**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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I'm a little hesitant to wade into this interesting and hotly contested debate about the relative value and performance of different empirical models, but think the discussion in this manuscript might benefit through clarification of a couple of points.

Firstly, the family of empirical models presented over recent years by Rahmstorf, Vermeer et al (including K11 in this paper) actually abnegates the whole concept of equilibrium sea level through their design. That is, there is no such property of the models. Under any arbitrarily small level of natural variability, the sea level will inevitably follow a random walk, drifting to arbitrarily large positive and negative deviations over time. (This feature of the models may be concealed either if natural variability is ignored, or if the base temperature is selected post-hoc as the empirical mean of the historical

temperature series.) I don't think the authors' discussion of a long-term trend really captures this behaviour very clearly, as the deviation from any fitted trend will also be arbitrarily large. How important this is in practical terms will depend on both the model parameters and the magnitude and spectrum of natural variability, of course.

A secondary point is the authors' criticism of the G10 model as unintuitive, on the grounds that the long-term equilibrium sea level rise is "used to explain" the initial rate of rise, despite the processes being largely unrelated. This seems rather unfair to me. While I accept that the intuition of the (much more experienced) authors may vary, it doesn't seem at all unreasonable or conceptually dubious to me that in such a highly simplified model as this one, both the initial rise, and the final equilibrium in sea level may be linearly related to the magnitude of a sustained step change in temperature - on the contrary, these seem the most obvious first-order approximations to make in modelling the system. This makes it inevitable that, as the temperature change varies and all other factors are held constant, these values are linearly related to each other, but this fact does not depend on there being any relationship in the other relevant physical processes, as the authors imply.

Note that an exactly analogous complaint could equally be made about the simple zero-dimensional energy balance equation which is commonly used to describe the Earth system (and which appears mathematically equivalent to the model of G10):

$$CdT/dt = F - lambda*T$$

In this model, in response to a sustained step change in forcing (F), the initial rate of warming (F/C) and the final temperature change (F/lambda) are both linearly related to the change in forcing, and thus to each other (as F varies, with C and lambda held fixed). However, the heat capacity and radiative feedback parameters are unrelated, and while the model has obvious limitations I would be surprised to see anyone criticise it specifically on the basis that it uses the long term change to explain the initial warming rate.

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