones that might come along in the future) might be improvements over the ones chosen. In general, the language employed should be more cautionary. Additionally, it would be very helpful if the numerical code included could easily be extended to include other parameterizations as they become available or for readers who disagree with the author's choices. Perhaps the code already can do this – I am not familiar with R so I could not check the code. Finally, it would be very useful to see a detailed error analysis which would include the estimated uncertainty with estimating each term/process in the parameterizations (i.e. molar volume, salinity dependence, air-side transfer velocity, water-side transfer velocity, etc) and ultimately the final gas transfer flux. Such an error analysis would highlight which parameterizations might need to be replaced soonest and would be very useful for any researchers planning on using calculated air-sea gas exchange fluxes.

Specific Comments

1) Abstract, Line 14: "to improve comparability." I'm not convinced that having a standard scheme for parameterizations would really improve comparability. If the parameterizations were perfect, then it would. But since they clearly are not, and some

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parameterizations might have more severe problems for certain gases (for example, neglecting bubbles is more severe for studying oxygen than for CO2), using a blanket scheme without paying attention to which gas it is for will not necessarily improve comparability.

2) “significant improvements under certain conditions” I wasn’t convinced that the new scheme offers significant improvements.

3) End of abstract: In the paper, the author mentions that some of these parameterizations (such as the one for diffusion coefficients) should only be used if direct data is not available. It would be good to state that as the final sentence of the abstract too.

4) Section 2 is title “New derivations introduced for this scheme”. I don’t think the word “new” is accurate – maybe the salinity-dependence parameterization could be considered new. But the air-side transfer velocity parameterization is not new – it rather is just a modification of the existing parameterization, i.e. it substitutes an existing parameterization for Cd for a constant in the standard Duce et al parameterization. Thus this section should have a different title.

5) P.254, line 20: I think the author means a blanket 20% solubility decrease has been applied compared to the freshwater solubility but could the author please clarify if that is indeed the case. As it is written, statement begs the question of 20% decrease from what?

6) P. 256, line 20: “we find closest fit to the Ks data . . .” It would be nice to include a figure that shows the data as a function of Vb and shows the fit.

7) P. 258, equation 11: u* should be defined the first time it is used – later in the paper, it is stated that u* is wind stress but that should be moved to here, the first time u* is used.

8) It would be useful to have more explanation of why certain parameterizations were chosen. This is really essential because the author is in some sense asking the reader

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to believe that these parameterizations are the ones that should be used in all cases. In other words, the author is expecting the reader to trust the author's opinions on all these parameterizations. In order for the reader to do so, the reader needs to be informed about why they were chosen. In some cases, the author explains his reasons and that is laudable. But in some cases, he does not. For example, on p.262, line 8, why should the scheme of Tsilingiris be used? What are other options? What makes that one the best? I would also like to see more information for choice of parameterization for molar volume and for choice of diffusion coefficient parameterization.

9) P. 264, line 18: Although the Nightingale parameterization is probably a reasonable choice, choosing it because it sits in the middle of other parameterizations is not a very good reason. If most of the existing parameterizations are wrong, then choosing one in the middle doesn't make it right! There are newer parameterizations that would be better choices, including some for example that explicitly include bubble processes (Stanley et al, 2009). This is one place where it would be very useful to include more information on newer gas exchange parameterizations and to make the code flexible enough that different users of the scheme could make their own decisions about which to use.

10) P. 269, line 25: "case by case" basis. I don't agree with the term case-by-case basis. Microlayer effects, such as surfactants, are important for all gases with water side gas transfer velocity and thus need to be applied much more widely than on a case by case basis (see Guitart et al, 2010, as well as several papers by Frew). It would be OK with me if the model doesn't include those effects since I'm not sure if a parameterization exists for them. However, to say "case by case basis" makes it sound like they are only important some of the time. Additionally, bubble effects have been shown to be important for many gases and thus need to be included with a wide range of gases and thus again used on more than simply a case by case basis.

11) P. 269, line 26: "suggested that this scheme should be adopted". This is another place where the caveat should be made that if some of the data is directly available for

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a gas, then the data should be used instead of parameterization.

12) P. 270. Line 1 “standard and intercomparable method.” I don’t think it is right to promote this scheme to become the standard one when the parameterizations are likely to be quickly out-dated. Some sentence should be added that as improved parameterizations are published in the literature, the improvements should be used in the scheme. Essentially, I think language should be more cautionary, both here and earlier.

13) P. 287, Fig.4: It is hard to tell the dashed lines representing Duce et al Sc and Duce et al with Large and Pond C_D apart in the legend. Also, it would be nice to see an inset that blows up the region of wind speeds less than 7 m/s to see what differences are in that region.

Technical Details

1) In many cases, when terms in the equation are explained, the phrase starting with “Where” on the line following the equation is capitalized whereas I don’t think it should be. In any case, the author should at least be consistent and either always capitalize “Where” or never do.

2) P.257, line 26: “methanal” should be “methanol”

3) P.261, line 8: “veloicites” should be “velocities”

4) P. 262, line 9: “represetative" should be “representative”

5) P. 263, line7: “reccommended" should be “recommended”

6) P.265, line 16: “vs” should be “vs.”

7) P. 280, caption: The second time that “Vb” is mentioned, it should be formatted the same as the first time.

8) P. 282: Table A1: could the headings of the columns be moved a bit so it is more clear which heading goes with which column?

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9) p. 290, Fig 7: Axis labels should not use slashes but instead us negative exponents, i.e. pmol m-2 s-1

Interactive comment on Ocean Sci. Discuss., 7, 251, 2010.