Patrick Frank
8 October 2017
Earth and Space Science Manuscript 2017EA000308
Response to Round 2 Reviewer #4

Summary Response

This reviewer:

1. Apparently never grasped the central point that linear extrapolation of forcing necessarily entrains linear propagation of error.
2. Has made substantively vacant arguments, items 3.1.1, 3.1.2, 3.2, 3.3, and 3.4.1
3. Misstated or misunderstood the analysis, items 3.3, 3.4.1, 3.4.3-3.4.5, 6.1, and 6.2
4. Apparently does not understand the meaning of "lower limit," item 4.1
5. Does not recognize the difference between precision and accuracy, items 5.1.1 and 6.2
6. Employs the equivocation fallacy, item 5.1.2
7. Does not understand that theory bias imposes an initial value problem onto each step of a model projection, item 5.2.

Thus, critical examination reveals this review as bereft of substantive content.

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

1.1 *This manuscript seeks to establish the reliability of global air temperature projections from GCMs as a function of fractional greenhouse gas forcing using a statistical approach.*

1.1 The reviewer's description is not quite correct. The author's approach is physical error analysis. The reviewer's "*using a statistical approach*" implies the approach is physics-free. However, physical error analysis tests the relevant physical theory; a standard of the physical sciences. The approach is scientific, therefore, not strictly statistical.

1.2 *It uses error propagation analysis to determine that there is a 15C uncertainty in air temperature for centennial-scale projections, among other findings regarding RF uncertainty. While the author does use a unique approach to looking at the predictability of the climate system, the manuscript in its present form has too many issues to be acceptable for publication, and this reviewer must therefore recommend its rejection.*

1.2 As shown below, the reviewer's conclusion has no critical basis.

2.1 *First, the manuscript makes truly extraordinary claims, including, most strikingly, that detection and attribution are impossible, based on a small number of calculations which contradict and flat-out ignore entire disciplines of geoscience.*

2.1.1 The author's conclusion is based on the demonstration that GCMs are unable to resolve the effect of CO_2 emissions upon the terrestrial climate. The conclusion against attribution is unavoidable.

Items 2.1.6.1 through 2.1.6.3 below show geoscience alone is never invoked attribute climate change. Model simulations are the sine qua non of attribution.

Attribution of physical cause strictly requires deduction from a falsifiable physical theory. When the theory is inadequate, and it is, attribution becomes impossible. The conclusion may be unexpected, but it is in no way vitiated by any reference to geoscience.

2.1.2 Physical error analysis and propagation of calibration error are a standard of practice in the physical sciences. Conclusions based on standard methods are not at all extraordinary claims. The conclusion apparently astonishes the reviewer, but it is not extraordinary.

2.1.3 The ±4 Wm^{-2} global annual longwave cloud forcing simulation error is ±114× larger than the global annual average 0.035 Wm^{-2} increase in CO_2 forcing. Why is it surprising that a calibration uncertainty so much larger than the perturbation should propagate to large uncertainties?

How is attribution possible, when the perturbation is so much smaller than model resolution? How can it be it known that CO_2 has a significant effect at all on the terrestrial climate absent a viable causal theory?

How is it possible to attribute a climatological effect that is presently unknowable?

2.1.4 The reviewer remarks upon, "*a small number of calculations.*" How many calculations does it take to indicate that one cannot simulate the impact of a perturbation that is 114 times smaller than the lower limit of model resolution?

2.1.5 Analytical attention is focused on the reliability of climate models. Other areas of geoscience are irrelevant to the manuscript analysis. The physical error analysis points to climate models alone, and is complete in that context.

2.1.6.1 The reviewer provided no citations validating his claim of attribution through geoscience.

However, to test the reviewer's claim the author examined several geoscience papers that claimed attribution: [*Gillett et al.*, 2008], [*Stott et al.*, 2010], [*Spagnoli et al.*, 2002], [*Hegerl et al.*, 2006], [*Hegerl et al.*, 2003], [*Huber and Knutti*, 2012]. Every single one of them relied on climate model simulations to attribute cause to CO_2 emissions.

This examination is not exhaustive, but not one of them referred to a model-free attribution method deriving from geoscience.

2.1.6.2 Section 4 of the IPCC AR4 Technical Summary (TS), "Understanding and Attributing Climate Change," begins with a statement defining attribution: "*Attribution evaluates whether observed changes are consistent with quantitative responses to different forcings obtained in well-tested models, and are not consistent with alternative physically plausible explanations.*" In every single approach to attribution discussed in the AR4 TS requires climate model simulations.

2.1.6.3 Likewise, Section 4 of the IPCC AR5 Technical Summary "Understanding the Climate System and its Recent Changes" begins with the statement: "*Understanding of the climate*

system results from combining observations, theoretical studies of feedback processes and model simulations. Compared to AR4, more detailed observations and improved climate models (see Box TS.4) now enable the attribution of detected changes to human influences in more climate system components."

Again, every single method of attribution discussed in the AR5 TS requires climate model simulations to establish the purported "anthropogenic signal."

Geoscience is never cited as a model-free approach to attribution. The reviewer's case is non-existent.

2.1.7 Absent a predictive physical theory, geoscience observations or trends would provide no proof of a CO_2 impact on climate.

[*Schmidt*, 2006] admits this point in a 2006 essay, wherein it is noted that, "*attribution (in the technical sense) of an observed climate change is inherently a modelling exercise. Some physical model (of whatever complexity) must be used to link cause and effect...*"

Climate models deploy the known physical theory of climate. They provide the only, repeat only, means to predict the impact on the climate of an energetic perturbation.

Interpretation of physical phenomena requires a predictive and falsifiable physical theory. Asserting significance by assignment provides no knowledge. This is basic to scientific knowledge. But is apparently unknown to the reviewer.

2.2 *For example, the statements that models alone are necessary to make warnings related to increasing atmospheric CO_2 concentrations are false: the paleoclimate and historical records alone provides a constraint on climate sensitivity that points to an ECS closer to 4 C/2xCO_2, with an upper bound of approximately 6. There are numerous other lines of evidence as well.*

2.2.1 The reviewer's claim here is insupportable on multiple grounds. Response items 2.1.6.1 through 2.1.7 alone are sufficient to fully refute reviewer item 2.2.

2.2.2 The reviewer assumes what he should have proved, merely by invoking ECS, namely that there is a significant impact of CO_2 emissions on the global air temperature.

However, as noted in 2.1.1, and 2.1.6.1-2.1.7, causal knowledge of an impact of CO_2 on climate requires a prediction from a falsifiable physical theory.

Instead, the reviewer assumes the CO_2 cause, interprets observations in light of his assumed cause, and then uses those observations to validate the assumed cause. This is arguing a logical circle. The reviewer missed a career in exegetics.

2.2.3 If CO_2 forcing cannot be causally implicated *at all* in the trend of the global air temperature record, and it cannot, how can the temperature record be used to constrain a magnitude for ECS?

2.2.4 The identical failure impacts the paleoclimate record. No changes in paleoclimate can be causally assigned to CO_2 forcing.

2.2.5 Current physical models do not have the resolution to determine the magnitude of ECS. Therefore, the reviewer's assertion of ECS is no more than a personally biased inductive inference supported by no causal theory of climate.

Proper derivation of ECS requires physical models, and only physical models.

The manuscript analysis demonstrates, beyond all rational doubt, that current physical climate models, right up through the CMIP5 versions, are unable to resolve the observable impact on climate, if any, of CO_2 emissions. By that account they are also unable to derive a magnitude for ECS.

3.1 *Second, the manuscript lays its foundation with a discussion about CO_2 radiative forcing. What is presented represents an elaborate reinvention of the wheel of radiative transfer theory, but one that again ignores decades of radiative transfer and spectroscopic research, based on laboratory data, quantum mechanics, and more recently, field observations.*

3.1.1 The reviewer's argument is complicated but substantively vacant. It is very easy to say that radiative transfer, spectroscopic research, laboratory data, quantum mechanics, and field observations have been ignored, without providing any substantive specifics. And yet, this is exactly what the reviewer has done.

How, precisely, does calculation of a photonic mean free path through clear air violate quantum mechanics? How does it ignore field observations, laboratory data or spectroscopic research?

The manuscript analysis specifically references and employs the NIST spectrum of dilute CO_2 in N_2. Does that not represent laboratory data appropriate to the calculation? Does it not reflect the appropriate spectroscopic research?

How does the fractional calculation based on the Figure 1b data of [*Manabe and Wetherald*, 1967] ignore radiative transfer, when in fact the calculations of these workers explicitly deployed the modern theory of radiative transfer?

It seems obvious that the reviewer has made hand-waving objections that sound portentous but in fact have no critical content.

The reviewer may not have noted the manuscript references to [*J. Houghton*, 1995; *John Houghton*, 2005]. Nothing in either of those papers conflicts with the author's mean free path calculation.

3.1.2 Nothing has been re-invented. The calculations producing Figure 1a use the clear-air mean free path of 15 μ radiation to estimate the onset of climatologically significant CO_2 forcing.

The author checked the literature for prior calculations of 15 μ mean free path and forcing onset, and found no published work.

The consulted basic climatological texts and papers included [*Aherns*, 2009; *Andrews*, 2010; *Bohren and Clothiaux*, 2006; *H G Houghton*, 1985; *Jacobson*, 2005; *Nazaroff and Alvarez-Cohen*, 2001; *Seinfeld and Pandis*, 2006; *Taylor*, 2005; *Wallace and Hobbs*, 2006].

Were the reviewer correct about an "*elaborate reinvention of the wheel of radiative transfer*," these sources would presage the manuscript discussion of mean free path, of the 15 μ *1/e* attenuation length, and of the $[CO_2]_{atm}$ necessary to the onset of non-negligible radiative forcing.

However, while the sources cover radiative physics at several levels, not one of them addresses any of these topics nor the climatological onset of CO_2 radiative forcing.

Wallace and Hobbs mention a *1/e* optical depth (p. 135), but do not identify it as the radiative mean free path.

Chapter 2 in Bohren and Clothiaux includes a very nice discussion of the *1/e* attenuation length (p. 53), where it is called the *e*-folding length but it is not identified as the mean free path. Their analysis does not discuss the radiative onset of CO_2 forcing.

The remaining examined sources did not mention a radiative mean free path at all or a *1/e* attenuation length. No text mentioned or discussed the atmospheric concentration of CO_2 necessary to initiate a climatologically non-negligible forcing.

The above search shows manuscript's assessment of the onset of climatologically significant CO_2 radiative forcing, and the concept of photonic mean free path are novel in the climatological field.

The reviewer claim 3.1 is thus incorrect throughout.

3.1.3 Once again the reviewer was critically unspecific. No published work was found to substantiate or sustain the reviewer's claim of, "*an elaborate reinvention of the wheel of radiative transfer theory."*

The reviewer has provided no evidence to the contrary and his dismissals are baseless.

3.2 *There are well-established databases (HITRAN, GEISA) that translate this information into tables that radiative transfer codes use. Correlated-k methods, though less accurate than line-by-line, are used within climate models, but what they do is far more rigorous than what is presented here.*

3.2 The reviewer is again incorrect. "*What is presented here*" is a calculation based upon the radiative transfer code of [*Manabe and Wetherald*, 1967].

Figure 1b displays the thermal effects of CO_2 under clear and cloud-covered skies, as originally presented in Table 4 of [*Manabe and Wetherald*, 1967]. These thermal effects are based upon their radiative transfer calculations.

The radiative transfer calculations of [*Manabe and Wetherald*, 1967] were entirely reliable, according to [*Pierrehumbert*, 2011], "*Though the first calculation of the warming of Earth due to CO_2 increase was carried out by Arrhenius in 1896, accurate CO_2 and water-vapor spectroscopy and a fully correct formulation of planetary energy balance did not come together until the work of Syukuro Manabe and Richard Wetherald in 1967.* ***With that development, the theory was brought to its modern state of understanding. It has withstood all subsequent challenges and without question represents one of the great triumphs of 20th-century physics.*** (author's bold)"

That is, the calculations of [*Manabe and Wetherald*, 1967] used the correct expressions for radiative transfer and provide reliable results.

Figure 1 Legend clearly revealed the source of the points in Figure 1b to be from [*Manabe and Wetherald*, 1967]. The reviewer could have resolved review item 3.2 in the author's favor merely by investigating the cited literature.

3.3 *Moreover, the approach makes subtle but important errors in the scaling of CO_2 forcing with rising CO_2, issues of diffusivity approximation, and how to go from a few radiative transfer calculations to a global average.*

3.3 The values in [*Manabe and Wetherald*, 1967] Table 4 in fact exactly reflect the changes in global average surface temperature with CO_2 forcing. The reviewer's concern is misplaced.

Their derived global values are plotted in Figure 1b. Therefore, there are no subtle errors in calculating a global average.

The scaling directly reflects the global fractions of clear sky and cloud-covered sky. How is direct scaling for fractional extent of sky-type subtly wrong? The reviewer does not say.

3.4 *The formulae from Myhre et al, 1998 cover how to do this properly. The calculations and results presented in Figure 1, which forms the basis for the rest of the discussion, are completely unacceptable for publication.*

3.4.1 Yet again, the reviewer has ignored the obvious. Manuscript lines 330,31: "*All greenhouse gas forcings used in eqn. 6 were calculated from the equations given in [Myhre et al., 1998].*" Thus, the author used the exact formulae that the reviewer supposes to have been neglected.

3.4.2 Figure 1a displays the obviously correct clear-air mean free path calculation. Figure 1b displays the correct CO_2/Temperature relationship calculated in [*Manabe and Wetherald*, 1967], which remains relevant.

The reviewer, therefore, apparently thinks that [*Manabe and Wetherald*, 1967] is "*completely unacceptable for publication.*"

3.4.3 Contrary to the reviewer's perceptions the rest of the discussion does not depend on Figure 1. Rather, the manuscript analysis depends upon the demonstration that climate model air temperature projections are linear extrapolations of GHG forcing.

Linear propagation of error rigorously follows from the demonstration of linear extrapolation. The large uncertainty in centennial projected air temperature in turn follows from that analytical dyad.

This again was clearly expressed in the manuscript, lines 400-405:

"*The success of eqn. 6 shows that GCM projections of emissions-driven future GASATs are just linear extrapolations of the fractional change in GHG forcing, moderated by the climate sensitivities of individual models. Therefore, a linear propagation of physical error (eqn. 2) is appropriate for estimating the accuracy of GCM GASAT projections [Vasquez and Whiting, 2006] (see also Section 5 in [JCGM, 100:2008]), and thus also their reliability.*"

However, once again the reviewer has missed a critically central point.

3.4.4 Further, had the reviewer consulted the values of f_{CO2} listed in Tables S1-S4 of the Supporting Information, he would have discovered that the 0.42 value derived from the work of [*Manabe and Wetherald*, 1967] was not in any way critical to any of the manuscript discussion or analysis.

3.4.5 The analytical dyad of item 3.4.3 does not depend at all on the mean-free path calculation or on the CO_2 fraction derived from the data of [*Manabe and Wetherald*, 1967].

Figure 1a,b merely provide an estimate for the fractional GH effect as deployed in climate models. This point is clearly made in manuscript lines 307-09: "*The significance of this result is its relevance to climate models. The result should not be seen as relevant to the terrestrial climate* (emphasis in the original)."

Figure 2 clearly demonstrates the accuracy of that estimate. All of this was apparently lost on the reviewer.

4. *Third, it is very unclear to this reviewer why the author chose only LWCF, when numerous other factors also contribute to surface air temperatures. For example, numerous researchers continue to point to shortwave-cloud effects as the primary source of model feedback uncertainty.*

4.1 The answer to the reviewer's question is found in manuscript lines 163-169, which include that, "*LWCF error therefore defines a lower limit of resolution for advanced climate models.* (emphasis added)"

It should be obvious that a resolution lower limit means other errors are not examined. The centennial ±15 C uncertainty in projected surface air temperature is a lower limit of projection uncertainty. Including additional sources of error will only expand the centennial

uncertainty bounds.

See also manuscript lines 171-174, all of Section 2.4 lines 515ff, especially lines 551-561 and 590-611, and lines 780-782 and 795-798, where the rationale for choice of LWCF error was abundantly described. See also the penultimate paragraph in SI Section 7.1.1.

4.2 The author provided an extended analysis of short wave contributions to tropospheric thermal flux and to forcing error in the first round response to reviewer #1. The present reviewer is welcome to consult that material.

However, to the extent that simulated LW and SW flux errors are anti-correlated, the total uncertainty in atmospheric thermal energy flux will remain about constant because the TOA flux is constant.

5.1 *Finally, there are a number of other serious issues with this paper, including, but not limited to, the discussion of error bars (lines 117-125) which ignores all of the work performed by literally hundreds of researchers to quantify uncertainty in models, ...*

5.1.1 In lines 117ff the author observes that all prior papers evaluating simulation uncertainty focus on model precision: the variance about an ensemble mean. The reviewer does not dispute this statement (and cannot), and thus apparently does not distinguish between precision and accuracy.

5.1.2 Given that dichotomy, "uncertainty" as used by the reviewer (precision) does not have the meaning of "uncertainty" as it appears in the manuscript (accuracy). The reviewer's item 5.1 therefore suffers from the equivocation fallacy: use of a given word assigned alternative meaning and used to falsely carry an argument.

5.2 *... and the discussion in the introduction which seems to approach climate change as an initial-value problem (lines 104-108), when it is, in fact, a boundary-value problem.*

5.2 The reviewer has ignored the point actually made in lines 105-108, which is that each simulation step initializes from an erroneous representation of the climate energy-state. This is a serious initial value problem that has apparently found no light in the modeling literature.

The errors are derived from incorrect theory, and are thus imposed onto every single step of a simulation. The step "*i*" input errors are compounded into the erroneous simulations of step "*i+1*." The combined errors in state *i+1* are propagated forward, and every step is burdened with an expanded physical uncertainty.

This point was further discussed in some detail in manuscript lines 795-823, however the reviewer did not address that discussion. It seems likely, therefore, that the reviewer did not read it.

The problem of propagated uncertainty from theory-bias was also discussed in SI Section 7.1.2 *The problem of continuing theory-bias*, and SI Section 7.2 *The problem of comparative error*. One doubts the reviewer saw these, either.

6. *In summary, the manuscript needs to build off of the large amount of research on model projections, especially that based on very well-established theory and observations (i.e., the radiative transfer) already performed in this field, if it seeks to provide an acceptable, statistically-based estimate of uncertainty in model projections. Furthermore, the manuscript is very far from being acceptable for publication, especially since it makes extraordinary statements based on the limited findings that selectively ignore large bodies of established research.*

6.1 The objections in item 6 have now been critically assessed and found meritless. Model projections: items 2.1.1-2.1.7, 2.2.1 - 2.2.5, and 5.1; radiative transfer: items 3.1.1 through 3.4.5.

6.2 The manuscript does not "*ignore large bodies of established research*." It points out that these bodies invariably and incorrectly portray precision as though it were accuracy.

This incorrect portrayal is summarized in lines 60ff of the Introduction, and that the literature is erroneous is established in lines 72-82, where the distinction between precision and accuracy is explicated.

The reviewer nowhere addressed this point, but instead proceeded as though estimates of model precision do indeed estimate model accuracy.

That is, the reviewer embraces and reasserts the very mistakes illustrated, addressed, and corrected in the manuscript.

It should be clear at this point that review #4 did not survive critical scrutiny. Review #4 has no diagnostic or analytical merit.

References:

Aherns, D. C. (2009), Meteorology Today, 9th ed., Brooks/Cole, Belmont.

Andrews, D. G. (2010), An Introduction to Atmospheric Physics, 2nd ed., 237 pp. pp., Cambridge University, Cambridge, UK.

Bohren, C. F., and E. E. Clothiaux (2006), Fundamentals of Atmospheric Radiation, 472 pp. pp., Wiley-VCH, Weinheim.

Gillett, N. P., D. A. Stone, P. A. Stott, T. Nozawa, A. Y. Karpechko, G. C. Hegerl, M. F. Wehner, and P. D. Jones (2008), Attribution of polar warming to human influence, Nature Geosci., 1(11), 750-754, doi: http://www.nature.com/ngeo/journal/v1/n11/suppinfo/ngeo338_S1.html.

Hegerl, G. C., T. J. Crowley, S. K. Baum, K.-Y. Kim, and W. T. Hyde (2003), Detection of volcanic, solar and greenhouse gas signals in paleo-reconstructions of Northern Hemispheric temperature, Geophysical Research Letters, 30(5), n/a-n/a, doi: 10.1029/2002GL016635.

Hegerl, G. C., T. R. Karl, M. Allen, N. L. Bindoff, N. Gillett, D. Karoly, X. Zhang, and F. Zwiers (2006), Climate Change Detection and Attribution: Beyond Mean Temperature Signals, Journal of Climate, 19(20), 5058-5077, doi: 10.1175/jcli3900.1.

Houghton, H. G. (1985), Physical Meteorology, 442 pp. pp., MIT Press, Cambridge, MA.

Houghton, J. (1995), Comment on "The roles of carbon dioxide and water vapour in warming and cooling the Earth's troposphere", Spectrochim. Acta, A51, 1391-1392.

Houghton, J. (2005), Global Warming, Rep. Prog. Phys., 68(6), 1343-1403, doi: 10.1088/0034-4885/68/6/R02.

Huber, M., and R. Knutti (2012), Anthropogenic and natural warming inferred from changes in Earth/'s energy balance, Nature Geosci, 5(1), 31-36, doi: http://www.nature.com/ngeo/journal/v5/n1/abs/ngeo1327.html - supplementary-information.

Jacobson, M. Z. (2005), Fundamentals of Atmospheric Modeling, 656 pp. pp., Cambridge University, Cambridge, UK.

Manabe, S., and R. T. Wetherald (1967), Thermal Equilibrium of the Atmosphere with a given Distribution of Relative Humidity, J. Atmos. Sci., 24(3), 241-259, doi: 10.1175/1520-0469(1967)024<0241:TEOTAW>2.0.CO;2.

Nazaroff, W. W., and L. Alvarez-Cohen (2001), Environmental Engineering Science, 694 pp. pp., John Wiley & Sons, New York, NY.

Pierrehumbert, R. T. (2011), Infrared radiation and planetary temperature, Physics Today, 64(1), 33-38, doi: 10.1063/1.3541943.

Schmidt, G. A. (2006), Attribution of 20th Century climate change to CO2, in *RealClimate*, edited, RealClimate.org, New York.

Seinfeld, J. M., and S. N. Pandis (2006), Environmental Chemistry and Physics, 2nd ed., 1232 pp. pp., John Wiley & Sons, New York, NY.

Spagnoli, B., S. Planton, M. Déqué, O. Mestre, and J. M. Moisselin (2002), Detecting climate change at a regional scale: the case of France, Geophysical Research Letters, 29(10), 90-91-90-94, doi: 10.1029/2001GL014619.

Stott, P. A., N. P. Gillett, G. C. Hegerl, D. J. Karoly, D. A. Stone, X. Zhang, and F. Zwiers (2010), Detection and attribution of climate change: a regional perspective, Wiley Interdisciplinary Reviews: Climate Change, 1(2), 192-211, doi: 10.1002/wcc.34.

Taylor, F. W. (2005), Elementary Climate Physics, 212 pp pp., Oxford University, Oxford, UK.

Wallace, J. M., and P. V. Hobbs (2006), Atmospheric Science: An Introductory Survey, 483 pp. pp., Academic Press, Amsterdam.