Clim. Past Discuss., 8, C1310–C1316, 2012
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Climate of the Past Discussions

CPD
8, C1310–C1316, 2012

Interactive Comment

Interactive comment on "On the differences between two semi-empirical sea-level models for the last two millennia" *by* M. Vermeer et al.

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In this manuscript, the authors present two semi-empirical sea level models (Grinsted et al., 2010, and Vermeer et al., 2011), calibrated against tide gauge and/or proxy data from North Carolina or Australiasia, and evaluate their performance against the proxy data sets. Given the widespread use of semi-empirical sea level models, and the absence of clearly superior alternatives, this comparison is a useful one. However, the comparison and its presentation are at the moment is in need of a clearer conceptual framing.

1 Lack of clarity in presentation of model selection

At its core, the problem tackled by this manuscript is one of model selection, which the authors could address either in a frequentist or Bayesian framework. (At the moment, they apply a somewhat ad hoc mixture of frequentist analysis, Bayesian analysis, and sensitivity analysis, without a clear distinction among the different frameworks.)

It currently takes significant effort to figure out which pairs of models and training data sets the authors use, and whether they have evaluated all the relevant combinations of the same. My (perhaps incomplete list) is:

- Models
  1. G10
  2. K11
  3. K11 with $a_1 = 0$ (which has been demonstrated to be formally equivalent to G10)
  4. R07
  5. VR09
- Sea level training data sets
  1. tide gauge compilation of Jevrejeva et al., 2008 [henceforth J08]
  2. North Carolina proxy record of K11 from 1100 CE-present, possibly including J08
  3. K11 1700 CE-present with an alternative GIA reconstruction, possibly including J08
  4. proxy record from New Zealand, with three different GIA adjustments
  5. proxy record from Tasmania, with three different GIA adjustments

CPD
8, C1310–C1316, 2012

- Temperature data sets
  1. Moberg et al., 2005
  2. Jones and Mann, 2004
  3. Mann et al., 2008
- Sea level validation data sets
  1. the same as the training set
  2. K11 NC proxy record

Not all 150 combinations are currently explored. Regardless of whether the authors choose to explore the complete set, it is very challenging to evaluate their results unless they directly compare the different models using the same training and validation data – and, at the moment, it is hard to tell whether and where they do that.

If the point of the evaluation is to validate the models for use in projections of future changes, I would strongly encourage the authors to use a validation data set that differs from the one used for training.

2 Opportunity for greater clarity in model presentation

As the authors acknowledge, the K11 model as presented nominally abolishes the core assumptions underlying the derivation of the semi-empirical formalism in R07: namely, that there exists an equilibrium sea level associated with a given temperature change, and that realized sea level decays toward that equilibrium level. K11, as written, has no equilibrium sea level. This omission could be readily fixed by the addition of an equation paralleling equation 4, e.g.,

$$\frac{dT_{0,0}}{dt} = \frac{1}{\tau_0}(T(t) - T_{0,0}(t)), \tag{1}$$

CPD
8, C1310–C1316, 2012

with τ_0 being long compared to the timescale of the data.

This representation hints at the fact that, not only is G10 formally equivalent to K11 with $a_1 = 0$, but both models are representative of a broader family of models, specified by:

$$\frac{dS}{dt} = \sum_i a_i(T(t) - T_{0,i}(t)), \tag{2}$$

$$\frac{dT_{0,i}}{dt} = \frac{1}{\tau_i}(T(t) - T_{0,i}(t)). \tag{3}$$

In a more general exploration of model selection, the authors might consider whether more than 2 terms might yield a model with greater likelihood than either G10 or K11.

It would also be more intuitive, at least for me, if the authors were to demonstrate formal equivalence of G10, K11 (and perhaps the more general form) by solving for the equilibrium sea level associated with each. For the generalized model, I get

$$S_{eq}(T) = \sum_i a_i \tau_i T + B. \tag{4}$$

If $i = 1$, then one gets $S_{eq}(T) = a_1\tau_1 T + B$, and equating this the G10 equation for equilibrium sea level readily shows the equivalency if $a_1\tau_1 = A$. This approach is more compact than the authors' current demonstration.

3 Proportionality of relationship between S_{eq} and dS/dt

The authors object to the proportionality in G10 between S_{eq} and dS/dt. While I understand their concerns, it seems to me that – given the role of heat capacity in buffering the relationship between realized and equilibrium temperature (i.e., radiative forcing) – this may be immaterial. If realized temperature approaches equilibrium temperature with a timescale τ_T, the response to a step function change in equilibrium temperature ΔT_{eq} will be given by

$$
\begin{aligned}
T(t) &= T(0) + (1 - e^{-t/\tau_T})\Delta T_{eq} & (5)\\
S_{eq} &= A(T(0) + \Delta T_{eq}) + B & (6)\\
&= A(T(0) + (T(t) - T(0))/(1 - e^{-t/\tau_T}) + B & (7)\\
&= A'(T(t) - T(0))/(1 - e^{-t/\tau_T}) + B' & (8)\\
dS/dT &= \frac{1}{\tau}(A'(T(t) - T(0))/(1 - e^{-t/\tau_T}) + B' - S(t)) & (9)
\end{aligned}
$$

so the short-term rate and long-term equilibrium responses are somewhat decoupled.

4 Differences between local and global mean sea level

The authors state (page 3559) that, in correcting the NC record for GIA, they assume that "all sea level changes [in] the last 2000 yr were caused only by vertical land motion." Surely, they do not mean this literally. If they do, their model should find no change in global mean sea level over the last 2000 years. I assume, instead, that they fit a linear trend to the last 2000 years to correct for GIA and subtract this.

This leaves the question: how to the authors take into account the uncertainties associated with using a single (GIA-corrected) local sea level record as a proxy for GSL? This is unclear as currently written.

5 Pre-1500 fits to the NC record

In their discussion of Fig. 1, the authors discuss discrepancies before 1100 CE, but do not clearly address the considerable mismatch between 1100 and 1500 CE. What is

CPD
8, C1310–C1316, 2012

their interpretation of this?

6 “Manual tuning”

The authors do not explain what they mean by “manual tuning” of parameters. Why did they not apply least squares analysis, which would be a common alternative to a full Bayesian analysis for the purpose of finding the modal set of model parameters?

7 Last Interglacial sea level

The authors write that “it is unclear if global temperature was cooler or warmer than present” during the Last Interglacial. This statement is correct based on the analysis of McKay et al. (2011), who suggest a peak LIG global mean SST $0.7 \pm 0.6°$C warmer than late Holocene pre-Industrial temperatures, and thus about $0.3 \pm 0.6°$C warmer than today (based on NOAA’s 2011 estimate of a global mean SST anomaly of 0.4°C). However, it is also irrelevant; the authors are discussing long-term equilibrium sea level responses, and McKay et al. do not suggest a high probability that the Last Interglacial was cooler than the late Holocene, the last time sea level might perhaps have been close to long-term equilibrium.

I would also refer the authors to Kopp et al. (2009) for estimates of Last Interglacial sea level, for which Dutton and Lambeck (2012) provide independent confirmation.

CPD
8, C1310–C1316, 2012

Interactive Comment

8 Technical comments

- On page 3554, where reference is made to the terms in the equation of R07 and VR09, please refer down to equation 3.
- Fig. 5 should be dropped or discussed.

References

Kopp, R. E., Simons, F. J., Mitrovica, J. X., Maloof, A. C., Oppenheimer, M., 2009. Probabilistic assessment of sea level during the Last Interglacial. Nature 462, 863–867, doi:10.1038/nature08686.

Interactive comment on Clim. Past Discuss., 8, 3551, 2012.

CPD

8, C1310–C1316, 2012

Interactive Comment