

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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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.

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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. 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. . . 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. No real discussion of that major difference between all the parameterisations and the EC CO2 observations is attempted?

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.

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The videos are not well explained and the point of them isn't really made clear.

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.

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... 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.

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