4 June 2016

Prof. Anthony R. Lupo
Department of Soil, Environmental, and Atmospheric Sciences
University of Missouri
Columbia, MO 65211

Dear Prof. Lupo,

Please find revised manuscript 3852317, "Propagation of Error and the Reliability of Global Air Temperature Projections," for resubmission to Advances in Meteorology.

The Introduction has been revised and extended to present the structure and logic of the analysis. The Figures are now in color, with the hope they are more accessible. The text is revised throughout for clarity. All this is in conjunction with the recommendations of reviewer #3.

Review #1 and Review #2 include far too many mistakes to enumerate here. Two mistakes, however, are pervasive and fatal: misperception of physical error as an energetic perturbation, and misperception of uncertainty in temperature as a physically real temperature.

Neither reviewer #1 nor reviewer #2 gave any evidence of understanding the analytical approach nor even of the meaning of propagated error.

These mistakes are so fundamental as to remove any critical merit from Reviews 1 and 2. The full responses to the reviews are appended to the end of this letter.

Response Letters #1 and #2 are necessarily lengthy (12 pp and 11 pp, resp.). For your convenience, they are headed by a summary and index of critical findings.

Review #1 and #2 exemplify the difficulties climate modelers apparently have with physical error analysis. It is respectfully suggested that further reviewers be restricted to physicists and physical meteorologists.

As before, meteorological and physicist reviewers might include:
Prof. Roberto Rondanelli, U Chile: ronda@dgf.uchile.cl
Prof. Yong-Sang Choi, EW University, Seoul: ysc@ewha.ac.kr
Dr. Randall J. Scalise, Southern Methodist University: scalise@smu.edu
Prof. Demetris Koutsoyiannis, Technical University of Athens: dk@itia.ntua.gr
Prof. David M. Harrison, University of Toronto: david.harrison@utoronto.ca
Prof. William Happer, Princeton University: happer@Princeton.EDU
Dr. Hyo-Jong Song, SUNY, Albany: hsong2@albany.edu
Prof. Anastasios Tsonis, University of Wisconsin: aatsonis@uwm.edu

Experts in validation and error assessment of numerical models include:
Dr. Jon C. Helton, Sandia National Labs: jchelto@sandia.gov
Prof. Christopher Roy, Virginia Tech: cjroy@vt.edu
Dr. William Oberkampf, Sandia National Labs: wloberk@sandia.gov

As noted previously, this work has been carried out on my own time and was not funded by any external agency or third-party donor.

Finally, thank-you very much for your consideration, and I await your reply.

Yours sincerely,

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

Author Response to Reviewer 1.

Summary

This reviewer:

1. never addressed the central point that linear extrapolation of forcing is subject to linear propagation of error.
2. has throughout mistaken the LWCF error statistic to be an energetic bias (items 2.1, 2.2.1-2.2.3, 2.3.1, 2.4.1, 2.7, 3.1). This mistake is pervasive, is fatal, and removes any scientific merit from the review.
3. has misconstrued ms. eqn. 6 (the PWM) to concern the terrestrial climate rather than climate models (items 1.1 and 1.2).
4. has inadvertently validated the manuscript analysis (item 1.3).
5. provided equation R4 which is riven with mistakes (items 2.3.1 - 2.3.10).
6. has mistaken the ±T uncertainty statistic to be a physical temperature. This follows from 1, above, and also represents a fatal mistake (item 2.4.2).
7. throughout has misconstrued the meaning of an error statistic, misconstrued propagated error, and showed no understanding of physical uncertainty.

Detailed Response:

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

1.1 Under "**1. The nature of the PWM**," the review began with a misconstrual that the passive warming model (PWM) represents the warming due to well-mixed greenhouse gases (GHGs).

Quoting the reviewer, "*According to the PWM, the total warming due to well-mixed atmospheric greenhouse gases (GHG) relative to a hypothetical state in which these* *gases were absent is*

$$"\ \Delta T_t = 0.42 \times 33K \times \frac{F_0 + \sum_{i=1}^{t} \Delta F_i}{F_0} \qquad (Eq.\ 6\ in\ the\ manuscript)\ "$$

Although the PWM is correctly written, the wrong meaning is appended. The PWM does not represent the warming due to GHGs. The PWM represents the observed behavior of climate models. The PWM emulates climate models, not climate response.

This distinction is absolutely critical. Failure to understand it has led the reviewer to make fundamental errors of review, as shown below.

The intent to emulate climate models, not climate, is presented immediately in the Introduction, and this meaning is applied to the PWM throughout the manuscript.

Section 2.2, which opens the full discussion of the PWM, states this distinction right in the title and in the very first sentence: the PWM is an emulator of climate models.

Further: manuscript p. 15 paragraph 1, describes the PWM as, "*represent[ing] the increasing GASAT projections of GCMs,*" i.e., representing GCMs, not the climate.

Ms. page 17, par. 2, ends with, "*Equation 6 is thus able to emulate the projected GASAT trends of virtually any current GCM.*"

The same meaning is implicit in the opening sentence of Section 3. Summary and Discussion.

This distinction of meaning is clear, is repeated, is critical, and has apparently escaped the reviewer.

1.2. The reviewer has repeated the same misconstrual on review page 2, first sentence: "*In essence, (R3) indicates that temperature depends linearly on radiative forcing.*"

This is not correct. The correct formulation would be, 'In essence, (R3) indicates that GCMs air temperature projections depend linearly on radiative forcing.' R3 (ms. eq. 6) says nothing about climate. R3 is about climate models.

Once again, this distinction is critical and central, and the reviewer has failed to grasp it.

1.3. In stating that, "*Global climate models give a similar [linear dependence on radiative forcing], although the coefficient of proportionality varies somewhat from model to model.*", the reviewer has inadvertently legitimized the entire manuscript analysis.

This is so, because linear propagation of GCM error follows directly upon GCM linear extrapolation of GHG forcing.

For example, Bevington and Robinson p. 48 under "Propagation of Error," for summations: $x = au + bv \quad \sigma_x^2 = a^2\sigma_u^2 + b^2\sigma_v^2 + 2ab\sigma_{uv}^2$. [1] That is, the uncertainty variance of a sum is the sum of the variances of the individual elements.

The identical treatment is found in equations A-3 and D-1 of the NIST Guideline for Evaluation and Expression of Uncertainty. [2]

The Bevington and NIST formulations generalize to manuscript eqn. 1 and eqn. 2.

The identical case applies to climate model projections of air temperature, which the reviewer has admitted are a sum from the linear extrapolation of GHG forcing.

2. Under the reviewer's, "**2. Why the main argument of the paper fails**"

2.1. The reviewer referred parenthetically to a, "*[bias] due to an error in the long-wave cloud forcing as assumed in the paper.*"

The manuscript does not assume this error. The GCM average long-wave cloud forcing (LWCF) error was reported in Lauer and Hamilton, manuscript reference 59, [3] and given prominent notice in Section 2.4.1, page 25, paragraph 1: "*The magnitude of CMIP5 TCF global average atmospheric energy flux error.*"

In 2.1 above, the reviewer has misconstrued a published fact as an author assumption.

The error is not a "bias," but rather a persistent difference between model expectation values and observation.

2.2. The reviewer wrote, "*Suppose a climate model has a bias in its energy balance (e.g. due to an error in the long-wave cloud forcing as assumed in the paper). This energy balance bias (B) essentially acts like an additional forcing in (R3),...*"

2.2.1. The reviewer has mistakenly construed that the LWCF error is a bias in energy balance. This is incorrect and represents a fatal mistake. It caused the review to go off into irrelevance.

LWCF error is the difference between simulated cloud cover and observed cloud cover. There is no energy imbalance.

Instead, the incorrect cloud cover means that energy is incorrectly partitioned within the simulated climate. The LWCF error means there is a ±4 Wm^{-2} uncertainty in the tropospheric energy flux.

2.2.2. The LWCF error is not a forcing. LWCF error is a statistic reflecting an annual average uncertainty in simulated tropospheric flux. The uncertainty originates from errors in cloud cover that emerge in climate simulations, from theory bias within climate models.

Therefore LWCF error is not "*an additional forcing in R3*." This misconception is so fundamental as to be fatal, and perfuses the review.

2.2.3The reviewer may also note the "±" sign attached to the ±4 Wm^{-2} uncertainty in LWCF and ask how "*an additional forcing*" can be simultaneously positive and negative.

That incongruity alone should have been enough to indicate a deep conceptual error.

2.3. "*... leading to an error in the simulated warming*:

$$ERR(\Delta T_t\text{-}\Delta T_0) = 0.416\times((F_t+B_t)\text{-}(F_0+B_0)) = 0.416(\Delta F+\Delta B) \qquad R4"$$

2.3 Reviewer equation R4 includes many mistakes, some of them conceptual.

2.3.1. First mistake: the ±4 Wm^{-2} average annual LWCF error is an uncertainty statistic. The reviewer has misconceived it as an energy bias. R4 is missing the "±" operator throughout. On the right side of the equation, every +B should instead be ±U.

2.3.2. Second mistake: The "ERR" of R4 should be 'UNC' as in 'uncertainty.' The LWCF error statistic propagates into an uncertainty. It does not produce a physical error magnitude.

The meaning of uncertainty was clearly explained in manuscript Section 2.4.1 par. 2, which further recommended consulting Supporting Information Section 10.2, "*The meaning of predictive uncertainty*." The reviewer apparently did not heed this advice. Statistical uncertainty is an ignorance width, as opposed to physical error which marks divergence from observation.

Further, manuscript Section 3, "*Summary and Discussion*" par. 3ff explicitly discussed and warned against the reviewer's mistaken idea that the ±4 Wm^{-2} uncertainty is a forcing (cf. also 2.2.2 above).

Correcting R4: it is given as:

$$ERR(\Delta T_t\text{-}\Delta T_0) = 0.416\times((F_t+B_t)\text{-}(F_0+B_0)) = 0.416\times(\Delta F+\Delta B)$$

Ignoring any further errors (discussed below), the "*B*" term in R4 should be *±U*, and ERR should be UNC, thus:

$$UNC(\Delta T_t\text{-}\Delta T_0) = 0.416\times((F_t\pm U_t)\text{-}(F_0\pm U_0)) = 0.416\times(\Delta F\pm\Delta U)$$

because the LWCF root-mean-error statistic $\pm U$, is not a positive forcing bias, $+B$.

2.3.3. Third mistake: correcting +B to $\pm U$ brings to the fore that the reviewer has ignored the fact that $\pm U$ arises from an inherent theory-error within the models. Theory error injects a simulation error into every projection step. Therefore $\pm U$ enters into every single simulation step.

An uncertainty $\pm U_i$ present in every step accumulates across n steps into a final result as $\pm U_t = \sqrt{\sum_{i=1}^{n} U_i^2}$. Therefore, $UNC(\Delta T_t\text{-}\Delta T_0) = \pm U_t$, not $\pm U_t\text{-}\pm U_0$. Thus R4 is misconceived as it stands.

One notes that $\pm U_i = \pm 4\ \mathrm{Wm}^{-2}$ average per annual step, after 100 annual steps then becomes $\pm U_t = \sqrt{\sum_{i=1}^{100} (\pm 4)_i^2} = \pm 40\ \mathrm{Wm}^{-2}$ uncertainty, <u>not error</u>, and $\pm T_{UNC} = 0.416(\pm 40) = \pm 16.6$ K, i.e., the manuscript result.

2.3.4. Fourth mistake incorporates two mistakes. In writing, "*a bias change $\Delta B = \pm 4\mathrm{Wm}^{-2}$ would indicate an error of ± 1.7 K*", the reviewer has not used eqn. R4, because the "±" term on the temperature error has no counterpart in reviewer R4. That is, reviewer R4 is ERR = $0.416\times(\Delta F+\Delta B)$. From where did the "±" in ±1.7 K come?

Second, in the quote above, the reviewer has set a positive bias "ΔB" to be simultaneously positive and negative, i.e., "$\pm 4\mathrm{Wm}^{-2}$." How is this possible?

2.3.5. Fifth mistake: the reviewer's *±1.7 K* is from $0.416\times(\pm\Delta U)$, not from $0.416\times(\Delta F\pm\Delta U)$, the way it should be if calculated from (corrected) R4.

Corrected eqn. R4 says ERROR = $\Delta\Delta T = 0.416\times(\Delta F\pm\Delta U) = \Delta T_F\pm\Delta T_U$ Thus the reviewer's R4 error term should be, '$\Delta T_F\pm$*(the spread from* ΔT_U*).*'

For example, from RCP 8.5, if $\Delta F_{2000\text{-}2100} = 7\ \mathrm{Wm}^{-2}$, then from the reviewer's R4 with a corrected $\pm U$ term, ERR = 0.416×(7±4) K = 2.9±1.7 K.

That is, the reviewer incorrectly represented ±1.7 K as ERR, when it is instead the spread in ERR.

2.3.6. Sixth mistake, the reviewer's B_0 does not exist. Forcing F_0 does not have an associated LWCF uncertainty (or bias) because F_0 is the base forcing at the start of the simulation, i.e., it is assigned before any simulation step.

This condition is explicit in manuscript eqn. 6, where subscript "i" designates the change in forcing per simulation step, ΔF_i. Therefore, "i" can only begin at unity with simulation step one. There is no zeroth step simulation error because there is no zeroth simulation.

2.3.7. Seventh mistake: the reviewer has invented a magnitude for B_t.

The reviewer's calculation in R4 (±4 Wm^{-2} → ±1.7 K error) requires that $B_t - B_0 = \Delta B = \pm 4\ Wm^{-2}$ (applying the 2.3.1 "±" correction).

The reviewer has supposed $B_0 = \pm 4\ Wm^{-2}$. However, reviewer's ΔB is also ±4 Wm^{-2}. Then it must be that $B_t - \pm 4\ Wm^{-2} = \pm 4\ Wm^{-2}$, and the reviewer's B_t must be ±8 Wm^{-2}.

From where did that ±8 Wm^{-2} come? The reviewer does not say. It seems from thin air.

2.3.8. Eighth mistake: R4 says that for any simulated ΔT_t the bias is always $\Delta B_t = B_t - B_0$, the difference between the first and last simulation step.

However, B is misconstrued as an energy bias. Instead it is a simulation error statistic, $\pm U$, that originates in an imperfect theory, and is therefore imposed on every single simulation step. This continuous imposition is an inexorable feature of an erroneous theory.

However, R4 takes no notice of intermediate simulation steps and their sequentially imposed error. It is not surprising then that having excluded intermediate steps, the reviewer concludes they are irrelevant.

2.3.9. Ninth mistake: The "*t*" is undefined in R4 as the reviewer has it. As written, the "*t*" can equally define a 1-step, a 2-step, a 10-step, a 43-, a 62-, an 87-, or a 100-step simulation.

The reviewer's $\Delta B_t = B_t - B_0$ always equals $\pm 4\ Wm^{-2}$ no matter whether "*t*" is one year or 100 years or anywhere in between. This follows directly from having excluded intermediate simulation steps from any consideration.

This mistaken usage is in evidence in review Part 2, par. 2, where the reviewer applied the ±4 Wm^{-2} to the uncertainty after a 100-year projection, stating, "*a bias change $\Delta B = \pm 4\ Wm^{-2}$ would indicate an error of ±1.7 K [which is] nowhere near the ±15 K claimed by the paper.*" That is, for the reviewer, $\Delta B_{t=100} = \pm 4\ Wm^{-2}$.

However, the ±4 Wm^{-2} is the empirical average <u>annual</u> LWCF uncertainty, obtained from a 20-year hindcast experiment using 26 CMIP5 climate models. [3]

This means an LWCF error is generated by a GCM across every single simulation year, and the ±4 Wm^{-2} average uncertainty propagates into every single annual step of a simulation.

Thus, intermediate steps must be included in an uncertainty assessment. If the ΔB_t

represents the uncertainty in a final year anomaly, it cannot be a constant independent of the length of the simulation.

2.3.10. Tenth mistake: the reviewer's error calculation is incorrect. The reviewer proposed that an annual average ±4 Wm^{-2} LWCF error produced a projection uncertainty of ±1.7 K after a simulation of 100 years.

This cannot be true (*cf.* 2.3.3, 2.3.8, and 2.3.9) because the average ±4 Wm^{-2} LWCF error appears across every single annum in a multi-year simulation. The projection uncertainty cannot remain unchanged between year 1 and year 100.

This understanding is now applied to the uncertainty produced in a multi-year simulation, using the corrected R4 and applying the standard method of uncertainty propagation.

The physical error "ε" produced in each annual projection step is unknown because the future physical climate is unknown. However, the uncertainty "u" in each projection step is known because hindcast tests have revealed the annual average error statistic.

For a one step simulation, i.e., 0→1, $U_0 = 0$ because the starting conditions are given and there is no LWCF simulation bias.

However, at the end of simulation year 1 an unknown error $\varepsilon_{0,1}$ has been produced, the ±4 Wm^{-2} LWCF uncertainty has been generated, and $U_t = \pm U_{0,1}$.

For a two-step simulation, 0→1→2, the zeroth year LWCF uncertainty, U_0, is unchanged at zero. However, at the terminus of year 1, the LWCF uncertainty is $\pm U_{0,1}$.

Simulation step 2 necessarily initiates from the (unknown) ε_1 error in simulation step 1. Thus, for step 2 the initiating ε is $\varepsilon_{0,1}$.

Step 2 proceeds on to generate its own additional LWCF error $\varepsilon_{1,2}$ of unknown magnitude, but for which $\pm U_{1,2} = \pm 4\ Wm^{-2}$. Combining these ideas: step 2 initiates with uncertainty $\pm U_{0,1}$. Step 2 generates new uncertainty $\pm U_{1,2}$. The sequential change in uncertainty is then $\pm U_0 = 0 \rightarrow \pm U_{0,1} \rightarrow \pm U_{1,2}$. The total uncertainty at the end of step 2 must then be the root-sum-square of the sequential step-wise uncertainties, $\pm U_{t=0\rightarrow 2} = \pm\sqrt{[(U_{0,1})^2+(U_{1,2})^2]} = \pm 5.7\ Wm^{-2}$. [1, 2]

R4 is now corrected to take explicit notice of the sequence of intermediate simulation steps, using a three-step simulation as an example. As before, the corrected zeroth year LWCF $U_0 = 0\ Wm^{-2}$.

Step 1: $UNC(\Delta T_t\text{-}\Delta T_0) = (\Delta T_1\text{-}\Delta T_0) = 0.416\times((F_1\pm U_{0,1})\text{-}(F_0\pm U_0)) = 0.416\times(\Delta F_{0,1}\pm \Delta U_{0,1}) = u_{0,1}$
Step 2: $UNC(\Delta T_t\text{-}\Delta T_0) = (\Delta T_2\text{-}\Delta T_1) = 0.416\times((F_2\pm U_{0,2})\text{-}(F_1\pm U_{0,1})) = 0.416\times(\Delta F_{1,2}\pm \Delta U_{1,2}) = u_{1,2}$

Step 3: $UNC(\Delta T_t\text{-}\Delta T_0) = (\Delta T_2\text{-}\Delta T_1) = 0.416\times((F_3\pm U_{0,3})\text{-}(F_2\pm U_{0,2})) = 0.416\times(\Delta F_{2,3}\pm\Delta U_{2,3}) = u_{2,3}$

where "*u*" is uncertainty. These formalisms exactly follow the reviewer's condition that "*t*" is undefined. But "*t*" must acknowledge the simulation annual step-count.

Each $t+1$ simulation step initiates from the end of step t, and begins with the erroneously simulated climate of prior step t. For each simulation step, the initiating $T_0 = T_{t-1}$ and its initiating LWCF error ε is ε_{t-1}. For $t>1$, physical error $\varepsilon_t = \sum_{i=1}^{t}\varepsilon_i$ but its magnitude is necessarily unknown.

The uncertainty produced in each simulation step, "*t*" is $u_{t-1,t}$ as shown. However the total uncertainty in the final simulation step is the uncertainty propagated through each step. Each simulation step initiates from the accumulated error in all the prior steps, and carries the total uncertainty propagated through those steps.

Following NIST, and Bevington and Robinson, [1, 2] the propagated uncertainty variance in the final step is the root-sum-square of the error in each of the individual steps, i.e., $\sigma_t^2 = \sum_{i=1}^{t}(u_i)^2$. When $u_i = \pm 4\ Wm^{-2}$, the above example yields a three-year simulation temperature uncertainty variance of $\sigma^2 = 8.3$ K.

As discussed both in the manuscript and in SI Section 10.2, this σ_t^2 is not an error magnitude, but an uncertainty statistic. The distinction is critical. The true error magnitude is necessarily unknown because the future physical climate is unknown.

The projection uncertainty can be known, however, as it consists of the known simulation average error statistic propagated through each simulation step. The propagated uncertainty expresses the level of ignorance concerning the physical state of the future climate.

2.4 The reviewer wrote that, "*For producing this magnitude of error in temperature change, ΔB should reach $\pm 36\ Wm^{-2}$, which is entirely implausible.*"

2.4.1. The reviewer has once again mistaken an uncertainty statistic for an energetic perturbation. Under reviewer section 2, ΔB is defined as, an "energy balance bias (*B*)," i.e., an energetic offset.

One may ask the reviewer again how a physical energy offset can be both positive and negative simultaneously. That is, a '±energy-bias' is physically incoherent. This mistake alone render's the reviewer's objection meritless.

As a propagated uncertainty statistic the reviewer's $\pm 36\ Wm^{-2}$ is entirely plausible because, a) it represents the accumulated uncertainty across 100 error-prone annual simulation steps, and b) statistical uncertainty is not subject to physical bounds.

2.4.2 The ±15 K that so exercises the reviewer is not an error in temperature magnitude. It is an uncertainty statistic. $\pm\Delta B$ is not a forcing and cannot be a forcing because it is an uncertainty statistic.

The reviewer has completely misconstrued uncertainty statistics to be thermodynamic quantities. This is as fundamental a mistake as is possible to make.

The ±15 K does not suggest that air temperature itself could be 15 K cooler or warmer in the future. The reviewer clearly supposes this incorrect meaning, however.

The reviewer has utterly misconceived the meaning of the error statistics. A statistical ±T is not a temperature. A statistical $\pm Wm^{-2}$ is not an energy flux or a forcing.

All of this was thoroughly discussed in the manuscript and the SI, but the reviewer apparently overlooked these sections.

2.5 In Section R2 par. 3, the reviewer wrote that review eqn. R1 shows the uncertainty is not independent of F_i and therefore cancels out between simulation steps.

However, R1 determines the total change in forcing, F_t-F_0, across a projection. No uncertainty term appears in R1, making the reviewer's claim a mystery.

2.5.2 Contrary the reviewer's claim, the average annual ±4 Wm^{-2} LWCF error statistic is independent of the magnitude of F_i. The ±4 Wm^{-2} is the constant average LWCF uncertainty revealed by CMIP5 GCMs (manuscript Section 2.3.1 and Table 1). GCM LWCF error is injected into each simulation year, and is entirely independent of the (GHG) F_i forcing magnitudes.

In particular, LWCF error is an average annual uncertainty in the global tropospheric heat flux, due to GCM errors in simulated cloud structure and extent.

2.5.3. The reviewer's attempt at error analysis is found in eqn. R4 not R1. However R4 also fails to correctly assess LWCF error. Sections 2.x.x above shows R4 has no analytical merit.

2.6 In section R2, par 4, the reviewer supposes that use of 30 minute time-steps in an uncertainty propagation, rather than annual steps, must involve 17520 entries of ±4 Wm^{-2} in an annual error propagation.

In this, the reviewer has overlooked the fact that ±4 Wm^{-2} is an annual average error statistic. As such it is irrelevant to a 30-minute time step, making the ±200 K likewise irrelevant.

2.7 In R2 final sentence, the reviewer asks whether it is reasonable to assume that model biases in LWCF actually change by ±4 Wm^{-2}.

However, the LWCF error is not itself a model bias. Instead, it is the observed average error between model simulated LWCF and observed LWCF.

The reviewer has misconstrued the meaning of the average LWCF error throughout the review. LWCF error is an uncertainty statistic. The reviewer has comprehensively insisted on misinterpreting it as a forcing bias -- a thermodynamic quantity.

The reviewer's question is irrelevant to the manuscript and merely betrays a complete misapprehension of the meaning of uncertainty.

3.1 R3 sentence 1, "*The author additionally assumes that energy balance biases in present day climate give a good order-of-magnitude estimate of the absolute change in bias when climate changes (ΔB in Eq. (R4)).*"

In R3, sentence 1, the reviewer has again mistakenly taken the LWCF error to be a bias in energy balance. It is not, as has been explained exhaustively above. The author has assumed nothing whatever about energy balance.

The ±4 Wm^{-2} LWCF error is the average of 520 CMIP5 simulation-year hindcasts of a changing climate. That error is therefore properly indicative of the uncertainty attending a futures projection of a changing climate.

The author's analysis concerns propagating the known average annual tropospheric LWCF inaccuracy of climate models, into the projections of future tropospheric air temperature made using those same climate models.

3.2. The reviewer is thanked for providing Figure R1. Figure R1 demonstrates a critical point the author has made elsewhere, namely that climate models do not provide a unique solution to the problem of the climate energy-state.

None of the Figure R1 climate models is known to be correct, or more correct than any other. Their baseline climates are different and so are their quadrupled CO_2 climates. No model appearing in Figure R1 is known to have produced a physically correct baseline climate or $4\times CO_2$ climate.

All of the Figure R1 LW cloud forcing estimates are therefore uncertain. And yet none of the points in Figure R1 have any uncertainty bars. Uncertainty bars would reflect the fact that the physical representations of the climate are not known to be correct and therefore that the correlation is not physically meaningful.

The fact that model outputs are correlated only speaks to the uniformity of the models. Correlation of model output lends no support to any supposition that the projections are physically correct.

One also notes that climate models are tuned to the same observables. [4-9] It is not surprising, therefore, that their outputs are correlated. Tuned correlations do not indicate projection accuracy.

3.3 Regarding Figure R2, the average one-year ±4 Wm^{-2} LWCF uncertainty reported by Lauer and Hamilton [3] (ms. ref. [59]) is already much larger than the standard deviation of the model results after 130 years.

Across 130 projection years, or any number of projection years, the meaning of this uncertainty magnitude is that the model simulation of the LW forcing response to 4×CO_2 has no obvious climatological meaning. That is, the projections convey no information about the physically true LW forcing of the future climate.

3.4 The reviewer wrote, "*The author is formally correct in that these intermodel differences only quantify the precision of the model results, not their absolute accuracy.*"

With this comment and Figure R2, the reviewer agreed that inter-model comparisons are about precision and give no indication of projection accuracy. In this, the reviewer therefore agrees with the author.

No more than this need be said about the meaning of Figure R1 and Figure R2. Neither Figure includes any information about model accuracy or projection accuracy, with respect to the true physical climate.

The reviewer's follow-up statement that, "*Nevertheless, Figs. 1 and 2 strongly suggest that the magnitude of present-day biases is not a meaningful measure for the uncertainty in the future change of the bias.*" amounts to a claim that Figures having no information about accuracy can nevertheless inform us about accuracy.

Again, to reiterate the central point, the reviewer's "*present-day biases*" (ΔB) is not a forcing bias, but an uncertainty statistic. This mistake perfuses the review and removes from it virtually any critical merit.

References:

[1] Bevington, P.R. and D.K. Robinson, Data Reduction and Error Analysis for the Physical Sciences. 3rd ed. 2003, Boston: McGraw-Hill. 320.

[2] Taylor, B.N. and C.E. Kuyatt., Guidelines for Evaluating and Expressing the Uncertainty of NIST Measurement Results. 1994, National Institute of Standards and Technology: Washington, DC. p. 20.

[3] Lauer, A. and K. Hamilton, Simulating Clouds with Global Climate Models: A Comparison of CMIP5 Results with CMIP3 and Satellite Data. J. Climate, 2013. 26(11): p. 3823-3845.

[4] Hargreaves, J.C. and J.D. Annan, Using ensemble prediction methods to examine regional climate variation under global warming scenarios. Ocean Modelling, 2006. 11(1-2): p. 174-192.
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[6] Rasch, P.J. and J.E. Kristjánsson, A Comparison of the CCM3 Model Climate Using Diagnosed and Predicted Condensate Parameterizations. J. Climate, 1998. 11(7): p. 1587-1614.
[7] Tebaldi, C. and R. Knutti, The use of the multi-model ensemble in probabilistic climate projections. Phil. Trans. Roy. Soc. A, 2007. 365(1857): p. 2053-2075.
[8] Kiehl, J.T., Twentieth century climate model response and climate sensitivity. Geophys. Res. Lett., 2007. 34(22): p. L22710.
[9] Knutti, R., Why are climate models reproducing the observed global surface warming so well? Geophys. Res. Lett., 2008. 35(18): p. L18704, 1-5.

Patrick Frank 4 June 2016
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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_{CO2} (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 20^{th} 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_{CO2} 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_{CO2} and F_{wv} to the SRES simulations. That is the derived F_{CO2} 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_{CO2} is very near the F_{CO2} 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_{CO2} 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_{CO2}."

6.3.1 F_{CO2} 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_{CO2} 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_{CO2} 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_{CO2} 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_{CO2} 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 $F_{CO2} = 0.42$ is sufficient to show that the PWM can successfully emulate the air temperature projections of advanced climate models. That is, when $F_{CO2} = 0.42$, 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_{CO2} 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_{CO2} = 0.42 coefficient was derived directly from parameters taken from Manabe and Wetherald. The reviewer has missed the point that F_{CO2} 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_{CO2} = 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_{CO2}.*"

7.3.3.1 Response items 7.3.2.3 and 7.3.2.4 demonstrate the validity of F_{CO2} = 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_{CO2} 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 <u>simultaneously</u> 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 methods standard in the physical sciences, as cited in the references. The irony of the reviewer's comment is not lost, given the reviewer's evident 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 or critical merit in this review and therefore no basis for the received conclusion.

References:

[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.

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

Response to Reviewer #3

The author thanks this reviewer for the constructive comments.

The manuscript has been revised throughout, following this reviewer's concerns and suggestions. The author hopes the reviewer finds it easier to read.

The following comments step through the reviewer's points.

Zero-order point: An alphabetically ordered "Table of Terms and Acronyms" has been added at the head of the manuscript.

In addition, the Introduction has been expanded with several new paragraphs that describe the logical structure of the analysis, including the rationale for Section 2.1, the mean-free-path and radiative transfer analysis.

p. 2: "Theory-bias" now is hyphenated throughout. This term is now also defined in the Table of Terms and Acronyms.

p. 5: "futures" is now removed and the phrase has been modified to "... X_N is a prediction of a future state..."

p. 7 (first point): "lower limit of resolution"; this term has now been defined with the added sentence, "That is, a GCM cannot resolve the effect of a tropospheric thermal flux perturbation that is smaller than its LWCF error."

This sentence now introduces the concept of a resolution lower limit, in addition to the discussion under Section 2.4.1.

p. 7 (second point): The rationale for the analysis in Section 2.1 is now given in paragraphs 13 and 14 of the Introduction.

To summarize, one wants to know where zero forcing occurs in the extrapolation of the Manabe temperature vs. CO_2 (ppm) plot (Figure 1b). Therefore, one must first know the ppm of CO_2 at which CO_2 forcing is zero. As it turns out, $CO_2 \leq 1$ ppm has zero net forcing.

p. 14: "PWM" is now defined where it first appears. The definition is also in the Table of Acronyms.

p. 16: i) Figure 2 is now in color. The author hopes this makes the Figure easier to interpret. All the other Figures are now in color, as well.

ii) The GISS model is now readily identifiable in Figure 2a. The relevant text has been revised for clarity.

iii) All the relevant figures now have the same "Anomaly Temperature (ΔC)" ordinate label.

pp. 18, 19: Figure 3 and Figure 4 have been remade and are now in color. The author hopes the reviewer finds them acceptable.

p. 22: i) Lag-1 is now defined in the Table of Terms and Acronyms. The meaning of a lag-1 plot is now included in paragraph 1 of Section 2.3.1.

ii) Figure 6 and legend have been revised for clarity. Much of the legend from prior Figure 6 has been revised and moved into the text.

iii) The opening paragraphs of Section 2.3.1 have been revised to clarify the method and meaning of the inter-correlation of model total cloud fraction (TCF) error.

iv) I am not sure what the reviewer means by "uncontrolled experiments." The TCF data are part of the 25-year (1980-2004) CMIP5 GCM hindcast experiment. In this experiment, the simulated cloud cover was compared with the cloud cover observed by satellite.

Figure 5 shows the error residual, namely the difference between the 25-year average of observed and simulated global cloud cover.

The reviewer is certainly correct that the terrestrial climate does not possess an averaged cloud cover. Nevertheless, construction of averages from observations can produce a very useful understanding of the compliance of a simulated climate with the physically real climate.

The experiment and the GCM outputs are described in Section 6 and Figure 10 of the Supplemental Information (SI). Reference to the SI is made in Section 2.3 paragraph 2 and Figure 5 legend.

Discussion of the meaning of Figure 6 has also been revised for clarity.

p. 23-24: The reviewer is thanked for this warning. Table 1 has been modified to remove the error. Originally, Table 1 was rotated 90^{o} to allow for more space and a larger font. However, doing so interfered with conversion of the document into a pdf. One hopes this can be rectified in any final production.

Minor points:
p. 37: this paragraph has been revised in light of the reviewer's recommendation.
p. 41: Reference 30 is fixed, thank-you.
p. 43: Reference 63 is fixed, thank-you
p. 43: Reference 64 is fixed, thank-you