From: **Patrick Frank** pfrank830@earthlink.net
Subject: Re: JOC-14-0623 - Decision on Manuscript
Date: January 21, 2015 at 7:11 PM
To: huth@ufa.cas.cz

PF

Dear Dr. Huth,

Those reviews are again incompetent and again misconceived.

Four of the five reviews have now been incompetent.

I am looking for an editor with the courage to be a scientist, Radan. If not you, who?

Sincerely wondering,

Pat

Patrick Frank
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email: pfrank830@earthlink.net
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These things are, we conjecture, like the truth;
But as for certain truth, no one has known it.

Xenophanes, 570-500 BCE
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On Jan 21, 2015, at 6:59 AM, huth@ufa.cas.cz wrote:

> 21-Jan-2015
>
> Dear Dr Frank
>
> Manuscript # JOC-14-0623 entitled "Propagation of Error and the Reliability of Global Air Temperature Projections" which you submitted to the International Journal of Climatology, has been reviewed. The comments of the referees,all of whom are leading international experts in this field, are included with this letter.
>
> In view of the comments of the referees I have no other option than to reject your manuscript from publication in the International Journal of Climatology. Please take this decision as final.
>
> Thank you for considering the International Journal of Climatology for the publication of your research.
>
> Sincerely,
>
> Dr Radan Huth
> International Journal of Climatology
> huth@ufa.cas.cz
>
> Referee(s)' Comments to Author:
>
> Reviewer: 1
>
> Comments to the Author
> I strongly recommend rejection of this paper, which presents an ill-designed analysis based on invalid assumptions and misconceived ideas about climate and climate models. Sorry. There are many aspects which I think are wrong and I think it's impossible to recover the analysis. For starters, the author thinks that a probability distribution function (pdf) only provides information about precision and it cannot give any information about accuracy. This is wrong, and if this were true, the statisticians could resign. He presents a statement about hindcasts in the paper which makes me think that he does not know what he is talking about. Also, a naive and simple linear framework for emulating global climate models (GCMs) is presented, that looks more like a fit to data. It is argued that it is skillfull, but given the initial fit and frocing data as input, this is hardly a tough test. Furthermore, this emulation framework muddles external forcing with feedbacks, and the treatment of errors assumes that each increment is independent of each other. The author looks at zonal means of cloud biases, and does not realise that the latitudinal structure is due to well-known phenomena and circulation patterns - we should not expect a white (or red) noise type stochastic structure of the residuals, because the cloude climate varies with latitude. There is also varying degrees of freedom, as the space 'converges' in the polar regions due to the geometry of a sphere. Also, the effect of clouds vary with latitude both due to the solar inclination and cooler poles. The best way to test the errors of the GCMs is to run numerical experiments to sample the predicted effects of different parameters, which indeed has been done and presentated in the IPCC reports - eg natural versus total forcings. Any analytical or simplified emulation must reproduce these kind of the results of such experiments - also the error bars. The most obvious indication that the error framework and the emulation framework presented in this manuscript is wrong is that the different GCMs with well-known different cloudiness biases (IPCC) produce quite similar results, albeit a spread in the climate sensitivities.

Reviewer: 2

Comments to the Author
The author concludes that the uncertainty in projections from climate models is at least an order of magnitude larger than seen in standard climate models.

Central to the analysis is the simple model defined in equation 6. This, I believe, is a variant of the step-response model of Good et al. (2011), which is not cited (it is hard to follow the derivation).

Using this model, the forcing error term of magnitude 4 Wm-2 is assumed. This is where the fundamental error of the paper lies. Although there may be a model spread in estimates of the mean radiative balance of the atmosphere, this is irrelevant when estimating the incremental (year-to-year) potential error in forcing. This will be much less.

Based on IPCC AR5 Chapter 12 fig 12.4, the total error in forcing at the end of the 20th century is of the order of +/- 1 Wm-2. Assuming year-to-year errors to be uncorrelated in time, a simple calculation, dividing this number by 100 (years), yields a year-to-year error of +/- 0.01 Wm-2. This would lead to an uncertainty in projected temperature that is much less than is claimed here and much closer to that seen in the CMIP5 models. Even if this calculation is over simplified, it is hard to see how a year-to-year error in radiative forcing could be anything like 4 Wm-2.

In addition to this fundamental flaw, the paper is very poorly written, contains sections that seem irrelevant to the main conclusions and does not adopt the standard approach of working with anomalies. There may well be further fundamental flaws in the irrelevant sections.

Good et al., GRL VOL. 38, L01703, doi:10.1029/2010GL045208, 2011