Patrick Frank 28 February 2016
Propagation of Error and the Reliability of Global Air Temperature Projections
Manuscript # JCLI-D-15-0797

Author Response to Reviewer 1.

Summary

This reviewer:

- ignored the demonstration of linear GCM behavior;
- never confronted the core of the analysis, that linear extrapolation of forcing directly entails linear propagation of error;
- apparently never understood that the manuscript is focused on climate models, not on climate (items 1.1, 1.3, 2.5, 5.3-5.5, 5.7-5.9, and 6.10);
- apparently does not understand the distinction between emulating model behavior vs. simulating climate (items 1, 2.6, 5.3-5.5, and 5.7-5.9)
- everywhere confused the ±K uncertainty with a physical temperature (items 1.3, 2.4, 4.5, 5.3-5.5, 5.7, and 6.3);
- showed no understanding of the meaning of uncertainty (items 1.2, 2.2, 2.4, 2.6, and 5.2-5.10)
- misconceived a ±K uncertainty to imply model oscillation between climate extremes (items 2.7, 4.5, and 5.3);
- supposed that both a forcing perturbation and a physical temperature can shift in two opposed directions simultaneously (items 4.5, 5.5, and 5.10.);
- shows such unfamiliarity with content as to indicate careless or scanty study and to have never consulted any relevant Supplementary Material (items 1.1, 1.2, 2.2, 2.6, 5.1., 5.9.1, 6.1.1, 6.5.2, 6.6.2, 6.8, and 7).

Detailed Response:

The reviewer is quoted in full in italics, followed by the author response. Review paragraphs, and sometimes sentences, are divided to attend individual points.

1.1 *This paper uses a simple linear model for estimating global mean temperature changes from forcing, ...*

> 1.1 The reviewer is mistaken. The linear model does **not** estimate the, "*global mean temperature changes from forcing*." It emulates climate model behavior.
>
> This distinction was prominently made. For example:
>
> Manuscript line 130: "The fractional contribution for the water-vapor enhanced (*wve*) greenhouse effect of CO_2 is thereby assessed, *as applicable to GCMs*." Note "*as applicable to GCMs*" does not mean 'as applicable to climate.'
>
> Line 136: "*a simple model is developed that captures the functional form of GCM GASAT projections*." "*The functional form of GCM projections*" does not mean 'the

physical form of the terrestrial climate.'

Line 154-157: "*To be kept in view throughout what follows is that the emulation model is not a climate model. The physics of climate is neither surveyed nor addressed; nor is the modeling of climate. Rather, the focus is on the behavior and reliability of climate models themselves.*"

The focus on the behavior of GCMs could not be clearer. But the reviewer failed to grasp the idea.

1.2. *... and a stepwise model for error growth to estimate the uncertainty in projections of warming in the future by climate models.*

1.2. The reviewer is incorrect. The analysis projects growth of uncertainty, not growth of error. The distinction is critical and again appears prominently in the manuscript. For example:

Manuscript line 672: "*Thus ±4 Wm^{-2} TCF error is a lower limit, and the derived confidence intervals are a minimum of uncertainty in projected surface air temperature.*"

Lines 675-685 specifically distinguish uncertainty from prediction. Line 683 explicitly recommends consulting Supplemental Material Section 10.2 *The meaning of predictive uncertainty*.

The distinction is further discussed in manuscript lines 692-714.

However the reviewer evidently overlooked all of this.

1.3. *The main conclusion is that the CMIP5 models all have an absolute uncertainty of ±15K for how much it will warm by the end of this century.*

1.3. The reviewer is not correct. In writing, "*how much it will warm*," the reviewer implies that the ±15 K is a range in predicted temperature. It is not. It is the uncertainty surrounding the predicted temperature. It is an ignorance width.

Uncertainty in temperature is not the predicted range of temperature. The reviewer has obviously confused these meanings.

The reviewer's comment indicates no cognizance of the meaning of uncertainty or of the distinction of uncertainty from model output.

The large uncertainty clearly means that the model output has no predictive significance.

Uncertainty is introduced very early in a college Physics (or Chemistry) major. In confusing uncertainty and output, the reviewer has made a freshman-level mistake.

2. *I recommend rejection on two grounds: 1) the methodology is not plausible and 2) the estimates of uncertainty are absurdly large, larger than any reasonable estimate of the projected warming, and not consistent with the tested variations in the projections or the physics of climate.*

2.1. Methodology: linear extrapolation of forcing (demonstrated) directly entails linear propagation of error.

The methodology is obviously correct (Bevington and Robinson 2003). The reviewer has conspicuously failed to grasp it.

2.2. With "*absurdly large,*" the reviewer has again confused temperature with uncertainty.

This persistent confusion shows that the reviewer is apparently unable to distinguish between a model output and its uncertainty.

Also: the reviewer either did not read or did not understand manuscript lines 675-685, or 692-714, or Supplemental Material Section 10.2 *The meaning of predictive uncertainty*.

These reviewer mistakes persist throughout (see below) and invalidate the review.

2.3. The manuscript uncertainty is indeed inconsistent with prior "*tested variations*" because no prior tests have ever propagated error. That inconsistency recommends the manuscript.

2.4. The reviewer's "*larger than any reasonable estimate of the projected warming,*" again shows that the reviewer is apparently unable to distinguish between uncertainty and output. Alternatively, the reviewer confused thermodynamics with statistics.

2.5. The "*physics of climate*" reference again shows that the reviewer has not understood that the manuscript is about climate models, and not about climate physics. This is a fundamental failure.

2.6. The "*physics of climate*" reference again shows that the reviewer has confused propagated uncertainty with model expectation values.

This confusion indicates the reviewer does not understand the meaning of propagated error, and indeed appears unqualified to review the manuscript.

The meaning of propagated error and uncertainty covered extensively in Supplemental Material Sections 10.1, 10.2, and 10.3. However, the reviewer has failed to consult them, despite a transparently worded Table of Contents.

2.7. Evident in item, 4.5 and 5.3 below, the "*physics of climate*" comment indicates that reviewer thinks the ± uncertainty means the model itself is oscillating between extremes. This is again a freshman-level mistake.

3. *The paper sets up a standard of absolute accuracy, which is not necessary for useful climate projections and is very hard to demonstrate, even for highly calibrated measurements.*

3.1.1. Presumably by "*standard of absolute accuracy*," the reviewer means an uncertainty of the model projections themselves, rather than of their anomalies. Projection uncertainties are standard in the physical sciences.

3.1.2. One wonders about the reviewer's view of "*useful climate projections*," given the reviewer must know that model errors are much larger than GHG forcings.

One is led to suspect the reviewer thinks that differencing from a climate base state removes projection error. Supplemental Material Section 7 shows there is no demonstrated evidence that is true.

3.1.3. The accuracy-relevant uncertainty standard is not "*very hard to demonstrate*." The manuscript shows that it is readily calculated for model temperature projections.

3.1.4. This is all quite apart from the manuscript uncertainties that demonstrate a complete lack of predictive utility.

4.1. *Overall the paper contains a lot of extraneous material, but on the key methodology for assessing uncertainty it remains a little vague.*

4.1 The reviewer is entirely unspecific, making the locus of purported vagary impossible to diagnose.

However, manuscript eqn. 1 and eqn. 2 and the associated text show exactly how uncertainty is assessed.

Section 2.4.1 shows how the annual average GCM long wave cloud forcing (LWCF) error enters into emulation eqn. 6.

Section 2.4.2. shows exactly how this error is propagated.

How is the above vague? Perhaps the reviewer finds propagation of error to be vague; a method standard everywhere in the physical sciences except climatology.

4.2. *Some things like the long discussion of radiative forcing are not novel or up-to-date, or needed for the analysis.*

4.2.1. Concerning novelty, the reviewer's complaint was investigated by examining (Aherns 2009; Andrews 2010; Bohren and Clothiaux 2006; Houghton 1985; Jacobson 2005; Nazaroff and Alvarez-Cohen 2001; Seinfeld and Pandis 2006; Taylor 2005; Wallace and Hobbs 2006).

These are all basic climatological texts and papers. Were the reviewer correct, these sources should presage the manuscript discussion of radiative forcing. While the sources cover radiative physics at several levels, none addresses the radiative onset of CO_2 forcing.

Wallace and Hobbs mention a *1/e* optical depth (p. 135), but do not identify it as the radiative mean free path.

Chapter 2 in Bohren and Clothiaux includes a very nice discussion of the *1/e* attenuation length (p. 53), where it is called the *e*-folding length but not identified as the mean free path. Their analysis does not discuss the radiative onset of CO_2 forcing.

The remaining examined sources did not mention a radiative mean free path at all or a *1/e* attenuation length. No text mentioned or discussed the atmospheric concentration of CO_2 necessary to initiate climatologically non-negligible forcing.

The above search shows manuscript is novel in climatology in its assessment of the onset of CO_2 radiative forcing, and the concept of photonic mean-free-path.

The reviewer has provided no evidence to the contrary.

4.2.2. Once again, the reviewer is critically unspecific. Nevertheless, no published estimate was found for the $[CO_2]_{atm}$ necessary to begin significant green-house forcing nor any climatological discussion of photonic mean free path.

4.1.3. The reviewer's dismissal is incorrect. Assessment of radiative forcing and its onset are critically necessary to the estimate of f_{CO_2}.

4.3. *Quite a bit of the text is not central to what is done.*

4.3. Unspecific again; diagnosis is impossible. What parts are not central?

4.4. *The method for assessing uncertainty is quite simple and not up to the state-of-the-art in studies of this nature.*

4.4.1. The "*state-of-the-art*" to which the reviewer refers concerns model precision, only. The manuscript analysis is about model accuracy.

This is a critically central distinction, and one made repeatedly in the manuscript. Nevertheless it escaped the reviewer.

4.4.2. There are no other "*studies of this nature*." None exist. Never before has error been propagated through projections of global air temperature. That singularity alone makes the present analysis state-of-the-art.

4.5. *Moreover it is not constrained by physics.*

4.5. Once again, the reviewer confuses an error statistic and a thermodynamic quantity. The ± uncertainty statistic is not a temperature.

The reviewer may wish to contemplate, indeed, how a temperature change can be simultaneously positive and negative (see 5.3, below). The reviewer here again makes a freshman-level mistake.

5.1. *The essence of the logic is contained in equations (2) and (6) and then (8), which includes an assumption that the error of the global mean energy balance is ±4Wm^{-2} in any year.*

5.1. The reviewer is mistaken. The ±4 Wm^{-2} annual average LWCF error is not an assumption. It is CMIP5 model hindcast physical error (Lauer and Hamilton 2013). This fact is prominently mentioned in manuscript lines 392, 469, and 474.

Section 2.3 is titled: "*CMIP5 model error in global average annual total cloud fraction (TCF)*." How was this missed?

To overlook the obvious is evidence of carelessness.

5.2. *This error is then accumulated year-by-year to produce the error estimates of ±20K in Fig. 8.*

5.2. The reviewer is mistaken. Uncertainty is accumulated; not error. This distinction is explained in detail in the manuscript, but fully lost on the reviewer.

5.3. *These equations are not physical. The results are not plausible in the sense that they predict an uncertainty in warming that encompasses violations of the second law of thermodynamics, such as the Earth cooling by 15K as a result of greenhouse gas increases, or warming by more than 15K.*

5.3. The reviewer here supposes that ±15 K uncertainty implies the model simulation is oscillating between hot house and ice-house temperature.

Note what the reviewer is suggesting. Every simulation step has a ±K uncertainty associated with it. The reviewer suggests these ±K are predicted physical temperatures. Thus for this reviewer, a modeled global climate can change positively and negatively in temperature, simultaneously.

Paraphrasing from SM Section 10, a simulated global climate clearly cannot be simultaneously colder and hotter – a physical incoherence that alone should have immediately alerted the reviewer that review criticism 5.3 is nonsense.

Having been forewarned by identical ventures in prior reviews, the reviewer's impressively naïve mistake is explicitly discussed manuscript lines 675-685. However, yet once again the reviewer overlooked a critical point and proceeded on to disaster.

Strangely, almost every single one of the climate modeler reviewers of this manuscript have made this same ludicrous error; to the point that the entirety of SM Section 10 of the Supplementary Material was dedicated to it.

But the reviewer clearly bypassed the SM.

5.4. *These results are not consistent with the physics in the models being evaluated or with the actual spread of projections produced by the models.*

5.4. The reviewer is wrong in both instances. First, the ±15 K centennial uncertainty is indeed consistent with the physics in the models, because that physics is found to produce an annual average ±4 Wm^{-2} error in simulated long wave cloud forcing when compared with the terrestrial climate.

Second, the ±15 K centennial uncertainty is indeed consistent with "*the actual spread of projections*" because the actual spread of projections only reflects GCM precision. The ±15 K centennial uncertainty reflects GCM accuracy (reliability).

5.5. *I don't believe it is physically possible for the models to produce climates that would be as different from current climate as the errors indicated by method are large.*

5.5. The reviewer is again mistaken. The ±15 K uncertainty statistic is not a temperature. It does not indicate a climate state. It is not, and cannot be, a temperature that is simultaneously positive and negative.

To reiterate: the ±15 K is not temperature. It instead indicates that no confidence can be put in the projection. The ±15 K does not in any way, shape, or form imply that the future temperature can be 15 K hotter, or 15 K colder, or 15 K hotter and colder simultaneously.

5.6. *The fault lies with the methodology used to estimate the error, not with the models or* *physics, in my opinion.*

5.6. Rather, the fault lies with the reviewer's evidently complete ignorance of error propagation, of the meaning of physical uncertainty, and of the distinction between accuracy and precision.

An understanding of all of these is necessary to critically approach the manuscript analysis. However, the reviewer has demonstrated none of them.

5.7. *To sustain a temperature difference of 15K would require a feedback process that would balance about ±30Wm^{-2} of enhanced longwave flux at the approximate rate of 2.0 Wm^{-2}K^{-1}. CO_2 forcing plus a doubling by water vapor feedback will produce only about 7 Watts, so the analysis assumes a cloud feedback big enough to increase this by a factor of 4 to get the high number, or a negative feedback so large it would cause a cooling from +3.5 Watts of CO_2 forcing. You don't expect to throw a blanket over an orange and expect it to freeze. Just does not pass the second law test. Not possible.*

5.7. There is no "*temperature difference of 15K*" in the manuscript (*cf.* items 4.5, 5.3, and 5.5).

The paragraph above, in discussing feedbacks and flux balances, etc., yet again evidences the mistaken supposition that the manuscript concerns climate physics. It does not. It mistakenly proposes, yet again, that ±15 K is a temperature. It is not.

The manuscript concerns the functional form of climate model temperature projections. It shows that these projections are linear in fractional forcing. Linear propagation of error directly follows. That is all; nothing more. Climate physics does not enter into this demonstration.

The reviewer here again demonstrates an utter lack of understanding of the analysis under review.

5.8. *The author needs to explain what mechanism currently in the climate models could produce such enormous feedbacks, since they do not exist in models or observations at present.*

5.8. The author need explain nothing of the kind, because the manuscript nowhere proposes that climate models produce enormous feedbacks. The reviewer continues misappropriating statistics to thermodynamics.

Reviewer item 5.8 displays no understanding of the analysis being reviewed.

5.9. *The method used to estimate error does not incorporate the model physics, but rather uses a simple linear growth model that allows the error to grow unhampered by physics. Climate models actually incorporate the laws of physics that would prevent such large excursions from happening.*

5.9.1. (Lauer and Hamilton 2013) estimated the error. The manuscript estimates uncertainty, which is something else entirely. This distinction clearly remained opaque to the reviewer.

Propagated statistical uncertainty can grow without bound. It is not bound by laws of physics. An uncertainty larger than the physical bounds means the model expectation value has lost all predictive meaning.

This was explained in manuscript lines 709-714 and again in 743-761, but the reviewer again missed this critical point.

5.9.2. No "*large excursions*" are described in the manuscript. The reviewer again mistakes a statistic for a temperature.

5.10. *The model of error used in the paper is simply too fanciful to be taken seriously.*

5.10.1 Fanciful, rather, is the reviewer's idea that global temperatures can move in two directions simultaneously. Or the reviewer's idea that ±4 Wm^{-2} is a forcing, that apparently must impact climate with synchronously opposed perturbations.

5.10.2. The reviewer supposes also that, e.g., (Bevington and Robinson 2003) are fantasists in advising propagation of error for the physical sciences.

6.1 *The author makes a crude estimate of the uncertainty of model radiation balance by estimating the error in longwave cloud forcing.*

6.1.1. (Lauer and Hamilton 2013) estimated the error in long wave cloud forcing, not the author; see also (Jiang et al. 2012). See manuscript lines 392, 464, and 474.

The reviewer has thus far not missed an opportunity to misunderstand.

6.1.2. The uncertainty width is more a lower limit estimate than crude. The reviewer's choice of language is noted.

6.2. *The annual mean error is much less than the estimate because of compensation between variance in shortwave and longwave cloud forcing and other causes.*

6.2.1. Compensating errors do not reduce projection uncertainty because the underlying physics is not known to be correct (*cf.* SM Section 7.3).

6.2.2. The estimate of (Lauer and Hamilton 2013) included both longwave and shortwave errors within 520 CMIP5 model hindcast simulation years. The average net LWCF error arose from the incorrectly simulated cloud cover.

That is, the compensating errors described by the reviewer were in operation during those 520 model years. The final ±4 Wm^{-2} LWCF error was emergent and persistent despite all error compensations.

6.3. *Thus this estimate of model error in any year is probably much less than this or the year-to-year variance in global mean surface temperature would be larger.*

6.3. The reviewer's reasoning is incorrect. Uncertainty does not impact the range or magnitude of expectation values.

6.4. *Also it is likely that this error is random, more than systematic, otherwise the models could not do a good job of simulating the current global mean temperature.*

6.4.1. Table 1 demonstrates LWCF error is systematic.

6.4.2. Models do a "*good job*" hindcasting air temperature because of model tuning and tendentious choices of parameter values (Bender 2008; Kiehl 2007; Stocker 2004; Tebaldi and Knutti 2007).

This hardly encourages confidence that a "*good job*" hindcast indicates a physically accurate description of the underlying climate state.

6.5. *Absolute Accuracy is not a reasonable goal at present, precision in anomaly projections is a useful goal:*

6.5.1. The manuscript demonstrates that accuracy is both a reasonable goal and critical to appraising the reliability of temperature projections.

6.5.2. Model precision reveals nothing of predictive accuracy, lines 56-81, 117-125, 560-568.

6.6. *Currently global climate models are tuned to simulate as well as possible the current climate, given the laws of physics that are incorporated in these models. Therefore they are not accurate in an absolute sense. I don't think anyone has claimed that they are accurate in an absolute sense, but rather are the best guide to how the climate will respond to greenhouse gas increases, albeit still uncertain to about a factor of 2, according to the latest assessments. Since climate models are tuned in different ways, it is argued that the diversity in their projections of change is also a guide to the uncertainty in these projections. The last point is an expression of confidence in the basic physics of the models. This confidence is based on a great deal of effort to test models against data, including real-world situations such as the response of the climate to the Pinatubo eruption, as well as natural cycles of weather and climate.*

6.6.1. The above paragraph is left in primarily for completeness. However, it only states the reviewer's view of standard practice and thus has no critical import.

6.6.2. Tuned models are not predictive, Supplementary Material Section 7.3.

6.6.3. Diversity of model projections is no guide to uncertainty in accuracy. Model ensemble means are not known to be more certain than any given single model run. The fact that ensemble means have greater statistical merit than individual runs, relative to observations, does not mean they are more accurate, because model means express physics that is identical to that of the individual model.

Low uncertainty resides in the physical accuracy of the theory -- the description of the physical state -- not in the statistical merit of the hindcast. Were the latter true, then any cubic spline fit to observables would be a good climate model.

No matter that a hindcast mean is statistically closer to an observable than individual runs, the uncertainty in the mean is the r.m.s. uncertainty of the ensemble members. This makes the uncertainty in the mean inevitably larger than the uncertainty in individual projections.

This is all discussed in manuscript section 2.4.4. Another missed understanding.

6.6.3. AM Figure S11 shows emulation en. 6 also does a "*good job*" with volcanic aerosols.

6.7. *Models can have fairly large biases in cloud properties, relative to both observations and to other models, and still give fairly consistent projections of climate change, within the range of uncertainty provided in consensus assessments.*

6.7. Inter-model consistency arises only because all the models are given parameter sets that reproduce the same observables.

6.8. *The current paper takes those biases and variances and aggregates them into uncertainties in projections of future climate as if it was an initial value problem, but in fact it is more of a boundary value problem, ...*

6.8. Manuscript Introduction lines 104-108 point out that every i^{th} simulation step provides the initial conditions of each subsequent $i+1$ step.

When the model suffers from theory-bias, the initial conditions provided by step i are physically incorrect. Therefore, every single $i+1$ step suffers from an initial value problem. That problem is never removed by model equilibration or spin-up, because it derives from theory-bias. I.e., the model itself injects new and further systematic error into every single simulation step.

This very point is extensively discussed in Section 2.4.2., lines 508ff, Section 2.4.3,lines 598-613, Section 3, lines 692-714, as well as Supplemental Material Section 7, "*Differencing and Systematic Theory-bias Model Error*."

However, the reviewer apparently missed all of it and the review certainly addresses none of it.

Given the above, propagation of error is the only route to a proper estimation of predictive reliability.

6.9. ... *since the response of individual models is strongly constrained by the energy budget at each time step.*

6.9. Statistical uncertainty -- the reliability estimate -- is not constrained by the energy budget.

6.10. *The simple model used does not satisfy a reasonable energy balance, and gives results that are a far outlier from other assessments that satisfy conservation equations.*

6.10. The reviewer's objection is misconceived. The simple model is meant only to emulate the output of GCMs. It does so successfully.

The simple model has no connection with the terrestrial climate (or thus with energy balance).

The simple model conclusively demonstrates that GCMs project temperature as a linear extrapolation of fractional forcing change. Linear propagation of error follows directly.

7. *Abstract: The abstract claims to evaluate CMIP5 simulations, but the only things used from the models are their projections for the global mean temperature history and the putative error in their annual mean budgets of* $\pm 4 Wm^{-2}$*, which I question.*

7. The reviewer is urged to resolve the question by consulting the cited literature, (Jiang et al. 2012; Lauer and Hamilton 2013).

Manuscript lines 604-613 and 687-690 note that the LWCF error, and other energetic errors, means that climate models do not partition energy correctly within the simulated climate-state.

This error in the physical climate state has a strong impact on the validity of CMIP5 climate simulations.

Following from the above, this reviewer has made such persistent and such fundamental mistakes that the review lacks any substantive or scientific merit.

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