Response to Reviewers

We would like to thank both reviewers for helpful and constructive comments.

Besides minor textual and methodological clarifications that we will address point-by-point in a revised manuscript and response, the primary concern raised by both reviewers is the idealized nature of the experiments presented in the paper. For example, Reviewer 1 recommends (similar to comments from Reviewer 2):

While in several places you mention that this is an "upper-bound on hydroclimate reconstruction skill" I think that this should be emphasised further, particularly in the abstract, but also while discussing the results. And it should be made clear that based on this study on its own you cannot tell if a skilfull DA reconstruction is actually a possibility now, merely that it is theoretically possible. To this end I think that a section describing what uncertainty this technique includes and what it does not include would be very useful.

This is a good idea and we will add further clarification on this point in the abstract of the revised manuscript and in new a summarizing paragraph in the Conclusion that includes all the idealizations and corresponding caveats that are necessary to interpret our results. This latter addition will include a discussion of the perfect model framework and the representation of proxy errors in pseudoproxy experiments.

Reviewer 2 also raises the following specific points that are classified as major issues:

…the method assumes that there is no model bias, that the model B matrix represents the true covariance structure. It is not clear how the residuals for the pseudoproxy are calculated and whether H is assumed to be known perfectly (or are the parameters of H degenerated, or is the VS-lite recalibrated somehow with the pseudoproxies?). Is R taken as diagonal? What noise model is used? And is R assumed to be known perfectly?

Given both the perfect model and pseudoproxy frameworks, we indeed make several simplifying assumptions including some brought up here by the reviewer. B represents the true covariance structure, H is known perfectly, R is diagonal, based on white noise, and known perfectly. We will add further clarification on these specifics in Sections 2.3 and 2.4 of the revised manuscript.

Another worry I have, in the examples given, is that the NINO index is specified with the same tree ring width series as the drought indices. Hence, the NINO index and drought are expected to be related as they are specified from the same tree ring width. Actually, this is seen in Fig. 9, which shows a stronger ENSO signal in the reconstructions using only tree ring width than in the "true" simulation. There is an element of circular reasoning here. In this context, localisation should be discussed. The author speak of localisation as an "ad hoc" method, but controlling this sort of circular reasoning would be one advantage that the authors should consider.

Respectfully, we are not clear on the meaning of this comment. As discussed in Section 2.4 of the paper (pgs. 6-7), the state vector contains the Nino 3.4 index (along with the other variables) and we use different climate model output (temperature, precipitation, etc.) to construct the pseudoproxies beforehand, but we are confused by the statement that the Nino index is "specified from the same tree ring width." Figure 9 shows the results from two reconstructions in which the first used only tree rings and the second used both corals and tree rings from the pseudoproxy network. A principal takeaway from Figs. 8 & 9 is that the specific proxy type used in these reconstructions doesn't matter very much for the reconstruction of ENSO (they have similar reconstruction features). As shown in Figs. 7 & 8, it is actually the reconstruction with tree rings alone that has a *weaker* ENSO signal because there is progressively less information about ENSO further afield from the central Pacific; with only remote tree-ring proxies available, adding localization would damp out even more information about ENSO and would presumably make the tree ring-only reconstructions of ENSO even worse. Moreover, as we note toward the end of Section 2.3 (pgs. 5-6), localization is actually mathematically unnecessary in the limit of very large ensemble sizes (though large ensemble sizes are a luxury we have here that isn't usually available for traditional uses of DA).

Reviewer 2 also raises some additional points that require responses beyond simple clarifications and additions (or other points addressed above) that we address below:

P. 3, l. 29: I think the boundary conditions require some further explanations. Is it sensible not to consider boundary conditions? Or would physical consistency be violated (e.g. by using a non-volcanic background during a volcanic year?

Several previous studies have shown that using an off-line DA approach, similar to the one that we employ, does not require boundary-condition specific priors for specific years, such as for volcanic eruption years (e.g., Steiger et al. 2014). As constructed in our manuscript, the prior contains years with volcanic eruptions that are sufficient for reconstructing volcanic eruption years. We will clarify this point in the revised manuscript.

P. 9, l. 4: It is not fully clear how the limitations were derived. As I understand the approach, the actual limitations are dependent on the climate conditions (couldn't the same VS-lite parameters make a tree moisture sensitive in one year but temperature sensitive in another one?).

It is correct that the parameters of VS-lite don't determine the growth sensitivities. This point is discussed in the text indicated by the reviewer where we note that we use the growth responses (these are optional outputs of VS-lite) to compute the limitations. We will further clarify this point in the revised manuscript.

P. 14, l. 3: In addition to El Niño, a look at the Atlantic Ocean might be interesting.

We agree that the influence of the Atlantic Ocean would be important for real-world investigations. Nevertheless, we have not explicitly reconstructed Atlantic modes/variables in this manuscript (such as the AMOC or AMO) and the dynamic investigations that we pursue are only examples of possible analyses, none of which are meant to be exhaustive. We therefore do not believe that an analysis of Atlantic influences on drought in the American Southwest would add significantly to the example pseudoproxy analyses.

P. 14, l. 12-14: I find this conclusion a rather dangerous one to make in a perfect-model set-up

The lines in the manuscript referenced here constitute an aside and are not integral to the paper. We will remove them from the revised manuscript.