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Climate of the Past Discussions

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

Anonymous Referee #4

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The manuscript describes a method that aims to include proxy data with different timescales into paleo-reconstruction. The method is tested in twin experiments to show that there is improvement in the reconstruction of time series of global mean temperature and AMOC strength when data of two different time scales are used. The manuscript is well structured and easy to read. The results are interesting (although not surprising). I recommend a major revision because of reasons given below:

I may not be an appropriate reviewer for this manuscript. I agreed to reviewing basically because of the title (and abstract) of the manuscript, but after reading the entire manuscript I am not sure why the method is called "data assimilation". At best, data assimilation methods are used ("spreading the information" from point values by means of a Kalman gain matrix) to cleverly interpolate between sparse observations. Also it was

not clear to me from the abstract (because I was confused by the term of "data assimilation"), that the aim of the method is reconstruction that does not require a dynamical model (as the term "data assimilation" had implied to me). Paleo-reconstruction is not my specialty, so most of my complaints are going to be about terminology and their unusual use that may lead to misunderstanding (I, certainly, was misled), rather than the reconstruction itself, which appears to be work reasonably well.

I always thought that data assimilation (which should really be called "model assimilation", because the model assimilates, i.e. changes with, the data, while the data are not changed) refers to using data, as they become available, to improve a model state, mostly in a sequential manner, in order to improve forecasts that again are corrected to by new observations and so forth. As far as I can see, the described method, at least in the way it is applied in the "off-line" mode, is a sequence of inversions that do improve the model state, but because there is no evolution of the model state that is affected by observations, I would not call this "data assimilation", but "(stationary) inversion". I think that the authors need to change their title and the overall terminology in the manuscript.

Adding information to a system of inverse equations should always improve the results. Not only should the solutions be closer to "truth", but also the posterior uncertainty estimates should become smaller. The authors only show the first part.

I suspect that adding an extra 100 observations to the one-times-scale only experiments would have a larger impact if the model states were allowed to adjust to (especially) the short-scale observations, but that's just my speculation.

The improvements between especially "only short time scale pseudo proxies" and both time scale pseudo proxies appears to be quite small for GMT for both models (Fig6) and also for AMOC for the GFDL-CM3, so I find it hard to exclude that this improvement is only by chance. I.e. are the results significant?

Conclusion: With only two models that behave similarly, it is quite bold to decide that

the “results are insensitive to the choice of climate model (Abstract, l15, Conclusion p3745, l17)

More details of my critique:

If I am mistaken about the stationarity of the inversion, i.e. this is really “data assimilation” in a dynamic sense, then the following two points apply (otherwise they are clear to me): p3735, ll1-10. It is not clear how this is done. I assume we talk about sequential data assimilation. The forward model that generates the prior stops at some point (1720) for the analysis step. During the analysis step the time mean over 20year (1700-1720) is updated and the annual mean of last year is updated. Or do you actually update each annual mean between 1700 and 1720 sequentially and at 1720 you update the 20year mean? p3735, ll11. Again I don’t understand the approach: The climate model results are updated with pseudo-proxy observations, but they are never integrated, meaning that the observations do not have any impact on the solution of the next forward simulation? What is the point of this? Computing weighted averages between model solutions and observations is not really data assimilation at all. If this is what is being aimed for, then the title of the manuscript is totally misleading.

minor comments: throughout the text: I don’t know what “to leverage” is supposed to me (can’t find it in any dictionary).

p.3731, l27 any method: do you mean “any study”? Because in the following you talk about methods that have been “used”, or studies that have been “performed”. I do not see, why DA methods a-priori exclude multiple timescales. You do not mention “adjoint methods” (a bad phrase for global inversions or state estimates) as the one in Kurahashi-Nakamura et al (which you cite at the end of the manuscript), that yield themselves naturally to incorporating all timescales within their assimilation window (given an appropriate observation operator that includes averaging in time, see below).

p.3732, l16 assesses -> assess?

p3734: Not clear to me: a non-linear H is applied to a temporal average \bar{x} and the update replaces the time mean in the decomposition. Why? The notion of a forward model (or observation operator, or proxy system model) H already includes averaging the prior state (i.e. you don’t do the decomposition, but you map the variable state to, say, a 20year mean) and H(x) should be directly comparable to the observations. In this context, why do you have to do it iteratively instead of an observation operation that maps the prior to all observations including the average to the corresponding timescales. In that way you would have a true multivariate inversion.

p.3735, l23-27: When the model has no skill, then all data assimilation methods fail.

p.3737, l16: How do you map from GMT to surface temperatures, and from AMOC strength to surface temperatures. I don’t understand how H(x) is constructed in this case. To me the “state variables” GMT and AMOC strength are derived from the “state”, which would to my mind include surface air temperature and ocean velocities. Or is H(x) a purely statistical correlation in this context?

p3739, l11 To my mind, the perfect model assumption and successful identical twin experiments are the necessary conditions that an algorithm has to satisfy, but it’s not enough to show the success of an inversion method.

Interactive comment on Clim. Past Discuss., 11, 3729, 2015.