

Ocean Sci. Discuss., referee comment RC2
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Comment on os-2021-4

Anonymous Referee #2

Referee comment on "Surface atmospheric forcing as the driver of long-term pathways and timescales of ocean ventilation" by Alice Marzocchi et al., Ocean Sci. Discuss., <https://doi.org/10.5194/os-2021-4-RC2>, 2021

This paper describes a novel and informative numerical experiment, in which the ventilation from the surface layer is characterized by injecting a different tracer each year, in a global ocean model that resolves inter-annual changes, with forcings corresponding to 1958-2017. Ventilation age distributions are then studied, and the technique enables a much more detailed analysis of ventilation than can be achieved in the real ocean by observations, which are limited to a small number of measured tracers, and where the yearly time resolution is usually not available. The approach will be of interest to all those studying ocean uptake of heat, carbon or transient tracers from the atmosphere.

Overall the paper is innovative and very interesting, and the presentation is clear and good. I have some relatively minor criticisms below that I think should be addressed to improve and clarify it before acceptance.

General

Model vs reality: my biggest criticism is that I found it frustrating that we aren't really given the information to know how to relate the results of the study to the real ocean. The study is of a model, but one forced with re-analyses for a specific modern time interval, suggesting we ought to be able to relate the results to the present day ocean. In much of the paper the authors are discussing inter-annual and decadal variability, which they would like to relate to the real world, but it's hard to tell how much faith to put in this. There are some important model aspects that don't accord with the real ocean -- we are told for example that it captures N. Atlantic and Southern mode water formation but that there is too-deep ventilation in the N. Atlantic. In the Southern hemisphere, while there is SAMW formation, there appears to be little or no AABW formation. So to what extent should we view it as just a model study, and to what extent a simulation of ventilation in the real ocean?

What would really clarify this for me would be a comparison with how this model's simulation of say, CFC distribution compares with that in the real ocean. I think it ought to be possible to do this without doing any more model runs since, as the authors point out, their tracers constitute a set of numerical Greens functions for transfer from the surface into the interior for each year of this 60 year period. Even if this is not possible, I think

there should be a section in the paper dealing more explicitly with the issue of the extent to which the model results are interpretable in the real ocean.

Minor:

Use of "Ventilation depth": The natural interpretation of this term would be the depth to which the tracer has penetrated. However, in the paper it is used to mean something rather different and quite specific: the definition makes clear (Line 176) that it is the column integral of the tracer, not an actual depth. However the tracer is defined as dimensionless so that the column integral has dimensions of depth. I would suggest at least a sentence after the definition to emphasise what is, and is not, meant by the term. Maybe the terminology should be changed – why not call it the "column integral" for instance?

Line 251: "Ventilation appears to be twice as effective in the Northern than the Southern hemisphere". This is a fascinating finding that deserves a good deal more investigation and some explanation. Is it due to the shortcomings of the model, or is it actually true in the real world? Even if it's a model effect it is worth understanding better. The comparison with a real tracer such as CFCs that I suggested above would help here.

Discussion line 317 and following on strength of AMOC and ventilation: I'm rather unclear as to why the authors concentrate on the residuals of fig 7, e.g we are not shown the correlation with the actual ventilation at 25 years and the AMOC, but rather the degree to which that is different (mostly, higher) than expected from the correlation with the ventilation at 1 year. I find it hard to interpret that finding (and clearly the authors do too: they say (line 330) "the mechanism is not fully clear" – I would say that is an understatement). Is there no correlation between the actual amount of ventilation at 25 years and the AMOC at the time of ventilation? (by eye I would think there probably is?)

Detailed comments:

Line 107: The FCT numerical scheme used has "moderate numerical diffusion". The authors don't give an estimate of "moderate" is, but for detailed tracer studies, numerical mixing should be small. It would be good to give a rough estimate, and they might comment on how this would affect their results.

Line 225: some typos here.