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 pervasively 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, <u>not</u> 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*:

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

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 ±*U,* is not a positive forcing bias, +*B*.

2.3.3. Third mistake: correcting +B to ±*U* brings to the fore that the reviewer has ignored the fact that ±*U* arises from an inherent theory-error within the models. Theory error injects a simulation error into every projection step. Therefore ±*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 $\pm U_i = \pm 4$ Wm^{-2} average per annual step, after 100 annual steps then becomes $\pm U_t = \sqrt{\sum_{i=1}^{100} (\pm 4)_i^2}$ = ±40 Wm^{-2} uncertainty, not error, and $\pm T_{UNC}$ = 0.416(±40) = ±16.6 K, i.e., the manuscript result.

2.3.4. Fourth mistake incorporates two mistakes. In writing, "*a bias change ΔB = ±4Wm*$^{-2}$ *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×$(\Delta F+\Delta B)$. 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., "±4Wm^{-2}." How is this possible?

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

Corrected eqn. R4 says ERROR = $\Delta\Delta T$ = 0.416×$(\Delta F\pm\Delta U) = \Delta T_F\pm\Delta T_U$ 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 Wm^{-2}, then from the reviewer's R4 with a corrected *±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 $B_t - B_0 = \Delta B$ = ±4 Wm^{-2} (applying the 2.3.1 "±" correction).

The reviewer has supposed B_0 = ±4 Wm^{-2}. However, reviewer's ΔB is also ±4 Wm^{-2}. Then it must be that B_t-±4 Wm^{-2} = ±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, $\Delta B_{t=100} = \pm 4\ \mathrm{Wm}^{-2}$.

However, the ± 4 Wm^{-2} is the empirical average annual 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., $0 \rightarrow 1$, $U_0 = 0$ 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, $0 \rightarrow 1 \rightarrow 2$, 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 = ±4 Wm^{-2}, the above example yields a three-year simulation temperature uncertainty variance of σ^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 ±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 *±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. *±ΔB* 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 ±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×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.

[5] Knutti, R., et al., Challenges in Combining Projections from Multiple Climate Models. J. Climate, 2010. 23(10): p. 2739-2758.

[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.