Patrick Frank 4 June 2016
Propagation of Error and the Reliability of Global Air Temperature Projections
Manuscript # 3852317

Author Response to Reviewer 2.

Summary

This reviewer:

- never addressed the central point that linear extrapolation of forcing is subject to linear propagation of error
- misperceived the manuscript to concern climate, rather than climate models (items 1.1, 1.2, 7.2 and 7.3.3). This is a fatal mistake.
- misconstrued the ±4 Wm^{-2} uncertainty statistic to be an energetic perturbation (items 2 and 7.6.4). This too is a fatal mistake.
- mis-equated propagated uncertainty with physical error (items 3 and 4).
- confused propagated error with subjectivist Bayesian uncertainty (items 5.1 and 5.2).
- misunderstood the significance of f_{CO_2} (items 6.2, 6.3.1-6.3.4, and 7.1-7.3).
- incorrectly supposed that differencing from a base-state removes climate simulation error (item 7.4.2).
- incorrectly supposed that a "±" uncertainty implies model oscillation (item 7.6.3). This is a freshman-level mistake.

Detailed Response:

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

1. The reviewer wrote that, "*The paper is seriously flawed [because] a formal application of the error propagation theory to the problem at hand. Eq.(2), ... is likely unable to estimate the true error accurately [because it] does not observe the equations of the Earth system dynamics, and formally increases the error each time step. As a result, it overestimates the true uncertainty, at least for sufficiently large time instants.*"

1.1 Reviewer statement 1 includes two fatal mistakes. The first fatal mistake is the reviewer's presumption that the error analysis concerns the terrestrial climate and must take into account "*Earth system dynamics*." The second mistake is to ignore that linear projections demand linear propagation of error.

Mistake 1: the error analysis is not concerned with the climate. It is concerned with the behavior of climate models. I.e., their air temperature projections are linear extrapolations of forcing.

This distinction is absolutely critical and is emphasized repeatedly: *cf.* manuscript Sections 2.1, 2.1.3, and 2.2.

The analytical focus on climate model behavior is repeated throughout the manuscript. However, the reviewer apparently did not grasp this critical point. The reviewer's focus on "*Earth system dynamics*" is thus wholly irrelevant.

1.2 GCM global air temperature projections are a linear extrapolation of greenhouse gas forcing. The manuscript fully demonstrates this fact.

Linear extrapolation fully justifies the application of error propagation eqn. 2 to GCM air temperature projections.

This absolutely critical point is repeatedly made: on manuscript p. 18, Figure 3 and text; on p. 27, Section 2.4.2, par. 2; on p. 34, Figure 9 and text; on p. 35, Section 3, par. 1, and in the extensively documented Supplementary Information Section 2 through Section 5, and Section 8.

However, the reviewer clearly failed to grasp this central point and apparently missed all of these multiple reminders and direct demonstrations.

2. "*Given the numbers, listed by the author (p.36: 'every calculational step imposes another ±4 W/m2 TCF average error onto the modeled climate response'), this 'sufficiently large time' should be at small as 10 years. My own experience of the Earth system modelling shows that such large imbalance of radiative fluxes would crash the model within several years at best.*"

2.1 The reviewer has mistakenly interpreted the ±4 Wm^{-2} long-wave cloud forcing (LWCF) error statistic as a radiative energy flux bias. It is nothing of the sort.

The ±4 Wm^{-2} average annual LWCF error arises from the difference between simulated and observed cloud cover.

It should be obvious that a statistical uncertainty is not a thermodynamic magnitude. Simulation errors are not uncompensated external flux biases.

It should also be obvious that a flux bias cannot be simultaneously positive and negative, i.e., ±4 Wm^{-2}. This incongruity alone should have alerted the reviewer to his/her mistaken thinking.

It is interesting to note that reviewer #1 made the identical mistake.

2.2 Differences between observations and simulations do not crash computers.

3. "*To be more specific, I would say that the formal application of the error propagation theory violates the fundamental conservation laws: it always increase uncertainty, while energetic constraints inhibit such increase for sufficiently large anomalies from initial value.*"

3. Once again, the reviewer has mistaken the ±4 Wm^{-2} statistic as an energy. The reviewer has also mistaken a propagated uncertainty statistic for a physical error.

The difference is fully explained on manuscript page 37, paragraph 2. The reviewer quoted this section, noted in item 2 above, clearly accessed the meaning of uncertainty, and did not grasp it.

Uncertainty statistics are not energetic terms and a large propagated temperature uncertainty is not a physical temperature. Physical conservation laws do not constrain statistical measures of ignorance.

As noted below, when error propagates into a very large uncertainty, the prediction becomes devoid of physical meaning.

Paragraph 2 ended, "*This is the meaning of propagated error: it indicates lack of knowledge – uncertainty – concerning the physical state of interest; it does not indicate anything of the state itself.*" Somehow, however, this explanation escaped the reviewer's grasp.

4. *This issue is well known for people dealing with numerical weather prediction: prediction error increases for a couple of weeks and then saturates.*

4. 1 Propagated error is not prediction error. It is a statement of predictive uncertainty.

Error is not uncertainty. Error is the difference between a simulation and observed physical reality. In contrast, uncertainty reflects the lack of knowledge about the fidelity of a predicted future state with the physically real future state about which nothing is known.
The uncertainty resulting from propagated error informs us of the reliability of the prediction, before the future state becomes observable.

4.2 Physical error saturates because it cannot exceed physical bounds. This is explained by Harlim, et al., as, "*A hallmark of chaos is the exponential growth of errors, where by* error *we mean the distance E(t) between two trajectories that are close to each other at time t=0. When trajectories are bounded, the exponential growth of E(t) cannot continue indefinitely; E(t) saturates near a value Es that is representative of the size of the chaotic attractor.*" [1]

That is, physical error must saturate at the physical bound.

However, a propagated uncertainty statistic can grow without bound, because statistics is not limited by physics. When uncertainty covers and exceeds the physical bound, it tells us that no knowledge is available concerning the phase-space position of the projected climate relative to that of the future physical climate.

Under this condition the projection expectation value has no physical meaning. That is, such a simulation conveys no information about the state of the future climate.

This is explained in detail in Supplemental Information Section 10.2 "*The meaning of predictive uncertainty.*" Manuscript Section 2.4.1, p. 25 and Section 3, p. 36 directed the reviewer to this explanation, but apparently to no avail.

A ±15 K propagated uncertainty in air temperature at the end of a centennial climate projection, means that the projected temperature increase has no predictive content. The projection reveals nothing about the state of the future climate.

5.1 *By the way, despite the statement made by the author and in some references cited by the manuscript, the statement that error analysis and propagation is ignored in climate studies is untrue. This error analysis is frequently employed for a calculation of Bayesian uncertainty,...*

5.1 Introduction paragraph 2 describes error analyses in published climate science. Therefore, author did not state that error analysis is ignored in climate studies.

Error propagation is not Bayesian. The reviewer did not cite any literature example of a Bayesian uncertainty propagated forward through a projection. I have found no such examples in my own literature searches.

5.2 "*... which now is a routine task for simplified climate models (e.g., for the Earth system models of intermediate complexity, EMICs; if interested, author could find the proper references himself).*"

5.2 That the reviewer did not cite such papers is regretted. The author has searched the climate literature for any example of error propagated through a GCM simulation, but without success.

However, a Google Scholar search of "*EMIC climate propagated error*" yielded the following three examples of Bayesian analysis:

i. The 2013 paper of Monier, et al., included an error analysis of the MIT Integrated Global System Model (IGSM). This model includes an Earth system model of intermediate complexity. [2] The analysis includes simulation root-mean-square errors vs. observations and simulation difference from an ensemble mean. The latter represents the precision-represented-as-error standard of analysis in climate science. Propagated error is nowhere in evidence.

ii. In Stott and Forest (2007) "*Ensemble climate predictions using climate models and observational constraints,*" Bayesian statistics are employed with EMIC simulations to judge the reliability of projected air temperatures. An uncertainty PDF is estimated based on 20th century simulation errors over observational periods of duration similar to the length of the futures simulation. However, nowhere is error uncertainty propagated forward stepwise through a simulation. [3]

iii. Urban and Keller 2010, "*Probabilistic hindcasts and projections of the coupled climate, carbon cycle and Atlantic meridional overturning circulation system: a Bayesian fusion of century-scale observations with a simple model,*" who also employ a Bayesian statistical model. [4] Herein error is not stepwise propagated into an estimate of projection uncertainty. Instead, all we get are future uncertainty pdfs based upon prior error pdfs. These pdfs are not error propagated stepwise through a projection. They do not show the increasing ignorance concerning the position of the projected climate relative to that of the future physically real climate.

The available evidence is that propagated error is absent in the uncertainty estimates of climate projections. Bayesian analysis is not propagated error. The manuscript criticism stands.

6.1 *Concerning the poor structure of the paper, I would mention Sects. 2.1.1 and 2.1.2. Which information from these Sections is used thereafter?*

6.1 This question is unfortunate. It should have been clear that the first coefficient of PWM eqn. 6 requires an objective estimate of the fractional contribution of water-vapor-enhanced greenhouse gas forcing as it is deployed within GCMs. This coefficient is derived in section 2.1.3.

Section 2.1.1 demonstrates that negligible forcing occurs at 1 ppm CO_2 The derivation in 2.1.3 requires this demonstration.

Section 2.1.2. demonstrates that CO_2 forcing follows $\log[CO2]_{atm}$ at >1 ppm CO_2. This is demonstration is also necessary to Section 2.1.3.

Thus the analyses in Sections 2.1.1 and 2.1.2 are critical to the validity of the subsequent analysis in 2.1.3.

The Introduction has been revised to clarify this logical sequence. Sections 2.1.1 and 2.1.2 have also been amended to highlight their contribution to what follows.

6.2 "*The definition of F_{CO_2} and F_{wv} (p.8), while correct for the Manabe and Wetherald (1967) model, is not applicable for the SRES and RCP simulations. The reasons are i) Manabe and Wetherald prescribed cloud amounts and cloud water+ice paths, ii) they neglected changes of other atmospheric constituents.*"

6.2.1 The analytical intent is to derive the fraction of water-vapor-enhanced CO_2 forcing alone, as applicable to climate models. Whether aerosols or other atmospheric constituents modify the total forcing is irrelevant to the pure case.

6.2.2 Figure 2b and Supplementary Figure S1 directly demonstrate the applicability of F_{CO_2} and F_{wv} to the SRES simulations. That is the derived F_{CO_2} produced an emulation that is well within the ensemble envelope of simulations from bona fide climate models.

6.2.3 Further, Supplemental Tables S1-S3 show that the derived F_{CO_2} is very near the F_{CO_2} values deduced for the CMIP3 models, GISS e-r, MRI cgcm2-1, CSIRO mk3-0, GFDL cm2-1, GISS aom, NCAR ccsm3-0, and GISS e-h.

The reviewer has ignored these direct evidences of validity.

6.3 "Cloud amounts and cloud optical properties are now interactive in climate models, and both SRES and RCP scenarios include aerosol emissions into the atmosphere. As a result, F_{CO_2} can not be calculated as a residue of unity and F_{vw}. Aerosol forcing is the most uncertain among all forcing agents (IPCC RA5, chap.7) and can not be neglected in the definition of F_{CO_2}."

6.3.1 F_{CO_2} is adjusted in the emulations of the SRES and RCP simulations of individual climate models. In these, it no longer has the Manabe-Wetherald value. This adjustment is introduced in manuscript page 17, and demonstrated in Supplemental Figure S2. Apparently the reviewer missed this discussion.

6.3.2 It is further relevant here to notice that the F_{CO_2} values derived from the various CMIP3 and CMIP5 climate models (Supplemental Tables S1-S4) vary between 0.365 and 0.815. These adjusted values allow for the aerosol and additional forcing agents.

6.3.3 The range of F_{CO_2} values among climate models also shows that cloud attributes, aerosol emissions, and other atmospheric constituents are either omitted or handled discrepantly among these climate models.

6.3.4 As the PWM, with adjustable F_{CO_2} is demonstrated capable of emulating CMIP3 and CMIP5 air temperature projections (Figure 2 through Figure 4, Figure 9, Supplemental Figure S1, and Figures S2-S8, the reviewer's criticism lacks force.

7."Eq. (6) (and its slightly extended version included in the Supplementary Information) is seriously flawed:"

The critical elements of this comment are taken in turn.

7.1. "i) it neglects the forcing from agents other than CO_2 and water vapour,"

This criticism is not correct. Other forcing agents are implicitly included in F_{CO_2} because this coefficient is adjusted to reflect the forcing deployed by each GCM for each SRES or RCP projection (Supplemental Figure S2).

The analysis demonstrates the persistent linearity of projections with forcing regardless of "*agents other than CO_2 and water vapour*." This invariable projection linearity completely justifies linear propagation of error (ms eqn. 2).

The original $F_{CO_2} = 0.42$ is sufficient to show that the PWM can successfully emulate the air temperature projections of advanced climate models. That is, when $F_{CO_2} = 0.42$, the PWM emulation is inside the model ensemble envelope and displays the correct slope (ms. Figure 2).

7.2 "*ii) it ignores climate inertia (according to the discussion in the Supplementary information, this was already mentioned by one the previous referees),"*

7.2.1 The purpose of manuscript eqn. 6 (the PWM) is to test the behavior of climate models. It is not itself a climate model. This straight-forward distinction appears to be lost on the reviewer.

The PWM as-is successfully emulates the temperature projections of CMIP3 and CMIP5 GCMs. Therefore there is no empirical reason to include climate inertia.

Reiterating the point: the PWM is not a climate model. It is not made to include the physics of climate. It is made to rest whether climate models linearly extrapolate CO_2 forcing into air temperature projections. This point is emphatically made in the Introduction, and carries through the entire manuscript. In this regard, the reviewer criticism is irrelevant.

7.2.2. The PWM is focused on the transient forcing of increasing GHGs as it is extrapolated using GCMs. Whatever climate inertia is deployed in climate models during the transient period is included in the derived F_{CO_2} for each projection.

Elaborating from 6.3.3, one notes that the latitude of the CMIP5 projections indicates the various models must also vary significantly in the way climate inertia is handled.

7.3.1 "*iii) it extrapolates the CO_2+WV forcing as calculated for the difference between the preindustrial climate (PI) and the climate of the "Earth without atmosphere'...*"

7.3.1 The reviewer is not correct. The CO_2+wv forcing is calculated as the difference between the pre-industrial atmosphere with CO_2 and the pre-industrial atmosphere without CO_2. It does not involve the "*Earth without atmosphere.*"

The method is given in the last paragraph of Section 2.1, the last sentence in Section 2.1.2, and the first two paragraphs of Section 2.1.3.

Section 2.1.3, par. 2 sentence 2 specifically mentions this point: "*Assuming that, <u>in the absence of CO_2, the global total cloud fraction (TCF) remains unaffected at 66.7%</u> [9, 46], the fractional greenhouse warming due to water vapor alone can be estimated directly from Figure 1b...*"

7.3.2 "*... to the forced climate change starting from PI. Because sensitivity (infinitesimal) of the forced response around the PI state is likely to differ from the mean (finite-response) sensitivity between PI-'Earth without atmosphere', items i and ii cast doubts on the value 0.42 which is used in Eq.(6).*"

7.3.2.1 Reviewer item *i* was shown incorrect in response 7.1. Reviewer item *ii* was shown to be irrelevant in responses 7.2.1 and 7.2.2.

7.3.2.2, the reviewer has mistaken the manuscript analysis to difference from "*Earth without atmosphere*." As the analysis differences from Earth with atmosphere plus water vapor, the criticism in 7.3.2 is misconceived.

7.3.2.3, the F_{CO_2} = 0.42 coefficient was derived directly from parameters taken from Manabe and Wetherald. The reviewer has missed the point that F_{CO_2} was only ever meant to represent the pure CO_2 case. Other forcing agents are irrelevant.

7.3.2.4, the emulation validity of the F_{CO_2} = 0.42 value is directly demonstrated in Figure 2.

7.3.3 "*Item ii can only be accounted for by adding finite heat capacity term C dT/dt to Eq.(6) (C is the climate heat capacity 10**9 J/m2, (Andreae et al., 2005, doi 10.1038/nature03671)), but the latter is not a remedy for the major uncertainty associated with the value 0.42 for F_{CO_2}.*"

7.3.3.1 Response items 7.3.2.3 and 7.3.2.4 demonstrate the validity of F_{CO_2} = 0.42.

7.2.3.2 As noted in 7.3.1, the PWM is an emulator of GCM behavior. It is not a climate model. Its success is thoroughly demonstrated. Adding a term for climate inertia is thus doubly irrelevant.

7.3.3.3 The value of F_{CO_2} is adjusted for each emulation. Various forcing elements are thereby included. The linearity of model projections is demonstrated to persist throughout.

7.3.3.4 It is interesting to note the logical incoherence in Andreae, et al., 2005 to which the reviewer grants such authority. Summarized and juxtaposed from the abstract: "*Atmospheric [aerosol uncertainties lead] to large uncertainties in the sensitivity of climate to human perturbations, ... and [uncertainties in] projections of climate change. ... Strong aerosol cooling in the past and present* *would then imply that future global warming may proceed at or even above the upper extreme of the range projected by the Intergovernmental Panel on Climate Change.*"

A logical extrapolation of the caution that aerosol uncertainties produce large uncertainties in human thermal impacts and in climate projections should lead to a conclusion that future global warming from human causes is presently unknowable.

Andreae, et al., 2005 instead very tendentiously conclude that unknown perturbations possibly imply a more extreme warming. Given the reviewer's dislike of tendentiousness, mentioned below, one would have anticipated the reviewer to disapprove of Andreae, et al., 2005.

7.4 *Author does not make a difference between the model bias for the initial state and uncertainty associated with projection (p.26, first paragraph). I note that this comments was already raised by one of the previous referees.*

7.4.1 It would have helped understand this criticism had the reviewer specified which of the previous comments was meant.

Manuscript page 26, first paragraph merely points out that the ±4 Wm^{-2} annual average LWCF error is ±150 % larger than the entire increased forcing since 1900, and ±114× larger than the ~0.035 Wm^{-2} annual increase in GHG forcing since 1979. How this neglects a difference between initial state bias and projection uncertainty is obscure.

7.4.2 However, perhaps the reviewer had in mind the comments of previous reviewer discussed in SI Section 7.1. This section points out that a theory bias imposes a calculational error into any equilibrated (spin-up) base-state climate. Theory bias further means that the erroneous base state climate is incorrectly projected forward.

That is, after calculating an erroneous base-state, an incorrect theory will erroneously project that state. The errors in the base-state will be compounded with further errors in the subsequently simulated state.

The average annual ±4 Wm^{-2} LWCF error inherent in climate models is therefore present in any base-state climate. The LWCF error is again made on projecting subsequent simulation steps. This is explained in detail on page 32, par 1. The reviewer has presented no solution to this problem.

The only possible conclusion is that theory-bias error is identically imposed in an equilibrated initial state and a projected state. There is no difference.

7.5 *I do not understand author's reasoning that 'climate is propagated through time as state magnitudes, not as anomalies' (p.32). The approach to make a Taylor expansion around some prechosen state is quite common in physics, and it is unclear why this approach is not suitable for climate studies.*

7.5 The author dies not dispute the use of Taylor expansions. The author merely pointed out that global climate is projected as climate state variables, not as anomalies.

7.6 "*Finally, Supplementary Information contains a rather lengthy discussion of the author with previous referees. At first, I agree with all listed comments made by previous referees and disagree with replies on these comments. Two examples of this are mentioned above."*

7.6 The reviewer specified agreement with prior review comments. Surveying the prior comments, the reviewer agrees that:

7.6.1 differencing against a base-state simulation removes projection errors (SI Section 7.1). This claim has never been empirically demonstrated, is not supported in the literature, and makes no appearance in the IPCC 5AR.

7.6.2 the PWM is successful only when forcing is linear (SI Section 8). This claim is refuted by Figure S11, Figure S12, and Figure S13 as well as by the 20th century emulations of Figure 4, Figure 8, and Figure S8.

7.6.3 the "±" of confidence intervals implies model oscillation (SI Section 10.1). This claim is freshman-level naive. The reviewer apparently believes that an uncertainty interval is a model expectation value, and further supposes modeled states can be simultaneously positive and negative.

7.6.4 an uncertainty statistic is an energetic perturbation (SI Section 10.3). It should not be necessary to point out that such a claim is nonsense.

Items 7.6.1-7.6.4 are obvious mistakes, but the reviewer apparently finds them convincing.

7.7 *I would say that paper is based on lack of knowledge.*

7.7.1 The manuscript is about error analysis, following the long-standing standard methods in the physical sciences cited in the references. The irony of the reviewer's comment is not lost, given the reviewer's lack of knowledge regarding uncertainty and physical error displayed in response items 7.6.1-7.6.4.

7.7.2 This lack of knowledge is reflected, in particular, in rather tendentious list of references.

7.7.2 The comment is impossible to evaluate as the reviewer provided no examples of tendentious references. Of the 94 manuscript references, 78 are to mainstream climate publications, 9 are to methods of error analysis and the calculation of uncertainty, 2 are the author's prior work, and the remainder are about CO_2 IR absorption. One is left wondering to what the reviewer refers.

7.7.3 I would suggest not to bother further referees for reviewing this flawed manuscript and just to reject it.

7.7.3 As demonstrated in the response items above, and in the Introductory Summary, there is little or no scientific merit in this review and therefore no basis for the received conclusion.

[1] Harlim, J., et al., Convex Error Growth Patterns in a Global Weather Model. Phys. Rev. Lett., 2005. 94(p. 228501.

[2] Monier, E., et al., An integrated assessment modeling framework for uncertainty studies in global and regional climate change: the MIT IGSM-CAM (version 1.0). Geosci. Model. Dev., 2013. 6(p. 2063-2085.

[3] Stott, P.A. and C.E. Forest, Ensemble climate predictions using climate models and observational constraints. Phil. Trans. Roy. Soc. A, 2007. 365(1857): p. 2029-2052.

[4] Urban, N.M. and K. Keller, Probabilistic hindcasts and projections of the coupled climate, carbon cycle and Atlantic meridional overturning circulation system: a Bayesian fusion of century-scale observations with a simple model. Tellus A, 2010. 62(5): p. 737-750.