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Geoscientific Model Development Discussions

Interactive comment on "Large ensemble modeling of last deglacial retreat of the West Antarctic Ice Sheet: comparison of simple and advanced statistical techniques" *by* D. Pollard et al.

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The submission can be an informative (and relatively succinct) comparison of two different approaches to making inferences about past ice sheet evolution given modelling and paleo observations. Some specific issues (including some mis-citations) are detailed below. There are four key deficiencies that have to be remedied (to change the above "can be" to "is"):

1) Currently there are no plots nor discussion of model fits to constraint data and as

such it is not clear whether this ensemble actually covers the constraint data.

2) The handling of data uncertainties for all the misfit metrics needs to be spelled out (some treatments are spelled out, but not all). Eg, TROUGH will have dating and downscaling/resolution uncertainties. If these uncertainties are ignored, the inferences based on these metrics are biased and incorrect.

3) how are data weighted within each class? If no weighting is done, then the statistical modelling is assuming all data/model residuals are not correlated, which is incorrect (though commonly implemented...).

4) There has to be justification for giving all data classes the same weight. There are only 8 RSL data sites, all located on the periphery of the ice sheet. There is no basis to give this geographically restricted data the same weight as, for instance, the RMS error between the dynamically modelled and observed present day ice sheet.

If the "advanced statistical method" does use a complete error model that addresses points 2-4 above, then this should be made clear in detail. Ie, are you saying that we can ignore all these issues, do simple latin hypercube sampling (albeit with a large enough sample, but still orders of magnitude smaller than required for proper MCMC), and get roughly the same result as a complete Bayesian calibration determination of the posterior (ie with a complete error/uncertainty model that accounts for uncertainties in the constraint data, structural uncertainties, and correlation between residuals and that covers the constraint data set)? If so, then this claim need to be much more clearly spelled out.

Once these (and the comments below) are addressed, I would agree with Nick Golledge as this being a methodological paper that is well-suited to GMD.

Specific comments:

How is relative sealevel computed? What visco-elastic earth model is used and is geoidal deformation computed?

"Tarasov et al. (2012) used Artificial Neural Nets in North American ice-sheet modeling to fill in parameter space between LE simulations, and have mentioned their potential application to Antarctica (Briggs and Tarasov, 5 2013)."

actually this was as much if not more of a "calibration" as the authors' "advanced statistical technique" and should be clearly stated as such. That 2012 paper also used MCMC to compute a posterior distribution of ensemble parameters given fits to paleo constraint data. The reason that "calibration" wasn't used in the title of that paper was 1) ensemble didn't cover data constraints (attaining coverage is a big challenge given the large size of the constraint data set), and 2) it had an incomplete error model especially with respect to quantifying structural uncertainties. Unfortunately, "Calibration" has become a poorly understood buzzword whose meaning is being watered down in some recent ice sheet relevant publications. To me, if "calibration" is not confidently estimating the probability distribution and thereby the uncertainties of predictions (with the unavoidable clear specification of uncertainties not accounted for), then it should not be called calibration. But this may be a loosing battle...

"Then the geometric (logarithmic) average of the 8 individual Si 's is taken to yield the aggregate score S for each run"

This choice makes no sense to me and needs to be justified. RMSE is effectively log(Gaussian). So your weighted score is (logGauss1*logGauss2*..)ˆ1/8. How does one interpret this? If you are using a non-Gaussian error model, then what is it?

"It differs from from the weighting in Briggs and Tarasov (2013) (their "inter-data-type"), which is algebraic and depends heavily (80%) on the fit to modern ice distribution."

This is incorrect. The weightings are for the RSME score components, but the final weighting is e to the power of the sum of these normalized components (ie assumes a pseudo-Gaussian error model). This is therefore not algebraic. Furthermore, Briggs, Pollard, and Tarasov (2014) should be cited instead. They give a corrected inter-data-type relative weighting of < 50% for present-day data (Coauthors should know the

papers their names are on, rap knuckles.., :)).

"3. Consistent with trends in recent Antarctic modeling studies (Ritz et al., 2001; Huy20 brechts, 2002; Philippon et al., 2006; Briggs et al., 2013, 2014; Whitehouse et al., 2012a, b; Golledge et al., 2012, 2013, 2014), the greater total Antarctic ice amount at the Last Glacial Maximum is less than in earlier papers, equivalent to 5 to 10m of global equivalent sea level below modern"

Incorrect citation of Briggs et al, 2014: Their confidence interval for LGM Antarctic ice volume excess has an upper bound of 14.3 m eustatic equivalent, with lower confidence is > 10 m, and one of their single best fit runs has an excess of 13.2 m. Furthermore, they raise the point that their (well our) model had insufficient grounding-line response compared to proxy paleo data, suggesting that LGM grounded ice volume could be under-estimated. So there is no basis to lump this in with other studies claiming </= 10 m of eustatic sealevel equivalent.

"For ELEV: the minimum squared mismatch of ice elevation and time, within the constraints of descending elevation trend, each relative to the observa tional uncertainties of elevation and time"

#Bit unclear. Is this the same error model as Briggs and Tarasov 2013?

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