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Interactive comment on "Comparing solubility algorithms of greenhouse gases in Earth-System modelling" *by* V. M. N. C. S. Vieira et al.

F. Joos (Editor)

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Dear authors

The support by reviewers to assess submitted manuscript is vital for Biogeosciences as it is for any other high quality peer-reviewed journal. The work by the reviewer is done on a voluntary basis and it is a considerable intellectual and time-consuming effort to thoroughly and carefully assess a manuscript. I thank both reviewers for their work and for their independent assessment of the manuscript following the procedures of Biogeosciences and its discussion format as explained on the website of Biogeosciences. I appreciate that both referees signed the reviews with their names.

Unfortunately, you suggest in your replies that the reviews are not independent and

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biased and that the review process is flawed. I emphasize that these suggestions are not justified, not appropriate, and not tolerable. As an editor I have to firmly reject such statements and insinuations. The review process has been carried out in an open and transparent manner and following the journals discussion format. Reviewer 2 rightly dismissed your claims as unfounded.

The reviewers were selected based on their scientific merits in the field. One of the referees was suggested by you. I asked both reviewers to evaluate your two papers on air-sea gas exchange and on solubility individually. In addition, I asked the referees for advice concerning a potential merge of the manuscripts into one single manuscript. This appears natural as the two manuscripts deal with closely related topics. It is in my editorial duty to evaluate whether closely related manuscript merit separate publication. The authors rightly responded to this query and there are no reasons to accuse the authors for doing so.

For future efforts, I would like to ask you to communicate on a factual basis.

Reviewer 2 wanted to respond to your reply and further clarify his assessment of your manuscript. He was not able to do so as the open discussion, following the journals procedures, closed shortly after your response. Instead he provided his response to the editorial office. Given the special circumstances discussed above, I add these additional comments by reviewer 2 at the end of this letter.

Turning to your manuscript, your work has been evaluated by two reviewers who are experts in the field. Both reviewers raise serious concerns regarding the quality of your manuscript and recommended that the manuscript should not be published in Biogeosciences.

I share the assessment by the reviewers. I am not able to accept your manuscript for publication in Biogeosciences as the novelty of the science appears not to merit publication. This conclusion is based on the review comments, my own reading of your article, the consideration of your replies to the review comments as published

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on the interactive discussion site and of a revised manuscript e-mailed to me. Your suggestions that the reviews are not independent did not influence this decision.

Potential options to proceed with your manuscript are outlined on the journal's "Frequently Asked Question" page: http://www.biogeosciences.net/general_information/faq.html#chapter7

I regret that I am not able to communicate a more positive outcome and thank you for time and effort that went into the preparation of your manuscript.

Yours sincerely,

Fortunat Joos

———————— Additional clarification by reviewer 2: ————————

My comments below pertain to this section in Dr Vieira's response to my review.

Reviewer comments:

It is unsurprising that the scheme of Johnson 2010 does not agree well with the empirically determined solubilities - my scheme is intended to be a stop-gap where such empirical data is unavailable i.e. for more 'obscure' trace gases - it is a generalisation of solubility on the basis of easily determined molecular properties. I'm rather pleased at how well it does compared to the de-facto standard solubilities! As I state in the abstract of Johnson 2010: " It is intended that the various components of this numerical scheme should be applied only in the absence of experimental data providing robust values for parameters for a particular gas of interest. "

Authors' comments:

The algorithm by Dr Sander and colleagues, which Dr Johnson adapted, also had their parameters empirically determined from data collected under robust laboratory experiments. Without any prejudice to the works by Dr Weiss, Dr Price, and colleagues, we fail to understand why their parameterizations are a priori unquestionably right,

deeming the works by Dr Sanders and colleagues to be a priori unquestionably wrong?

When Dr Johnson published his adaptation, which included a comparison to the solubilities estimated by other authors for several gases as CO2, CH4 and N2O (in Johnson 2010, Table 1), he compared his estimates of CO2 and CH4 solubilities to those by Teng and Yamasaki (1998) and Yamamoto et al (1976), respectively, and not to the works by Dr Weiss. Therefore, Dr Johnson could not be considering the works by Dr Weiss as the de-facto solubilities and could not be referring to them in his abstract.

Even now, Dr Johnson is not convinced about those being the unquestionably right parameterizations estimating the de-facto solubilities, as he wrote in his comment: "That is not to say that the Weiss solubility data are unquestionable - someone somewhere should probably check them some time as the whole community relies on them as far as I am aware.

Reviewer's reply:

There are 3 reasons why I believe that comparing the empirically derived solubility functions of Weiss and coworkers to those of Sander / Johnson are inappropriate, which I did not provide in detail in my original review:

1) Weiss and coworkers provide solubility terms with complex polynomial fits to measured data whereas Sander's solubility temperature dependence term is a much simpler, generalised relationship based on a single parameter value.

2) Weiss's groups' solubility data for each gas they studied is determined from a series of consistent and carefully controlled experiments, whereas the data contained in Sander's database is necessarily broad, from data from various sources. Commonly multiple values for T dependence for a given gas in Sander's database were averaged to give the ball applied in the scheme of Johnson 2010. Thus it is further

3) When applying the salinity dependence in Johnson 2010 I derived a general empirical relationship to apply to all gases, thereby making the scheme much less precise for

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any given gas than a study of the T and S dependence of the solubility specifically of that particular gas.

This does not mean that the Sander database of solubilities is WRONG, it's about what is appropriate to use in different situations.

As I said I do not deem the work of Weiss unquestionable and it is most important that we are confident in their parameterisations - for important and close-to-equilibrium gases like N2O and CO2, a couple of percent error in the solubility function could have huge effects for ocean emission/uptake estimates. However, comparison with a more derived and generalised approach is not an appropriate test, nor is it evidence in itself that Weiss and co should be questioned.

I remain (completely independently) unconvinced that this work merits publication on its own in biogeosciences

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Interactive comment on Biogeosciences Discuss., 12, 15925, 2015.