Patrick Frank 22 September 2014
Propagation of Error and the Reliability of Global Air Temperature Projections
Manuscript # JOC-14-0316

Summary

- The review has unaccountably focused on weather prediction, which is no part of the manuscript analysis (Items 2.1, 2.2, 2.4, 3.1, 3.3).
- The dismissal of novelty is misplaced (Item 2.1.2).
- The review misconstrues the analysis, which concerns the behavior of climate models, not the behavior of climate itself (items 2.3.1, 2.4, 3.2, 4.1).
- The review confuses error growth with growth of uncertainty (items 3.2.4, 3.3.3).
- The review has confused confidence intervals with air temperatures (item 4.2).
- The review reflects the incorrect assumption that climate models produce unique solutions to the problem of the climate energy-state (item 4.3).
- Overall, the review has ignored the central finding that GCM air temperature projections are just linear extrapolations of tropospheric forcing, and that the linear propagation of error directly follows from this result.

Detailed response to Reviewer 3

Reviewer comments are presented in full, indented, numbered, and in italics. Author responses follow.

The reviewer is thanked for the thoughtful commentary, though regrettably misconstrued.

> 1. *If this paper is correct, it is a profoundly important paper. If this paper is flawed then to publish it would be profoundly misleading. The question is, which is it.*

1. The reviewer is thanked for agreeing that the paper is profound, whether importantly or misleadingly so to be determined.

> 2.1.1. *This paper shows that models cannot predict the weather years ahead of time, due to the accumulation of errors.*

2.1.1. It is unclear how the reviewer came to this view. Nowhere is the manuscript concerned with weather prediction.

From beginning to end, the manuscript analysis concerns simulated global average climate. Not weather. The term "weather" does not occur once in the manuscript.

> *2.1.2. This is already known, so the result is not novel.*

2.1.2.1. Novelty resides in showing that climate model air temperature projections are linear extrapolations of tropospheric forcing.

2.1.2.2. Likewise, novelty resides in showing that error propagates linearly through such projections.

2.1.2.3. Likewise novel are the physically based confidence intervals calculated for air temperature projections.

> 2.2. *We have known this formally since Lorenz. Unfortunately the author misunderstands this result and claims that the models cannot predict climate.*

2.2. As there is no analysis whatever of weather prediction in the manuscript, the reviewer's reference to the work of Lorenz as regards weather prediction has no significance.

Likewise, the reviewer's diagnosis of author misunderstanding is misplaced, resting as it does on the reviewer's mistaken ascription of weather prediction.

The reliability of model predictions of global average air temperature is the point at issue. Unfortunately the reviewer's opening statement has no connection to the central subject of the analysis.

> 2.3.1. *The equation that he has applied, eq. (2), does not however apply to climate ...*

2.3.1. The analysis demonstrates that climate model global air temperature projections are strictly linear extrapolations of tropospheric forcing. Equation 2 is completely general for propagating error through a linear summation. It is not clear how comment 2.3.1 can possibly be correct.

> 2.3.2. *... which is the average state of the atmosphere not the instantaneous state.*

2.3.2. Climate model global air temperature projections represent the transient response to instantaneous changes GHG forcing. This understanding is not controversial, e.g., [Lacis, AA *et al.*, 2010] and [IPCC, 2013b] WG1 Chapter 9. The manuscript assesses this transient air temperature response.

The equation for propagated error remains the same because the final air temperature state follows a linear summation.

> *2.4. It might conceivably apply to weather – and he proves weather is not predictable on longer timescales but it is a mis-application of Equation 2 to apply it to climate.*

2.4. The reviewer statement certainly does not follow from anything appearing in the manuscript. The manuscript does not concern weather, or weather prediction, or weather models. Nor does the manuscript concern the physical climate.

The manuscript concerns the observed behavior of climate models, and the reliability of their global average air temperature simulations.

Thus far, the reviewer has not addressed any topic that is actually present within the manuscript.

3.1. *I think it is actually very arguable whether Equation (2) does apply to the weather state.*

3.1 Eqn. 2 is never applied to a weather state within the manuscript. Nor is it ever suggested that eqn. 2 should be applied to a weather state. The reviewer's comment is completely extraneous to the manuscript analysis.

3.2. The equation assumes that successive errors are independent and additive, yielding a random walk. This is not how dynamical systems behave, because in the short term errors grow exponentially (in effect, are multiplicative) and must be characterised not by a linear growth parameter as assumed in this manuscript, but by an exponent (the Lyapunov exponent).

3.2.1. The behavior of dynamical systems is of no concern to the manuscript analysis. The observed behavior of climate models is the concern.

3.2.2. It is demonstrated, not assumed, that climate models project global average air temperature as a linear extrapolation of tropospheric forcing. This finding is central to the analysis, and fully demonstrated in manuscript Figure 2 and Auxiliary Material (AM) Figure S1, Figures S3-S8, and Figure S11.

The fact of linear behavior alone fully justifies application of eqn. 2 to the growth of projection uncertainty.

3.2.3. The error correlation matrix in manuscript Table 1 shows that model total cloud fraction (TCF) error comes from theory-bias. Therefore, TCF error will necessarily ramify through every calculational step of a climate simulation.

The ±4 Wm^{-2} of [Lauer, A and Hamilton, K, 2013] is a multi-year, multi-model annual average TCF error. [Lauer, A and Hamilton, K, 2013] demonstrate that TCF error persists across the sequential calculations in a climate simulation. As an annual multi-model average, TCF error can be applied to estimate the reliability of any CMIP5 model simulation.

A multi-model, multi-year average TCF error is independently applicable to every calculation in a step-wise annual series, and projection uncertainty will therefore grow additively when the calculation is linear; which it is.

3.2.4. Finally, the reviewer is confusing error growth with growth of uncertainty.

The growth of error concerns the actual divergence of the expectation value from the true value. The growth of uncertainty concerns the diminishing state of human knowledge -- an ignorance width. These are very much not the same thing.

For example [Taylor, BN and Kuyatt., CE, 1994], subsection 2.3, note that, "*The difference between error and uncertainty should always be borne in mind. For example, the result of a measurement after correction (see subsection 5.2) can unknowably be very close to the unknown value of the measurand, and thus have negligible error, even though it may have a large uncertainty.*"

In a futures projection, the growth of error is unknown because there are no comparative observables. However, the growth of uncertainty is indeed known -- by propagation of known errors, e.g., ±4 Wm^{-2} TCF error, through the projection.

The distinction between error and uncertainty is discussed extensively in manuscript section 2.4.3, and even more thoroughly in AM Section 10.1.

> 3.3.1. *Thus the situation is even worse than the author believes for weather,*

3.3.1. The manuscript does not concern weather, and the author has stated no beliefs about weather.

> *3.3.2. and instead of months of predictability, as implied by Fig. 5 with linear error growth, there are only a couple of weeks of actual predictability.*

3.3.2. Figure 5 concerns centennial global climate, not weather. This fact can be ascertained by direct inspection of Figure 5. The SRES scenarios traverse 100 years, not a few months.

SRES projections concern global climate, not weather. The IPCC defines the SRES scenarios as "*Projections in Future Changes in Climate*," not changes in weather; cf., [IPCC, 2007] Summary for Policy Makers, p. 12.

> 3.3.3. *Over the long term, the dissipative character of dynamical systems (including the climate system) causes the errors to be bounded by the dimensions of the attractor.*

3.3.3.1. The manuscript is not concerned with dynamical physical systems, or with the physics of climate. It is concerned with the observed behavior of climate models. This distinction is explicitly stated in the Introduction, on original page 8, lines 27ff.

This focus has now been elevated to paragraph 2 in the Introduction of the revised manuscript, so that the import is not lost on the reader.

3.3.3.2. As noted in 3.2.4, the analysis evaluates the growth of uncertainty, not the growth of error. Uncertainty magnitude can grow beyond the bounds of the attractor. When the magnitude of uncertainty exceeds the bounds of an attractor, the simulation no longer transmits any physical meaning.

> 3.3.4. *The author is strongly urged to consult a textbook on dynamical systems theory since the application of (2) is a very basic error. If he does not want to read dynamical text books by climate scientist, read some by applied mathematicians.*

3.3.4. The author appreciates the kindness this advice, but again notes that the uncertainty analysis concerns the behavior of climate models, not the behavior of dynamical systems.

Climate models are demonstrated to linearly extrapolate tropospheric forcing. Application of eqn. 2 is therefore entirely justified.

> *4.1. The author's calculation is completely inapplicable to climate modeling.*

4.1 As noted already several times, and as stated in the Introduction, the analysis concerns the behavior of climate models, not modeling of the physical climate.

Equation 2 is fully justified by the demonstrated linear behavior of climate models. In the original manuscript, this justification is stated in the Abstract, line 22; Introduction p. 8, line 1; Section 2.2 p. 22, line 41; section 2.4.2, p.32, lines 46ff; Section 3, p. 44, lines 43ff.

The reviewer has not addressed this argument at all. The review comments are confined to physical dynamical systems, and have not considered the structure of uncertainty consequent to the fact that climate models exhibit linear behavior.

> *4.2. Indeed if we carry such error propagation out for millennia we find that the uncertainty will eventually be larger than the absolute temperature of the Earth, a clear absurdity.*

4.2. The reviewer is here confusing a confidence interval (CI) with a model expectation value. It is astonishing how often this mistake appears in the comments of climate modelers. Please see comment 3.3.3.2. regarding the relation between a CI and the bound of a physical model.

If a projection CI extends beyond the absolute temperature of Earth, the straight-forward interpretation within physics is that the simulation is physically meaningless.

The reviewer may consult AM section 10 to relieve this misapprehension.

> *4.3. In reality climate models have been tested on multicentennial time scales against paleoclimate data (see the most recent PMIP intercomparisons) and do*

reasonably well at simulating small Holocene climate variations, and even glacial-interglacial transitions. This is completely incompatible with the claimed results.

4.3. Reviewer 1 made a similar declaration. Therefore a similar treatment is given here.

The reviewer is offering the PMIP results as though climate models produce a unique solution to the problem of the climate energy state. Only unique solutions have determinative authority. Figure R3-1 below shows that any given simulated air temperature, or any simulated climate observable, does not represent a unique climate energy state. Rather, multiple simulated paleo-climate states can produce the same paleo air temperature.

A "correct" paleo-temperature therefore does not indicate that the energy-state of the paleo-climate is correctly simulated. The air-temperatures of non-unique climate states have no discretionary physical meaning, and do not permit exclusionary decisions.

This conclusion is demonstrated in Figure R3-1 below, taken from [Rowlands, DJ *et al.*, 2012], which displays thousands of perturbed physics simulations of centennial air temperatures made using the UK Met HadCM3L. "Perturbed physics" means that model parameters are varied across their range of physical uncertainty. Each member of an ensemble of modeled states in a perturbed physics experiment is of equivalent weight. None of them are known to be more correct than any of the others.

In Figure R3-1, the two black horizontal lines at $\Delta T = 1$ K & 3 K have been added. These lines traverse the ensemble, showing that hundreds to thousands of simulated climate states that differ in total energy, can nevertheless produce the same air temperature anomaly.

Alternatively, after a simulation of 100 years, the identical energy-state of the 2080 climate has simulated anomaly expectation values spread across 6 K.

These results show that climate models do not produce unique solutions to the climate energy state. The same conclusion follows from the large spread of simulated observables in any ensemble of projected climate, e.g., Figure AI 1 in Annex I the IPCC 5AR [IPCC, 2013a].

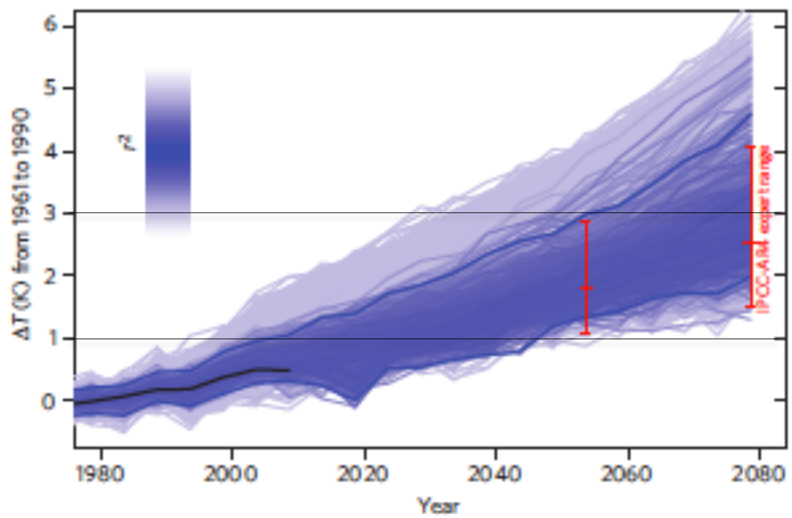

Figure R3-1. Original Legend: “**Evolution of uncertainties in reconstructed global-mean temperature projections under SRES A1B in the HadCM3L ensemble.**” The horizontal black lines at 1 K and 3 K were added by the author.

Figure R3-1 shows that simulated paleo-temperatures can not represent a unique solution to the physical state of the paleo-climate.

Hundreds or thousands of simulated climate states may produce the same air temperature anomaly. It cannot be known whether any one of the underlying climate states producing those air temperatures represent or even approach the physically correct paleo-climate.

Any correspondence in observables therefore does not verify the physical validity of climate models.

The thick black line within the simulation envelope of Figure R3-1 represents observations. Even those air temperature simulations near the observation line cannot be known as ‘more correct’ because the climate states they represent are not unique solutions to the underlying energy state. All the projected states, including those near the observed state, will be associated with large uncertainties; uncertainties that make no appearance in the original figure.

The problem of non-unique solutions was tacitly admitted by [Tebaldi, C and Knutti, R, 2007] who noted that, “*Once scientists are satisfied with a model, they rarely go back and see whether there might be another set of model parameters that gives a similar fit to observations but shows a different projection for the future.*” The meaning of this statement is that the same observables can be produced by multiple different projected or hindcasted climate states.

Tebaldi and Knutti's statement also brings up the problem of anti-correlated parameter errors, discussed in Section 3, p. 50, line 22 ff.

With anti-correlated parameter errors, climate observables can be reproduced in a simulated climate state with completely erroneous underlying physics.

The reviewer has failed to recognize this problem of climate modeling, and its very serious impact on the uncertainty in simulated climate observables.

Because of this problem, the "*reasonably [good simulations of] small Holocene climate variations, and even glacial-interglacial transitions*" are no more than exercises in false precision. The confidence intervals resulting from a proper propagation of error through a paleo-climate projection would undoubtedly be very wide, and would remove any fancy that visual correspondence represented physical fidelity.

> *5. In summary, the claims made in this paper are profoundly important if they are right. Unfortunately, they are wrong at several important levels and the paper has to be outright rejected.*

5. The reviewer has regrettably ignored the thorough demonstration that climate models linearly extrapolate forcing.

The review has inappropriately criticized the analysis in terms of weather prediction and the behavior of dynamic physical systems. Neither topic makes any appearance in the manuscript. Further, as demonstrated above, neither topic has any bearing on the uncertainty analysis presented in the manuscript.

The reviewer has confused error growth with the growth of uncertainty, and has confused physical error with an uncertainty statistic. The reviewer, also mistakenly, does not distinguish between a confidence interval and a model expectation value.

The reviewer also apparently does not realize that climate model predictions are non-unique. Non-unique but "correct" observables, which are typical of models deploying tuned anti-correlated parameters [Kiehl, JT, 2007], do not constitute any proof of model validity. Large parameter uncertainties necessarily produce large CIs in any model projection.

These difficulties remove the scientific force from the review and completely vitiate the reviewer's final conclusion.

References:

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