Patrick Frank 7 September 2014
Propagation of Error and the Reliability of Global Air Temperature Projections
Manuscript # JOC-14-0316

Response to Reviewer 1

Summary

- The reviewer has mistaken error statistics for energetic perturbations, e.g., items 8.1, 18.2, 18.3, and 20.16.2.
- The reviewer has mistaken propagated error for model response, e.g., items 13.1, 13.3, 18.3 and 20.16.2.
- The above mistakes cause the reviewer's Stefan-Boltzmann analysis to be entirely in error, items 18ff.
- The reviewer has incorrectly supposed that the PWM does not emulate GCM projections with non-linear forcing, e.g., item 5 and Figure R1-1.
- The reviewer has repeatedly confused a mean of errors with the error of a mean state, items 7.2, 8.2, and 20.13.
- The reviewer incorrectly implied that GCMs produce unique predictions, e.g., item 13.5 and Figure R1-2.
- The reviewer has several times unintentionally validated the manuscript error analysis, e.g., items 5.4, 5.5, 16.2, and 18.3.
- These mistakes remove any scientific force from the review.

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

Detailed Response
Part I

> 1. *This paper, which has its genesis in a similar exposition by Frank (2008) in 'Skeptic' magazine ...*

1. The origin of this paper was the author's discovery that climate model air temperature projections are just linear extrapolations of greenhouse gas forcing. The Skeptic paper was the first peer-reviewed publication exploring the consequences of this discovery.

> 2. *... and has gone through a number of previous submissions to the technical literature.*

2. The cover letter to the IJOC editor explained that the manuscript has gone through full submissions twice at JGR-Atmospheres. Reviewer 1 participated in the second JGR submission. Unfortunately, the mistakes present in that first review re-appear here in the second.

3. *I have previously reviewed this manuscript and since the submission has not changed in any important way, my review here is necessarily similar to my earlier one. I note a few of the more trivial errors have been corrected, but the fundamental conceptual problems remain.*

3. It appears the editor at JGR-Atmospheres did not provide this reviewer with the author response document. Had the editor done so it seems unlikely Reviewer 1 would have chosen to repeat his prior review.

4. *The author purports to emulate the result of GCM calculations under specific scenarios with a linear, instantaneous function of the forcings.*

4. "Purports" is incorrect. Emulation is thoroughly demonstrated in manuscript Figures 2 and 6, and Auxiliary Material (AM) Figures S1 and S3 through S8.

5. *This is only successful in situations where the forcing is basically linear since the thermal inertia of the climate system is ignored.*

5.1. The reviewer is incorrect. AM Figure S11 is reproduced below for convenience. The emulation with non-linear forcing is well within the envelope of variation produced by advanced GCMs [Knutson, TR *et al.*, 2013; Lewis, SC and Karoly, DJ, 2013].

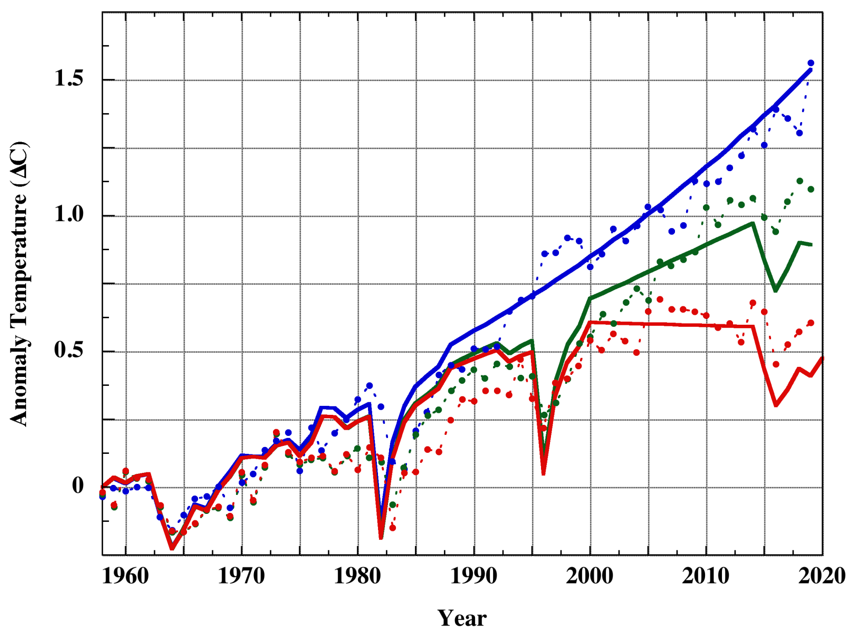

Figure R1-1 (AM Figure S11): (points), scenarios A, B, and C including non-linear forcings from the Agung and El Chichon volcanic events, from [Hansen, J *et al.*, 1988], and; (lines), the same scenarios calculated using manuscript eqn. 6, the scenario forcings [Schmidt, GA, 2007], and the volcanic perturbations.

Also, compare Figure R1-1 with WG1 AR5 Figure 9.8a in Chapter 9, Figure 10.1a in Chapter 10, or Figure 1 of Box 11.1 in Chapter 11 [IPCC, 2013]. The PWM calculation is well within the GCM envelope.

5.2. The 20th century also included non-linear tropospheric forcing. Successful emulations of that air temperature record appears in AM Figures S12 and S13. The validity of eqn. 6 as a GCM emulator is fully demonstrated.

5.3. Future GHG emissions are invariably projected as smoothly varying, i.e., the reviewer's "basically linear," [Moss, RH *et al.*, 2010; van Vuuren, DP *et al.*, 2011]. In his comment 5, therefore, the reviewer has acknowledged the success of PWM eqn. 6 within the scope of the analysis.

5.4. Nevertheless, the successful emulations in Figure R1-1 show that non-linear trends in GCM projections of air temperature follow only from the linear extrapolation of non-linear forcings. That is, GCM air temperature projections are <u>always</u> linear extrapolations of forcing, no matter that the forcing itself can be non-linear.

The accurate emulations demonstrate the linearity of GCM air temperature projections. The full force of the error propagation necessarily follows. The relevance of the uncertainty analysis then inescapably includes projections incorporating non-linear forcings.

In item 5, therefore, the reviewer has inadvertently validated the entire manuscript analysis. Figure R1-1 is proof of point.

5.5. Reviewer comment 5 implicitly concedes that climate models in fact linearly extrapolate GHG forcing when that forcing is smoothly varying. In this, the reviewer has inadvertently, and for a second time, validated the force of the manuscript uncertainty analysis.

> 6. *From an emulation of the GCM temperature projections, the author derives a completely erroneous uncertainty due to cloud forcing biases ...*

6. The author did not derive the cloud forcing uncertainty, nor did that uncertainty come from the GCM emulation equation 6. The reviewer's ascription is incorrect. The cloud forcing uncertainty is from [Lauer, A and Hamilton, K, 2013]. This fact was specified on manuscript pages 23 and 30.

> 7. *... and then purports to 'propagate' errors from the mean state energy flux into absolutely nonsensical (and self-evidently wrong) temperature projections of the future and past. This is fundamentally flawed and cannot be remedied to make it publishable.*

7.1. "*purports to 'propagate' errors*" Manuscript eqn. 2 is the standard propagation of error in the physical sciences. [Bevington, PR and Robinson, DK, 2003]

Manuscript error propagation strictly conforms to the published recommendations of the Joint Committee for Guides in Metrology (JCGM) and of the US National Institute of Standards and Technology (NIST) for the expression of uncertainty. [JCGM, 100:2008; Taylor, BN and Kuyatt., CE, 1994]

The same method of error propagation is found in reliability analyses of other non-linear physical models. [Vasquez, VR and Whiting, WB, 2005]

The phrase, "*purports to propagate errors,*" is therefore wrong on its face. Errors are in fact propagated.

7.2. "*mean state energy flux*" The reviewer has mistaken the mean error of all modeled states for the error in a modeled mean state.

The ±4 Wm^{-2} CMIP5 GCM cloud-forcing uncertainty reported by [Lauer, A and Hamilton, K, 2013] is not the error in a mean state flux. It is the mean of model flux error.

The mean model error is, $\pm\bar{\sigma}_N = \sqrt{\frac{1}{(N-1)}\sum_{i=1}^{N}\sigma_i^2}$, i.e., the root-mean-square of the expectation value errors of N individual models.

In contrast, the error in a modeled mean state is, $\pm\sigma_m = \sqrt{\sum_{i=1}^{n}\sigma_i^2}$, i.e., the root-sum-square of the sequential errors made by a given model in the "n" steps of calculation to the final state representing the mean set of conditions.

The first is the mean of the errors produced by N individual models when calculating a given state; a multi-model mean. This is the cloud forcing error presented in [Lauer, A and Hamilton, K, 2013].

The second is the final uncertainty obtained by propagating the errors made by one individual model through the sequence of n calculations to the final state of the mean state of an ensemble of states. The two errors are not the same at all.

7.3. "*nonsensical (and self-evidently wrong) temperature projections*" The emulations invariably reproduce the projections of advanced bona-fide climate models. The emulations are thus self-evidently pertinent. The reviewer has denied an object demonstration.

The successful emulations demonstrate that climate model air temperature projections are just linear extrapolations of tropospheric forcing. The factually demonstrated linearity of GCM output completely justifies the linear propagation of error (ms eqn. 2).

Author Response Part II

Overall [reviewer] comments:

8. *The confusion between errors in the mean state and errors associated with a perturbation are pervasive (equivalent to conflating the error in the constant and first derivative of a complex function). See below.*

8.1. The reviewer has confused an error statistic with an energetic perturbation. The ±4 Wm^{-2} following from total cloud fraction (TCF) error is not an energy. It is a statistical mean of errors; the CMIP5 mean error in simulated long-wave cloud forcing.

To apprehend this distinction, the reviewer might contemplate the physical impossibility of an energetic perturbation that has simultaneously positive and negative magnitudes, i.e., ±4 Wm^{-2}.

Confusion of an uncertainty with an energy perfuses the reviewer's entire commentary.

8.2. Further, as noted in item 7.2 above, the ±4 Wm^{-2} cloud forcing error is not the error of a mean state, but the multi-model mean of error.

The reviewer is confusing model error in the simulation of a mean state with a statistical mean of model errors.

9. *The author's claim that published projections do not include 'propagated errors' is fundamentally flawed.*

9.1 None of the published literature critically examining GCM error includes, suggests, or even mentions propagated error.

9.2 The reviewer has provided no example of published propagated GCM error.

10. It is clearly the case that the model ensemble may have structural errors that bias the projections, but these are not derivable in the manner the author claims.

10. As noted in item 6, the author did not derive any "*structural errors that bias the projections*." The CMIP5 cloud forcing error was determined by [Lauer, A and Hamilton, K, 2013]

11. Indeed, he has not demonstrated that any of the errors in the climatology that he highlights are even correlated to differing model outcomes, let alone dominant sources of projection spread.

11. Manuscript Table 1 demonstrates the high correlation of cloud fraction error among CMIP5 models.

    The manuscript is nowhere concerned with "*differing model outcomes*" or "*dominant sources of projection spread*". Neither topic makes any appearance in the manuscript. Reviewer comment 11 is thus an irrelevance.

    The manuscript is concerned with the impact of systematic theory-bias error on the reliability of model projections. Section 2.4.2 (p. 32) centrally discusses the meaning of propagated error and uncertainty.

12. The application of naive error propagation theory for a compound measurement to this case completely ignores the feedbacks and complexity of the models that render any assumption of statistical independence between subsequent errors null and void.

12.1. When an observation-based error is empirically demonstrated, ontological statistical assumptions are superfluous to subsequent uncertainty analysis. The ±4 Wm^{-2} long-wave annual cloud forcing error is empirical and observational.

Both [Lauer, A and Hamilton, K, 2013] and [Jiang, JH *et al.*, 2012] demonstrate that TCF error persists across a very large number of sequential calculational steps. This frees TCF error from any statistical assumptions concerning independence of subsequent-step error.

12.2. The " *feedbacks and complexity of the models* " is rendered entirely irrelevant by the factual demonstration that model projected air temperature is linear and simple.

12.3. The average CMIP5 cloud forcing error reported by [Lauer, A and Hamilton, K, 2013] included 27 models integrated over 20 years of simulated terrestrial cloud cover. It therefore summarizes an empirically-derived error that persisted across a multi-model 540-year average.

The reference against observations, the integration over multiple models, and the persistence across long-baseline averaging, demonstrate that the ±4 Wm^{-2} long-wave cloud forcing error persistently deviates climate model simulations.

The final-state impact of persistently repetitive error is found by propagating that error. There is no escape from this standard of science.

13. The demonstration of the fallaciousness of the reasoning demonstrated here is obvious upon assessing any other climate change simulated by the models. For instance, looking the last glacial maximum, the same models produce global mean changes of between 4 and 6 degrees colder than the pre-industrial. If the conclusions of this paper were correct, this spread (being so much smaller than the estimated errors of +/- 15 deg C) would be nothing short of miraculous. It isn't because they aren't.

13.1. The reviewer has mistaken the confidence interval (CI) for a thermodynamic magnitude; the same reviewer mistake was discussed in item 8. It is disappointing to find so basic a misstep in a critical review.

13.2. The ms shows that GCMs linearly extrapolate forcing. Therefore, model thermal flux error (long wave TCF error) propagates linearly through the projection. Reference to simulations of other climates is an irrelevance.

13.3. The reviewer has supposed that a CI of ±15 C implies a physical ΔT = 15 C. Mere inspection of Figures 5 and 6 show this is not correct. Model expectation values are completely independent of confidence intervals. Models are tuned to yield discrete expectation values, and do so. Confidence intervals tell us whether those discrete values have any meaning.

A CI is an ignorance width. A CI larger than any possible physical response means that the predicted state conveys no physically meaningful information. A CI = ±15 C means the simulated climate has no knowable correspondence with the physical state of the future climate.

13.4 The reviewer's 4 to 6 C difference has scientific force only if GCMs produce predictively unique solutions to the climate energy-state. However, Figure R1-2 shows that GCMs do not produce a unique air temperature for any given climate energy-state.

13.5. For GCMs, multiple climate states can produce a given air temperature (see Figure R1-2 below). Therefore, reproducing a known air temperature does not mean that the GCM has correctly reproduced the underlying climate state.

For example, Figure R1-2 below, taken from Rowlands, et al., [Rowlands, DJ *et al.*, 2012], displays thousands of perturbed physics centennial simulations made using the UK Met HadCM3L.

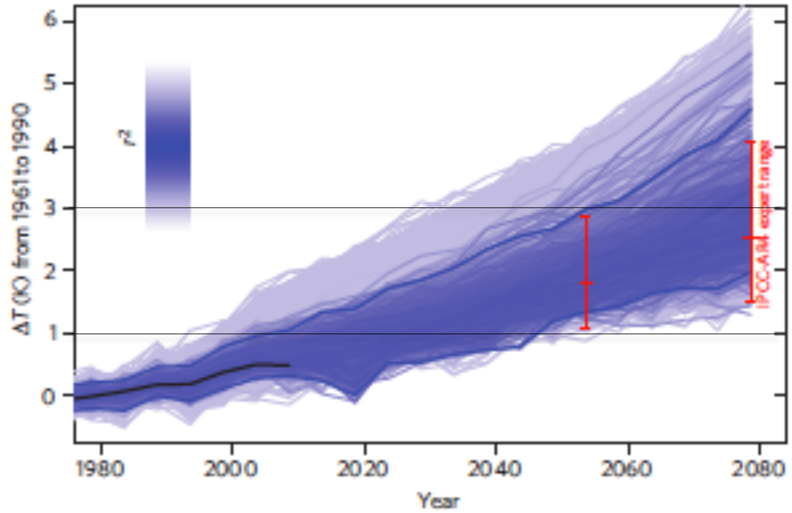

Figure R1-2. Original Legend: "**Evolution of uncertainties in reconstructed global-mean temperature projections under SRES A1B in the HadCM3L ensemble.**"

The two black horizontal lines at ΔT= 1 K & 3 K have been added. These lines traverse the ensemble, showing that hundreds to thousands of different simulated climate states can produce the same air temperature anomaly.

Figure R1-2 shows that the 4-6 C paleo-temperature in reviewer comment 13 is not a unique solution. It therefore does not verify the physical validity of climate models.

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

The thick black line within the simulation envelope, part of the original figure, represents observations. Even those air temperature simulations near the observation line cannot be known as 'more correct' because the underlying climate states they represent are not unique solutions to the energy state. All the projected states, including those near the observed state, will be associated with large uncertainties; uncertainties that are left only implicit in the above figure.

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

13.6 Review comment 13 incorrectly identified a ±15 C CI as an actual spread in projected air temperatures. It is not. This very basic misunderstanding unfortunately ramifies throughout the review.

The meaning of a confidence interval as an ignorance width is first discussed on manuscript p. 35, line 10ff, and in detail in Section 3, p. 45, lines 10-56. Section 10 in the AM, "*The Meaning of Uncertainty in Model Projections*" is entirely given over to this topic. However, the reviewer has apparently overlooked these guides.

13.7 In general, boundary conditions do not limit propagated uncertainty. They limit model response. When uncertainty grows beyond physical bounds, no physical meaning remains in the predicted state. In the absence of a unique solution none of all the possible alternative iso-energetic states can be eliminated.

This is the message of the large CIs. The same message is implicit in Tebaldi and Knutti, 2007.

> 14.1 *Going more deeply into the methodology here, equation 6 is the key assumption, that temperature changes are the instantaneous response to the normalized forcing relative to some t0.*

14.1 Eqn. 6 represents a hypothesis, not an assumption. The hypothesis is that GCMs linearly extrapolate forcing when projecting an air temperature trend. The hypothesis is fully validated in Figures 2 and 6, and AM Figures S1, S3-S8 and S11.

> 14.2 *However, by the logic so far, it is clear that this delta(T) does not include any feedbacks to radiative forcing (since both the '0.42'was calculated holding everything else constant).*

14.2 The reviewer has again misunderstood the analysis, which pertains to the behavior of climate models, and not to physics.

Generalized eqn. 6 successfully emulates GCM air temperature projections. Mere inspection of this success makes obvious that further feedbacks are irrelevant to the error analysis.

> *14.3 The F0 is in the right ballpark for the radiative forcing associated with complete removal of the main GHGs. The formula give 13.9 K for the pre-industrial climate (i.e. 1900 in this case), and so the difference in temperature from the pre-industrial is given by Tanom=13.9 * Fanom/F0 = 0.41 * Fanom. Note that this gives a climate sensitivity to 2xCO2 (Fanom=4W/m2) of ~1.6C, somewhat higher than the expected 1.2 deg C no-feedback response generally estimated. The match of this PWM to the GCM output is simply because the transient climate response (TCR) (which takes into account ocean thermal inertia etc.) is close to 1.6 C, but this is entirely fortuitous.*

14.3.1. The reviewer has conceded that ms eqn. 6 produces projections that match those of GCMs that do include ocean thermal inertia. I.e., "*The match of this PWM to the GCM output...*"

This admission vitiates the reviewer's own criticism, here and in item 5 above, that ms eqn. 6 does not include a term for ocean thermal inertia. Such a term is demonstrated to be superfluous. The reviewer has inadvertently verified this point.

14.3.2. The 0.42 coefficient of the PWM is derived from the climate model of [Manabe, S and Wetherald, RT, 1967] and applied to the PWM response to forcing.

The PWM (ms eqn. 6) successfully emulates output to the transient climate response of GCMs in part because the "0.42" coefficient automatically includes the constant relative humidity modeled by [Manabe, S and Wetherald, RT, 1967]. Thus, the correspondence to climate model output is hardly "fortuitous."

15. Once the author decides to fit his model to the individual models, he is simply calculating an empirical TCR and all of the foregoing justification is moot.

15.1. Model-tuning also calculates an empirical TCR. The reviewer is criticizing a technique ubiquitous in climate modeling.

15.2. The justification of error propagation lays in the demonstration that GCMs linearly extrapolate GHG forcing when projecting air temperatures. The use of PWM coefficients relevant to individual GCMS in no way makes moot the central truth that GCMs linearly project forcing. The reviewer's criticism thus misses the point.

The demonstrated linearity of output completely justifies linearly propagated uncertainty. The successful emulations of generalized eqn. 6 extend this justification to multiple advanced GCMs.

15.3. GCMs all have different TCRs, e.g., see ms Figure 2, Auxiliary Figures S1, S3-S8, or any multi-model projection ensemble. Figure R1-2 above shows that under conditions of "perturbed physics," even a single GCM has varying TCRs. The reviewer is therefore criticizing ms eqn. 6 for successfully emulating a property of GCMs.

16. Of course, this will only work with scenarios that have roughly linearly increasing forcings. Any stabilisation or addition of large transients (such as volcanoes) will cause the mismatch between this emulator and the underlying GCM to be obvious.

16.1. AM Figure S11 (response Figure R1-1 above), and AM Figures 12 and 13 refute this reviewer claim. Eqn. 6 is successful with non-linear forcings.

16.2. As already noted (item 5), the error analysis specifically concerns air temperature projections resulting from proposed trends in future tropospheric GHGs. As these trends are "roughly linear" in the reviewer's sense, the admission in item 16 has the reviewer inadvertently validating the error analysis.

> 17.1. *The author than takes the uncertainty in the TOA energy fluxes (for which he uses +/-4W/m2) and then assumes that this is also the uncertainty in the annual forcings.*

17.1. The reviewer is incorrect. There was no such assumption. [Lauer, A and Hamilton, K, 2013] (p. 3833) describe their statistic as the annual "rmse," (root-mean-square-error) error. I.e., "*The overall comparisons of the annual mean cloud properties with observations are summarized for individual models and for the ensemble means by the Taylor diagrams for CA, LWP, SCF, and LCF shown in Fig. 3.*" Underlining added.

The Legend to Lauer and Hamilton Figure 3: "*Taylor diagrams showing the 20-yr annual average performance of the (top) CMIP3 and the (bottom) CMIP5 models for (left to right) CA, LWP, and ToA SCF and LCF as compared to satellite observations*." Underlining added.

That the ±4 Wm^{-2} is an annual average long wave cloud forcing error of CMIP5 GCMs is a fact, and was not assumed.

> 17.2. *...and then assumes that this is also the uncertainty in the annual forcings. This is clearly nonsense, precisely for the reasons he earlier mentioned - i.e the cloud forcing errors are systematic - not random.*

17.2. The reviewer has failed to explain how a set of systematic multi-year multi-model errors cannot have an annual average.

> 17.3. *Assuming that there is an additional random component in the forcings of this size produces error bars that are effectively a random walk and therefore will increase without bound over time. This neither matches what the models actually do, nor is it physically justifiable.*

17.3.1. The author has not assumed any "additional random component." The reviewer assessment is thus misplaced. Manuscript Section 2.4.2 provides the physical justification for the increase in CI with projection step. In short, uncertainty in the projected physical state increases with every error-burdened calculation within a step-wise simulation. See also ms p. 48, lines 46ff.

17.3.2. The "*[not] physically justifiable*" comment indicates that reviewer has also misunderstood the meaning of a confidence interval. When errors are step-wise correlated, as are TCF errors, their propagated CIs necessarily grow without bound. When the CI is very large the calculated expectation value has no physical meaning. This is the standard of scientific error analysis.

> 17.4. *(For instance, even after forcings have stabilized, this analysis would predict that the models will swing ever more wildly between snowball and runaway greenhouse states. Which, it should be obvious, does not actually happen). Given that the PWM is supposed to be an emulator of the GCM results, this a-physical result somewhat undermines its utility.*

17.4.1. The reviewer is once again mistaking a confidence interval for a thermodynamic magnitude. Plus/minus CIs do not imply model oscillation. They do not describe model behavior. Models always produce discrete expectation values. CIs describe the uncertainty in those discrete values.

17.4.2. The PWM is demonstrated to be an emulator of GCM results. None of the reviewer's criticisms have undermined this conclusion.

> *18. I will give (again) one simple example of why this whole exercise is a waste of time. Take a simple energy balance model, solar in, long wave out, single layer atmosphere, albedo and greenhouse effect. i.e. sigma Ts^4 = S (1-a) /(1 - lambda/2) where lambda is the atmospheric emissivity, a is the albedo (0.7), S the incident solar flux (340 W/m2), sigma is the SB coefficient and Ts is the surface temperature (288K). The sensitivity of this model to an increase in lambda of 0.02 (which gives a 4 W/m2 forcing) is 1.19 deg C (assuming no feedbacks on lambda or a). The sensitivity of an erroneous model with an error in the albedo of 0.012 (which gives a 4 W/m2 SW TOA flux error) to exactly the same forcing is 1.18 deg C. This the difference that a systematic bias makes to the sensitivity is two orders of magnitude less than the effect of the perturbation. The author's equating of the response error to the bias error even in such a simple model is orders of magnitude wrong. It is exactly the same with his GCM emulator.*

The reviewer should have consulted AM Section 10.2, which explicitly showed that the Stefan-Boltzmann analysis in 18 is in error. Nevertheless, the salient mistakes are reiterated in 18.1 - 18.3 below.

18.1. The reviewer has written the lambda or albedo error statistic as a positive offset. This is a mistake. The error statistics should be written as ±0.02 or ±0.012, respectively, i.e., as the root-mean-square. The respective uncertainties are then ±4 Wm^{-2}; again not the constant positive 4 Wm^{-2} offset represented by the reviewer.

18.2. The ±4 Wm^{-2} is not an energetic perturbation. It is an uncertainty statistic. The ±4 Wm^{-2} does not produce an expectation value. It produces a confidence interval. As noted above, this mistake permeates the review. As elsewhere, this mistake completely vitiates the reviewer analysis.

18.3. As the ±4 Wm^{-2} is not an energy, the 1.19 ºC and 1.18 ºC calculated by the reviewer are not model responses and are not correctly represented.

They are the model uncertainties in calculated temperature produced by a ±4 Wm^{-2} uncertainty induced by lambda or albedo error.

Their correct representation is ±1.19 ºC and ±1.18 ºC, respectively, because the "perturbation" used by the reviewer is a ±4 Wm^{-2} error statistic, not a positive 4 Wm^{-2} energy magnitude.

The corrected uncertainty values, ±1.19 ºC and ±1.18 ºC, are similar in magnitude to the GCM projection uncertainties calculated using the PWM. Propagating those uncertainties through an air temperature projection will produce CI envelopes analogous to those presented in the ms.

The reviewer has thus inadvertently validated the manuscript error analysis for a third time.

Reviewer comment 18 unfortunately shows no understanding of the meaning of physical error, or the meaning of propagated error.

TCF error is a lower limit of resolution of the model regarding the thermal energy flux of the troposphere. Sequential propagation of that error into projected future climate states causes the subsequent states to be less well known than the initial state. The uncertainty in future air temperatures can only increase with each calculational step.

This explanation was covered in detail and repeatedly in the manuscript, e.g., under Section 2.4.2 line 20ff, Section 2.4.3, line 8ff, and especially p. 38 line 27 to p. 40 line 13, and in Section 3 p. 48, lines 23-48 and, p. 49, lines 3-38. The review comments display no awareness of this pertinent discussion.

19. The summary conclusion section is based wholly on the unsupported and erroneous results from the previous sections and does not need to be reviewed in depth.

19. The review reflects a persistent and very serious misunderstanding of physical error, of physical uncertainty, and of the meaning of a confidence interval. The review criticisms of the meaning and effectiveness of the PWM are therefore uniformly incorrect. These criticisms, including item 19 above, lack any scientific force and can be set aside.

20. *Minor comments:*

20.1. *p3. l18. IPCC AR4 high end temperatures are twice this.*

20.1. Nevertheless, 3 ºC is the typically offered mean warming prediction, [IPCC, 2007] TS 4.5, p. 65, and [IPCC, 2013] TS 5.3, p. 81.

20.2 *p6. l37. The author is simply asserting that uncertainties in published estimates are not 'physically valid' - an opinion that is not widely shared.*

20.2. The reviewer has neglected manuscript p. 4, lines 15-37, viz., the entire prior discussion of the distinction between precision and accuracy.

The reviewer also neglected the follow-on manuscript sentence:
"Instead, the standard uncertainties derive from model variability, which is a measure of precision rather than of accuracy."

Model variability -- precision -- is not a measure of physical accuracy. This view is hardly controversial and is indeed widely shared. See the Guide to the Expression of Uncertainty. [JCGM, 100:2008]

Published GCM projection uncertainties are consistently variation around a multi-model mean, reflecting precision, not accuracy. Model precision (self-similarity) has no intrinsic physical meaning.

20.3. *p6. l44. This is not true. Structural uncertainties (which go beyond individual parameter uncertainties) are very often assessed, most recently in the AR5 report. 'Systematic energy flux errors' are not a specific thing which can be propagated either.*

20.3.1. The lines in question point out the absence of propagated error in the climate modeling literature. The reviewer dismisses this, but has provided no example of propagated model error.

20.3.2. Assessment of physical errors ("structural uncertainties"), e.g., as in [Jiang, JH *et al.*, 2012; Lauer, A and Hamilton, K, 2013], is not propagation of error.

20.3.3. [Lauer, A and Hamilton, K, 2013] assessed CMIP5 models to produce a specific $\pm 4 Wm^{-2}$ average annual error in long-wave cloud forcing. This error is persistent through multi-year climate simulations.

Propagation of persistent error through a multi-step calculation is a very standard means of evaluating the uncertainty in a final state. See, for example, Section 5.2 of [JCGM, 100:2008], "Determining the Combined Standard Uncertainty; correlated input quantities." The reviewer's dismissal is superficial and incorrect.

20.4. *p9. l48. This is fundamentally wrong. Calculating the magnitude of the* *greenhouse effect by removing a single absorber (in the case CO2) does not produce the effect of what remains due to the spectral overlaps. The paper by Schmidt et al (JGR, 2010) shows clearly that this will underestimate the effect of CO2 (i.e. their Table 1).*

20.4.1. [Manabe, S and Wetherald, RT, 1967] included the effects of overlapping gas absorptions (water vapor, CO_2, and O_3), as described in [Manabe, S and Strickler, RF, 1964], when they calculated the variation of temperature with $[CO_2]_{atm}$ under clear and cloud-covered skies. The reviewer's criticism is thus unfounded.

20.4.2. [Manabe, S and Strickler, RF, 1964] noted (p. 371 and Figure 6c) that, "*The influence of CO_2 upon the temperature of the earth's surface is about 10 deg.*" This 10 K represented the greenhouse temperature difference between an atmosphere including CO_2 + H_2O vs. one with H_2O alone.

Under the contemporary (1967) estimate of 58% average global cloud cover, the methodology of manuscript Figure 1b yielded the very comparable 12 K [Frank, P, 2008].

20.4.3. [Schmidt, GA *et al.*, 2010] cited by the reviewer employed, "*the IPCC AR4 version of GISS ModelE to calculate the instantaneous changes in radiative fluxes to changes in individual LW absorbers...*"

However, different GCMs clearly calculate different responses to the thermal impact of GHGs (see [Kiehl, JT, 2007], [Schmidt, GA *et al.*, 2010], ms Figure 2, and AM Figure S1 and Figures S3-S8).

The reviewer's argument thus unjustifiably privileges Model E as definitive.

20.4.4. The fact that the derived f_{CO_2}=0.42 fraction contributes successfully to emulating GCM air temperature projections completely vitiates the force of reviewer item 20.4 as regards the manuscript uncertainty analysis.

Likewise vitiating is the fact that virtually any GCM global air temperature projection can be emulated merely by varying f_{CO_2} between 0.37 and 0.94 (Tables S1-S3).

20.5. *p10 to p13: this is all completely irrelevant for what is actually being done. Firstly, the whole calculation is contradictory to the earlier claim that this paper is purely concerned with GCM results and not the real world. Secondly, the answer would have been better derived directly from sensitivity studies from radiative/convective models themselves (ie. Ramanathan and Coakley, 1993). Third, the answer is wrong because of the neglect of spectral overlaps (which are as present in the models as they are in the real world).*

20.5.1. Please refer to the caveat expressed on manuscript p. 8 line 29: "The analysis is not an investigation of the physics of climate, but of the behavior of GCMs." That is, the analysis is excused from treating the physics of climate, not from physics itself.

20.5.2. The reviewer probably refers to [Ramanathan, V and Coakley, JA, 1978], which does not estimate the onset of CO_2-induced warming. Determining this onset is the focus of manuscript section 2.1.

However, [Ramanathan, V and Coakley, JA, 1978] do state (p. 468) that, "*At small wave numbers* $\nu = 1/\lambda \leq 2500\ cm^{-1}$ ($\lambda \geq 4\ \mu m$) *the extinction of radiation due to scattering in the earth's atmosphere is small in comparison with the extinction due to absorption, and as a result the scattering of long-wave radiation is often neglected altogether.*" This validates the 15 μ absorbance mean free path calculation in manuscript section 2.1.1.

[Ramanathan, V and Coakley, JA, 1978] likewise specify the cosine relation "*between the ray path and the vertical,*" as was also used in the manuscript calculation.

Additionally, the $[CO_2]_{atm}$ necessary for the onset of significant forcing was not evaluated in [Schneider, SH, 1975]; the paper referenced by Ramanathan and Coakley as summarizing the effect of increasing CO_2 on the global surface temperature.

20.5.3. The mean free path with respect to absorption by CO_2 is not sensitive to absorption by water. The fact that the transmitted intensity of surface LW radiation is reduced by simultaneous absorption by water vapor affects the mean free path of a 15 μ photon through the atmosphere. It does not affect the intrinsic *1/e* 15 μ mean free path transmittance of $[CO_2]_{atm}$ itself, which is dependent on $[CO_2]_{atm}$ alone.

20.6. *p16. l47. There is no 'asymptote' at 1ppm CO2.*

20.6. On the positive abscissa *y=log[f(x)]* gets arbitrarily close to, but never reaches, *f(x)=1*. This well-known limit is commonly called a vertical asymptote, as an internet search of "logarithmic vertical asymptote" will reveal for the reviewer.

20.7. *p17. l17-40. All of the calculation here is just wrong. First, the attempt is being made to calculate the no-feedback contributions to the 33K GHE. The results of Hansen et al (1988) are applicable here and show that about 7K of the 33K is directly attributable to the single factor removal of CO2. Allowing for overlaps (as discussed in Schmidt et al (2010)), you would get a slightly larger value of ~10K. Note that percentage attribution of the temperature change is not the same as for the LW absorption (because of the non-linear dependence of LW on T^4).*

20.7.1. The manuscript analysis incorporates the calculations of [Manabe, S and Strickler, RF, 1964], which gave the 'more correct' 10 K estimate. Item 20.4.2 observed that when the methodology used the contemporaneous global fractional cloud cover value available to those authors, it produced a very similar CO_2-alone contribution of 12 K, [Frank, P, 2008].

20.7.2. The reviewer is now privileging the results of GISS Model II as definitive (see 20.4.3). Other models produce other values.

> 20.8.1. *p18 l51. The author's definition of the 'water-vapour enhanced' CO2 forcing confuses a forcing with the feedback and is fundamentally confused.*

20.8.1. Manuscript page 9, line 18 defines water vapor enhanced CO_2 forcing as, "*the sum of intrinsic CO_2 forcing with the calculated positive feedback following from the condition of atmospheric constant relative humidity.*" with reference to [Held, IM and Soden, BJ, 2000].

That is, effect of water vapor is described as a feedback. The reviewer apparently missed this definition, which is completely consistent with the reviewer's outlook. There is no confusion at all of feedback with forcing.

Further, "water-vapor enhanced" is an adjectival phrase modifying "CO_2 forcing." The phrasing does not imply that water vapor itself is a forcing. Rather, it implies that water vapor affects CO_2 forcing in such a way as to increase the magnitude.

The concept that water vapor "enhances" CO_2 forcing is also found in the literature. It ramifies throughout [Harvey, LDD, 2000], is also found in [Ramanathan, V et al., 1979] section c, p. 4953: "*Since the troposphere tends to conserve relative humidity, the tropospheric H_2O content would increase with increased tropospheric temperature, with both effects, resulting from increased CO2, enhancing the tropospheric downward longwave emission to the surface.*" and in [Philipona, R *et al.*, 2005], "*... strong water vapor feedback, enhancing the [anthropogenic greenhouse] forcing...*"

The sources show that "water-vapor enhanced CO_2 forcing" is a concept found in the literature. None of the other reviewers have taken issue with it.

> 20.8.2. *Since it is a forcing, the temperature is fixed for this calculation, therefore so is the relative humidity and therefore the answer is exactly the same as for the CO2 forcing itself.*

20.8.2. The response fraction of eqn. 6, which determines the calculated temperature, is derived from the constant relative humidity calculations of [Manabe, S and Wetherald, RT, 1967], cf. Table 4.

The constant absolute humidity calculations of [Manabe, S and Wetherald, RT, 1967] yield the zero water-vapor feedback condition; the reviewer condition of CO_2 forcing itself. Figure R1-3 and the associated text below show this result.

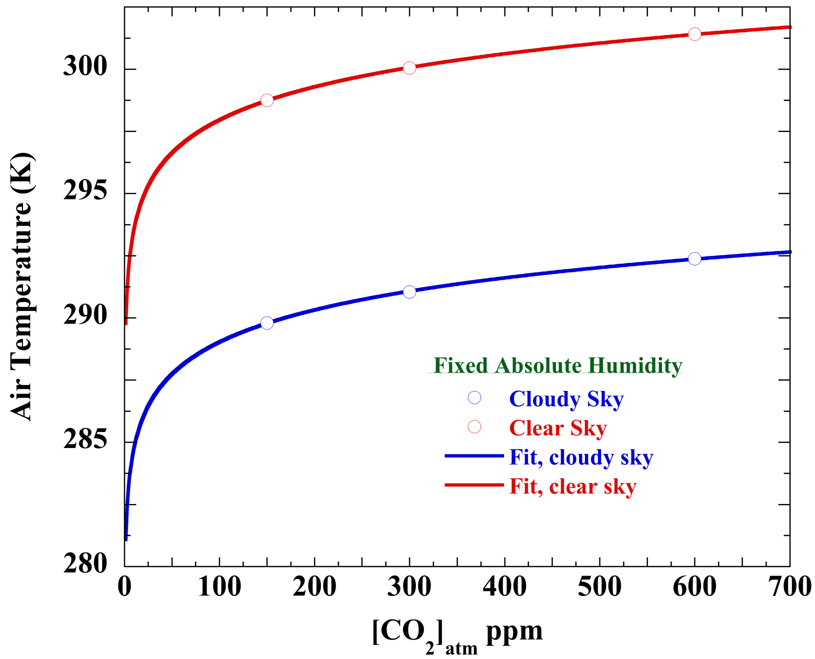

Figure R1-3: The equilibrium surface temperature under conditions of clear (o), or cloud-covered sky (o), fixed absolute humidity, and varying concentrations of atmospheric CO_2, taken from Table 4 of [Manabe, S and Wetherald, RT, 1967]. The fits are: clear sky, $T(K) = 1.92\times\ln(CO_2)_{ppm} + 289.13$, $R^2 = 0.9998$; cloud-covered sky, $T(K) = 1.86\times\ln(CO_2)_{ppm}+280.46$, $R^2 = 0.9997$.

From the fit-intercepts at $[CO_2]_{atm}$=1 ppm and global average cloud fraction = 0.67 [Jiang, JH *et al.*, 2012], the equilibrium surface temperature under conditions of negligible CO_2 forcing is 283.35 K. For T_{TOA} = 254 K and the unperturbed surface T_{GH} = 33 K, the derived fraction of unperturbed greenhouse warming due to CO_2 itself is 0.11 (3.65 K).

This fraction is not here represented as physically correct. Rather 0.11 is the fraction derived from the calculations of [Manabe, S and Wetherald, RT, 1967]. This result shows that eqn. 6 will yield a completely different answer using the uninfluenced forcing of CO_2 alone. The reviewer suggestion in item 20.8.2 is thus not correct.

> 20.8.3. *The description of the results from Lacis et al (2010) is wrong. Their result is that ~20% of the GHE is attributable to CO2, and this has nothing to do with the model's climate sensitivity.*

20.8.3. The IPCC 5AR Summary for Policy Makers, Section D2 "*Quantification of Climate System Responses*" defines climate sensitivity as, "*the change in global mean surface temperature at equilibrium that is caused by a doubling of the atmospheric CO_2 concentration.*" That is, the climate sensitivity for any GCM is the simulated climate response to a combination of the magnitude of the bare increase in CO_2 forcing and the magnitude of the induced corresponding water vapor feedback. These magnitudes apparently vary among GCMs, in view of the disparity of simulated climate responses to identical changes in climate energy states, e.g., 5AR Technical Summary Figure TS.9 and TFE.6, Figure 1.

[Lacis, AA *et al.*, 2010] find that GISS Model E calculates a CO_2 forcing fraction of 0.2 contribution to the greenhouse effect. The disparity of simulated air temperatures produced by alternative GCMs distinctly implies that other models must include CO_2 forcing fractions different from GISS Model E.

> 20.9. *p21. l20-41: this is complete nonsense. The author is equating the attribution of the climatological GHE to the changes of GHE as a function of the feedbacks. This makes no sense whatsoever.*

20.9. The analysis on p. 21 lines 20ff compares the GISS Model E fractional instantaneous CO_2 GHE reported by [Lacis, AA *et al.*, 2010] to that of other climate models. [Lacis, AA *et al.*, 2010] describe their experiment this way:

"*We used the GISS 4° × 5° ModelE to calculate changes in instantaneous LW TOA flux (annual global averages) in experiments where atmospheric constituents (including water vapor, clouds, CO_2, O_3, N_2O, CH_4, CFCs, and aerosols) were added to or subtracted from an equilibrium atmosphere with a given global temperature structure, one constituent at a time for a 1-year period. ... By normalizing these fractional contributions to match the full-atmosphere value of GF, we obtained the fractional response contributions shown in Fig. 1.*"

That is, Lacis, et al., calculated the "*instantaneous*" forcing: the transient response fraction, not the equilibrium climatological fraction.

The slopes of the projections in manuscript Figure 2a are determined by the CO_2 response function of each climate model. The slopes therefore reveal the relative magnitudes of the individual model CO_2 response functions.

Figure 1 in [Lacis, AA *et al.*, 2010] shows the GISS Model E instantaneous CO_2 fractional response is 0.2, i.e., 20% of the GHE. The same GISS Model E 20% fractional instantaneous forcing for CO_2 was reported in [Schmidt, GA *et al.*, 2010]

It is entirely appropriate to estimate the transient CO_2 responses of other climate models by comparison with the GISS transient response.

> 20.10. *p24 l34: that models have systematic biases is well known (marine stratus decks, double ITCZs, excessive southern ocean absorption). I have no idea what 'theory-bias' has to add to the already extensive literature on why these biases exist.*

20.10. The manuscript analysis makes no attempt to discuss the "why" of systematic TCF error. Systematic TCF error is assessed to address its contribution to uncertainty in projected future climate air temperatures.

The manuscript is unique in addressing the meaning and impact of systematic error on the reliability of air temperature projections. Such an accounting has never before been published.

Theory-bias introduces a systematic error of unknown magnitude into every single calculational step of a climate simulation, including that of the zeroth state. It is impossible that uncertainty will not accumulate across a projection.

> *20.11. p 26. Fig 4: this is completely irrelevant. Of course a smooth function over latitude with perhaps 2 or 3 degrees of freedom will show latitudinal correlations.*

20.11. Figure 4 shows model TCF errors with respect to observations. Were the cloud errors spatially random, they would not be smooth functions, latitudinal autocorrelation would be absent (apart from chance persistence), and cloud error would disappear in multi-year averages.

This point was explicitly made on p. 26, lines 54ff.

The point of Figure 4 is to test the above proposition. In so doing one finds model TCF errors are not spatially random. Table 1 then takes the next step, showing TCF error is correlated among models.

> 20.12. *p27 l6: why not just look at the spatial errors? of course they are non-random!*

20.12. The author agrees with the reviewer that the spatial errors appear non-random by inspection. However, analysis required demonstrating the case.

> 20.13. *p36 l27-37: this is the main paper error complete in one line. i.e. errors in the mean are not the same as errors in the perturbations.*

20.13. As noted in items 7.2 and 8.2, the $\pm 4 Wm^{-2}$ is the mean of errors, not the error of the mean. The reviewer has consistently gotten this distinction wrong.

It should not be a mystery that theory bias will produce increasing uncertainty in sequentially projected states, even if the perturbations are perfectly known.

Likewise, it should not be a mystery that the average of theory-bias errors made by an ensemble of models, with respect to an observable, will provide a general indication of model reliability for any given single state simulation. Nor should it be a mystery that propagation of the average error will provide a valuable and valid general estimate of uncertainty in a sequential projection of future states.

Error propagation is a standard of physical science. See, for example, section 3.16.2 "*Propagation of Error*" in [Emery, WJ and Thomson, RE, 2004]. See also the prescribed method and meaning of error propagation in [Bevington, PR and Robinson, DK, 2003] and [Miller, JC and Miller, JN, 1988]. It is a true puzzle why this should be opaque to the reviewer.

20.14. *p37 l20-25: Of course the AR5 mentions that it is plotting anomalies and what the baselines are.*

20.14. Manuscript 37/20ff refers removing subsequent projection error by differencing against an 1850 base-state simulation. Removal of error has little to do with production and examination of climate change anomalies. The reviewer comment is a non-sequitur.

20.15. *p37. l39 onwards: I fail to see how 'theory-bias' as a concept leads to a specific conclusion that models are partitioning energy incorrectly among climate modes.*

20.15. TCF error explicitly demonstrates that theory-bias has induced an incorrect partitioning of energy within the simulated climate state.

Climate models partition the available solar energy into the coupled physical subsystems of the global climate (atmosphere, oceans, cryosphere, etc.). Theory bias causes an incorrect partitioning of flux density among these subsystems, and produces errors in the simulated state – errors of the energy density within, and flux through, the climate sub-states.

Even if the total climate energy state is perfectly known, incorrect partitioning of that energy will produce physically incorrect climate states and sub-states. For any given total state-energy, theory-bias means that multiple simulated climates can result, exhibiting different observables. This is exemplified in Figure R1-2 and discussed in item 13.4.

TCF error is one such example. Theory biased models simulated the cloud fraction energy state incorrectly. As the total energy is determined by radiant insolation, ± cloud error alone must produce autologous ∓ errors elsewhere among other climate subsystems.

20.16.1. *p40 l46 etc. All of this assumes that the errors always add and never interact.*

20.16.1. The reviewer is confusing error with uncertainty. The reviewer has also ignored the immediately prior analysis, which is directed specifically to climate model error and includes neither of the reviewer assumptions.

For example, p. 38 lines 28ff (ε_{mi} is model error in the i^{th} simulation step): "... *theory-bias* ε_{mi} *is not known to be step-wise constant in structure, sign, or magnitude.*"

Lines 50ff: "*As the signs and magnitudes of the* ε_{m0} *and* ε_{mi} *are not known,* $\Delta\varepsilon_{m0,i}$ *may be larger or smaller than* ε_{m0}, ε_{mi}."

20.16.2 This is not valid for GCMs given the large negative feedback associated with the Planck response. This reduces the magnitude of error growth significantly.

20.16.2. The reviewer is again mistaking an error statistic for a model perturbation and a confidence interval for a model response. Statistical uncertainties are not thermodynamic magnitudes, even when the uncertainty is physically derived. Models do not radiate away uncertainty as the terrestrial climate does heat.

The growth of uncertainty, not the growth of error, determines the reliability of projections of future climate. The reason is that in a futures projection the magnitude of error is unknown. This is made clear in p. 38 line 27 through p. 41 line 37. The magnitude of error in projected future climates cannot be known. However, the magnitude of projection uncertainty can be known, and that uncertainty grows without bound with projection length.

20.17. *p42-3 etc. This is patently absurd. The scenarios A, B and C are clearly distinguishable and make clear predictions as a function of the different scenarios.*

20.17.1. In light of the foregoing, it remains true that the 1988 GISS Model II projections A, B, and C are lost within their uncertainty envelopes and have no predictive value. The same diagnosis is true for any surface air temperature projection made using any climate model up to and including a CMIP5 GCM.

20.17.2. The reviewer comment includes the implicit assumption that GISS Model II represents a fully competent theory of climate.

20.18. *p45. This response to earlier criticisms is very poor. The author claims that models are unable to predict GMSAT better than +/- 15 C because of propagation of errors associated with clouds. Yet models do not oscillate with variances that large.*

20.18. A projected GMSAT uncertainty of ±15 C does not mean the model oscillates with this or any other variance. Supposing so represents a complete misunderstanding of confidence intervals (CI).

Section 10 in the Auxiliary Material provides an extended discussion of the meaning of a CI, with illustrative quotes and references to appropriate literature. This discussion has apparently escaped the reviewer.

20.19.1. *The author now suggests that these errors are not random in time, but rather systematic (constant in time),...*

20.19.1.1. The author never suggested that TCF error is random.

20.19.1.2. The "systematic" of systematic error need not mean constant in time. Such error can vary under the impact of uncontrolled variables [Bevington, PR and Robinson, DK, 2003; Coleman, HW and Steele, WG, 2009]. Systematic means deterministically causal, as opposed to randomly fluctuational. That distinction is not unique to the author.

> 20.19.2. *... and yet, the estimate of LGM temperature differences (as assessed above) are all within a couple of degrees, an order of magnitude smaller than the author's claim.*

20.19.2.1 Please see author response 20.18. Confidence intervals are not model expectation values.

20.19.1.2. As noted in items 13.4 and 13.5, the reviewer is here again assuming that the simulated LGM temperatures are unique solutions to the climate energy state. They are not. As non-unique solutions they have no predictive force, and do not validate the fidelity of climate models.

> 20.19.3. *Therefore if the error is neither random in time nor systematic, where pray is it to be found?*

20.19.3. The relevant error is found where [Lauer, A and Hamilton, K, 2013] reported it to be: in simulated TCF.

This error produces an uncertainty in forecasted (and hindcasted) climate states. The fact that model expectation values take the same reasonable magnitudes that the models are constructed to produce in no way dismisses that uncertainty.

Namely, the uncertainty that arises from theory-bias and from poorly known parameterized quantities adjusted to produce offsetting errors.

The review has regrettably but consistently evidenced no understanding of the larger meaning of physical error, of error statistics, of confidence intervals, or of standard error analysis in physics. This overwhelming lack ramifies throughout the review, and in and of itself removes all critical force.

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