

Reviewer #2 Evaluations:
Recommendation: Reject
Grammar improvements needed: No
Commentary: No
Willing to review a revision: No
Do you have a potential conflict of interest?(Required): No
Annotated: No

Reviewer #2 (Comments to Author):

Review of Patrick Frank's "Propagation of error and the reliability of global air temperature projections" revised manuscript

By Ronan Connolly

("Reviewer #5" in the author's submitted list of responses to the reviewers)

In January 2017, I was asked to review an earlier draft of the author's manuscript which had been submitted to ESS. After much consideration and careful review, my opinion at the time was that the main statistical analysis in the manuscript was fundamentally flawed, but that there were still some valid points and arguments which were important and could be of value to the climate science community, if the author were prepared to restructure the paper to focus on those important points instead. With that in mind, I had recommended the article should be "returned to the author for major revisions". However, I understand that the final editorial decision was to reject the manuscript.

Despite this, the author has decided to resubmit his rejected manuscript to ESS essentially unaltered, albeit with some small changes addressing a few minor technical points and typos identified by the reviewers. 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").

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>. 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."

(*) Note: *[The audience member was actually a co-author of mine, Willie Soon. Dr. Soon apparently also had recommended to the organisers of the meeting that the author present the talk. 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).]*

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

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. The author debated this critique in the comments section of Dr. Brown's accompanying blog post: <https://patrickbrown.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.

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. After all, most of us have received negative reviews at some stage which have been simply flat-out wrong. And perhaps there is some truth to the author's theory that most of the negative reviews have come from a particular sector of the scientific community, i.e., climate modellers (*).

(*) Note: *[Although, I wonder whether he knows for sure that all of the negative reviews he had received by July 2016 were exclusively from climate modellers, especially given his comments on how he dislikes having non-anonymous reviewers. For what it's worth, I am not a climate modeller, and in fact have been openly critical of the reliability of the CMIP3 and CMIP5 GCMs which the author criticises in the manuscript, e.g., see Soon, Connolly & Connolly, 2015 (Earth-Science Reviews, vol. 150, p409-452) or Connolly, Connolly & Soon, 2017 (Hydrological Sciences Journal, vol. 62, p1317-1340). Also, 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. 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.]*

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. The other four reviewers of the previous version all appear to have recommended rejection. And, since the author has explicitly stated that he is uninterested in making the major revisions I had recommended would be necessary to make the manuscript worthy of publication, my recommendation also reverts to "Reject". So, in my view, the decision of whether to consider accepting this current manuscript probably comes down to whether or not he has successfully proven that the five reviewers of the previous version (including me) were all "mistaken", "misguided" and "[without] critical merit".

With that in mind, 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.

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

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

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:

- "Science by assertion", i.e., insisting that he is correct... because he says so
- 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!)

- Claiming I was "confus[ed]" or suffering from some "misperception" and had not understood his arguments (not true!)
- Ignoring/dismissing them

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

As mentioned above, his response to my review is rather lengthy - comprising 8 sections, each with multiple subsections and even sub-subsections. However, below I summarise and address what appear to be his chief objections to my review:

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

My comments, criticisms and recommendations would have been the same if I had remained anonymous. I merely appended my name after I had finished my review and ticked the appropriate box on the ESS website. However, I am of the opinion that if a reviewer is prepared to publicly stand over the validity and relevance of their review comments (which I was, and still am), then they shouldn't need to rely on the shield of anonymity.

As a reviewer of the manuscript, I was already aware of the author's identity (e.g., it's included within the manuscript). So, if the author genuinely believes that knowing my name introduces "personality [to] the process", and that this might reduce the scientific objectivity of his responses, then this is purely on his side of the process.

2. 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".

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, 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".

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".

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.

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". 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".

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".

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.

3. 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.

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. 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" and this has led to a certain mystique about the output from GCMs among the scientific community - and also the wider public, e.g., see this popular 2015 Bloomberg webpage: <https://www.bloomberg.com/graphics/2015-whats-warming-the-world/>

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

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.

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.

4. 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.

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. Independently, all four of the other reviewers raised similar concerns over this value, 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/>).

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>

So, 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.

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.

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.

5. 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.

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.

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.

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

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?

6. 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".

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.

The author's responses to the other reviews

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.

Firstly, I will reiterate that I agree with the author that the distinction between precision and accuracy is often missed, and there is an over-reliance on inter-model comparisons instead of encouraging more comparisons of model results to experimental data. This is apparent from several of the reviews, and these were two of the good points of the author's manuscript I had noted in my original review. However, the main focus of his analysis is flawed, i.e., his attempted quantification of the uncertainties associated with GCM temperature projections.

Between the five reviewers there seems to have been a range of views on how reliable (or not) the GCM global temperature projections are. Reviewers #4 and #6 both concede that the current GCMs are not perfect, but argue that their global temperature trend projections are still useful. It is difficult to ascertain Reviewer #3's views from the limited extracts of his/her review provided. However, Reviewer #1 seems to believe that the GCM projections have some reliability but acknowledges the possibility that there could be systemic biases common to all the current GCMs. On the other hand, I (Reviewer #5) have been highly critical of the reliability of current GCM hindcasts (and by extension, their projections). For instance in Soon, Connolly & Connolly, 2015 (Earth Science Reviews, vol. 150, p409-452), we argued that the CMIP5 GCM hindcasts did a poor job of simulating natural climate change, and that after correcting for urbanization bias, the hindcasts were unable to reproduce the observed 20th century Northern Hemisphere temperature trends. Similarly in Connolly, Connolly & Soon, 2017 (Hydrological Sciences Journal, vol. 62, p1317-1340), we demonstrated that the CMIP5 hindcasts were unable to reproduce 20th century Arctic sea ice trends.

With that in mind, it is striking that between all five of us, we have independently identified several of the same key criticisms more than once. Dr. Brown's online critique of the author's 2016 talk also separately identifies several of these criticisms as well: <https://patrickbrown.org/2017/01/25/do-propagation-of-error-calculations-invalidate-climate-model-projections-of-global-warming/>

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.

It seems that multiple reviewers have repeatedly and independently identified several key criticisms of his analysis. However, rather than actually addressing these recurring criticisms, his typical response seems to be to imply that his critics are stupid and/or ignorant. This approach of "shooting the messenger" seems to be the author's default approach to dealing with any criticism of his analysis.

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>

Final remarks

In my earlier review of this manuscript, I showed that the main analysis of the paper was seriously flawed and invalid. 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". However, the author apparently is uninterested in my suggested restructuring and insists on continuing with his flawed and invalid analysis.

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

Therefore, I recommend the manuscript should be rejected.

Reviewed by:

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