

Interactive comment on “Multi-time scale data assimilation for atmosphere–ocean state estimates” by N. Steiger and G. Hakim

N. Steiger and G. Hakim

nathanjs@uw.edu

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Referee #1 incorrectly characterizes the reconstruction method employed in this paper as identical to a previous paper, "Assimilation of time-averaged pseudoproxies for climate reconstruction". As claimed evidence for this, Referee #1 cites the portions of the two papers dedicated to writing out and explaining the terms in the update equation of data assimilation. Certainly we have not changed the update equation itself, which is classical, and its terms have retained their same meaning. We note also that the method for the first paper is in the appendix of that paper and not the section cited by Referee #1. This first paper cited applies assimilation at annual time scales and cannot be trivially applied to proxies of arbitrary time scales for several important reasons. The purpose of the present paper is to overcome these issues and present a proof of con-

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cept on the reconstruction approach at longer time scales. As discussed in the section of the paper specifically detailing the method (line 25 pg 3733 through line 10 pg 3735 and Fig. 1) the key innovations here are the processes of assigning specific priors to each year of the reconstruction, assuring temporal coherency of all these priors, and then revising the assimilation process itself such that proxies are assimilated singly for their entire length, not assimilating all proxies for a given year and then moving on to a new year. The explication of this process and tests of its skill in new contexts and new models for both atmosphere and ocean variables are highly novel and non-trivial extensions of the previous paper. To make an analogy, Evenson (1994) solved the well-known Kalman filter equations with an ensemble technique. That detail in solution is critical and has led to hundreds of papers exploring other aspects.

Referee #1 also claims that the paper makes "mistakes in mathematical terminology." However, these purported mistakes are not in fact mistakes; some appear to be rather pedantic suggestions where the writing was clear to the reader, and other suggestions appear to be based on misunderstandings or taking pieces of statements out of context. Taking each claimed mistake in turn: we understand the point concerning Eq. 2, but does the reader not know the quantity referred to? The prior and the posterior VECTORS do indeed contain the variables of the model employed (see, for example, chapter 5 of Kalnay 2003); yes, technically when non-linear and non-Gaussian effects may be present there is no proof that this solution is "best," but again, this is a pedantic point; in the paper we never claim that $H(x_b)$ is the "true value of the observations" but rather quoting the paper in full "the true value of the observations are estimated by the prior through $H(x_b)$ " (line 7 pg 3733); note the key word "estimated" since, in theory, the prior is drawn from the same distribution as the true state. As written in the text at lines 18–20 pg 3733 the "non inverse" part of K (note that "numerator" conveys this point more simply) is $\text{cov}(x_b, H(x_b))$ where the phrase "covariance between the prior and the prior estimated observations" refers to x_b and $H(x_b)$ respectively. This phraseology for $H(x_b)$ is consistent within the text as written.

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Certainly we are happy to correct errors in the text and to clear up terminology where appropriate, however, we think each of these alleged mistakes are unfounded.

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