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Response to reviewer #2 (round 1 reviewer #5)

This review is no more than a disgraceful polemic. The editor would have done better to exclude it on the grounds of bringing ill repute to the Journal.

Summary Response:
This review:

1. Is analytically vacuous throughout
2. Inadvertently validated the manuscript study (items 7.2.2, 7.3.2)
3. Was expressly dishonest (items 1.3.1, 1.3.2, 1.4, 1.5.1, 1.5.2, 2.1, 3.2.1, 5.2.1, 6.9, 6.10.2.2, 9.2.4, 12.8, and 12.9)
4. Repeatedly misrepresented Dr. Connolly's own prior review (items 1.3, 5.3.1, 5.3.2, 6.10.3.2, 7.4.1, 9.2.2.2, 9.3.1, 9.3.2, 10.2, 10.4.2, and 12.4)
5. Repeatedly misrepresented the author and other reviewers (items 2.1, 3.2.1, 4.1, 5.2.1, 6.1.1, 6.1.2, 6.2.1, 6.2.3, 6.3-6.5, 6.7.2.2.1, 6.10.1, 6.10.2.2, 7.1, 8.1.3, 9.1, 9.2.2.1, 9.2.3.1, 9.2.3.2, 9.3.1.1, 10.1, 10.4.2, 12.4, 12.5, 12.6.1, 12.7)
6. Expressed uncritical agreement with the obvious and serious analytical mistakes of others (items 2.2.1.1-2.2.1.5, 2.2.2)
7. Uncritically reiterated falsified arguments (items 1.2, 1.5.1, 2.2.3, 3.1.3, 4.1, 6.1.3, 6.4.1-6.4.6, 9.2.4)
8. Inadvertently refuted Dr. Connolly's own arguments (items 6.7.2.2.2, 6.8.1)
9. Falsely claimed priority in showing linearity in climate model projections (item 7.4)
10. Revealed that Dr. Connolly is professionally unfit to review (items 1.4, 2.2.1.6, 6.9.1, 6.9.3, 6.9.4, 6.10.2.3, 6.10.2.4, 6.10.4, 7.1, 9.3.1.2, 9.3.2, 10.1.2, 10.2, 10.4.1, 12.1)
11. Never showed cognizance of the analytical core of the study, namely that linear extrapolation of forcing entrains linear propagation of error (items 3.1.2, 3.1.3). That is, Dr. Connolly rejected the study without ever indicating that he understood it.

In other parts of his review,

- Dr. Connolly showed no understanding of manuscript eqn. 6.
- Dr. Connolly showed no understanding that eqn. 2 propagates LWCF model calibration error, not air temperature.
- Dr. Connolly showed no understanding of the fundamental difference between a statistical average and a magnitude average.
- Dr. Connolly showed no understanding of the significance of the error in simulated cloud cover.
- Dr. Connolly showed no understanding of the meaning of the LWCF ±4 Wm^{-2}/year calibration error statistic.
- Dr. Connolly nowhere showed any understanding that linear extrapolation of forcing is subject to linear propagation of error.
- Dr. Connolly showed no understanding that LWCF enters the tropospheric thermal energy flux.
- Dr. Connolly showed no understanding that LWCF calibration error conditions the simulated tropospheric thermal energy flux.
- Dr. Connolly showed no understanding that CO_2 forcing enters and becomes part of the tropospheric thermal energy flux.
- Dr. Connolly showed no realization that annual uncertainty in simulated LWCF obscures the small annual radiative forcing change due to CO_2.

In short Dr. Connolly showed no understanding of any of the critically central concepts in the manuscript analysis. He showed no ability to mount any sort of cogent review. His review is utterly without merit and should be set aside.

Detailed Response:

The reviewer is quoted in italics, and the indented author response follows.

Unnecessary and shallow introductory complaints are deleted. However, certain points of critical failure or dishonesty require attention.

1.1 *After much consideration and careful review, my opinion at the time was that the main statistical analysis in the manuscript was fundamentally flawed, ... Despite this, the author has decided to resubmit his rejected manuscript to ESS essentially unaltered, ...*

1.1 Dr. Connolly has ignored the demonstration of the fatal errors, critical mistakes, and shallow analyses in his prior review. This tactic of neglect has allowed Dr. Connolly to then complain that the manuscript was not revised in line with his critical rubbish.

1.2 *Instead of attempting to modify his manuscript in light of the major criticisms made by all five reviewers (including myself), the author has chosen to write lengthy responses to each of the reviews claiming that they: "[have] no critical merit" ("Review #1"); "[are]...misconstrued... mistaken...[and] confused" ("Review #3" and "Review #4"); "fundamentally misguided" and unable to "[survive] critical scrutiny" ("Review #5"); as well as involving "the mistake[s] of a naive college freshman" ("Review #6").*

1.2 Dr. Connolly has here complained that the author critically evaluated the reviews and found them very much wanting. The fact of their error evidently had no impact on Dr. Connolly.

The editor should note that Dr. Connolly has complained but did not substantiate his complaint. That is, he does not anywhere authenticate his complaint by showing an actual mistake in the author's critical assessment of the round 1 reviews.

The author suggests that he cannot do so. The lack of substance in Dr. Connolly's prior and following provide definitive proof the author is correct in this surmise.

Dr. Connolly complaint in 1.2 is critically vacuous.

1.3 *In the author's response to my review he refers to a July 2016 talk he gave based on an earlier (apparently very similar) draft of the manuscript, and he provided a link to an online video of this talk: https://www.youtube.com/watch?v=THg6vGGRpvA.*

1.3.1 Dr. Connolly is disingenuous. In his Round 1 review, Dr. Connolly misattributed to [*Frank*, 2008], a comment the author made in his 2016 video presentation. Dr. Connolly's familiarity with the comment and his misattribution indicate that he had watched the video prior to writing his first review.

In his response, the author merely corrected Dr. Connolly's misattribution. Dr. Connolly did not discover the video through the author's correction of Dr. Connolly's mistake.

1.3.2 Dr. Connolly went on to write, "*I recall Dr. Soon mentioning the talk, but I had not watched it when I was reviewing the previous draft. Instead, my review at the time was based on the author's submitted manuscript, supplementary information and the relevant literature (both cited and non-cited).*"

It is impossible to credit Dr. Connolly's claim of having watched the video only after his first round review, when, in that very review, Dr. Connolly repeated a statement the author made only in the video presentation.

1.4 *In the Q&A at the end of this talk (starting at ~36 min), one of the audience members (*) asked the author how he had fared in publishing his analysis. The author replied:*

"I have been trying to publish this work for 3 years. I've gone through review processes now at 6 different climate journals, including some of the major ones, like Journal of Climate. There have been 16 reviews that I have responded to, of which 13 were by climate modellers, and the 3 that were not climate modellers all recommended publication. The 13 by climate modellers of course recommended rejection.

"I don't know how to say this without sounding self-serving, but every single climate modeller review was incompetent. And by incompetent, I mean they made mistakes that were typical of naïve, undergraduate freshmen."

1.4 Dr. Connolly ended his quote there, which was very dishonest. He cut short the quote immediately before the author listed some of the naïve mistakes these reviewers in fact did make.

This listing followed immediately after the quoted comment, and is what Dr. Connolly knowingly excised:

"*For example ... they neither respect nor understand the distinction between precision and accuracy.*

"They don't understand propagated error, and they think that those error bars that I showed you -- these error bars -- mean that the climate itself -- the model -- is oscillating rapidly between a hothouse and an ice-house climate, when in fact they don't mean anything like that.

"They don't understand the distinction between an energetic perturbation and an error statistic. So they see that ±4 Wm^{-2} of error as a perturbation on the model, which it is not.

"So they make these really, really basic mistakes that, if you're a physicist or a chemist, you learn about in your first year of your undergraduate study; how to begin to

propagate errors through your measurements. And none of them seem to know anything about that."

The itemized mistakes are indeed those of naïve freshman, and do indeed indicate scientific incompetence.

That Dr. Connolly should knowingly exclude them when they are centrally relevant provides clear evidence of a malign bias in this reviewer.

...

1.5.1 *So, in addition to the five negative reviewers of his previous submission to ESS, he apparently has also received a further 13 negative reviews for this manuscript from six different journals (with 3 positive reviews).*

1.5.1 Dr. Connolly clearly ignored that those reviews demonstrated analytical incompetence. Or perhaps Dr. Connolly agrees that precision is identical to accuracy, and that uncertainty bars indicate model oscillation.

The fact that Dr. Connolly explicitly failed to include the author's description of the obviously naïve mistakes made by those reviewers has allowed Dr. Connolly to here misrepresent these reviewers as having made legitimate criticisms.

Dr. Connolly's commentary in 1.4 - 1.5.1 is thus very dishonest.

1.5.2 *On the final slide of his talk, he thanks another four "critical reviewers" (Christine Adams, Chris Essex, Ross McKitrick & Carl Wunsch). However, tellingly he stresses that these four reviewers did "not necessarily [agree] with all the stated conclusions".*

1.5.2 Dr. Connolly is again disingenuous. The author stressed nothing concerning these reviewers. The final slide says, "*Listing these names does not necessarily imply agreement by any party with all the stated conclusions*." That is, the author merely provided a standard acceptance of responsibility for all content.

The author thanked the reviewers but passed over the quoted statement in silence; content to merely enter it into view.

It is a fact in evidence in the video that author stressed nothing concerning the reviewers.

Dr. Connolly's dishonesty is again visible.

1.5.3 *So, it is quite possible that some of these additional named reviewers shared some of the concerns raised by myself and the other ESS reviewers.*

1.5.3.1 Dr. Connolly here speculates freely, opportunistically, prejudicially, again without substance, and again malignly.

All the pre-publication reviews of the Skeptic manuscript were positive. Prof. Carl Wunsch wrote that although he hadn't run the numbers he nevertheless agreed with the analysis.

Dr. Connolly also apparently did not notice the acknowledgement in the Skeptic article thanking Prof. McKitrick for carrying out and communicating all the Phillips-Perron tests. Prof McKitrick was among the named reviewers attracting Dr. Connolly's attention. His participation in the study is hardly the behavior of a negatively disposed reviewer.

Thus readily available evidence discourages Dr. Connolly speculations. However, as is his patent wont, Dr. Connolly has overlooked clear disproofs of his favored position.

1.5.3.2 Dr. Connolly again ignored that both his review and the others he mentioned were demonstrated, **demonstrated**, to be critically meritless. As he apparently received the author's responses, Dr. Connolly has no excuse for such ignorance.

2.1 *After viewing the author's 2016 talk, I also found a detailed video critique by Dr. Patrick T. Brown on the author's 2016 talk as well as a similar analysis in a 2013 AGU Poster: https://www.youtube.com/watch?v=rmTuPumcYkI. Many of the criticisms made by Dr. Brown are similar to those made by the five ESS reviewers, and it is worth viewing for this fact alone.*

2.1 Dr. Connolly again betrays his ignorance. Only one of the ESS reviewers reiterated a Dr. Patrick Brown argument (reviewer 1, round 2); and then only one of them. I.e., the reviewer similarity leaderboard is *one and one*, not Dr. Connolly's *many and all*.

And, in fact, Dr. Brown had only two main arguments, not many. His arguments are identical to his mistakes, and can be summarized.

The first is that Dr. Brown treats the annual average ±4 Wm^{-2} LWCF model calibration uncertainty of [*Lauer and Hamilton*, 2013] as a constant single-sign physical offset error. That is, Dr. Brown treats ±4 Wm^{-2} as a constant, positive-sign 4 Wm^{-2} error.

This is obviously wrong.

Dr. Brown's second argument, and mistake, consists of insisting the dimension of the annual average LWCF calibration uncertainty statistic is Wm^{-2} rather than Wm^{-2}/year. ESS reviewer 1, round 2, made the similar argument comprising the identical mistake.

Of all the reviewers, only round 2, reviewer 1 made this mistake.

It is not true, therefore, that "*Many of the criticisms made by Dr. Brown are similar to those made by the five ESS reviewers...*" Only one criticism is similar, and it is mistaken.

Thus, Dr. Connolly has either knowingly inflated the case or has made a knowledge claim in ignorance. Either eventuality requires measured dishonesty.

2.2 *The author debated this critique in the comments section of Dr. Brown's accompanying blog post: https://patricktbrown.org/2017/01/25/do-propagation-of-error-calculations-invalidate-climate-model-projections-of-global-warming/. However, in my opinion, the*

author's counter-rebuttal of Brown's critique was invalid, and I agree with much of Brown's negative review.

2.2.1.1 Dr. Brown showed no understanding of a calibration experiment. The author pointed this out in his February 5, 2017 at 8:28 pm post, and provided a detailed methodological explanation of calibration.

See also the author's posts on February 12, 2017 at 6:31 pm and February 20, 2017 at 1:21 pm.

Dr. Brown responded to the author's observation with resentment, e.g., his post on February 14, 2017 at 3:21 pm. One cannot blame him for being upset. However, he clearly did nothing to inform himself of the meaning of a calibration experiment before replying.

2.2.1.2 Dr. Connolly agrees with Dr. Brown. Nevertheless, the ±4 Wm^{-2} is a calibration uncertainty statistic, not a physical error magnitude. Apparently Dr. Connolly sees no problem with conflating physical magnitudes with uncertainty statistics.

2.2.1.3 Dr. Connolly therefore either likewise knows nothing about the conduct or meaning of a calibration experiment and the statistical dimensions it entails, i.e., the per-unit denominator, or else is blinded by the judgmental prejudice in evidence above (items 1.4, 1.5.2, 1.5.3.1), or is perhaps merely arguing disingenuously.

2.2.1.4 Apparently, also, Dr. Connolly thinks that ±4 Wm^{-2} statistic is instead a positive-sign +4 Wm^{-2} offset physical error. This mistake is worse than naïve. To be direct: an uncertainty statistic is not physical error.

2.2.1.5 He further evidently thinks that the dimension of the annual average Wm^{-2}/year statistic does not include 'per year.' Instead he has it as the dimension of a discrete physical magnitude, 'Wm^{-2};' apparently unable to distinguish a statistical average from a magnitude average.

Supporting such foolish mistakes is beyond fatuous for a PhD scientist.

2.2.1.6 Granting his training, the fact that Dr. Connolly actively supports Dr. Brown in so many obvious mistakes either shows a reflexive negative prejudice, amply in evidence above, and/or training so poor as to produce an inability to grasp a high-school level concept of empirical science, i.e., dimensional analysis.

In either case, Dr. Connolly has self-revealed either professionally or by temperament that he is unfit to render a critical review of science.

2.2.2 The author further notes that Dr. Connolly expressed his support for Dr. Brown's dimensional mistake in the face of the dimensional derivation of the [*Lauer and Hamilton*, 2013] calibration statistic explicitly and obviously present in the revised manuscript.

2.2.3 One observes that Dr. Connolly again did not substantiate his declaration of belief with any critical examples, or with any critical argument at all. He merely stated his support. His unbuttressed belief has no more critical force than a declaration of religious preference.

3.1 *Of course, the fact that there have already been so many negative reviews of this manuscript does not itself mean that the author's analysis is flawed.*

3.1.1 Dr. Connolly is thanked for this admission. Dr. Connolly has ignored, however, that the author has demonstrated that the case he admits possible is the case factually at hand.

3.1.2 In fact, the author demonstrated beyond doubt that both Dr. Connolly and all the other negative reviewers have ignored the basic analytical justification of the manuscript analysis, which is that linear extrapolation of forcings is subject to linear propagation of error.

3.1.3 Dr. Connolly below declared that he is "*very familiar with propagation of errors*," and yet has completely overlooked this very obvious justification repeatedly made: in the abstract, lines 23-24; in the manuscript, lines 150-153, 400-406, 613-623, and 770-772.

Dr. Connolly's expertise in propagated error has apparently led him to overlook the obvious case for propagated error.

3.2.1 ... climate modellers (*) ... (*) Note: *... the author claimed in his 2016 talk that his reviewers were (allegedly) unaware of the standard statistical approaches for the propagation of errors through measurements* ***because they (supposedly) hadn't studied undergraduate chemistry or physics*** (author's bold).

3.2.1 The author nowhere said climate modelers were unaware ***because*** they hadn't studied, etc. The author noted that the mistakes were those of naïve undergraduates. The latter is not a mere allegation but a factual observation, and not the causal declaration Dr. Connolly imputes.

The author honestly has no idea why these reviewers are so obviously ignorant of physical error analysis.

It is also curious that Dr. Connolly expressed agreement with such obviously mistaken arguments with himself having claimed (below) an expertise in propagation of error.

Nevertheless, Dr. Connolly has not missed the opportunity for another dishonest representation.

3.2.2 *So, I should probably note that, like the author, my undergraduate degree was in chemistry, and I am very familiar with the propagation of errors.*

3.2.2 Dr. Connolly's studies have obviously not helped him here. And his review to this point brings no credit to the profession.

4.1 *However, the author has chosen to resubmit his rejected manuscript - essentially without any substantive revisions - on the basis that he claims to have satisfactorily shown that all his reviewers were wrong, i.e., he is basically claiming that everyone is out of step except for him.*

4.1 On the contrary: the author made no mere claim. The author has demonstrated that the negative reviews were without critical merit. That demonstration of scientifically and analytically meritless content included Dr. Connolly's review, which one surmises is the source of his prejudicial antipathy in evidence here.

4.2 *... for the rest of this review I will focus specifically on whether or not the author has shown the reviewers (including myself) to be wrong in their criticism of his analysis. I argue that he has not done so and that the reviewers have shown the author's analysis to be critically flawed. Therefore, I recommend rejecting the manuscript.*

4.2 The author welcomes this challenge, and shows below that Dr. Connolly's second round review is as bereft of analytical probity as that of his first round. Dr. Connolly's second round review differs from his first in being bereft of professional integrity.

5. ***The author's chief objections to my review ("Review #5")***

5.1 The author has written a lengthy (24 page) response to my previous review (11 page).

5.1 Dr. Connolly begins with a specious complaint. The author quoted Dr. Connolly's 11 pages and responded. It should be no surprise that a quote-and-response should be twice the original length.

5.2 *Throughout his response, he repeatedly claims that I have "mistaken" or "misunderstood" his arguments and that my assessments are "misconceived", "wrong" and "[lack] analytical rigor". He asserts this with such confidence that anybody reading his response without having read both the manuscript and my review could be forgiven for initially assuming my review was somehow seriously flawed, ill-considered and irrelevant.*

5.2.1 Dr. Connolly has lifted the enquoted phrases from the author's Summary. They do not occur repeatedly throughout the response.

For example, "*wrong*" occurs only in Summary item 11, and response item 7.3.2.3. The "*lacks analytical rigor*" occurs only in Summary item 11; "*misconceived*" occurs only in Summary item 2; "*misunderstood*" occurs twice in the Summary Response, and only three times in the body of the author's response.

Dr. Connolly's complaint proves to be an exaggeration for effect, and is another example of the dishonest rhetorical approach in evidence to this point of the review.

5.2.2 Dr. Connolly's review was, in fact, "*seriously flawed, ill-considered and irrelevant*." The round 1 twelve-point Summary itemized his mistakes, which were many, fundamental, and fatal, and directed the reader to those response sections in which Dr. Connolly's mistakes were exposed.

For example, Dr. Connolly's round 1 review immediately supposed manuscript eqn. 6 to concern climate physics.

This mistake alone is enough to demonstrate a misconception fatal to his review. This same mistake ramified throughout his review, as demonstrated in Round 1 response items 1.1, 1.2.1, 4.4, 5.1, 5.2, 5.3, 5.4, 5.5, 5.6, 5.8, and 5.9.4, rendering Dr. Connolly's round 1 review critically vacant.

5.3.1 *However, a careful inspection of his response reveals that the "objections" he dedicates most space to are dramatically inflated mischaracterizations of trivial differences in how we are describing essentially the same points.*

5.3.1 Dr. Connolly is attempting to minimize his fundamental mistakes. For example in his round 1 he characterizes emulation eqn. 6 as, "*a simple, semi-empirical analytical model for the global temperature response to greenhouse gas concentrations.*"

He is fundamentally mistaken. Eqn. 6 emulates climate model output. It has nothing whatever to do with global temperature response to anything.

To misconstrue eqn. 6 in this way is to fundamentally misperceive the entire subsequent analysis. Here in his round 2 review, Dr. Connolly's new argument is that fundamental misperceptions are "*trivial differences*." The major thrust of this claim is to avoid the obvious diagnosis of, 'wrong.'

For example in his round 1 review Dr. Connolly went on to criticize the author because, supposedly, "*He then claims (or assumes?) that the physics underlying his model are essentially the same as the physics built into GCMs ...*" whereas no such claim or assumption can be found anywhere in the manuscript.

Thus, Dr. Connolly's own round 1 text refutes his round 2 claims.

The manuscript clearly states that eqn. 6 is not about physics but rather is an emulator of climate model output (*cf.* lines 126-130, 150-153). That is, eqn. 6 merely shows the arithmetic structure of air temperature projections.

These mistakes are not *trivial differences*. They manifest a thorough misunderstanding of the entire analysis, and ramify throughout Dr. Connolly's round 1 review. Dr. Connolly's fatally mistaken understanding renders his review critically irrelevant.

Yet here he passes off such mistakes as *trivial differences*.

5.3.2 *That is, many of the so-called "critical point[s]" which he claims I have "mistaken" and "missed" are actually points where I was agreeing with him, but just describing them in a slightly different manner.*

5.3.2 Dr. Connolly is again wrong in his characterization. Item 5.3.1 exemplifies his very fundamental mistake.

For another example, he supposed that a linear least square (LLSQ) fit, "*between the GCM projections and the changes in GHG forcing*," is equivalent to the demonstration that one linear equation can emulate the temperature projection of any climate model under any scenario. That is, he claims a linear fit is equivalent to a deductive demonstration that all GCMs merely extrapolate temperature as a linear function of GHG forcing.

However, no LLSQ fit or set of fits can demonstrate the generality. Dr. Connolly is equating an inductive inference (his LLSQ fits) to a deductive conclusion from eqn. 6 (all GCM air temperature projections are linear extrapolations).

In Dr. Connolly's inductive approach, it is never known whether the next test will fail. The generality can never be deduced.

In short, Dr. Connolly's claim flies right into the face of the scientific method. This is not a *trivial difference*.

Then, on the basis of his completely erroneous assessment, Dr. Connolly went on to dismiss eqn. 6 as unimportant to the study. In fact, as a successful emulator of GCMs, eqn. 6 is absolutely central to the study.

Dr. Connolly's linear fits do not permit any assessment of projection uncertainty.

In short, Dr. Connolly's errors are fatal, and he has here trivialized them.

6. *On the other hand, when it comes to the more substantive points where I was disagreeing with his analysis, his responses typically comprise one or more of the following:*

6. These claims are taken in turn, and shown to be groundless.

6.1 • *"Science by assertion", i.e., insisting that he is correct... because he says so*

6.1.1 Was it the author's mere assertion or insistence that Dr. Connolly's depiction of eqn. 6 as a, "*semi-empirical analytical model for the global temperature response to greenhouse gas concentrations,*" is wrong and fatally wrong?

Eqn. 6 is strictly an emulator of GCM air temperature projections. This meaning was repeated throughout the manuscript, and nevertheless was completely lost on Dr. Connolly.

These are real mistakes. Demonstration of basic misconstrual as such is not, "*"Science by assertion", i.e., insisting that he is correct... because he says so.*"

6.1.2 Dr. Connolly claimed the propagated uncertainties are "*invalid*," and "*unrealistically large and inappropriate.*"

His rejection rests entirely on the utterly mistaken notion that calculated air temperature plays a part in the eqn. 6 emulations. Thus, Dr. Connolly wrote:

"*while the global surface mean temperatures in a given step do have an influence on the*

projections of the next steps, this is generally an indirect influence (through feedback loops, etc) and is definitely not the only (or even a major) factor. This can be demonstrated using a simple thought experiment: if the GMST of one year was solely a function of the GMST of the previous year, then GMST would be effectively constant over time. Instead, the global warming projected by the CMIP5 models is mostly a consequence of rising GHG concentrations."

Dr. Connolly's analysis is wholly wrong. Where in eqn. 6 is GMST propagated year-by-year? Air temperature does not enter anywhere into the calculation, or into the error propagation. Air temperature is an extensive variable from equation 6, not an intensive variable within it.

Dr. Connolly's "*thought experiment*" is analytical nonsense. And yet, his entire rejection of propagated uncertainty rests upon it.

The propagated uncertainty bars reflect CMIP5 LWCF calibration uncertainty, not GMST. The step-wise uncertainty propagation does not depend at all on the air temperature of the previous year.

Dr. Connolly's mistakes are objectively real. They are not merely, "*by assertion", i.e., insisting that [I am] correct... because [I] says so*."

Dr. Connolly's mistakes were all plainly explained in round 1. He is obviously wrong and apparently unable to accept the fact.

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6.1.3 Further, this sentence,

> "*if the GMST of one year was solely a function of the GMST of the previous year, then GMST would be effectively constant over time.*"

is meaningless because Dr. Connolly has left "*function*" unqualified.

It is clear from "*constant over time*' that Dr. Connolly had '[*unitary] function*' in mind. However, when left unqualified, *function* could be non-linear or exponential, or you-name-it. This logical lacuna shows yet again the poor quality of thought that typified Dr. Connolly's review.

The last sentence is particularly ironic, because emulated air temperatures calculated using eqn. 6 are wholly, "*a consequence of rising GHG concentrations*." Dr. Connolly has recognized that fact about GCM projections, but is apparently unable to recognize exactly that same trait as obviously central to the eqn. 6 emulations.

Emulation of that trait in eqn. 6 is exactly what qualifies it to be used for error propagation. This was exhaustively demonstrated but Dr. Connolly failed to grasp it.

Dr. Connolly's "*thought experiment*" is wrong, it is irrelevant, it is negligent, and it merely demonstrates that he apprehends nothing of what he attempted to analyze.

These are examples of the obvious and deep mistakes that Dr. Connolly has called, "*trivial differences*," and supposes were dismissed, "*because he said so.*"

Dr. Connolly's dismissals are shallow and disingenuous. Obvious catastrophic mistakes perfused Dr. Connolly's review. He remains unable to see them.

6.2 • *Repeating an argument from his original manuscript and/or the supplementary information, and implying (or even claiming) that I had somehow skipped over it when I was reviewing the paper (not true!)*

6.2.1 In his round 1 review Dr. Connolly wrote that the author, "*shouldn't claim the projections "...are just linear extrapolations..."*"

In fact, the author demonstrated that air temperature projections are in fact just linear extrapolations. Dr. Connolly here demurs about an established fact of his round 1 review.

6.2.2 Further, author round 1 response item 5.10.2 described the linear relation $N = F-\alpha\Delta T$ from [*Andrews et al.*, 2012; *Gregory et al.*, 2004], pointing out that $\Delta T = (F-N)/\alpha$, will successfully emulate the air temperature projection of any CMIP5 model, when the GCM-specific value of α is known.

This item showed that linearity of climate model output was recognized in the literature. Thus the peer reviewed literature itself refuted Dr. Connolly's demand to set that finding aside. This is not "*repeating an argument*".

6.2.3 Author round 1 response item 7.5 addressed Dr. Connolly's attempt to dismiss error propagation on the grounds that LWCF error is systematic.

This item showed [*Vasquez and Whiting*, 2006], recommended systematic error in linear physical models to be propagated as $\pm u_{sys} = [\sum_{i=1}^{m} \varphi_{s,i}^2]^{1/2}$, which exactly the approach taken in the manuscript.

This is not "*repeating an argument*".

6.3 • *Claiming I was "confus[ed]" or suffering from some "misperception" and had not understood his arguments (not true!)*

6.3.1 Items 5.2.2 and 5.3.1 above demonstrate Dr. Connolly's confusion, misperception, and lack of understanding concerning critical aspects of the manuscript analysis.

6.3.2 Further examples of confusion and misunderstanding include:

1. that Dr. Connolly wrongly claimed the author assumed that satellite cloud data sets are perfect, merely showing Dr. Connolly did not understand clear statements that the author was estimating a lower limit to model resolution (lines 166, 173, all of Section 2.4 line 515*ff*, line 554-55, and line 780).

2. that Dr. Connolly did not understand the difference between no significant observational change and no simulation error (R1 item 7.4.2)
3. that Dr. Connolly did not understand that an uncertainty in simulated LWCF means the 0.035 Wm^{-2} annual average change in atmospheric thermal flux due to CO_2 is irresolvable within that uncertainty (R1 item 7.4.3)
4. that Dr. Connolly did not understand that the uncertainty in a simulation anomaly is the root-sum-square of the uncertainties in the baseline trend and the projected trend (R1 7.4.5.
5. that Dr. Connolly did not understand the what or how of manuscript error propagation (R1 items 7.5, 7.6.1, 7.6.2, and 7.7)
6. that Dr. Connolly's "thought experiment" indicated his utter confusion of what went into emulation eqn. 6 (R1 item 7.7)

These many examples are not exhaustive, but are sufficient to show that Dr. Connolly was indeed and remains, confused, suffering from considerable misperception, and still does not understand the author's arguments.

6.4 • *Ignoring/dismissing them*

6.4 By now it should be very clear that the author neither ignored nor dismissed Dr. Connolly's review comments. Rather, he showed Dr. Connolly either knew not whereof he wrote, or had recommended massive and utterly fatuous deletions.

6.5 *He seems to have also taken this approach with his responses to the other reviewers.*

6.5 Even a casual inspection of the author's other responses will show that Dr. Connolly is quite wrong.

Perhaps he did not view those responses and therefore pejoratively opined out of factual ignorance. Perhaps Dr. Connolly read and did not understand the author's responses, in which case he has demonstrated incompetence. Or perhaps Dr. Connolly has construed a lie.

There are no other possibilities.

6.6 *As mentioned above, his response to my review is rather lengthy - comprising 8 sections, each with multiple subsections and even sub-subsections.*

6.6 Dr. Connolly's round 1 review was 13 pages long, comprising 8 sections, and each with many unnumbered sub-sections. He has no grounds whatever to complain of the author's comprehensive response.

6.7 However, below I summarise and address what appear to be his chief objections to my review:

6.7.1 He objects to me choosing the journal's (commendable) option to revoke my anonymity.

The other reviewers had the good sense to remain anonymous. Dr. Connolly's reason for appending his identity might be found in the self-conceit that perfused his first round review.

The remainder of Dr. Connolly's complaint here was deleted out on grounds of his tedious self-exculpation. However, Dr. Connolly attempted to put the problem of personality conflict on the author. The inanity of his idea is readily apparent in the realization that personality conflict requires two personalities. In signing his review, Dr. Connolly consciously supplied the second and opened that gate.

Further, given the dishonesty that perfuses Dr. Connolly's round two comments (replacing the self-conceit in evidence in round 1), Dr. Connolly has clearly opted to pass through the gate that he, himself, opened.

6.7.2.1 He objects to me describing his Passive Warming Model (i.e., Equation 6) as "a simple, semi-empirical analytical model for the global temperature response to greenhouse gas concentrations".

6.7.2.1 The reason being, of course, that Dr. Connolly was wrong to so describe it.

6.7.2.2 *His main objection seems to be that if he did this he might be interpreted as implying that his model had some predictive power whereas his model is specifically a model of GCM projections of global temperature response to greenhouse gas concentrations,...*

6.7.2.2.1 Dr. Connolly has misrepresented the author's concern. The author objected to Dr. Connolly's characterization of eqn. 6 because it was wrong; not because the author might be misinterpreted.

Eqn. 6 is not a physical model for the global response to GHG concentrations. Eqn. 6 is a model for how GCMs project air temperature, which is a numerical analogy model.

The distinction is orthogonal. Physics is not numerical analogy. Dr. Connolly is plain wrong; and to miss this point is to misconstrue both science and the entire manuscript analysis, which Dr. Connolly has done.

6.7.2.2.2 *... and he is using this model to prove that the GCMs have effectively no predictive power with regards to that parameter. In his 2016 talk he referred to it as a "climate model model".*

6.7.2.2.2 Dr. Connolly here has betrayed evidence that he knows eqn. 6 is a climate model model, i.e., a numerical analogy of GCMs. He thus has thus implicitly contradicted his previous description of eqn. 6 as a physical model. He has unselfconsciously refuted himself.

6.8 *This "objection" is repeated in 11 out of the 54 subsections in his response (1.1, 1.2, 4.4., 5.1, 5.2, 5.3, 5.4, 5.5, 5.6, 5.8, 5.9), i.e., ~20% of his objections. And he repeatedly implies that it is a "critical point" which I have "misunderstood"*.

6.8.1 The author thanks Dr. Connolly for demonstrating that the author repeatedly addressed Dr. Connolly's mistaken thinking.

In listing these author responses, Dr. Connolly has also demonstrated that he knows the author did not reply in terms of, "*Science by assertion, i.e., insisting that he is correct... because he says so*", as Dr. Connolly had it.

In this admission, Dr. Connolly has refuted his own complaints, noted in items 6.1-6.4.

6.8.2 It should be obvious that Dr. Connolly has misunderstood the meaning of eqn. 6. He repeatedly claimed eqn. 6 is a semi-empirical physical model of the global air temperature response to GHGs. It is nothing of the sort.

Eqn. 6 shows the arithmetical form of GCM air temperature projections; nothing more. Dr. Connolly appears to never have apprehended this fact. Nor, as indicated by his insistently mistake-grounded complaint, does he evidence a capacity to ever apprehend it.

6.9 *To me this is a somewhat circular logic, which kind of puts the cart before the horse, i.e., because his conclusions are that the uncertainties associated with GCM temperature projections are too large to have predictive power, he doesn't want to even suggest that his semi-empirical model might have some predictive power. Instead, I think the language I had used to describe his model is equivalent, but yields a more insightful and objective approach, i.e., describe his model as a semi-empirical analytical model for the global temperature response to greenhouse gas concentrations, then argue that it is actually a good emulator of GCM projections, then argue that neither the GCM projections nor his semi-empirical model have predictive power with regards to global temperature trends.*

6.9.1 Dr. Connolly's very bad logic: eqn. 6 demonstrates that, "*the uncertainties associated with GCM temperature projections are too large to have predictive power.*" Nevertheless, Dr. Connolly's position is that eqn. 6 "*might have some predictive power*."

How does such nonsense grace a scientist?

6.9.2 Dr. Connolly's claim of circularity rests upon his entirely unfounded speculations concerning author intent ("*...he doesn't want...*").

Dr. Connolly imputes to the author desires the author does not have.

6.9.3 One could never validly argue for eqn. 6 to be, "*a semi-empirical analytical model for the global temperature response to greenhouse gas concentrations,*" as Dr. Connolly has it. This supposes that eqn.6 is a physically valid theory of the terrestrial climate, i.e., able to falsifiably predict climatic response to an energetic perturbation.

But eqn. 6 incorporates no physics; none of the fast feedbacks available to the climate, such as cloud-type response, enstrophic dissipation of thermal energy, convection, evaporation and condensation, radiation of latent heat, and so forth.

How can any serious scientist suppose a simple equation linear in fractional forcing have any predictive power for climatic response?

Granting his status as a scientist, Dr. Connolly's suggestion is incredible. Eqn. 6 incorporates no physics and has no predictive power. Dr. Connolly betrays a hopelessly inadequate understanding of the meaning of theory in science.

His supposition of theory is entirely baseless. Eqn. 6 cannot be a semi-empirical condensation of a scientific theory nowhere in evidence.

6.9.4 It is not "*equivalent*" or "*more insightful*" to wrongly describe a numerical emulator as a semi-empirical physical model. In so describing eqn. 6, Dr. Connolly is abusing language and abusing science itself. The latter pertains because Dr. Connolly is assigning predictive power where none exists.

Dr. Connolly's item 6.9 equivalency argument is false and a self-serving illocution.

6.10.1 *In other words, I had described this part of his analysis as him having developed a "model for the global temperature response to greenhouse gas concentrations" I then described how he had argued this "model is a reasonable approximation of the global temperature projections of current GCMs".*

6.10.1 The ideas in the enquoted clauses appear nowhere in the manuscript. The author did not argue these points. Dr. Connolly imputed the ideas to the author in his round 1, and is here falsely assigning them, **again**, to the author.

This, despite the author's round 1 response having pointed out in exhaustive repetition that Dr. Connolly is mistaken.

Manuscript line 393 provided the author's view of eqn. 6 as, "*able to accurately emulate the projected GASAT trends of virtually any current advanced GCM.*"

This description is inequivalent to a "*semi-empirical model*" or "*reasonable approximation of*," as Dr. Connolly has it.

To accurately emulate is not to reasonably approximate; accurate emulation is what the author demonstrated, not reasonable approximation.

Dr. Connolly's item 6.10 is again false and a self-serving misconstrual.

6.10.2 *The author claims that I should have - from the outset - just referred to his model as a model of the models' projected global temperature response to greenhouse gas concentrations. That is, to use the terminology from his 2016 talk, I had referred to his model as "a model used to emulate climate models", while he insists it is just a "climate model model*".

6.10.2.1 Dr. Connolly recognizes here that he should have accepted eqn. 6 as the author presented it, rather than go on to impose upon it his own mistaken interpretation.

Dr. Connolly nowhere in his round 1 referred to eqn. 6 as "*a model used to emulate climate models.*" Even the word "emulate" does not appear in his round 1.

In his round 1 "Comments on Sections 2.1 and 2.2," Dr. Connolly described eqn. 6 as "*a simple, semi-empirically derived model of the expected global SAT increase from an increase in GHG.*"

That is, Dr. Connolly wrongly represented eqn. 6 as a physically valid model.

6.10.2.2 Dr. Connolly's 6.10.2 claim of having described eqn. 6 as, "*a model used to emulate climate models*," completely misrepresents his actual description. It is clear that Dr. Connolly remembers his original and mistaken description of eqn. 6 (*cf.* items 6.7.2.1 and 6.9 above). Therefore he is here consciously misrepresenting his prior description.

In his 6.10.2, Dr. Connolly has constructed yet another falsehood.

6.10.2.3 In any case, Dr. Connolly is correct that he should have referred to eqn. 6 as an emulator of GCM air temperature projections, which it manifestly is.

However, following his original mistake, and the author's correction, Dr. Connolly has insistently departed from his obligation to accurately portray the analytical content of the manuscript. Insistent error does not recommend Dr. Connolly as a valid reviewer.

6.10.2.4 The manuscript should be reviewed on its internal merits, not on the author's language in a 2016 oral presentation. Dr. Connolly's use of external language is irrelevant to a review of the manuscript. Indeed to introduce external language violates the ethical obligation to provide a fair-minded review. Again: Dr. Connolly had an ethical obligation to review the manuscript on its own internal merits. He has violated that obligation

6.10.3 *However, this is hardly the "critically central point" he claims it to be, since my main criticisms of his model and his use of the model in his analysis revolve specifically on its role as "a climate model model."*

6.10.3.1 Dr. Connolly's inability to appraise eqn. 6 as an emulator depends exactly upon his wrongful insistence that it is a semi-empirical physical model. As he is thoroughly mistaken in insisting this, his criticism is thoroughly misguided.

It is indeed critically central that eqn. 6 emulates climate model air temperature projections, because it is exactly the successful emulation that justifies the subsequent propagation of uncertainty. Dr. Connolly has provided no evidence that he understands this obvious logic.

6.10.3.2 Dr. Connolly has misrepresented his "*main criticisms*" to be the author's use of eqn. 6 as a climate model model.

In fact, his main (and only) criticism can be found in Dr. Connolly's own round 1, "*7. Comments on Section 2.4*," in the paragraph beginning, "*Yes, climate models projections are step-wise in nature.*" and ending, "*So, the author's use of equation 2 for estimating the uncertainty of GMST projections is invalid.*"

The sole basis for Dr. Connolly's judgment is his Section 7 "*thought experiment*" supposing that, "*global mean surface temperatures in a given step have an influence on the projections of the next steps.*"

Dr. Connolly's surmise is that the manuscript uses eqn. 2 to propagate an error in global mean air temperature. It does not. Eqn. 2 propagates ±4 $Wm^{-2}year^{-1}$, the LWCF model calibration error statistic.

This is very evident in manuscript eqn. 7 and eqn. 8, plus surrounding text. Nowhere is error in air temperature propagated.

The uncertainty in annual projected air temperature arises strictly from the uncertainty in simulated atmospheric longwave thermal energy flux, as represented by the LWCF model calibration error.

Dr. Connolly's "*thought experiment*" utterly missed the point. His criticism of manuscript eqn. 2 is completely misguided.

6.10.4 *In subsection 1.2, he also objects to me suggesting that the model implies the GCM's projected global temperature trends are linearly related to greenhouse gas concentrations - while his model actually implies a linear relationship to greenhouse gas forcing. That's a valid objection, except that in the context of my review it is irrelevant, since the points that I was making merely referred to the relevance of a potential linear relationship.*

6.10.4 If we understand Dr. Connolly, he has admitted his fundamental mistake but has excused it on the grounds that the concept of "linear" occurred in both the author's presentation and in his mistaken description.

To be clear: Dr. Connolly wrongly imputed physical validity to eqn. 6. However, eqn. 6 is an arithmetical emulator. His mistake is as basic as is possible to get within science. Dr. Connolly assigned physical meaning where none exists.

7.1 He objects to me calling for a more rigorous assessment of how reliable an emulator of GCM temperature projections his linear model is. Instead, he insists that it is an exact emulator.

7.1 The term "*exact emulator*" occurs in the author's round 1 response, not in the manuscript. The manuscript describes eqn. 6 as able to "accurately emulate" GCM air temperature projections. A manuscript review should focus on the manuscript. Instead, he has again constructed a polemical response to the author's description of Dr. Connolly's ill-conceived criticisms. Doing so violates the ethics of review.

7.2 *If he could satisfactorily show that his linear model was a very good emulator for GCM global temperature projections, then in my opinion this would actually be a useful result which would surprise many in the climate science community.*

7.2.1 Dr. Connolly's statement here is truly incredible.

Exactly very good emulation of GCM global temperature projections is demonstrated in manuscript Figure 2, Figure 3, Figure 4, Figure 8, and Figure 9, as well as Supporting Information Figure S1-1, Figure S3, Figure S4, Figure S5, Figure S6, Figure S7, Figure S8, Figure S12 and Figure S13, each and all.

How could Dr. Connolly be unaware of this shatteringly obvious demonstration of accurate emulation? It is beyond stunningly incompetent to have not apprehended overwhelming evidence right before his eyes.

7.2.2 As Dr. Connolly has described the results obviously in evidence as a, "*result [that] would surprise many in the climate science community*," one may legitimately conclude he has agreed that the manuscript is publishable.

7.3 *Many climate scientists appear to be under the impression that the current GCMs are very complex computer models which include a lot of different "natural and anthropogenic forcings" ... So, if the author could quantitatively demonstrate just how well the various GCM temperature projections are approximated by a simple linear model of greenhouse gas forcing, then this would be an important result.*

7.3.1 GCMs in fact <u>are</u> complex computer models that include a lot of different forcings.

7.3.2 As noted in response item 7.2, and as even a casual inspection of the manuscript will show, the author has quantitatively demonstrated that eqn. 6 successfully emulates the air temperature projections of virtually any GCM, up through the CMIP5 versions.

Thus, Dr. Connolly has here implied that the manuscript is worthy of publication.

7.3.3 The manuscript goes on to show that these GCMs have no predictive value.

7.4 *We have actually alluded to this almost linear relationship in Soon, Connolly & Connolly, 2015 (Earth Science Reviews, vol. 150, p409-452).*

7.4.1 The author has examined [*Soon et al.*, 2015] to evaluate Dr. Connolly's claim, and found item 7.4 to be a misrepresentation.

Dr. Connolly appears to be referring to their Figure 29, which shows the, "*Attempts to fit the residuals from the solar model using changes in greenhouse gas concentrations.*" This is a LLSQ fit to air temperature residuals remaining after Soon, et al., had removed the (assumed linear) impact on air temperature of changes in Total Solar Irradiance.

The temperature residuals vary about zero and to any impartial observer should imply no systematic effect left in evidence. No matter, [*Soon et al.*, 2015] fitted the residuals to relate GHG radiative forcing with air temperature and unsurprisingly found a correlation indistinguishable from zero.

In the words of [*Soon et al.*, 2015],
"*There does not appear to be much of a relationship between the residuals and either of the datasets, suggesting that greenhouse gases do not appear to have much influence*

on Northern Hemisphere surface air temperatures, after the above solar relationship has been accounted for."

This empirical conclusion is correct, no matter the dangling preposition. That is, [*Soon et al.*, 2015] allude to the opposite of a relationship between GHG radiative forcing and air temperature, i.e., to no relationship.

In 7.4 Dr. Connolly proposed that they had alluded to a linear relationship. In fact, they alluded to a non-relationship.

Further, [*Soon et al.*, 2015] nowhere mention that GCM air temperature projections are linear in GHG radiative forcings.

The only possible conclusion from this evaluation is that Dr. Connolly has misrepresented the content of his own paper. No such relationship was alluded.

7.4.2 One also sees in this paper the source of Dr. Connolly's affection for LLSQ fitting. [*Soon et al.*, 2015] is littered with them.

7.5 *But, it could be useful to have a more quantitative assessment of exactly how close to linearity the relationships are for each model, along with a discussion of the residuals.*

7.5.1 As noted in item 7.2, that the manuscript provides exactly that quantitative assessment is in obvious and overwhelming evidence.

7.5.2 It was pointed out in round 1 response item 5.9.5.2, that the residuals consist primarily of simulated climate noise. Their analysis would be analytically worthless.

8.1 *However, it is not sufficient to repeatedly insist that the relationship is exact. He should quantify the deviations from linearity (of each of the models - not just the multi-model mean) and describe the residuals. If the residuals are very small, that's great, but he should demonstrate this rather than merely asserting it.*

8.1.1 One need merely inspect the relevant Figures within the manuscript and in the Supporting Information to understand the residuals are analytically trivial.

8.1.2 Relevant to any residuals, it is pointed out in Supporting Information Figure S2 Legend and related text that, "*Standard forcings were used [Meinshausen et al., 2011], which may be different from the RCP forcings employed within the GCM.*"

There is further discussion in the text, "*All eqn. 6 SRES simulations employed the standard IPCC forcings. In their projections, the various GCMs may depart more or less from the standard IPCC SRES forcings, accounting for at least some of the occasional excursions from eqn. 6 simulations.*"

Where is the analytical benefit from examining emulation residuals of those GCM projections for which the explicit forcings are not known? Where the [*Meinshausen et al.*, 2011] RCP forcings were apparently used unchanged, the emulations are little short of exact.

Dr. Connolly requires a meaningless exercise.

8.1.3 The author did not merely assert what is so obviously in evidence.

8.2 *He makes similar assertions in 6 of the 54 subsections (1.3, 5.5, 5.6, 5.7, 5.9, 5.10), i.e., ~11% of his response.*

8.2.1 Following response items 7.5 and 8.1.1-8.1.3, and Dr. Connolly's evident neglect of pertinent information in the SI, and his insistence on an analytically vacant exercise, his list in review item 8.2 merely presents further examples of Dr. Connolly's marvelously obdurate attachment to his mistakes.

8.2.2 Examination of the content of Dr. Connolly's list of author responses will demonstrate that Dr. Connolly has no grounds for complaint.

9.1 *He objects to the idea that he has poorly justified his [plus minus]4 W/m2 estimate of "the cloud error" and the appropriateness of his usage of this particular value for his analysis.*

9.1 The ±4 Wm^{-2}/year LWCF annual average calibration error was derived by [*Lauer and Hamilton*, 2013], not by the author (*cf.* Dr. Connolly's "*his ... estimate*").

The "*appropriateness*" was clearly explained in the opening paragraphs of Section 2.4, namely that LWCF error meant that the atmospheric thermal flux was simulated poorly.

It went on to note that GHG radiative forcing entered the atmospheric thermal energy flux and became part of it. The effects of perturbations below the limit of model resolution cannot be simulated.

This is very clear justification for using the LWCF uncertainty of [*Lauer and Hamilton*, 2013]. Nevertheless, Dr. Connolly evidently does not understand it.

9.2.1 *His use of this [plus minus]4 W/m2 value is a major component of his analysis, yet his rationale for choosing it is poorly justified, quite subjective and I had (and still have) several concerns with its validity.*

9.2.1 As noted above, the ±4 Wm^{-2}/year uncertainty was well justified, even in the first round manuscript, e.g., Section 2.4.1, lines 501-523. This section ended, "*Therefore, simulations of the climatic response to changes in GHG atmospheric forcing are limited by ±4 Wm^{-2} of uncertainty in the magnitude of thermal energy flux within the troposphere.*"

The rationale was first presented in the Introduction, lines 163-171, and further elaborated in lines 792-817.

A limit on model resolution is the obvious message of model calibration error. This analytical conclusion is standard in science, is inescapable, and is apparently beyond Dr. Connolly's grasp.

The rationale is even made even clearer in the revised manuscript, where the full derivational logic is provided in lines 517-541, along with the dimensional analysis yielding ±Wm^{-2}/year.

Step-by-step derivational details are given in the revised Supporting Information Section 6.2.

Dr. Connolly has passed over all of this in silence, preferring instead to construct a foolish polemic (below).

In this manifest extreme of ignorance and negligence, Dr. Connolly demonstrates he is unfit to review any scientific manuscript.

9.2.2 *Independently, all four of the other reviewers raised similar concerns over this value, ...*

9.2.2.1 No other reviewer raised concerns over the ±4 W/m^2 value of LWCF error.

9.2.2.2 Dr. Connolly's specific objections to LWCF error are found in his round 1 "*Comments on Section 2.4,*" and are here summarized as,

1. the mistaken notion that the LWCF error is a spatial correlation (it is not);
2. that it may not be a global uncertainty (it is), and;
3. that global mean cloud cover did not change much (irrelevant to the magnitude of simulation uncertainty).

Therefore, Dr. Connolly did not raise concerns over the ±4 W/m^2 value of LWCF error, either.

No other reviewer raised these or similar objections.

Only round 1 reviewer 4 expressed reservations about the LWCF error, but this concern was that the true LWCF uncertainty might be different were the models not tuned.

Thus, Dr. Connolly has, yet again, constructed a specious argument.

9.2.3 *... as did Dr. Brown in his online critique of the author's 2016 talk (https://patricktbrown.org/2017/01/25/do-propagation-of-error-calculations-invalidate-climate-model-projections-of-global-warming/).*

9.2.3.1 Dr. Brown's argument was that the ±4 Wm^{-2}/year LWCF error statistic was a constant-sign physical offset error (it is not), and that the "per year" denominator was wrongly included (it was not).

Therefore, Dr. Brown did not raise concerns over the ±4 W/m^2 value of LWCF error.

The set-aside of Dr. Brown's first argument obviously follows from the "±" attached to the ±4 Wm^{-2}/year LWCF calibration error statistic. I.e., "±" disproves both that the statistic has a constant sign and that it is a physical offset error.

The set-aside of Dr. Brown's second argument follows from the derivations of the LWCF error statistic, demonstrating the units are $Wm^{-2}year^{-1}model^{-1}$.

This derivation was provided on Dr. Brown's blog site. However, Dr. Brown ignored it. So did Dr. Connolly.

The derivation was provided in even more detail in the revised manuscript. However, neither the obvious nor the demonstrated have any apparent influence on Dr. Connolly's thinking. Evidently Dr. Connolly's tactic of debate is to ignore analytical disproofs of his favored positions.

9.2.3.2 Dr. Brown's arguments are not similar to those of Dr. Connolly, making review item 9.2.3 another specious claim.

Merely the fact of taking issue with using the LWCF error statistic derived by [*Lauer and Hamilton*, 2013] does not justify citing widely varying arguments as "similar."

Further, the author showed Dr. Brown's concerns were wrong. There is no corroboration for Dr. Connolly in a misguided argument.

9.2.4 *Additionally, during Dr. Brown's debate with the author on his blog, Dr. Brown physically contacted one of the researchers who wrote the paper the author's [plus minus]4 W/m2 value was taken from:*

> *"I have contacted Axel Lauer of the cited paper (Lauer and Hamilton, 2013) to make sure I am correct on this point and he told me via email that "The RMSE we calculated for the multi-model mean longwave cloud forcing in our 2013 paper is the RMSE of the average *geographical* pattern. This has nothing to do with an error estimate for the global mean value on a particular time scale."."*
> *- Dr. Patrick Brown, February 1, 2017. https://patricktbrown.org/2017/01/25/do-propagation-of-error-calculations-invalidate-climate-model-projections-of-global-warming/#comment-1443*

9.2.4 Dr. Connolly has neglected to reveal that the author's February 5, 2017 at 8:50 pm post resolved Dr. Brown's objection.

In his "Point 1" the author worked step-by-step through the LWCF error derived in [*Lauer and Hamilton*, 2013].

The author analytically showed the LWCF error statistic is of dimension $\pm Wm^{-2}$/year/grid-point. [*Lauer and Hamilton*, 2013] summed this grid-point LWCF uncertainty across the globe to produce the global annual average calibration error statistic the author used in the manuscript analysis.

The author summarized his response to Dr. Lauer's explanation by observing that,

> "*The crux issue is that [Dr. Lauer] referred the error to a "particular time scale." The LCF error they calculated is an error estimate for the global mean simulated value on*

*an **averaged** time-scale. The mean error is a representative **average** time-scale error, not a **particular** time-scale error.*

"*It should be clear to all that an average of errors says nothing about the magnitude of any particular error, and that an annual average says nothing about a particular year or a particular time-range.*"

[*Lauer and Hamilton*, 2013] themselves call their derived global error statistic, a 20-year annual mean bias.

An annual mean statistic is 'mean magnitude per year.' The RMSE 20-year annual mean bias statistic is '±mean magnitude per year.'

The author's posted response thus fully resolved Dr. Brown's concern. But Dr. Connolly chose to uncritically cite Dr. Brown, without at all investigating the author's reply. This is rhetorically opportunistic, but scientifically dishonest.

9.2.5 S*o, regardless of whether the author's use of this [plus minus]4 W/m2 value is actually valid or not, it is clearly something that all of the reviewers (including me) find to be poorly justified and highly contentious. Presumably, many of the readers of ESS would have similar concerns with it.*

9.2.5.1 Dr. Connolly has made another incredible argument. He suggests that even if the author is correct (which he is), the wrongly conceived objections and concerns of others should impact publication.

9.2.5.2 In making this argument in round 2, Dr. Connolly has ignored the new and detailed derivations that **establish the very point** he contests, even after supposedly examining the revised manuscript in which they appear.

9.3.1 *However, while 13 of the 54 subsections (6.1, 6.2, 6.3, 6.4, 6.5, 6.6, 6.9, 7.3, 7.4, 7.5, 7.6, 7.7, 8.4), i.e., ~24% of his response, refer to the concerns I raised on this point, the actual modifications he made to the manuscript on these points were minor, and did not adequately address these concerns.*

9.3.1.1 The author's revisions were minor because Dr. Connolly's concerns were wrong, were without critical force, and were unworthy of incorporation. Even a casual reading of the cited response items is sufficient to establish these judgments.

9.3.1.2 The author thoroughly addressed Dr. Connolly's concerns with a major new analysis opening revised Section 2.4.1.

Further, new Supporting Information Section 6.2 goes through the entire dimensional analysis and the derivational logic of the CMIP5 LWCF error reported in [*Lauer and Hamilton*, 2013].

9.3.1.3 Dr. Connolly has passed over these centrally important additions in complete silence. Indeed, he evinces no knowledge of them whatever. His empty dismissal is indistinguishable from the sham criticism of someone who has not even read what he has criticized.

9.3.2 *Therefore, if the author wishes to continue using it for his analysis, it is absolutely essential that he provides a much better rationale and justification, as well as satisfactorily address the many concerns raised by the reviewers (including me) on this point.*

9.3.2 It should be clear, following response items 9.1 through 9.3.1, that that Dr. Connolly's argument is empty of content.

All of his cited criticisms involved mistakes such as supposing that CMIP5 LWCF error did not impact the resolution of a simulation, or (incredibly) that a statistical average does not include a 'per-unit' denominator, or that a "±" statistical uncertainty is instead an energetic perturbation.

In his item 9, Dr. Connolly did not make any new critical arguments. Indeed, he made no critical arguments at all. He merely declared his disagreement and referred to old and resolved criticisms. He then went on to ignore the extensive new analysis in the revised manuscript.

It is almost as though he wrote his round 2 review without having read the revised manuscript.

It is an element of charity to suppose that these object failures indicate only that Dr. Connolly is critically incompetent.

10.1 *He claims that I (and apparently the rest of the reviewers) don't understand how to deal with the propagation of errors. Hence, he insists that our objections to the inappropriateness of his approach are mistaken.*

10.1.1 Where Dr. Connolly wrote "*claims*" the author provided demonstrations.

Where in Dr. Connolly's review is propagation of systematic error properly described? Round 1 response item 7.5 provided the correct approach, with citation to [*Vasquez and Whiting*, 2006]. Dr. Connolly has ignored that provision.

The revised manuscript cited the Joint Committee for Guides in Metrology [*JCGM*, 100:2008] as well as [*Garafolo and Daniels*, 2014; *Vasquez and Whiting*, 2006] as precedent for the author's approach. Dr. Connolly has ignored those citations and that precedent, as he has ignored everything else of relevance in the round 1 response and in the revised manuscript.

10.1.2 Response items 1.4, 2.1, 2.2, 9.3.2.1, and 10.4.3 summarize some of the reviewer mistakes concerning propagation of error. Dr. Connolly is invited to select which of them he sees as correct.

The Summary at the head of each author of the five responses gave directions to the location of these and other mistakes in each review. Dr. Connolly has apparently received these responses.

However, he has neither cited, nor refuted, nor even contested a single one of the author's corrections. He has merely dismissed them by argument from a spurious consensus (various reviewers chanced variable mistakes).

Dr. Connolly's empty and unsupported dismissals do not constitute review arguments. They are no more than a jejune polemic.

10.2 *16 of the 54 subsections (1.3, 1.4, 1.6, 1.8, 1.9, 4.1, 5.9, 7.1, 7.4, 7.5, 7.6, 7.7, 7.8, 8.2, 8.3, 8.4), i.e., ~30% of his response to my review, includes him claiming I don't know anything about the propagation of errors and uncertainties and that I didn't understand the arguments he was making.*

10.2 As shown below, Dr. Connolly has misrepresented the content of the listed response items. None of them, not one, contest his knowledge of error propagation. He could well have made the exposed mistakes while knowing the method.

Some items do not discuss error propagation at all. All of them, however, show that Dr. Connolly did not understand the manuscript analysis.

Very significant is it that Dr. Connolly nowhere showed the author was mistaken in criticizing his review. Dr. Connolly nowhere refuted the author's arguments. Dr. Connolly nowhere provided more incisive arguments refuting the author.

Throughout, he has merely complained that the author contested his views. He has also called upon external authorities to support his rejectionism, but without demonstrating that any of them are substantive.

In short, Dr. Connolly's rejoinders are utterly inadequate. Specifically:

1. Response item 1.3 does not discuss propagation of error at all. Item 1.4 corrected Dr. Connolly's meaningless neologism, "*uncertainty errors*." Items 1.6, 1.8, 1.9 and 4.1 said nothing of Dr. Connolly's knowledge or understanding of error propagation. Item 5.9 discussed Dr. Connolly's mistaken view of eqn. 6, not his knowledge of error propagation.

2. Item 7.1 disagreed with Dr. Connolly's unfounded suggestion to delete the error propagation. Item 7.4 discussed Dr. Connolly's mistaken views of cloud error, not propagation of error.

3. Item 7.5 described the [*Vasquez and Whiting*, 2006] method of propagating systematic error, which Dr. Connolly subsequently and consistently ignored. It did not contest Dr. Connolly's knowledge or understanding.

4. Item 7.6 contested Dr. Connolly's view of what was propagated, i.e., uncertainty not error, not his understanding of propagation.

5. Items 7.6.2 and 7.7 showed Dr. Connolly mistakenly assigned the cumulated uncertainty to temperature, rather than, correctly, to model LWCF thermal flux calibration error. They did not contest his understanding or knowledge of error propagation.

6. Item 7.8 merely summarized items 7 as indicating Dr. Connolly's failed criticisms, none of which concerned his knowledge or understanding of methodological error propagation.

7. Item 8.2 merely took issue with Dr. Connolly's unsubstantiated recommendation to drop error propagation from the manuscript.

8. Items 8.3 and 8.4 merely again contest Dr. Connolly's subjective opinions. In 8.4, for example, Dr. Connolly incorrectly suggested that use of propagation eqn. 2 was inappropriate. He based this view on his entirely wrong "*thought experiment*" (round 1 response item 7.6.2) and his mistaken suggestion that systematic error does not propagate as an uncertainty (round 1 response item 7.5). See also item 6.10.3.2 above.

Thus, none of the listed items questioned Dr. Connolly's knowledge or understanding of error propagation. They expose his various analytical mistakes, mistakes that may well have been possible despite his purported methodological understanding.

One notes again that Dr. Connolly has nowhere shown that the author's assessments of his mistakes are themselves incorrect.

10.3 *For the record, I am very familiar with the propagation of errors. I read his manuscript and supplementary information thoroughly and I had carefully considered the arguments he used to justify his approach. But, the approach he has taken is flawed and inappropriate.*

10.3 It is very clear that Dr. Connolly's purported familiarity with error propagation has not helped him.

Dr. Connolly claims to have read the manuscript and SI thoroughly, and yet his new review displays no familiarity with the revised sections. He just baldly dismissed them as inadequate, and without any analysis at all.

He also showed no cognizance of the analytical message of any of the Figures.

Perhaps Dr. Connolly claims to have read the prior version thoroughly. Nevertheless he was badly mistaken to suppose that eqn. 6 was a semi-empirical physical model, and that uncertainty in GMST is propagated through eqn. 6 (*cf.* the cited response references in numbered item 8 above).

His claims of flaw and inappropriateness were thoroughly demonstrated as profoundly misguided.

10.4.1 *In my original review, I explained in considerable detail several reasons why his approach was flawed.*

10.4.1 Dr. Connolly's original review was thoroughly wrong. His claims that the author's analysis was flawed did not itself withstand critical scrutiny.

Viz.:

- Dr. Connolly did not understand eqn. 6.
- Dr. Connolly did not understand the significance of the error in simulated cloud cover.
- Dr. Connolly did not understand the meaning of the LWCF ±4 Wm^{-2}/year calibration error statistic.
- Dr. Connolly nowhere showed any understanding that linear extrapolation of forcing is subject to linear propagation of error.
- Dr. Connolly did not understand that LWCF enters the tropospheric thermal energy flux.
- Dr. Connolly showed no realization that annual uncertainty in simulated LWCF obscures the small annual radiative forcing change due to CO_2.

In short Dr. Connolly showed no understanding of any of the critically central concepts in the manuscript analysis. He showed no ability to mount any sort of cogent review.

10.4.2 *Reading through his responses to the other reviewers (and also Dr. Brown's online critique), it is apparent that they also independently came to the same conclusion for similar reasons.*

10.4.2 The author has already shown the item 10.4.2 claim of similarity to be false (items 2.1, 9.2.2, and 9.2.3).

10.4.3 *I haven't seen the 16 reviews which the author had already received before his ESS submission, but I wouldn't be surprised if many of the 13 negative reviewers had also reached their conclusion for similar reasons.*

10.4.3 Dr. Connolly can be reassured that the other reviewers found a way to their own brand of nonsense.

For example only prior reviewers suggested that the ±15 C centennial uncertainty meant that the model was wildly oscillating between ice house and hot house climates. And yet, at least three reviews did so.

Only prior reviewers supposed that the Planck feedback compensates for simulation uncertainty.

Only prior reviewers supposed that ±4 Wm^{-2}/year is an energetic perturbation.

Only prior reviewers asserted that to calculate a global average of cloud forcing is an assertion that all clouds are identical.

Dr. Connolly should feel relieved on being informed that these examples of truly naïve mistakes are far from exhaustive.

10.4.4 *The late biochemist and science fiction writer, Isaac Asimov, once jokingly quipped that: "people who think they know everything are a great annoyance to those of us who do". But, how long is the author going to insist that everyone is out of step except for him?*

10.4.4 The author has only claimed that those who are demonstrated to be wrong, are in fact wrong. That demonstration includes Dr. Connolly. It seems likely this demonstration powers Dr. Connolly's dishonest, unpleasant, and scientifically vacuous polemic to which the author has been exposed.

11. *He claims that the recommendations I made for how he could restructure the manuscript to refocus on some of the genuinely useful aspects of the paper "would leave nothing publishable".*

11. Dr. Connolly recommended removing half of the Introduction, all of Section 2.1 and 2.2 (concerning radiative mean free path and CO_2 fractional forcing), Section 2.3 then perforce becomes irrelevant and would suffer removal. He then wants to remove all of Section 2.4 deriving and assessing propagation of error and all of conclusion Section 3.

This leaves only a discussion of precision vs. accuracy and the analytical nonsequitur of cloud error. How is that publishable?

Dr. Connolly recommended gutting the manuscript, and then below goes on to deny recommending that the manuscript be gutted

11.2 *I disagree. In my original review, I identified several good points and arguments in the paper which could easily be fleshed out and developed into a short, but potentially very important, paper. However, since the author is apparently uninterested in doing this, then my recommendation is that the article should be rejected.*

11.2 Dr. Connolly writes that he did not recommend excising the analysis, after recommending that the manuscript analysis be discarded.

As shown in exhaustive detail above, Dr. Connolly's second round review is not a review. It is a dishonest polemic. It is empty of critical content.

His first round review was demonstrated as thoroughly misguided and wrong, wherever it was not irrelevant.

Dr. Connolly has recommended rejection without having demonstrated cause.

12. **The author's responses to the other reviews**

12.1 *I have not seen the other four reviews in completion, but much of their assessment can be inferred from the quoted text in the author's responses to the reviewers.*

12.1 The author has left in Dr. Connolly's 12.1 because Dr. Connolly admits there to having received the author's responses. The full critical assessment above of Dr. Connolly's polemic shows that he has misrepresented them. He now has does not have the excuse of ignorance for having done so.

12.2 Dr. Connolly's subsequent soliloquy has been removed as irrelevant

12.3 *With that in mind, it is striking that between all five of us, ...*

12.3 On being corrected Dr. Connolly is likely to assert the author should have known he meant to write **'among** all five of us...'

12.4 *... we have independently identified several of the same key criticisms more than once.*

12.4 The author has shown Dr. Connolly has consistently misrepresented this case (*cf.* response 10.4.2 above and the cited items within).

12.5 *Dr. Brown's online critique of the author's 2016 talk also separately identifies several of these criticisms as well: https://patricktbrown.org/2017/01/25/do-propagation-of-error-calculations-invalidate-climate-model-projections-of-global-warming/*

12.5.1 Dr. Brown's arguments were unique to him, apart from the year^{-1} mistake, which also appeared in round 2 review #1. Thus, Dr. Connolly has once again made a specious argument.

12.5.2 The author demonstrated that Dr. Brown's arguments were wrong throughout.

That is, the LWCF calibration error statistic in [*Lauer and Hamilton*, 2013] is global, annual, and includes a year^{-1} denominator; the ±4 Wm^{-2} is not a positive-sign physical offset error; calibration error statistics do indeed propagate through subsequent calculations.

Dr. Connolly has evidently not grasped any of this. His agreement with Dr. Brown's mistaken analysis does not inspire any confidence in Dr. Connolly's scientific judgment. This is especially true in light of the added sections concerning the [*Lauer and Hamilton*, 2013] statistic in the revised manuscript.

12.6 *This demonstrates that the major criticisms which we have collectively identified with the author's analysis are not unique to the climate modelling community as he had claimed. Instead, his main analysis is fundamentally flawed and invalid.*

12.6.1 Dr. Connolly again misrepresented the case. The author did not claim the mistakes were unique to the climate modeling community. The author said, in his 2016 presentation that, "every single climate modeler review was incompetent." That judgment does not indict the entire community. It indicts the specific reviewers.

12.6.2 An agreement of error among those demonstrated to be erroneous is no indication that the author's analysis is flawed or invalid. Dr. Connolly has made an argument from

flawed and invalid authority. In view of the extraordinarily poor thinking Dr. Connolly has displayed throughout his professionally and ethically depauperate excuse of a review, his misjudgment here is hardly surprising.

12.7 *It seems that multiple reviewers have repeatedly and independently identified several key criticisms of his analysis.*

12.7 The author has shown they have done no such thing.

12.7 *However, rather than actually addressing these recurring criticisms, his typical response seems to be to imply that his critics are stupid and/or ignorant.*

12.7.1 The author addressed the all reviewer criticisms exhaustively and in detail. Dr. Connolly has received the other responses and should know this beyond any doubt.

Despite this, he has positively averred here that the author did not address these criticisms. Dr. Connolly is either ignorant of the content of those responses and has produced a facile falsehood, or does know the content and has composed a deliberate falsehood.

12.7.2 Let me be specific here. The author has shown the other reviews were misguided. This said nothing of the intellectual quality of the reviewers.

However, following from Dr. Connolly's appalling display here, the author is ready to conclude the Dr. Connolly himself is both stupid and ignorant.

12.8 *This approach of "shooting the messenger" seems to be the author's default approach to dealing with any criticism of his analysis.*

12.8 As is usual with a dishonest polemicist, Dr. Connolly has finished by accusing the author of the failing on display in Dr. Connolly's own polemic. As evidence one observes that not once did Dr. Connolly address any of the analytical responses the author provided. He merely dismissed them baldly, and with prejudice and pejoration.

The author demonstrated that Dr. Connolly's round 1 review was wrong in every important particular. Dr. Connolly neither addressed this demonstration, nor refuted it, nor modified his criticism in light of it.

Instead he has attacked the author personally, and misrepresented both the author's responses and the content of the other reviews.

12.9 *Apparently, I am not the first person to notice this, as Dr. Brown made a similar observation after several weeks of debating the author on his "propagation of error and the reliability of global air temperature projections" analysis on Brown's blog:*

> *"Hi Dr. Frank,*
> *I was hoping that we would be able to have a productive scientific discussion on this topic but I am pretty pessimistic that there is much hope for that moving forward. One prerequisite for a truly productive discussion is that both parties are charitable*

and do their best to understand the substantive points being made by the other party. However, it seems to me that your primary goal is not to understand my arguments but rather to score 'debate points' by any means necessary. Specifically, you have a tendency to look past the substance of a point being made in order to create a straw man, destroy it, and then declare victory...all while exuding condescension. This may make you look intelligent and authoritative to some 3rd party observers but it does not actually make you any more correct."
- Dr. Patrick T. Brown, February 14th 2017. https://patricktbrown.org/2017/01/25/do-propagation-of-error-calculations-invalidate-climate-model-projections-of-global-warming/#comment-1469

12.9 As is now usual when making this sort of argument, Dr. Connolly has taken refuge in dishonesty. He excluded the author's response to Dr. Brown made in the post of February 20, 2017 at 1:21 pm. In that post, the author detailed Dr. Brown's complaints and, one-by-one showed them to be unfounded.

But this is of no apparent interest to Dr. Connolly, whose main intent, after analytical misrepresentation, appears to be character assassination.

13 **Final remarks**

13.1 *In my earlier review of this manuscript, I showed that the main analysis of the paper was seriously flawed and invalid.*

13.1 Dr. Connolly did nothing of the kind. The author showed his review was wrong throughout.

To reiterate a central point, Dr. Connolly supposed the propagation of error to be invalid on the grounds of a "*thought experiment*" that focused on uncertainty in GMST. Compare this to the propagation itself, which involved the LWCF calibration error statistic.

His assessment was entirely without analytical merit. The author demonstrated this (*cf*. round 1 response items 7.6.2 and 7.7, as well as item 6.10.3.2 above). Nevertheless, Dr. Connolly has both ignored the demonstration and has reiterated his mistake.

13.2 *On the other hand, I also identified several useful points and arguments which were (and still are) valid. In recognition of these positive aspects, I had suggested that the paper could be restructured and revised with a different emphasis and I therefore recommended that the article should be "returned to the author for major revisions".*

13.2 As noted above, Dr. Connolly recommended removing manuscript Sections 2.1, 2.2, most of 2.3, 2.4, parts of the Introduction, and all of the conclusion, keeping only part of the Introduction and Section 2.3.1.

This he did on the basis of his wrong-headed review. And, after having been demonstrated to being wrong-headed, Dr. Connolly has insistently reiterated his mistakes. This is to be hard-headedly wrong headed.

13.3 *However, the author apparently is uninterested in my suggested restructuring and insists on continuing with his flawed and invalid analysis.*

13.3 The author has insisted on the original analysis because it is correct. The author is uninterested in Dr. Connolly's restructuring because it is an eminently senseless idea and is based on Dr. Connolly's utterly flawed and invalid review.

13.4 *The author has made a few minor alterations to the manuscript since the previous submission in an attempt to address a few relatively minor issues and fix a few typos.*

13.4 Again, Dr. Connolly offers an incredible opinion in light of the extensive new analysis in revised manuscript Section 2.4 and in Supporting Information Section 6.2. These are not *relatively minor*.

Dr. Connolly apparently here betrayed a likelihood of not having read the manuscript he has so poorly assessed.

13.5 *However, none of these alterations have satisfactorily addressed any of the substantive major criticisms identified by the reviewers.*

13.5 Following the object evidences of Dr. Connolly's extremely poor and repeatedly dishonest commentary, and following the authors extensive and definitive responses to all the reviewers, the author can only observe that item 13.5 is another example of Dr. Connolly's failure of scientific integrity.

14 Therefore, I recommend the manuscript should be rejected.

14 Dr. Connolly has left his recommendations to be without merit or worth.

15 Reviewed by:
Dr. Ronan Connolly
Independent scientist, Dublin, Ireland

15 The author has retained Dr. Connolly's identifier, which he insisted upon and defended, in deference to the pride he must feel in his work.

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