Reviewer #2: Review of "Propagation of Error and the Reliability of Global Air Temperature Projections" by Frank, submitted to J. Climate

This manuscript is an impostor. It pretends to be a research paper, but it is not. A research paper makes a good-faith effort to provide relevant context as defined by prior work and then goes on to show how the prior work is extended (perhaps by showing flaws in it). By contrast, the present manuscript uses scientific-looking reasoning and referencing but blatantly ignores or misrepresents prior work. Furthermore, the manuscript contains fundamental errors in the technical development it claims to present as novel. Hence I cannot but recommend rejection.

Major comments

1. The framing is disingenuous. Following a superfluous and lengthy reminder of the distinction between precision and accuracy, it is first claimed that error propagation is ignored in the discussion of uncertainty of climate projections (line 110 onward). Then the quote from Smith (2002) is used to insinuate that climate modelling ignores elementary (high-school level) codes of good scientific practice concerning uncertainty estimates (line 117 onward). Both statements are so misleading that they are effectively wrong (see major comments 2 and 3).
2. Linear propagation of information and, in principle, error is investigated systematically by the so-called tangent-linear model or its transpose, the adjoint (e.g., Hall et al. 1982; Errico 1997; Marotzke et al. 1999). However, application to a full-blown climate model is conceptually and technically extremely challenging, because turbulent instabilities limit the utility of the linearization (e.g., Lea et al. 2000; Köhl and Willebrand 2002) and because accumulation of systematic error is very hard to trace quantitatively (e.g., Rauser et al. 2011). The premise stated here, that linear error propagation is ignored in climate modelling, is hence plain wrong. What the author would need to show is that he can overcome the formidable limitations identified to date.
3. Any serious look, however brief, into the IPCC report shows how carefully uncertainty is estimated, given the inherent limitations. For example, Figure 12.1 of WGI AR5 Chapter 12 (Collins et al. 2013) lists the ensembles of the state-of-the-art climate models, and the surrounding text goes to great lengths in explaining how uncertainty of the projections is estimated. Ignoring this, as the present manuscript does, is disingenuous.
4. Claiming that the well-known systematic error in cloud radiative effect (CRE) swamps any smaller energy flux ignores the presence of compensating errors (e.g., WGI AR5 Ch9, page 766; Flato et al. 2013).
5. Section 2.1 goes through a tortuous derivation of the decades-old linear relationship between forcing and response (e.g., Gregory et al. 2004) - but arriving at a fundamentally wrong version of this relationship (see below).
6. Equation (8), formulating an energy-balance framework leading to the

relationship between forcing and response, leaves out a crucial term - the damping from changed radiation to space. Furthermore, there is no justification for adding a systematic error as a random "forcing" in a Markov process, because it is then implied that the errors are uncorrelated in time. This latter property, together with the absence of damping, means that equation (8) simulates a simple random walk (e.g., Wunsch 1992), and its standard deviation after 100 years comes out trivially for the current example as 0.42 x 4 x (number of years)^(1/2) K ca. 16 K. Thus, the manuscript with its fatally flawed energy balance merely repeats a decades-old example from random walk.

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