Patrick Frank
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Response to Round 2 Review #5 (round 1 reviewer #1)

Summary Response

This reviewer:

1. Never grasped the critical center of the analysis, which is that linear extrapolation of forcing necessarily entrains linear propagation of error
2. Ignored the analytical refutations of his round 1 review, items 1 and 4.1.1
3. Has repudiated his own prior review, item 2.1.1
4. Has inadvertently validated the manuscript analysis, items 2.1.2 and 3.1
5. Does not realize that LWCF enters the tropospheric thermal energy flux, item 2.2
6. Does not know that fortuitously cancelling errors does not remove uncertainty, items 3.2.1, 4.2.1, and 4.4
7. Is wrong about model TOA biases, items 4.2.2 and 4.2.3
8. Is overly simplistic regarding correlated SW error, item 4.1.1 through to item 4.3

Conclusion: critical analysis of this review reveals no analytical merit.

The reviewer is quoted in italics below, followed by the indented response.

1. *Although the author has added some more analysis of shortwave and ozone forcings, the concerns from the previous review have not been addressed.*

   1. To the contrary, the author addressed every single one of this reviewer's previous concerns.

      1. This reviewer began the round 1 review with a mistaken view that GCM emulation eqn. 6 represents a physical model of climate. This fatal mistake permeated the reviewer's entire argument. However, the reviewer has not acknowledged making this very basic critical error.

      2. The author showed the reviewer's concerns about SW forcing were misplaced.

      3. The author showed that the reviewer fatally misconstrued the ±4 Wm^{-2} LWCF calibration uncertainty statistic to be a +4 Wm^{-2} energetic perturbation.

      4. The reviewer supposed his invented perturbation would impact the TOA energy balance, without ever wondering how a perturbation could be simultaneously positive and negative, i.e., ±4 Wm^{-2}.

      5. It is difficult to envision a more basic mistake than the reviewer's misconstrual of a '±' statistical uncertainty to be a positive energetic perturbation.

6. The author's critical assessment demonstrated the entire the round 1 review to be misconceived. Nevertheless, this reviewer begins, incredibly, by contending that, "*the concerns from the previous review have not been addressed.*"

The author addressed every concern this reviewer raised. Critical analysis left nothing of the previous review to survive into the second round.

In short, examination of the round 1 response will demonstrate that this reviewer's first round concerns were without critical merit.

This reviewer's new objections are point-by-point taken up next.

2.1 *Taking Eq (6) as an emulation of how models predict the warming effect of the radiative forcing, we can then use Eq (6) to predict the warming due to doubling CO_2 for the models.*

2.1.1 The reviewer has now repudiated his own round 1 review by agreeing that eqn. 6 is strictly an emulator of climate model air temperature projections. Consider that the reviewer began his prior review with this mistaken appraisal of eqn. 6:

> "*The main idea of this paper is to use the current average greenhouse effect to establish a linear relation between the increase of the surface temperature in response to the radiative forcing from greenhouse gas.*"

That is a fatal mistake (see round 1 response items 1 and 3.1) the entire first round review followed from it.

Now the reviewer agrees that eqn. 6 strictly emulates the air temperature projections of climate models, and is absent from any presumption of physically valid theory. The reviewer has thus destroyed his own previous review.

2.1.2 With his admission concerning eqn. 6, the reviewer has also inadvertently validated the manuscript analysis.

If eqn. 6 is an emulator of how models project (not "predict") warming due to radiative forcing (and it is), then propagation of uncertainty through eqn. 6 will indicate the uncertainty in those projections ("predictions").

The reviewer's admission in item 2.1 is QED for the manuscript analysis.

2.2 *Because there is no uncertainties as large as +/-4W/m^2 in estimating the extra radiative forcing from the increased amount of CO_2, this prediction should not have error as large as +/-15C.*

2.2 The uncertainty is in the simulated tropospheric thermal energy flux, not in the CO_2 radiative forcing. Manuscript Section 2.4.2 developed this point in detail, but the reviewer has apparently ignored it.

CO_2 radiative forcing enters into the atmospheric energy flux bath and becomes part of it. Any simulation must resolve the effect of CO_2 radiative forcing from the general thermal effects of the background tropospheric thermal energy flux.

This means climate models must resolve the impact of an annual 0.035 Wm^{-2} increase in tropospheric thermal energy flux against a flux background simulated to not better than ±4 $Wm^{-2}year^{-1}$. How does the reviewer propose to accomplish this?

Manuscript lines 514-529 and 542-552 discussed this point in some detail. Section 2.4.2 further describes the problem of resolving a perturbation smaller than the uncertainty.

Further, from manuscript lines 792ff:

> *"Annual LWCF error alone represents ignorance of the atmospheric energy flux magnitude ranging across 8 Wm^{-2}. Compare this to an annual 0.035 Wm^{-2} increase of GHG forcing. It cannot be known how TCF changes in response to a 0.035 Wm-2 perturbation, or even whether TCF changes [at all]."*

Uncertainty in simulated atmospheric thermal energy flux is an obvious bound on the ability of models to resolve the impact of radiative forcing of CO_2 emissions within that very same atmosphere.

However, this obvious bound is apparently invisible to the reviewer.

It is ludicrous to suppose that one can resolve a perturbation of 0.035 Wm^{-2} given a simulation uncertainty of ±4 Wm^{-2} for <u>the very climate subsystem</u> within which the 0.035 Wm^{-2} perturbation occurs.

Nevertheless, this basic of scientific reasoning apparently escaped the reviewer's grasp.

3.1 *+/-4W/m^2 uncertainties in the longwave cloud radiative forcing due to cloud fraction errors in current GCMs are in general model biases instead of random errors.*

3.1 The author thanks the reviewer for noting what the author has demonstrated, namely that the LWCF error is systematic and stems from model theory error.

As a systematic theory bias error, LWCF simulation error must propagate into and through every step of a climate projection. By his admission, therefore, the reviewer has implicitly validated the manuscript analysis.

3.2 *These biases are usually balanced by other biases, e.g., in the shortwave radiative effects from clouds so as to keep a balanced energy as total.*

3.2.1 Apparently the reviewer does not know that fortuitously cancelling errors do nothing to remove simulation uncertainty.

The analytical reason for this is that statistical uncertainties combine as the root sum square, meaning that the sign distinctions of errors are lost.

The physical reason for this is that fortuitously cancelling errors provide no information concerning the underlying physical system. Physical ignorance remains undiminished no matter that a "right answer" is obtained fortuitously.

Physical uncertainty then propagates forward with the stepwise increase in ignorance of phase-space error.

The reviewer's claim is that fortuitous error cancellation in a calibration hindcast proves an error-free projection.

Such a claim is groundless.

3.2.2 The reviewer makes this claim despite knowing that error cancellation is typically obtained by a tendentious tuning of parameters [*Collins et al.*, 2011; *Kiehl*, 2007; *Rasch*, 2012].

4.1 *When discussing prediction of air temperature, it is not only the thermal energy flux that is relevant. The reflected shortwave radiation is very important too; less downward shortwave to surface can cool down the surface and thus the air.*

4.1.1 Item 4.2 in the round 1 response to this reviewer showed that reflected shortwave made no direct contribution to tropospheric thermal energy flux.

The author also showed that the SW component to atmospheric heating was in its contribution to surface transmitted intensity (STI).

Figure 4 of the author's previous response showed further that the intensity profiles of STI and TOA reflected SWR are negligibly correlated.

This analysis demonstrated that the correlation of the TOA LWR and SWR profiles is not directly relevant to the uncertainty in tropospheric thermal energy flux.

4.1.2 It further does not matter to the uncertainty analysis that more reflected shortwave cools the surface. What matters is the known and published large uncertainty in simulated tropospheric thermal energy flux. This uncertainty determines the CMIP5 resolution lower limit of the tropospheric thermal flux that determines air temperature.

The ±4 Wm^{-2} uncertainty is the surviving average LWCF uncertainty of CMIP5 simulations, no matter that all the projections included anti-correlated simulated reflected shortwave.

4.2 *Actually, current climate sensitivity spread has a lot to do with the shortwave radiative feedback from low clouds. The models can have biases in both thermal energy flux and reflected shortwave flux, but unlikely to have a large bias in the total flux at TOA.*

4.2.1 The reviewer again supposes that fortuitously cancelling errors removes uncertainty in climate simulations and in projected air temperatures.

4.2.2 The author consulted the IPCC AR5 and AR4 reports to check the reviewer's supposition that simulations are unlikely to have large biases at the TOA. Figure R1 shows the verdict.

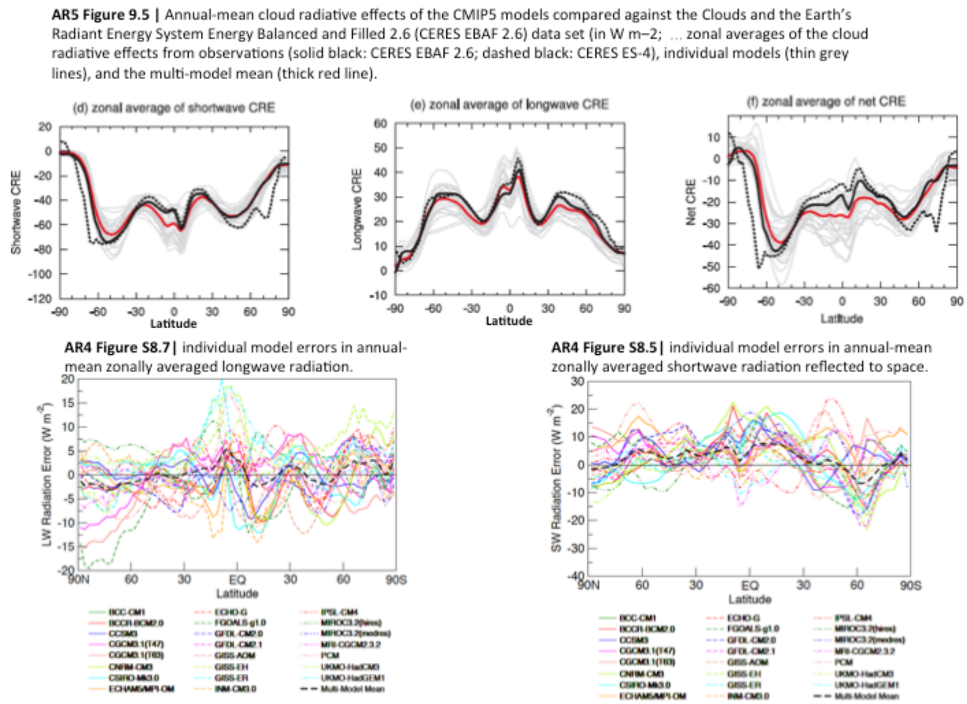

Figure R1: Top. AR5 Figure 9.5, simulated and observed annual average TOA flux and error. Bottom. AR4 Figures S8.7 (longwave) and 8.5 (shortwave) model simulated TOA annual flux error.

It is quite clear that the reviewer is wrong. There are large model errors in both shortwave and longwave simulated TOA flux that will easily produce an annual average ± 4 Wm^{-2}year^{-1} model calibration uncertainty.

4.2.3 The unlikelihood of large bias the reviewer supposed rests primarily upon of model tuning [*Bender*, 2008; *Chen and Frauenfeld*, 2014; *Rasch*, 2012; *Sanderson and Knutti*, 2012].

For example, from [*Lauer and Hamilton*, 2013],

"*The better performance of the [CMIP5] models in reproducing observed annual mean SCF and LCF therefore suggests that this good agreement is mainly a result of careful model tuning rather than an accurate fundamental representation of cloud processes in the models.*"

"*Careful model tuning*" does not remove projection uncertainty.

4.2.4 One is further led to wonder at the meaning of error when the residuals reflect models that have been adjusted by hand to reproduce the test metric. Error magnitudes are thereby artificially (and unjustifiably) reduced. Apparently the conundrum of error that is not the physical error does not concern the reviewer.

4.3 *Therefore, in considering the prediction of air temperature, we must consider both long-wave and short-wave fluxes together.*

4.3 As noted several times, fortuitously cancelling error does not remove uncertainty.

4.4 *The cross term (sigma_u,v)^2*dx/du*dx/dv will be negative. Adding contribution from shortwave flux reduces the total error obtained using the propagation method.*

4.4.1 The reviewer's full propagation equation is,

$$\sigma_u^2(\frac{dx}{du}) + \sigma_v^2(\frac{dx}{dv}) + 2\sigma_{u,v}^2(\frac{dx}{du})(\frac{dx}{dv}) . \qquad \text{R1}$$

Here, *dx* is the change in atmospheric thermal flux, *du* is the change in LWCF and *dv* is the change in SWCF. Note the second term that is missing from the reviewer's critical assessment.

Several points apply.

- In the event of tuning to TOA, SWCF and LWCF must necessarily be anti-correlated in order to maintain a constant TOA flux. This anti-correlation therefore includes an artificial imposition of the conditions of simulation.

- Any consequent anti-correlation of error that includes this artificial imposition is therefore not known to indicate the behavior of the true physical error.

- The reviewer implicitly claims that the cross term in eqn. R1 is larger in magnitude than the second term -- the second term that is neglected in the reviewer's analysis.

- In their study of surface irradiance, [*Kato et al.*, 2012] "*assume that the shortwave and longwave components are independent, but that upward and downward irradiance uncertainties are correlated.*" That this correlation of uncertainties must be assumed means that the correlation is not known. Therefore, the reviewer's assertion of anti-correlation does not rise above speculative.

- [*Kato et al.*, 2012] also state that, "*Modeling error includes the error in the inputs and assumptions in the model.*" Input errors are not included in the LWCF uncertainty propagated in the manuscript. However, Kato, et al., Table 3 provides the global uncertainty in observed downward and upward shortwave to be ±4 Wm^{-2} and ±3 Wm^{-2}, respectively, for a combined global observational uncertainty of ±5 Wm^{-2}. Observational uncertainty is not a constant bias that subtracts away. This observational uncertainty represents the uncertainty in the model calibration target observation, and thus enters directly into any simulation. This uncertainty should thus also propagate into and through any climate projection, because the physically correct short wave irradiance is unknown.

- The ±15 C centennial uncertainty in projected air temperature is therefore a lower limit. Section 2.4 introduced this point. Manuscript line 515ff pointed out that, "*[LWCF] error represents a range of atmospheric energy flux uncertainty within which smaller energetic effects cannot be resolved.*" The average annual increase in CO_2 radiative forcing of 0.035 Wm^{-2} is exactly one such smaller energetic effect. The point is repeated in manuscript line 722.

4.5 *Note that it is confusing to have expression (sigma_u,v)^2*dx/du*dx/dv in Eq 1, because (sigma_u,v)^2 may be negative in case of a negative correlation.*

4.5 Manuscript eqn. 1 is the error propagation equation as it is given in every reference the author consulted. If $\sigma_{u,v}^2$ is negative, then one adds a negative value. The reviewer's confusion is resolved in freshman high school algebra.

All the concerns of this reviewer are now resolved in favor of the manuscript analysis.

References:

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