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Biogeosciences Discussions

Interactive comment on "Improving estimations of greenhouse gas transfer velocities by atmosphere–ocean couplers in Earth-System and regional models" *by* V. M. N. C. S. Vieira et al.

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Reviewer's comment

The first of two papers by Vierra et al presents a new scheme for coupled region and ESMs for calculating the air-sea flux of GHGs. Whilst the modelling and measurements may well be of publishable quality it is impossible to tell because the paper is very hard to follow. pages 15903-5 contain an extensive account of a series of models, presenting quite a lot of not very important information. Like some other parts of the paper this makes it very difficult to follow. Maybe this could all be summarised in a useful table of which model uses which parameterisations.

. . . The introduction doesn't really manage to set the scene for the work presented, neither does it outline aims, objectives or the plan for the work and the way it is presented in the paper. This makes the rest of the paper very hard to follow as the reader never knows what to expect! I'm not sure what the authors mean by "The competing formulations were tested with simulated data relative to the European shores" The big blocks of text describing the model with inline equations are extremely hard to follow and understand. This needs to be simplified and made clearer, and equations spaced out. I found the first time I read the paper that I had got to the discussion without really appreciating what the authors had done or why - the paper is very hard to follow and could do with a careful restructure and simple statements of what was done and why early on to frame the methods results and discussion sections.

... How do the schemes presented here compare to NOAA COARE. What is their advantage over COARE? The authors need to carefully rewrite the paper making clearer the motivations for their work and the significant findings from it before any decision on whether the work is publishable is made. As it stands the paper is not publishable as it is too difficult to follow and results and their significance are not clear. The videos are not well explained and the point of them isn't really made clear.

Authors' replies:

the irrelevant information in the introduction was removed, the introduction was reformulated in order to "get to the point", and the final paragraph of the introduction states much clearer the work's aims and the steps taken to achieve them.

the methods were reformulated for a much clearer presentation. Most equations imbedded in the text were extracted and presented separately. Longer paragraphs were split. The model presentation is now much clearer with concise specific sections ending with their specific equations.

the results were presented in greater detail. The links within the sequence of results were improved, but also the links of the results with the methods enabling them. The re-

lations with the videos were substantially improved. We have a text for video captions. But this needs be coordinated with Biogeosciences production office.

the discussion was improved. We get to the point with greater clarity and more soundly based in the results.

The COARE algorithm is acknowledged as a keystone in the subject area for the last decades. Nevertheless, its mathematical structure disables its application in this situation, the reason why the bulk of the scientific community (both reviewers included) has preferred the alternative mathematical structure that was also adopted in our work. We introduced a new (final) paragraph in the discussion devoted to this issue.

Our framework allows for the estimation of the atmosphere-ocean exchange of nearly all gases in the biosphere. These include, besides GHGs, aerosols like DMS with a notable effect on Earth's heat budget and climate. A small paragraph about this was introduced in the discussion.

Reviewer's comment

It feels rather like Wanninkhof 1992 kw parameterisation and models that employ it are being demonised here. For sure, modellers could do to catch up with some of Wanninkhof's more recent work on the best wind speed based parameterisations, but they implicitly (admittedly with considerable error bars) account for much of the processes discussed in Vierra's work, and thus is a decent first order parameterisation - in the absence of anything better at a similar level of simplicity it is not unreasonable for models to currently used wind speed driven approaches. However the need for progress into other forms of gas exchange parameterisation is a real one.

Authors' replies:

Regrettably, our work was misunderstood as demonizing Dr Wanninkhof's work and the ESM implementations using it. Thus, we reformulated the text to recognize the validity and relevance of Dr Wanninkhof's work. Using the reviewer's own words, "the need for

progress . . . is a real one", and that was our aim when we proposed replacing simpler formulations by our more complex framework in order to model the coastal ocean with its inevitable finer resolution. Some co-authors in this work have been, and still are, active participants and/or collaborators in the above mentioned ESM implementations that we were accused of demonizing.

The argumentation that Dr Wanninkhhof's formulation "implicitly (admittedly with considerable error bars) account for much of the processes discussed in Vierra's work, and thus is a decent first order parameterisation - in the absence of anything better at a similar level of simplicity" fails for the following reasons:

when applied to the coastal ocean, these are not true error bars in the sense that they may contain systematic error when applied to specific locations due to their missrepresentation of the processes at a local scale. We further quantified that error by comparing with our more complex formulations and obtained only roughly 50% of the overall gas volume exchanged between atmosphere and ocean over a 66h period and the whole area modelled. We consider this is quite an astonishing statistic to consubstantiate or point.

Any formulation at a similar level of simplicity will inevitably fail tremendously when applied to the coastal ocean, as shown by Fig2 and Video 5.

Our framework is not at a similar level of simplicity. It also accounts for the formation of bubbles with breaking waves, current drag with the bottom, and can easily be updated with formulations for other factors

Reviewer's comment:

That said, ultimately the authors are presenting a revised/ improved wind-speed / micromet. based parameterisation and do not account for bottom driven turbulence or other drivers.

Authors' replies:

This is not true: bottom driven turbulence was included in former equation 1 (now equation 2) and its related section above. We simply could not test these specific issues with our data. Other drivers are also included or can easily be updated.

Reviewer's comment:

It is unsurprising that including the two-layer model does little to the estimated transfer for the gases concerned - these gases are all rather too insoluble to expect a large effect.

Authors' replies:

Besides being unsurprising, due to our work, it no longer lacks demonstration about the effect of the two-layer model on the estimated transfer velocity of the gases concerned. Furthermore, we disagree with considering "little" a 5% difference in the overall flux of N2O, known to have a greenhouse gas effect 298 times higher than CO2.

Reviewer's comment:

The authors claim that their model based approaches are finer scale and more accurate than Wannikhof's 1992 parameterisation but their data falls either side of his parameterisation so I'm not sure what progress has been made . . . Furthermore, Wanninkhof's 1992 formulation stacks up pretty well next to the methods presented here and no worse than the rest of them relative to the observations.

Authors' replies:

Compared to our's, the parameterization by Wanninkhof (1992) was unable to adapt its kw to the changing conditions in sea-surface roughness and atmospheric stability. That was the reason why ours fell iteratively on both sides of Wanninkhof's formulation, with the upper bound corresponding to rougher sea-surfaces and the lower bound corresponding to smoother sea-surfaces. This improvement is clearly presented and explained in our work. Later, we demonstrated its implications when we tested all along the European coastal ocean, resulting in the before mentioned 50% gas volume

transferred.

Interactive comment on Biogeosciences Discuss., 12, 15901, 2015.