

From: **Patrick Frank** pfrank830@earthlink.net  
Subject: Re: FOR-17-0244  
Date: April 17, 2018 at 9:50 PM  
To: Young, Peter p.young@lancaster.ac.uk  
Cc: Derek Bunn dbunn@london.edu

PF

Dear Peter,

I presently have a manuscript under review at the Journal of Chemical Physics. The reviews raised points to be addressed. The JCP editor conditioned acceptance on their resolution.

I presently have a manuscript under review at the Journal of Forecasting. The review raised points to be addressed. The JF editor rejected the manuscript.

My CV includes 70 peer reviewed papers. The JCP response is standard. The JF response is not.

This is a well-trodden path for me. It has always ended the same way.

A new submission will have new reviewers who will raise new points. The logic of rejection is repeated.

If you will condition publication on resolution of the present review and an acceptable re-write, I will undertake it. However, I am unwilling to participate in a losing game.

With respect to your comments, the coefficients of the linear emulator are indeed derived from fits to individual model data sets.

Manuscript page 9, last paragraph, directs to the Supporting Information where the method is presented in detail. That explanation can be moved into the manuscript

You wrote it worries you that the emulation equation is a static relationship, while the climate is dynamic, quasi-periodic, etc. However, stated repeatedly in the manuscript is that the emulator is not concerned with the climate, or with climate physics. It is meant to reveal the behavior of climate models, and only that.

The emulation equation does that extremely well. It is therefore the climate models that are static with respect to projected temperature. It should thus instead worry you that the climate models, which the equation emulates so well, apparently project temperature as a static relationship with GHG forcing.

That is the point of the analysis after all: \*climate models\* merely linearly extrapolate greenhouse gas forcing. That's all they do. This is what you should worry about.

The worry that the model emulator does not reflect the climate itself is not relevant to the meaning and use of the equation.

Finally, you may be interested to know that the global temperature anomaly record is nearly meaningless. The field completely ignores systematic measurement error, which is not Gaussian and does not average away. Nor does systematic error arising from uncontrolled environmental variables subtract away. An estimated lower limit of residual measurement uncertainty is (+/-)0.5 C.

If you like, I have published on this problem. The paper is open access here:

[http://meteo.lcd.lu/globalwarming/Frank/uncertainty\\_in%20global\\_average\\_temperature\\_2010.pdf](http://meteo.lcd.lu/globalwarming/Frank/uncertainty_in%20global_average_temperature_2010.pdf) (869.8 KB)

Even more incredibly, every group including BEST ignores the limited resolution of the historical thermometers.

The field neglects all this in favor of self-serving assumptions that all measurement error is random and, implicitly, that all temperature sensors have infinite resolution.

In any case, please let me know if you will accept to continue the first JF submission.

If you decide not, then please have the grace to change the web-decision to 'decline' rather than "reject," because the decision to exclude will have been made in the absence of any justifiable analytical cause.

Thank-you for your consideration, and best wishes,

Pat

Patrick Frank  
Palo Alto, CA 94301

email: [pfrank830@earthlink.net](mailto:pfrank830@earthlink.net)

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These things are, we conjecture, like the truth;  
But as for certain truth, no one has known it.

Xenophanes, 570-500 BCE

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On Apr 16, 2018, at 5:03 AM, Young, Peter <[p.young@lancaster.ac.uk](mailto:p.young@lancaster.ac.uk)> wrote:

Dear Patrick,

First, can I say that your statement "Honestly, your response is beyond comprehension", is not the kind of language that encourages an editor to look again at a paper that has been rejected. In future, you might think a little more about about the way you communicate with those who are trying to do their unpaid job in as fair a manner as possible.

However, I can see you are frustrated by your experiences trying to publish material in the standard climate literature, so I will ignore this indiscretion on your part. I should stress that I have no connections or particularly good feelings towards the climate community because I too have had papers rejected by them on what I considered spurious and questionable grounds. So you should not think that any comments I make, or indeed those made by the referee in this case, are anything but our own feeling about your paper based on our own knowledge and experience. I can assure you that we are both fair in our judgements and nothing would give us more pleasure than to be able to accept a paper of yours, provided it is suitable for the JOF and is reasonably comprehensible to its readership.

On the above basis, I have now had time to look again at your paper, as well as the quite comprehensive review of the paper that was prepared by a referee. I should say that I originally looked at your paper and felt that, in style and content, it was not really suitable for JOF. This is usually the first stage in the reviewing procedure and, if I have such reservations, I usually send it to a single referee without, in any way, passing on my reservations to the referee. Indeed, I selected this referee because I know he is very fair in his judgement and is one of the few people in the forecasting community who knows about climate data and climate models. On this basis, I feel that I could do no better at reviewing the paper than this referee. His review is quite comprehensive and his critique of the paper seems most appropriate to me. If we put the technical criticisms on one side (although I think these are important and need to be answered), then the main problem that he sees with the paper is that you have made little, if any, effort to communicate satisfactorily with a forecasting audience.

I cannot but agree with the referee in this last regard; indeed it seems to me that you may well have submitted the same paper to JOF that you had rejected by a climate science paper. I hope that, with hindsight and if this is the case, you will see this was a major mistake. If you want to have a paper published in a journal then you must take the trouble to look at the kind of papers that have appeared in that journal in the past, as well as the style and detailed content of such papers. This is not an easy task, as I know from my own past experience writing papers for journals ranging from ecology, through engineering and the environment, to macroeconomics. On the contrary, it is very hard work, requiring much time and energy. But it is an essential task if you want your paper to be published.

In order to illustrate this latter point, I attach a paper of mine on global surface temperature modelling and forecasting that has appeared very recently on the International Journal of Forecasting (IJF). This was a paper I submitted to a climate journal and that was rejected by them on what I believe were entirely spurious grounds. In order to make it suitable for the IJF, I had to modify the paper quite a lot and it took me a long time. For instance, you will see that, because I am using a 'hybrid' continuous-time modelling and forecasting methodology that is not that well known to the forecasting community, I have gone to considerable pains not only to explain this methodology and even compare the results I obtain with those obtained using a more standard discrete-time forecasting approach used by most readers of JOF, but also to explain my models in the context of the traditional climate models (with which most IJF readers are unfamiliar). So you need to do the same kind of thing if you wish a paper, based on your work, to be acceptable to the JOF.

My considered conclusion, therefore, is that, if you wish to submit a new version of your paper to JOF, as a new submission, then I will be pleased to process it. However, it would need to be revised very substantially so that it is in a form that responds fully to the above criticisms. In particular, the paper needs to be shortened by removing some of the technical detail that will be incomprehensible to almost all of the forecasting audience and which, I believe, is not essential to the paper. It should have a style and content that is reasonably matched to the forecasting community and the readership of JOF, taking into account past publications in the journal. And, of course, it should respond fully to the technical comments of the referee because these would surely arise again if they are not responded to. This will, I am afraid, mean a lot of hard and time-consuming work on your part. Consequently, I need to stress that there can be no guarantee that the revised paper would be considered acceptable for publication because it would have to go through the same reviewing procedure as that used for all newly submitted papers, with at least two referees.

Finally, I should say that, on my reading of the paper, there was one thing, in addition to the points raised by the referee that worried me. Having analysed the globally averaged climate data myself, there is no doubt in my mind that these are data from a stochastic, dynamic system that appears to be characterised by short and long time constants, as well as long-term quasi-cycles. In this regard, it worries me that your 'emulation' model is a static relationship and, as the referee states, "the parameters are not estimated by fitting to a data series, they are derived from first principles": in other words, they depend on your hypotheses about the nature of the system and how it can be represented. I feel that, in any new submission, you would need to address these important issues.

Best wishes,

Peter

Prof. Emeritus Peter Young,  
Systems and Control Group.

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Lancaster Environment Centre,  
Lancaster University, UK.

My recent book Recursive Estimation and Time Series Analysis: see <http://www.springer.com/engineering/control/book/978-3-642-21980-1>

Our recent book True Digital control: see  
<http://eu.wiley.com/WileyCDA/WileyTitle/productCd-1118521218.html>

For information on the CAPTAIN Toolbox and latest paper downloads:  
[http://captaintoolbox.co.uk/Captain\\_Toolbox.html](http://captaintoolbox.co.uk/Captain_Toolbox.html)  
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Lancaster Environment Centre Web Page:  
<http://www.lancaster.ac.uk/lec/about-us/people/staff-list/all/peter-young/>

On 14 Apr 2018, at 03:59, Patrick Frank <[pfrank830@earthlink.net](mailto:pfrank830@earthlink.net)> wrote:

Dear Prof. Young,

From your extended silence, I gather you are disinclined to further consider FOR-17-0244.

Is that correct?

Please let me know.

Thanks for your consideration,

Pat

Patrick Frank, Ph.D.  
Palo Alto, CA 94301  
email: [pfrank830@earthlink.net](mailto:pfrank830@earthlink.net)

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These things are, we conjecture, like the truth;  
But as for certain truth, no one has known it.

Xenophanes, 570-500 BCE  
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<PCY\_IJF\_2017.pdf>

