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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.

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Reviewer comments

The second paper by Vieira et al presents two different parameterisations for the solubility of CH4, N2O and CO2 - that of Johnson 2010 and the classic Weiss and colleagues empirically derived relationships used widely in GHG science and presented in Sarmiento and Gruber. The paper is rather hard to follow (paragraphs longer than a page. . .) and there are a few non sentences and other minor typographical errors which I will not raise here.

Authors' comments

Grammar and syntax were improved, paragraphs shortened, and the presentation of

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the algorithms was optimized. Several equations where separated from the text where they were embedded.

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

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should probably check them some time as the whole community relies on them as far as I am aware. If science funding and scientific publishing worked properly the results of Weiss and co workers would have been validated many times over by labs around the world given their importance...".

Unsurprisingly, the competing formulations agree remarkably well, often leading to similar results, with mismatches being seldom of the order of 5% and never more. Nevertheless, we demonstrated that these 5% mismatch occur in critical situations with a potential for significant impact in Earth-System Modelling.

Reviewer comments

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Authors' comments

The conclusion of our conclusion was precisely the importance for Earth-System Modelling that someone does check sometime. We demonstrated that the forecasts about the marine and atmospheric storages of GHG can vary substantially with small changes in solubility estimates. Nevertheless, we were careful enough to never take sides, even in the own title. We provide a software and framework allowing the users to choose their preferred parameterization and to compare between each other, and against measured data. It is confusing why all this is important as long as not done by us.

Reviewer comments

Therefore I do not see that this is a worthwhile publication in its own right and would be better integrated into the associated paper on transfer velocities, reduced down to

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a few paragraphs.

Authors' comments

We disagree based on the reasons presented above. About adapting this work to a few paragraphs in its companion paper, we already replied to the exact same suggestion in Dr Wanninkhof's review. We were expecting for an independent review as standard procedure in ISI indexed publications.

Our framework allows for the estimation of the solubility of nearly all gases in the biosphere including, besides GHGs, also aerosols like DMS with a notable effect on Earth's heat budget and climate. So far there are no alternatives to the sequence of updates: Sanders (1999) -> Johnson (2010) -> Vieira et al (2013, 2015a,b).

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