Record of dialogue between Mike Jonas and Sage Publishing

2 Jan 2018 - I (Mike Jonas) began trying to submit the paper to Sage Publishing's *Journal of Ocean and Climate: Science, Technology and Impacts* (SAGE Publishing is a trading name of SAGE Publications Ltd, Registered in England No.1017514). The editor is Miguel Angel Morales Maqueda.

Due to technical problems with their website, it was not recorded as submitted until 5 Mar 2018.

22 Apr 2018 email from me to the editor:

> *I submitted my paper - OCS-18-0002 - on 5 Mar 2018 to the Journal of Ocean and Climate: Science, Technology and Impacts. 7 weeks later, it is still "Awaiting Reviewer Selection". I emailed Clare Legge on 14 Apr 2018 with my concern that 'reviewers have still not been assigned'. I received an 'out of office' reply, saying that my email would be attended to from 16 Apr 2018. It is now one week later and I have heard nothing.*
>
> *I can understand if you are reluctant to publish the paper, because it shows that the very powerful IPCC and the climate models are in error. However, I have made the paper as straightforward and clear-cut as possible, while still being completely thorough. The case presented by the paper is simple and solid. If journals are not prepared to publish such papers, then the future of science is grim. If you are not prepared to process the paper in good faith, then I have no choice but to withdraw it.*
>
> *If I have misinterpreted the situation, then I apologise.*

23 Apr 2018 email from the editor to me:

> *Many thanks for your message. I sincerely apologise for the excessive delay in sending your article for review. I have only recently taken over the position of Editor-in-Chief of the journal and I must confess I am overwhelmed by the amount of work that this requires. We are also still recruiting associated editors.*
>
> *In any case, I was hoping to handle your paper directly, as polar oceanography is one of my fields of expertise. Your paper is indeed somewhat unorthodox and controversial. Because of that, I wanted first to get in touch with you and discuss certain aspects of your work before sending it for review. My tardiness in doing so is entirely due to time management difficulties on my part. Please be reassured that the delay is not driven by political considerations.*
>
> *Your results are interesting and, in fact, not inconsistent with the known fact that the Southern Ocean sea ice cover has experienced a positive, although weak (about half a million square kilometres per decade at most), expansion since the beginning of the satellite era. The negative SST trends you deduce from the Reynolds data set would be consistent with this increase in sea ice extent. Using different observational datasets other researchers (e.g., Liu and Curry, 2010, attached) have also found negative trends in relatively large oceanic areas around Antarctica. The attribution of this trends is still a matter of scientific debate. I think that your paper could be a valuable contribution to the literature but I feel that, before sending the manuscript for review, your findings should be put into the context of existing research and your conclusions should re-examined after some further, and perhaps somewhat more nuanced, analysis of the implications of your results. I do not know whether this could be a valuable option for you but, would you be interested in having a*

Skype conversation with me at some point in the next few days to exchange views as to how to proceed with this paper? I would be grateful if you could reconsider you decision of withdrawing the paper until we have had a conversation.

24 or 25 Apr 2018 - The editor called me on Skype and asked whether would be prepared to "nuance" the paper's findings. I replied that the paper's findings were logically sound and that "nuancing" them would be the wrong thing to do because the findings were inescapable and should be expressed clearly. At no time during the conversation had the editor suggested that any part of the paper was unsound in any way - and remember, polar oceanography was their area of expertise.

Crucially, in answer to a direct question, the editor gave an assurance that he would not make any decision based on reviews without giving an opportunity to reply. I agreed to leave the paper with the journal, after making it clear that my right of reply to reviews was a crucial factor in this decision.

20 Jun 2018 - Reviews by "Reviewer #1" and "Reviewer #2" were sent to me. Both recommended rejection.

25 Jun 2018 - I sent in a very polite and comprehensive point-by-point reply, together with a revised paper. Every issue raised by the reviewers was dealt with.

24 Jul 2018 - The journal advised that Reviewers #1 and #2 now didn't have time to review the revised paper, and that they were looking for new reviewers.

16 Aug 2018 - The journal advised that 12 more reviewers had been approached to review the paper, and had all declined.

28 Sep 2018 email from the editor to me (note that there is a new reviewer, and I have not been given any opportunity to reply):

I write you in regards to manuscript # OCS-18-0002.R1 entitled "Southern Oceans Sea Surface Temperatures contradict a key element of the IPCC Report" which you submitted to Journal of Ocean and Climate: Science, Technology and Impacts.

In view of the criticisms of the reviewer(s) found at the bottom of this letter, I am afraid that your manuscript has been denied publication in Journal of Ocean and Climate: Science, Technology and Impacts.

As editor in chief, I regretfully need to make this final decision in agreement with the recommendation of the two reviewers and the associate editor. I do this regretfully because there are elements of your paper that are, in my view, of scientific interested and worthy of publication. The negative trends in summer SSTs in ice-free regions between ~65S and ~52S are very much worthwhile reporting. Other authors have published articles on related issues (see Liu and Curry, 2010, https://doi.org/10.1073/pnas.1003336107, and Maheshwari et al., 2013, http://dx.doi.org/10.5402/2013/392632) using different methodologies and data sets, but your results do not just duplicate theirs and are novel in certain ways. I am in particular intrigued by the fact that the trends you calculate exhibit areas of nearly uniform gradients (negative between 72S and 65S, positive between 65S and 48S and negative again equatorward of 48S) separated by narrow regions of abrupt gradient change. I wonder why this is and it might be telling us about the physics of the system. I would have also welcome an analysis and discussion of the causes and implications of these observations, which are somewhat counter-intuitive in the context of a predominant global warming are puzzling

and merit investigation. Having done and presented this valuable analytical work, you then move to concluding that climate models are wrong because they do not reproduce these trends. In a sense, you are right as current models have many serious problems because of their poor resolution and their crude parameterisations of key processes. Every single piece of work that uses observational data sets could be used to criticise model performance. The conclusion that models are imperfect is, as a result, hardly new or illuminating. Models, nevertheless, encapsulate the best of our current understanding, however incomplete, and so their output need to be taken seriously (as the IPCC does) even with a pinch of salt. In regard, specifically, to the problem of polar amplification, the existence of an albedo-temperature feedback is a fact of basic thermodynamics. It cannot seriously be contested. Models might produce excess/unrealistic climatic warming in the Southern Ocean because of an inadequate representation of the surface albedo (the albedos of sea ice and marine snow are very variable and difficult to model) or because of problems with many other aspects of the ocean and sea ice physics (e.g., lateral and vertical mixing, upwelling) or the atmospheric dynamics (winds, precipitation). These problems are all very well known. They are notoriously difficult to address and need hardly to be rehearsed in yet another paper unless new observational or theoretical insights are put forward that can help to solve, or at least remediate, them.

Thank you for considering Journal of Ocean and Climate: Science, Technology and Impacts for the publication of your research. I hope the outcome of this specific submission will not discourage you from the submission of future manuscripts and we are indeed open to considering revised versions of the present manuscript (as new submissions, not resubmissions) if you wish to continue this scientific discussion.

- 12 Oct 2018 - I wrote to Kiren Shoman, the senior person (according to the journal's website) responsible for editing.

  A SERIOUS COMPLAINT about one of your editors

  Earlier this year, I submitted a paper to your Journal of Ocean and Climate: Science, Technology and Impacts. The editor is Miguel Angel Morales Maqueda.

  The editor advised me that I would have the right to respond to reviews of the paper before the decision was made on whether to publish.

  The editor has now reneged on this undertaking, and has rejected the paper without giving me any opportunity to respond to one of the reviewers. I do not think that I have been treated fairly, and I would like to register a serious complaint. I am writing to you because the Sage Publishing website says that you are the Editorial Director UK of Sage Publishing, and the journal's editor is I believe based in the UK.

  The sequence of events for the paper in question is as follows:
  - *2 Jan 2018 : I attempted to submit the paper but had technical problems with the journal's website.*
  - *5 Mar 2018 : The editorial assistant eventually managed to bypass the technical problems and register the paper - OCS-18-0002 - for the Journal of Ocean and Climate: Science, Technology and Impacts*
  - *24 Apr 2018 : The editor phoned (actually Skype) to discuss the paper. During the conversation he assured me that I would have the right to respond to any review before any editorial decision was made on whether to publish. (Other emails from the editor also indicated that I would have a right to respond).*

- *19 Jun 2018 : I was advised that the paper had been reviewed, and I was invited to respond to the reviewers and to revise the paper.*
- *24 Jun 2018 : I provided detailed responses to the reviews, and submitted a revised paper.*
- *28 Sep 2018 : The editor advised that the paper had been rejected. I had not had any opportunity to respond to the reviews of the revised paper.*

The reviewers of the original paper were "Reviewer 1" and "Reviewer 2". The reviewers of the revised paper were "Reviewer 2" and "Reviewer 3". My understanding was that I would have the right to respond to all reviews, but I was not invited to respond to the last two reviews. In particular I was never given any opportunity to respond to "Reviewer 3". In my opinion, the review of the revised paper by "Reviewer 2" had no substance and the review by "Reviewer 3" had very serious errors, but I was not given the opportunity to explain any of this.

I wish to register my dissatisfaction with the way I was treated by the editor. It was particularly harmful, given the amount of time (nearly 9 months) that I ended up wasting on the process.

- 22 Oct 2018 email from Jessica Villarreal, Executive Assistant to Kiren Shoman:

I am writing to you on behalf of Kiren Shoman, VP, Editorial, at SAGE Publishing in London to acknowledge receipt of your letter dated 12 October 2018. Kiren has passed the letter on to the appropriate SAGE editor who will be in touch shortly to respond. Please do not hesitate to get in touch if there is anything I can do to be of assistance in the meantime.

- 6 Nov 2018 email from Clare Legge, Publishing Editor, STM Journals, SAGE Publishing:

Your letter dated 12th October regarding your submission to Journal of Ocean and Climate: Science, Technology & Impacts has been passed to me as the Publishing Editor for the journal.

Your original submission was reviewed by two reviewers. Both of these reviewers recommended that it be rejected, which you were advised of in the decision email dated 19th June. The Editor-in-Chief indicated that he agreed with the reviewers and the Associate Editor handling your paper, but based on some interesting elements of science in the paper offered you the opportunity to revise it. He stated at this point that the major revision decision did not constitute a promise of final acceptance.

Having recommended that your original manuscript be rejected, one reviewer declined to re-review your revised manuscript, and hence another reviewer was invited. As you will have seen in your decision letter, both reviewers once again recommended your paper be rejected. As the Reviewers and Associate Editor were in agreement that your paper should be rejected at both original submission and revision stage, and based on the feedback of the reviewers at revision stage, the Editor-in-Chief took the decision to reject your manuscript.

Your paper followed the standard peer review process of the journal and as per our Editorial policy, the final decision on all manuscripts rests with the Editor-in-Chief, who takes into consideration all reviewer comments before making an informed decision.

If you wish to respond to the reviewer comments please submit an updated version of your manuscript as a new submission. If the Editor-in-Chief feels that the re-submitted paper adequately addresses the points and concerns raised by the previous reviewers then the paper will be sent out for peer review in line with the journal's Editorial policy.

- 6 Nov 2018 - My reply:

Please can you forward the content of this email to Kiren Shoman.

For Kiren Shoman and the board of Sage Publications

Please would your board look further into this matter as a serious breach of process, or at the very least a very serious breach of trust which reflects very poorly on your journal.

On 12 Oct 2018 I wrote to you with a complaint about the editor of your Journal of Ocean and Climate: Science, Technology and Impacts. *The complaint was that I had been denied the opportunity to respond to a review of a paper that I had submitted.*

I have now received an email from an associate of the editor, claiming that due process was followed but providing no new information. In particular, it confirmed that I was not given an opportunity to reply to one of the reviews. I wish to object to this treatment in the strongest possible terms. The editor had assured me personally that I would have the right to reply to reviews before an editorial decision was made, and this was an important factor leading me to submit the paper to that journal. In other words, I believed that I would be treated fairly. As I explained before, the editor then reneged and did not allow me to respond to one of the reviews.
I also request that the board take into consideration that the editor's own comments to me are quite bizarre, because they were written in support of his decision not to publish, but logically they all support the paper. The details are as follows:
*My paper documented the testing of a statement in the latest IPCC report. The IPCC statement was itself based on output from climate models, so the test was also a test of the climate models. The test used satellite sea surface temperatures of the southern oceans. My paper detailed a specific and somewhat striking way in which the models had failed. The paper also explained why it was a decidedly non-trivial failure. In his email to me, rejecting the paper, the editor said: "*I am in particular intrigued by the fact that the trends you calculate exhibit areas of nearly uniform gradients (negative between 72S and 65S, positive between 65S and 48S and negative again equatorward of 48S) separated by narrow regions of abrupt gradient change. I wonder why this is and it might be telling us about the physics of the system.*". This must surely mean that I have found something new and interesting and therefore (presumably) worthy of publication and of further investigation.*
*The editor says of climate models: "*you are right as current models have many serious problems because of their poor resolution and their crude parameterisations of key processes*" and the problems are "*notoriously difficult to address*". That is absolutely in line with the findings in my paper, and "*you are right*" appears to be a clear endorsement by the editor.*

*The editor went on to say: "*Models, nevertheless, encapsulate the best of our current understanding, however incomplete, and so their output need to be taken seriously (as the IPCC does) even with a pinch of salt.*". In the world that I live in, science that requires a pinch of salt is bad science and needs to be exposed to scrutiny.*
*So the situation now is that I have presented a new, specific and striking way in which the models have failed, and which surely (if published) can be helpful to people trying to address the problems in the models. But the paper has been rejected after a flawed process because ..... because the paper is fine but we would rather treat the models with a pinch of salt than learn more about them????? As I said in the paper, "*The fact that the IPCC recognises that it has a problem does not mean that the problem can be ignored. It means that they really do have a problem.*".*
Please note that I do not address the cause of the observed temperature pattern, as that is beyond the scope of the paper. At this stage I simply show that the climate models have a significant failure. If I can find the cause of the temperature pattern I will attempt to get it published, but in the meantime I am happy for others to benefit from my findings to date and to search independently for the cause.

Apologies for the length - it is difficult to explain properly and to be brief.

- 10 Nov 2018 - email from Clare Legge. Note that (a) I still couldn't get the issue to the attention of Kiren Shoman, (b) the email ignores the fact that the editor had assured me that I would have right of reply to reviews - instead they make it sound like allowing me to reply to the first two reviews was them being especially nice, and (3) there weren't three more rejections.

As Jessica advised, Kiren is currently out of the office travelling and she has therefore forwarded your email to me to respond.
I am sorry that you feel you have been treated unfairly in the way that your manuscript was processed. Normally, if a reject recommendation is made by two reviewers and the Associate Editor after the first round of reviews then the paper would be rejected. In the case of your paper, the Editor-in-Chief granted a decision of major revision to provide you with the opportunity to respond to the reviewer comments.
Upon receiving a further three reject recommendations at revision stage, the Editor-in-Chief made the decision to reject your manuscript. This decision was made in accordance with our Editorial policy and after the due peer review process had been followed.
At SAGE we are proud to support the Editorial independence of our journal Editors and we are in support of the final decision of the Editor-in-Chief to reject your manuscript in this instance.
If you do have any further comments, I have copied in the Publisher for STM Journals, Richard Jansz-Moore.

- 10 Nov 2018 - My response.

Thank you for your reply, but it does not address the issue and I request that this matter be dealt with by Kiren Shoman and/or the board of directors and I am prepared to wait for a while.
The issue is that the editor reneged on his assurance that I would have the right to reply to reviews. After I had replied to the first two reviewers, one of whom then withdrew, he ambushed me with a third reviewer and did not give me any opportunity to reply.

The reviews themselves, and the editor's treatment of them, are a separate issue and I would welcome the opportunity to present my side of the story to Kiren Shoman and/or the board. I don't think that the events to date reflect at all well on the way that the business of the journal has been conducted.

- 13 Nov 2018 email from Richard Jansz-Moore, Publisher, SAGE Publishing.

As Clare outlined in her previous email I am the Publisher for STM journals at SAGE; I have responsibility for overseeing the portfolio of journals that Journal of Ocean and Climate: Science, Technology and Impacts is included in.
We have fully investigated the procedure undertaken by the Editor on your paper and are satisfied that he has followed due process as required, in accordance with peer review expectations.
In normal circumstances a submission that received "reject" decisions from all reviewers and the Associate Editor would expect to be rejected; the fact that the Editor gave you the opportunity to respond on the initial reviews fulfils his assurance that you would have right to reply. The Editor made his decision based on the cumulative evidence of all reviewers' comments received and his own final opinion. There is no obligation for an Editor to continue to send papers out for peer review; though you retain the right to submit a new version of the manuscript as a new submission.
I have discussed this matter with both the Associate Vice President, Open Research, and the Vice President, Global STM Journals and they are in full agreement with our findings.
SAGE as a company are committed to editorial independence and fully support the editor's decision.

- 13 Nov 2018 - My response. Note: "<u>My Side of the Story</u>" was included it so that the response to Reviewer #3 would be somewhere in Sage's records.

Thank you for your interest in this matter, but I find it very curious that you can rule that I had the right to reply to reviews when I clearly did not: my very first sighting of Reviewer #3's review was in the email advising me of the editor's rejection of the paper.
I also very clearly said in my last email that I would welcome an opportunity to present my side of the story, and you made your decision without even asking what that might be. In the resolution of any dispute, I would have thought that it was normal procedure to listen first to both sides.
I reject absolutely your assertion that due process was followed, because the process as described to me by the editor was not followed.
My understanding is that Kiren Shoman is your editorial director, and I request that this matter be resolved at that level.
My side of the story is as given below (also attached as a pdf).
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<u>My Side of the Story</u>
My case is that my paper has genuine merit, that the review process was not followed correctly, and that the failure of process resulted in an editorial decision being made without relevant information having been considered.
In conversation with the editor, I made it clear that having the right to respond to reviews was important to me. I knew that my paper would be controversial, even though its content was scientifically and mathematically solid. I was assured personally by the editor that I

would have the right of reply to reviews, and he later reneged on this assurance by giving me no opportunity to respond to one of the reviewers.
I confess that I did have some doubts as to whether the paper would be dealt with fairly, but I hoped that the editor, who is knowledgeable about the Antarctic, would be able to see the merit in the paper. As it turned out, I was right on both counts - the editor did see the merit in the paper, but it was not dealt with fairly.
I responded thoroughly to the initial reviews and revised the paper accordingly, and one reviewer then withdrew. The second reviewer persisted in recommending rejection but without providing any substance on which that opinion was based. I would have expected the editor to notice that (more detail below). The third reviewer was way off target, as detailed below. If I had been able to respond to that review, then I could have pointed that out to the editor, but I was not given that opportunity.
<u>The Review Process</u>
The review process as described to me by the editor was not followed. The editor assured me that I would have the right to respond to reviews, but later reneged on that assurance by not giving me the right to respond to one of the reviewers.
In his email of 12 Nov 2018, Richard Jansz-Moore said "the fact that the Editor gave you the opportunity to respond on the initial reviews fulfils [their] assurance that you would have right to reply*". There were three reviewers. I was only given the opportunity to reply to two of them. The editor's assurance that I would have right to reply was not fulfilled.*

<u>The Merits of the Paper</u>
The journal editor has stated clearly and in some detail that the paper has merit. For the benefit of others less familiar with climate models and with the Antarctic, I provide the following information:
Climate modellers must understand the laws of physics and the climate mechanisms, and how to implement them. The climate mechanisms are the movements of matter and energy within and between the oceans, the atmosphere, the land, the biosphere, the cryosphere, and space.
If it is accepted (as I do) that the climate modellers understand the laws of physics, then any major error in the models must be in their understanding of the climate mechanisms or in their implementation of those mechanisms. We know that our understanding of the mechanisms is incomplete, and we know that the implementation of the mechanisms is unsatisfactory (as acknowledged by the editor). It is therefore not exactly earth-shattering for me to state in the paper that the major model failure which I identified and quantified shows that "a failure of the temperature in the sea-ice latitudes to behave as projected would indicate that the climate models' underlying mechanisms or their implementation are invalid*", but it does need to be said because this is a truly serious model failure over a large area and a long period. It necessarily follows that* "There must therefore be one or more important large-scale climate processes that are not reasonably represented in the models*".*
The paper itself is very simple. Using sea surface temperatures of the southern oceans, it tests the hypothesis that surface warming is amplified by sea ice- and snow-related feedbacks near the poles. I found and explained a simple way of quantifying the rate of change of the surface temperature in the southern oceans by latitude. It uses nothing complicated because it doesn't need to be complicated. It is particularly relevant because it shows clearly that the greatest rate of cooling occurred very close to where the greatest amplified warming was expected. Now I know that scientists nowadays are very used to detailed inspection of minutiae using advanced statistical analysis, but in this case it was simply not needed. The entire picture is presented fully just by Figures 2 and 4 in the paper and their explanation.
<u>The Reviews</u>
Reviewer #1's comments were fully dealt with, and the reviewer then withdrew.
Reviewer #2 in their second review (no chance for me to respond) said "I do not see substantial evidence or additional statistical evidence ...*". Now to my mind this can be rejected on three fronts. <u>Firstly</u>, it's waffle. I think the usual term in the field of science is "hand-waving". In other words, it's a broad unsubstantiated statement. If there were holes in the evidence or the logic, then surely the reviewer should identify them, so that the editor*

could determine whether there was any substance to the reviewer's assessment. Secondly, it's nonsense. The evidence in Figures 2 and 4 as supported in the paper's content is truly substantial because it covers the whole of the southern oceans over a 36-year period. Thirdly, it's ridiculous. When there is such clear and simple evidence that the models got the whole of the southern oceans diametrically wrong over a long period, how could there possibly be a need for additional statistical evidence - there is nothing more to say, and to bring in anything more complicated would simply cloud the issue.
I was not given the opportunity to respond to Reviewer #3. They said my paper "looks more as a complaint to the IPCC organization. As you know the IPCC has a protocol for addressing errors in IPCC Reports*". But this isn't that kind of error. An error in an IPCC report is an error in interpretation or presentation of information obtained from peer-reviewed papers - that's what the IPCC does, they do no actual research. My paper is new information (a quantified pattern of southern oceans' surface temperature by latitude) and analysis, which should be published in a peer-reviewed journal so that it can be used by the IPCC in the preparation of their next report, and by climate modellers to help improve their models.*
Reviewer #3 also said "I did not find the quote about the hypothesis - Surface warming is amplified by sea ice- and snow-related feedbacks near the poles – Do we have the same document?*". The exact wording was my paraphrase, but I thought this was pretty clearly presented, with appropriate references, in Section 2 THE HYPOTHESIS:*

> The fifth IPCC Report (Collins, 2013) states: "**Feedbacks associated with changes in sea ice and snow amplify surface warming near the poles (Hall, 2004; Soden et al., 2008; Graversen and Wang, 2009; Kumar et al., 2010).**". Hall (2004) found that surface albedo feedback accounts for about half the high-latitude response to external forcing by CO2.

In their other comments, Reviewer #3 made similar errors to Reviewer #2. For example, they say "You just get focus saying that the climate models are wrong and the mechanism and implementation are invalid, but you don't propose how to improved, what kind of consideration they need, border conditions, nest, etc, something that helps to improve this analysis.*". This is an unreasonable statement and is way outside the scope of the paper. You have to understand something about what the models have got wrong before you can fix them. The editor's comments showed that I had found something new and informative (*"very much worthwhile reporting", "do not just duplicate [others] and are novel in certain ways", "might be telling us about the physics of the system*"). That is quite enough to justify presenting the ocean temperature pattern in the public domain for others to use, and to highlight the fact that the models have a major fault. ie, it is quite enough to justify publication of the paper.*
Reviewer #3 continues with detailed comments, many of which are as irrelevant or misguided as those that I have highlighted above. I would be happy to respond in more detail or to revise details in the paper if required, but at this stage I suggest that it is sufficient to consider the higher-level issues.

- 13 Nov 2018 email from Richard Jansz-Moore:

> *While Kiren Shoman is Editorial Director at SAGE her area of responsibility is for books. The people I mentioned in my previous email, Associate Vice President, Open Research, and the Vice President, Global STM Journals, are the same level as her, but have oversight for journals.*
>
> *The matter was discussed and resolved at the level that you have requested. After investigating we are satisfied with the process carried about by the Editor.*

It is curious that they left it to the final email to say that Kiren Shoman was the wrong person to complain to. Surely, if that was true, they would have said that right from the beginning.

There has been no further correspondence.