Many thanks David and co-authors! This is all great, the reviewer comments have been addressed adequately. Based on my own reading, I still have a few minor points I would like to raise and ask you to address in the final version:

(i) you state that "it is difficult to detect changes in the carbon inventory of the ocean with measurements of DIC" (p.8 first line) and never provide a good explanation. At the same time you argue that the added alkalinity should and can be measured to some extent. The signal-to-noise ratio of additional alkalinity and additional DIC should be very similar. Why is measuring DIC not given a more positive consideration? Could you please explain or rephrase the possible use of DIC measurements?

It is true that TA and DIC will have similar signal-to-noise after a few months to years in the ocean flow. However, we meant that TA increase should be measured in the near field after deployment, and that it would be impossible to use an increase in DIC in the far field to determine CDR.

We have added the following sentences to the section that discusses alkalinity:

It is worth noting that measuring a TA increase near the OAE deployment point may be possible, but once the OAE-perturbed water has dispersed in the ocean flow, the signal-to-noise ratio will likely be too low to make any accurate quantification. This is also the case for attempting to quantify CDR using DIC, as discussed below.

(ii) the citation of other papers of the Guide should be "Fennel et al. (2023, this Guide)" etc.

Fixed

(iii) the DA discussion in chapter 4, page 9, bottom: In ocean-only models, the counterfactual condition could use the same physics and the same initial conditions for ocean biogeochemistry as the DA solution for the OAE condition, but for the counterfactual just run without OAE. This neglects possible (but usually unlikely) direct effects of OAE on the ocean physics. It assumes that DA produces dynamically consistent trajectories (Ensemble filters or adjoint), which would be required for mass conservation anyway, with mass conservation likely being very useful if not mandatory for models used in MRV. If this is correct, wouldn't the counterfactual problem be much smaller than you indicate?

This is true if you are only assimilating physical variables. However, if you are assimilating BGC variables, there is still a problem.

Sorry for bringing this up only now. Happy to learn that my concerns are not valid. If they are, they should be straightforward to address via a few changes in the text.

Thanks and best wishes,
-Andreas