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Climate of the Past Discussions

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 a1 = 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
- 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.

The list appears on the whole correct. It however does not highlight the important comparisons from the incidental ones (e.g., for New Zealand and Tasmania, we showed for interest the effect of increasing or decreasing GIA by 0.15 mm/yr; this is nowhere further used). Often we included extra data sets to pre-empt suspicions that certain behaviour might be due to data-set choice.

Exploring all 150 (!) possible combinations certainly was never our ambition. We created the below Table 1, showing the main comparisons and their nature (like: exploratory, quality-of-fit, out-of-sample test). We also introduce abbreviations that we use also in the figures, for greater clarity:

K11full The full model formulation used in Kemp et al. (2011), including a_1 and b terms

K11 The simplified model formulation, in which $b = 0$

G10* The Kemp et al. model formulation in which $b = 0$ and also $a_1 = 0$, which we show to be formally equivalent to

G10 The model formulation used in Grinsted et al. 2010.

And for sea-level data:

K11 The data used in Kemp et al. (2011), including the parameter estimates from Vermeer and Rahmstorf (2009) from the tide-gauge period used as a prior

NC The North Carolina proxy data only, from 1100 AD onward

NC-new The North Carolinaproxy data from 1700 AD onward

NC-old The North Carolina proxy data before 1700 AD

TNZ Tasmania and New Zealand data from Gehrels et al. (2012), joint data set

J08 The sea-level data from the Jevrejeva et al. (2008) tide-gauge sea-level reconstruction.

We should have moved less central material to Supplementary Information, as it clutters the main text and figures.

Table 1. The numerical experiments depicted in the various Figures. A polynomial fit summary of the NC sea-level data, with one-sigma uncertainty band, is included as background in all graphs even when not used in fit or out-of-sample test

				Fig. 4	
	Fig. 1	**Fig. 2**	**Fig. 3**	**(a)-(d)**	**(e)-(g)**
Test type	Exploratory		Out-of-sample	Fit	
Model	G10	K11full[a]	G10*, K11	G10*	K11
Temperature	Moberg[b]	M08[c]	M08	M08	
Sea level (fit)	J08	K11	NC-new	TNZ	
Sea level (test)	-	-	NC-old	-	

[a](and others for illustration)
[b](and others for illustration)
[c](and others for illustration)

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.,

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

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:

$$\begin{aligned} \frac{dS}{dt} &= \sum_i a_i \left(T\left(t\right) - T_{0,i}\left(t\right)\right), \\ \frac{dT_{0,i}}{dt} &= \frac{1}{\tau_i}\left(T\left(t\right) - T_{0,i}\left(t\right)\right). \end{aligned}$$

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

$$S_{eq}\left(T\right) = a_i \tau_i T + B$$

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.

We appreciate the elegance and power of this approach, which is now on the record here. We decided against including it in our manuscript, as we are using a version of the formalism where $\tau_2 \to \infty$, making $T_{0,2}$ conveniently a constant. This is a formal device introducing its own complications (on which James Annan commented).

R07 discussed extensively the existence of an equilibrium sea level to which true sea level asymptotically approaches – and then proceeded to use a linear approximation for the initial rate of rise, since that (i.e. the 21st century) was what interested him. It is obvious that if that initial rate of rise were continued forever, sea level would rise forever, which would simply be an abuse of an approximation outside its range of validity.

3 Proportionality of relationship between Seq 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

$$\begin{aligned} T(t) &= T(0) + \left(1 - \exp\frac{t}{\tau_T}\right)\Delta T_{eq}, \\ S_{eq} &= A\left(T(0) + \Delta T_{eq}\right) + B = \end{aligned}$$

$$\begin{aligned} &= A\left(T(0) + \left(T(t) - T(0)\right)/\left(1 - \exp\frac{t}{\tau_T}\right)\right) + B = \\ &= A'\left(T(t) - T(0)\right)/\left(1 - \exp\frac{t}{\tau_T}\right) + B' \\ \frac{dS}{dt} &= \frac{1}{\tau}\left(A'\left(T(t) - T(0)\right)/\left(1 - \exp\frac{t}{\tau_T}\right) + B' - S(t)\right), \end{aligned}$$

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

Our concerns would be allayed if it is recognized to be a formal device, valid for a short time. However, G10 explicitly used data constraints from the last interglacial and last glacial maximum, thus treating as factual this proportionality of final equilibria far from the present state to the current rate of rise. We remain concerned that the G10 model with estimated parameter values generates equilibrium sea-level changes that are much smaller than geological reconstructions of sea level indicate was the case.

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.

Our language was too sloppy here. We were referring to the study of Engelhart et al.

(2009) where this assumption is made for the last several thousand years for 19 regions along the U.S. Atlantic coast. We agree that there is some circularity, but it is widely accepted that global ice volume changed little in the last 2000-4000 years and therefore there was no trend in the eustatic contribution to sea-level change. Other factors such as ocean density and circulation changes may have driven variability around a stable mean. This assumption is shared by Earth-Ice models, which also display a systematic misfit to reconstructed sea level (also, e.g., King et al., 2012).

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 their interpretation of this?

In the paper's Figure 1, we show the original hindcast result from G10, and various replications of it. For information, also the polynomial representation of the K11 result is plotted in, and a variant obtained by assuming a different GIA value (dashed line). The proper comparison is, however, with the G10 hindcast in black and grey. We believe it would help to change the positioning of legend items to make clearer that the K11 polynomial really is background information here.

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?

Table 2. Parameter values used for the integration curves in Figs. 1 and 2. These (except τ) were estimated by a least-squares fit to the sea-level target; for those marked with a plus sign (+), only sea-level data post- 1100 AD was used

Line style	Temperature data	Sea-level target	A (m K)	B (m)	τ (yr)	S_0 (m)
Blue	Moberg et al. (2005)	G10	1.290	0.760	208	0.010
Magenta	Mann et al. (2008)	G10+	1.622	0.495	208	0.125
Blue	Moberg et al. (2005)	K11+	11.260	8.772	4000	−1.334
Magenta	Mann et al. (2008)	K11+	12.301	5.692	4000	−1.161

We replaced manual tuning with least-squares fit, the results of which are included Table 2. The results did not change much, but an element of arbitrariness was removed. We only used sea-level data post 1100AD (especially with the Mann et al., 2008 and G10 model) because including the earlier data generated spurious hindcasts, suggesting problems with the early part of the temperature proxy data or the North Carolina sea level record. For the third and fourth rows, we just were consistent with Kemp et al.

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 ± 0.6◦ C warmer than late Holocene pre-Industrial temperatures, and thus about 0.3 ± 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.

We agree that McKay et al. (2011) is an irrelevant reference and its use here incorrect. We don't need it as the point we are making here (that Milankovich-driven behaviour is less suitable for this kind of analysis) is not controversial.

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. Thanks.
- Fig. 5 should be dropped or discussed.

  Indeed discussed, as we believe the issue illustrated in this graph (that the very short τ values produced by G10 model fits tends to lead to low estimates of sea-level rise over multiple centuries) is important.

References

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