Page 1/3

Aug 13, 18 15:23

ttrev

Review of : A statistical method to validate reconstructions of late-glacial relative sea level - Application to shallow water shells rated as low-grade sea-level indicators

It is great to see a study that considers how to rigorously define conditional probability distributions for RSL for paleo contexts. However, the current submission has a major flaw. The SLI residuals are not independent and this must be explicitly accounted for. The current formulation explicitly assumes independence but then contradicts this with a 1/N normalization. The consequence of SLI dependence is clear, for instance, when considering the whole Dyke RSL database for North America. The spatial-temporal density of RSL datapoints varies greatly with resultant variations in datapoint redundancy. Without taking this density variation explicitly into account, use of your scoring scheme for say deglacial ice sheet model calibration will give results with model-data fits biased to where datapoints density is highest, even if the sectors where this occurs represent just a small area fraction of the LGM North American ice complex. Until this is addressed, the statistical method is invalid.

I should also note that this flaw might have been avoided with a more careful consideration of the existing litterature (which is not evident in the reference list), eg Briggs and Tarasov, 2013 and Love et al, 2016.

I do not understand the choice of journal. This submission would seem to me much more appropriate in GMD especially since the novelty here isn't the theory (this is standard Bayesian and probability theory) but the actual implementation. The first line in the abstract also delineates this as a methodology paper: "In this study, we propose a statistical method to validate sea-level reconstructions using geological records known as sea-level indicators (SLIS)." Futhermore, the paper focus is on the method with the viscosity results only provided as an example : "findings are only meant to explain the method and not actually to constrain models."

The paper would also strongly benefit from more concrete details on implementation (probably best included in the supplement) to enable others to do so (especially since the software toolbox is not being made available).

Submission to GMD though requires provision of necessary code/software. This then raises an inequity between the two journals, submit to CPD and avoid the need to provide required code.... I'll defer the appropriate journal choice to the Chief Editor who should have a clearer sense of journal scope. I would like to see a statement from the editor clarifying how to resolve the scope intersection between GMD and CP with respect to software availability.

I would also like to see explicit consideration of tidal range and wave impacts, especially given the significant tides in Hudson Bay along with the well-known "storm-beach" displacement of SLIs.

Once these issues (and the points below) are addressed, I would see this submission as worthy of publication in GMD (or CP if justified by the chief editor).

Printed by Lev Tarasov

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Page 3/3

Aug 13, 18 15:23	
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How is this implemented? And how is pa(t) retrieved from oxcal? # oxcal is a complex enough application that a bit of guidance here # would help others with their own implementation.

ttrev

Assuming that the conditional probabilities of the individual SLIs, P_i, are independent, the joint probalility eq 12

the 1/N_data normalization in eq 12 breaks the stated assumption of # independent conditional probabilities. The likelihood is the joint # conditional probability given by P in eq 11. ln(L) would just be # SUM(ln(P_sli_i) if the residuals were truly independent. Anyway, # there is no basis to assume all the SLI residuals are # independent.