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Climate of the Past
Discussions

Interactive comment on "Last Glacial Maximum and Deglacial Abyssal Seawater Oxygen Isotopic Ratios" *by* Carl Wunsch

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My thanks to Olivier Marchal for again making very helpful comments. In response, starting with the "Major Comments":

1) As in previous work on this topic, the present analysis requires an assumption about the initial

profile of δ^{18}O (here, at t = 100 kyr). In some of the calculations reported in the ms, the initial profile

is taken as the measured profile. Whereas this approach appears sensible, it does seem to violate

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one of the assumptions of the Kalman filter, i.e., the assumption that the errors in the state (here

the core δ^{18}O profile) extrapolated from the model and the errors in the data are uncorrelated at

every time (e.g., Bryson and Ho 1975, Applied Optimal Control, Taylor & Francis, 1975; transition

from eq. (12.2.10) to eq. (12.2.11) on p. 350 of that textbook). Specifically, if the states extrapolated

from the model are calculated from initial conditions that are constrained from terminal data, as

done here, then the errors in the extrapolated state for the terminal time and the errors in the

terminal data are expected to show some correlation. The author is well aware of the assumption of

independence between extrapolated state errors and data errors in the Kalman filter (see, e.g.,

Wunsch 2006; p. 196). Unless my interpretation of the filter's assumptions is incorrect, I would

suggest that the apparent violation of this assumption in the present analysis be addressed or at

least discussed in the ms.

The point is correct, that a priori errors correlated in time are not accounted for properly in the basic sequential estimation method. Here, however, one must distinguish between the use of identical data for the initial and final states, and a very different *assumption that the corresponding errors are in any way related. I've assumed that the ways in which the initial state would differ from the correct one have an entirely different error structure from the deviation at the terminal state. So, by way of example, terminal errors could be dominantly analytical ones, while deviations at $t = -100$ ky would be dominated by a whole host of processes for which the analytical error at $t = 0$ would likely be completely negligible. (I've added a clarifying sentence.)*

2) The present study assumes, again as in previous work, that the δ^{18}O flux at the core bottom

vanishes. The author is quite upfront with this assumption (p. 3, last paragraph). However, as also

acknowledged in the ms (p. 3, bottom), the data do not provide evidence for a vanishing vertical

gradient of δ^{18}O and hence of a vanishing vertical δ^{18}O flux at depth in the cores (fig. 3). As stated in

the ms, the problem could be reformulated to determine the δ^{18}O fluxes at the core bottom, instead

of the core top δ^{18}O values, from the pore water δ^{18}O data (p. 3, last paragraph). I think that the

present study would be even more interesting if it also investigates this other problem, i.e., whether

the pore water δ^{18}O data could be explained by changes in δ^{18}O flux (or δ^{18}O value) at the core

bottom rather than by changes in δ^{18}O value at the core top. In fact, that this could be the case is unclear to this reviewer, since the downward effective velocity induced by the postulated decrease

of vertical diffusivity with core depth would compete with vertical diffusion in transmitting upward,

along the core, information at the core bottom. It would be useful to test whether this intuition is

quantitatively grounded on the 100-kyr time scale in a future version of the manuscript.

The bottom boundary condition is troublesome. But relaxing it to permit finite vertical diffusion from below would not add much to what we already know: the result will depend directly upon the assumptions concerning the magnitude and sign of w, *and the magnitude of* k *as well as guesses at the statistics, at least, of the temporal variations there. I hope that someone will pursue this (I might), but the message of the present paper already suggests so much freedom in guessing the correct physical situation that I am loathe to explore yet another one.*

3) As mentioned in the above comment, the present analysis assumes that the vertical diffusivity of

δ^{18}O (call it kappa) decreases linearly with depth along the cores. A vertical gradient in kappa induces

a vertical effective velocity (p. 2), in this case a movement of δ^{18}O down-core. This movement tends

to propagate downwards the information provided by boundary conditions at the core top. As a

consequence, it should exert some influence on the controllability of the system and on the results

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of this study, although the ms suggests that a uniform kappa would make little difference (p. 4, top).

I think that a future version of the ms should clarify the basis for the assumption of a decrease of

kappa with depth along the cores. For example, is the assumption based on data of sediment

porosity and (or) tortuosity? The paper of Wunsch (2015) does report the measured vertical profiles

of porosity for the sediment cores, but whether these measurements truly require kappa to

decrease with depth is unclear since kappa also depends on other sediment properties such as

tortuosity.

Numerical experiments, not shown, demonstrate that the "induced" vertical velocity only quantitatively modifies the results for these values of k. A full discussion of the physics governing advection/diffusion in a core, including such zero-order issues as the utility of the one-dimensional assumption, would be a major undertaking for someone more fully competent in flows in porous media at high pressures. I also added some words about the assumptions concerning the sediment-water interface physics.

4) Most of the calculations reported in the ms seem to assume that the data error variance (R) is

“about 10 times larger than the value in Adkins and Schrag (2001)” (p. 9). Could this assumption be

justified? If data error variance is poorly understood, I would recommend that calculations with

different R (i.e., with different data errors for all terminal data, not only for data near the measured

δ^{18}O maximum) be conducted in order to further test the robustness of the results.

Such experiments have been done, but don't really change anything. I've added some sentences about sensitivity to terminal data errors—where the major issue, not resolved, is whether the observed structures are signals or noise.

SPECIFIC POINTS

Abstract and everywhere in the manuscript: replace "salinity/chlorinity" with "salinity (chlorinity)". *Ok*

Line 4: "... by them."*Ok*

p1, line 12: "... that the deep ocean ..." *Ok*

p1, line 16: "Recently, Miller (2014) and Miller et al. (2015) have ..." left.

p1, line 20: "... Adkins and Schrag (2003), Miller (2014), and Miller et al. ..."*Ok*

p2, line 4: "... and the model ..." *Ok*

p2, eq. (1b): There should be a minus (not plus) sign in front of the last term on the left-hand side. yes

p3, line 1: I think that symbols for chemical elements (here oxygen) are generally not italicized. *Ok*

p3, line 9: "... is an estimate of their uncertainty". *Ok*

p3, 1st full paragraph: the last sentence may need to be rephrased (a verb seems to be lacking). ?*I think ok?*

p3, line 24: "... based upon measurements in corals ..." (sea level curves are not based on f06418O values

in corals). *ok*

p4, line 4: "... to the results (see the Appendix)." *Ok*

p4, line 19: "... matrices A, B, ..." (drop comma after "matrices"). *Ok*

p5, line 16: "Lagrange multipliers, or adjoint, methods and the Rauch..." *Ok*

p5, line 24: I think that the observability matrix for the present system would be more

conventionally defined as the partitioned matrix (e.g., Gelb et al. 1974):

= [0|0| ... ( )],

which has a similar form as the controllability matrix (5). *True, but I've written a special case, now noted.*

p6, line 9: "... 100,000 yr ..."*Ok*

p6, line 24: "...f06418Ow distribution ..." (drop comma). *Ok*

p7, "... likely connected to the extreme volatility of dynamical properties in the equatorial Pacific

Ocean and is not further discussed here": this could be elaborated or dropped. *Disagree. MS. explains why not further discussed.*

p7, line 17, and everywhere in the manuscript: replace "... and/or ..." with "... and (or) ...". *A journal style choice. I will wait and see.*

p8, line 18: please define the matrices P0, Q, and I. *Dropped.*

p8, line 20: "... k decreases linearly with core depth from ... at z = h to ... at z = 0 m." *Ok*

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p9, line 3: “. . . conditions, figure 8 shows . . . ” *Ok*

p9, line 11: “. . . and the terminal data uncertainty was strongly reduced in the vicinity . . . ” *Ok*

p9, lines 22-23: “. . . (about 10 times .. in Adkins and Schrag [2001] and now meant . . . error), P0 = . . . ”. *Dropped*

p9, last sentence: is it meant “It would appear that this record is not consistent with the prior d18O *Dropped*

profile at 100 kyr within its stipulated error bars”? Some clarification would be useful.

p10, section 3.3: Please also explore cases with a sea-level prior and zero initial conditions, and

briefly describe the results in the ms. *Too many cases already!*

p10, section 3.4: Please also explore the case with zero initial conditions and briefly describe the

results in the ms. *Same as above*

p10, line 23: “. . . rough summary would include: . . . ”*Ok*

p10, line 4: “(1) Physical transport of f06418O is one-dimensional (vertical)”. Although this seems to be

common in the literature, I would suggest not to use the division sign in non-mathematical

expressions, such as “Physics/chemistry”, “diffusivity/porosity”, “Advection/diffusion”, etc. *Again a journal style decision. I don’t think much danger of confusion.*

p11, line 23 (in Appendix): “. . . from the bottom of the core at z = 0 m to the top of the core at z = h”. *Ok.*

In the development following (A1), I would suggest first to introduce the change in variable ($\zeta =

. . . ”$) and then to set $c = \hat{c}(\zeta) . . . ”. The ordinary differential equation between (A1) and (A2)

should have ζ as the sole independent variable. *yes.*

p14, line 16 (Reference list): have “Olver . . . ” starting on a separate line. *Ok*

Fig. 1 could be enlarged. *Ok*

Caption of Table 2: “. . . $\boldmath{x}_t$ . . . ” *ok*

Figs. 7-16 could all be enlarged. *Yes*

Caption of fig. 7: “. . . with k linearly increasing from . . . at the core bottom (z = 0 m) to . . . at the core

top (z = h).” In panel (c), I interpret the solid line as the difference between the filter estimate of

d18O and the measured f06418O, but I am not sure. *Fixed*

Panel (e) could be zoomed in, perhaps on the last

kyr, to better see the changing control and its estimated error near t = 0 kyr. ??

Caption of fig. 10: Please define R in the main text. *Ok*

Fig. 17: the legend indicates that the solution with constant kappa is a numerical one, whereas the

caption indicates that it is an analytical one. Please clarify. The initial conditions and the time for

which the solutions are displayed could be specified. *Ok. Fixed*

Olivier Marchal