

Ocean Sci. Discuss., author comment AC2
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Reply on RC2

Alice Marzocchi et al.

Author 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.,
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We thank the reviewer for the very positive comments and for the helpful suggestions, which we will address in our revised manuscript.

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.

- In the revised manuscript, we will highlight the favourable comparisons between our results and available observations (such as times of deep convection in the subpolar North Atlantic) and include additional discussion about how which model biases may affect our results and which aspects will not impact our conclusions. In addition, we have performed simulations that also include CFCs, which we will include in the revised manuscript to show a comparison between the distribution of modelled and observed CFCs for the mid-Atlantic section.

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?

- As suggested, we will clarify the definition in the revised manuscript and specify that this metric represents the column integral. As proposed in addressing a similar comment by Reviewer 1, we now propose to name this metric “ventilation thickness”.

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.

- In the revised manuscript, we will add further discussion of this aspect of the results and comparisons with the additional CFCs simulations where possible and helpful.

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?)

- We will re-examine the suggested correlation and provide further discussion on the possible interpretation of these findings in our revised manuscript.

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.

- We will comment further on how the scheme may affect tracers’ dispersal and our results.

Line 225: some typos here.

- Thank you. We have corrected these.