

# Submission to the Inter-Academy Council Independent Review of the Policies and Procedures of the Intergovernmental Panel on Climate Change

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1. *What role(s), if any, have you played in any of the IPCC assessment processes?*

I was an expert reviewer for Working Group I of the Fifth Assessment Report (AR5).

2. *What are your views on the strengths and weaknesses of the following steps in the IPCC assessment process? Do you have any recommendations for improvement?*

a. *Scoping and identification of policy questions*

I take it you mean IPCC operational policies, since the IPCC is supposed to be neutral with respect to government policy. IPCC policies are frequently referred to by people who want to appeal to the authority of the IPCC documents. In particular, the claim that the IPCC review process is “objective, open and transparent”<sup>1</sup> is sometimes cited as grounds for treating the assessments as authoritative to the point of near-infallibility. But my experience is that the written policies of the IPCC are not always followed, and there do not appear to be any consequences when they are breached.

For example, in Annex I of the IPCC procedures,<sup>2</sup> Review Editor responsibilities include the following: “Review Editors will need to ensure that where significant differences of opinion on scientific issues remain, such differences are described in an annex to the Report.” I was involved in areas where there were significant differences of opinion on scientific issues, particularly with respect to peer-reviewed evidence of contamination of surface climate data, improper estimation of trend uncertainties and methodological flaws in the hockey stick graph highlighted by the IPCC in the TAR. None of these differences were resolved during the review process, yet no such Annexes appeared, creating a false impression of consensus.

After the publication of the AR4 I found that important text had been altered or deleted after the close of the review process, and the Lead Authors of Chapter 3 had fabricated evidence on page 244 of the WGI report, by introducing a claim that statistical evidence of surface data contamination in two published, peer-reviewed articles was statistically insignificant, when the articles show no such thing. The paragraph was inserted after the close of peer review and was never subject to external scrutiny. That Lead Authors are able to insert evidence and rewrite the text after the close of review makes a mockery of the idea that the IPCC reports are peer-reviewed, and undermines the claim that they contain the consensus of experts. Consequently I believe that the policies governing IPCC operations are inadequate to ensure reliable conclusions. I will make some recommendations for change in subsequent sections.

b. *Election of bureau including working group chairs*

c. *Selection of lead authors*

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<sup>1</sup> <http://ipcc.ch/pdf/ipcc-principles/ipcc-principles-appendix-a.pdf> Sect. 4.2.4.

<sup>2</sup> <http://ipcc.ch/pdf/ipcc-principles/ipcc-principles-appendix-a.pdf> Sect. 5.

These steps appear to be under the control of a small circle of people committed to a predetermined view on global warming. The controlled selection of Lead Authors, in combination with the fact that the review process is toothless, guarantees that the report contents are predictable given the names of the Lead Authors. Indeed there is not much point even publishing the report any more: once the list of Lead Authors is known, we can all guess what the conclusions will be. I am sure that there are many areas in the IPCC report where the conclusions will be sound. But in the areas where I have detailed knowledge and experience, it has not been the case.

One example of this problem was the selection of Michael Mann as Lead Author of the TAR paleoclimate chapter. It is obvious that there were many experienced experts who would have been more obvious choices, whereas Mann was a new Ph.D. with one prominent study in the area, namely the hockey stick graph.<sup>3</sup> His 2 papers on the hockey stick proposed a major revision to a view that many experts, including the IPCC itself, had held in the 1990s, concerning the relative magnitude of the Medieval Warm Period. But the study and its methods were brand new and had not been subject to any real scrutiny, nor would they be until Steve McIntyre began trying to replicate them in 2003. It is difficult to explain Mann's promotion to Lead Authorship based on his experience or professional status at the time of the TAR, but it does make sense if we take the view that the IPCC looks for individuals it believes will be able to articulate a narrative they want to see promoted.

Furthermore, now that this process has been in place for about 15 years, a feedback loop has emerged in which scientists who serve as Lead Authors for the IPCC gain prominence in their fields, thereafter exerting greater influence on the journal publication process, thereby smoothing the path to publication of studies that reinforce the IPCC view, and blocking the publication of studies that do not. These kinds of issues came out with disturbing clarity in the CRU emails, and have been discussed anecdotally for many years. The IPCC is no longer a neutral observer of research, it now affects and distorts the flow of research itself.

#### *d. Writing of working group reports*

A major problem with the IPCC is that the assignments for Lead Authors (LA's) often put them in the position of reviewing not only their own work, but also that of their critics. There is too much conflict of interest built into the report-writing process, and what few safeguards are in place are ineffective.

An important example concerns the question of surface climate data quality. Chapter 3 of the AR4 covers the measurement of observed climate change. A key data set for this purpose is produced by the Climatic Research Unit (CRU) at the University of East Anglia. The CRU publishes a gridded climate data set called TS 2.x that they specifically warn is unsuitable for IPCC work because it is contaminated by the effects of urbanization and land-use change. They also publish the CRUTEM data under the leadership of Phil Jones, which they claim is filtered in

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<sup>3</sup> See, for example, the comments by Judith Curry at <http://www.examiner.com/examiner/x-9111-Environmental-Policy-Examiner%7Ey2010m5d4-Global-warming-Interview-with-Dr-Judith-Curry-Part-1-Cuccinellis-Witch-Hunt>.

such a way as to remove the contaminating influences, revealing an accurate climate signal. I and others have published studies testing this claim and finding evidence against it. If the claim is wrong, this would have serious repercussions not only for estimating the magnitude of post-1980 climate warming but also for attributing the observed changes to greenhouse gases. Unfortunately the IPCC appointed Jones to write the chapter that assessed the question of CRU data quality, creating a conflict of interest at the heart of one of the most important discussions.

It was clear to me as a reviewer that Jones (and the other LA's) were disinclined to give this literature a proper review in the IPCC report, since they kept any mention of the studies that refuted their claims out of the drafts that were seen by reviewers. I objected to this, as did at least one other reviewer, but the Second draft was likewise silent on the issue. Then, after the review period closed, they inserted some unsubstantiated statements claiming that my and others' findings against the CRU data were statistically insignificant. As a result of the CRU email leak we now know this was a premeditated strategy. On July 8 2004, a year before the first IPCC draft was produced, Jones wrote to Michael Mann regarding my first paper on the temperature data problem (which he denotes as 'MM'),<sup>4</sup>

The other paper by MM is just garbage - as you knew. De Freitas again. Pielke is also losing all credibility as well by replying to the mad Finn as well - frequently as I see it. I can't see either of these papers being in the next IPCC report. Kevin and I will keep them out somehow - even if we have to redefine what the peer-review literature is!  
Cheers  
Phil

By contrast, in the two paragraphs immediately prior to the above quotation, Jones discusses some results by a colleague named Adrian Simmonds that appeared to rebut earlier work of Eugenia Kalnay and Ming Cai, who had shown that much of the measured warming over the US portion of the CRU data was attributable to land-use change, thus adding to the evidence against Jones' position. Jones pushed for publication of the Simmonds paper with the intent to use it in the IPCC chapter he was writing.

It may be the case, in principle, that Jones' judgment on both papers was correct (though I don't think it was). But the problem is that because Jones was acting in a conflict of interest, his decision as IPCC LA to exclude this evidence, and later to misrepresent it, cannot be taken at face value as simply an expert judgment.

Another example concerns the treatment of the conflicting evidence about tree ring climate reconstructions at the time of the TAR. As noted above, the IPCC made an odd selection by choosing Michael Mann to write the chapter, since he was new to the field and had only just recently obtained his Ph.D. At that point there were three studies presenting hemispheric mean temperature histories back to the Medieval era. One was by Mann, and the others were by, respectively, Briffa (which would appear in print in 2000) and Phil Jones et al. Not all the studies, and indeed not all the studies' authors, supported the view that the 1990s could be

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<sup>4</sup> <http://www.eastangliaemails.com/emails.php?eid=419&filename=1089318616.txt>

ranked as the warmest decade of the millennium. In principle that should not be viewed as a problem. The task of the IPCC is to summarize the science, and if the science is uncertain then that is what the summary should say. It would only be considered a problem if an author wanted to make it appear that all the evidence confirmed his own results.

The study that departed the most from his own results was the Briffa series. Briffa was unpersuaded by Mann's reconstruction, as evidenced an email of his on September 22 1999.<sup>5</sup>

I do believe , that it should not be taken as read that Mike's series (or Jone's et al. for that matter) is THE CORRECT ONE. I prefer a Figure that shows a multitude of reconstructions...For the record, I do believe that the proxy data do show unusually warm conditions in recent decades. I am not sure that this unusual warming is so clear in the summer responsive data. I believe that the recent warmth was probably matched about 1000 years ago. I do not believe that global mean annual temperatures have simply cooled progressively over thousands of years as Mike appears to and I contend that that there is strong evidence for major changes in climate over the Holocene (not Milankovich) that require explanation and that could represent part of the current or future background variability of our climate.

Briffa's study departed from Mann's in showing relatively large natural variability over the previous centuries, and in showing a conspicuous decline in temperature in the last few decades. This apparently posed a problem for Mann, as he explained in a remarkable email the same day.<sup>6</sup>

I would be happy to add Keith's series. That having been said, it does raise a conundrum: We demonstrate (through comparing an extratropical averaging of our northern hemisphere patterns with Phil's more extratropical series) that the major discrepancies between Phil's and our series can be explained in terms of spatial sampling/latitudinal emphasis (seasonality seems to be secondary here, but probably explains much of the residual differences). But that explanation certainly can't rectify why Keith's series, which has similar seasonality\*and\* latitudinal emphasis to Phil's series, differs in large part in exactly the opposite direction that Phil's does from ours. This is the problem we all picked up on (everyone in the room at IPCC was in agreement that this was a problem and a potential distraction/detraction from the reasonably consensus viewpoint we'd like to show w/ the Jones et al and Mann et al series.

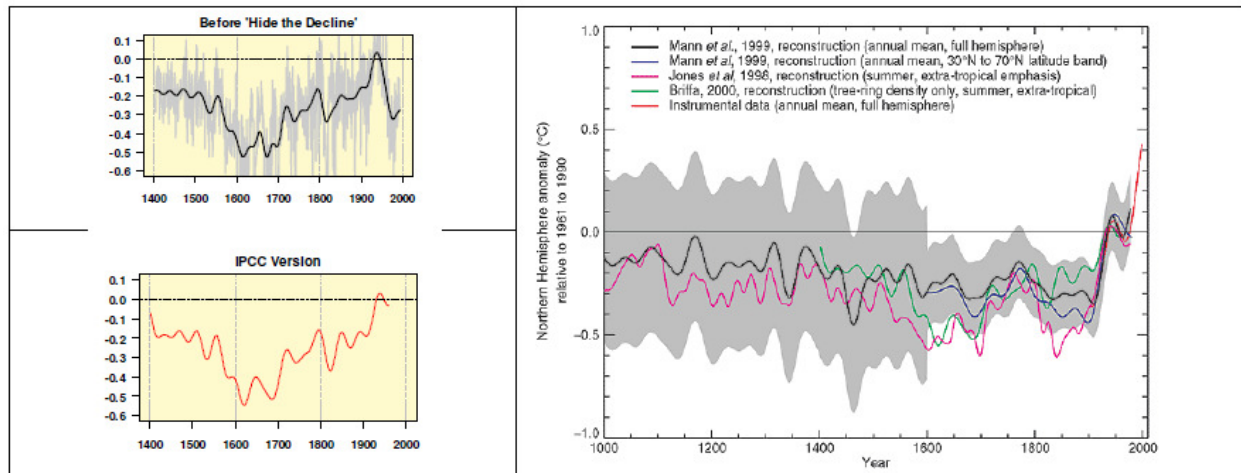
Here again the problem is not that experts are assessing evidence and making judgments. The problem is that a Lead Author who is in a conflict of interest has sole discretion to impose a judgment. Mann (and Jones) dealt with Briffa's counterevidence by simply deleting the divergent data. Figure 1, taken from a recent presentation by Stephen McIntyre<sup>7</sup> at Trinity College,

<sup>5</sup> <http://www.eastangliaemails.com/emails.php?eid=138&filename=938031546.txt>.

<sup>6</sup> <http://www.eastangliaemails.com/emails.php?eid=136&filename=938018124.txt>

<sup>7</sup> Stephen McIntyre, "Climategate: A Battlefield Perspective" presented at Trinity College, University of Toronto, March 2010. See <http://climateaudit.org/2010/05/06/trinity-college-presentation-march-2010/>

University of Toronto, shows the contrast between the data Briffa supplied by email to Mann, the version after Mann processed it, and the diagram that appeared in the IPCC Report (IPCC 2001). As is quite clear, the declining post-1960 data was removed. There was no discussion of this in the 2001 IPCC Report. In the 2007 Report the same trick was applied. This time at least one expert reviewer noticed it and objected, but the objections were dismissed.



Top left – Briffa reconstruction re-plotted from data in Climategate email; bottom left – digitized Briffa reconstruction in IPCC 2001 (where it is the green reconstruction.). Right – IPCC Figure 2-21.

I would add one further observation: in all the examples of which I am aware the IPCC authors only seem to be troubled by evidence that supports the positions of people they dismiss as “skeptics.” They do not struggle with evidence that supports their global warming narrative: they simply use it. I am unaware of any examples in which IPCC authors discuss tweaking the evidence to boost a skeptical position. Tweaking only ever seems to be used to undermine a skeptical conclusion.

#### *e. Review processes*

It is not actually a “peer review” process, such as academic journals use, instead it is more like a limited public comment process. No one is assigned the role of reviewing a particular section or chapter. It is conceivable that parts of a report might not be read by any reviewers: there is nothing in the IPCC procedures that prevents this. I will make recommendations later on to remedy this.

The number of comments submitted is a good indication of how carefully the report was read. Although there are over 140 governments in the IPCC, only 22 national governments submitted any review comments on the WGI Second Order Draft (governments did not review the First Order Draft). The European Commission also contributed comments on two chapters, bringing

the total to 23 government entities.<sup>8</sup> Of the 2,010 comments submitted, over half were from only two countries: the United States and Australia. Not one African country submitted a comment, nor did any Middle Eastern or Arabic countries, nor did Russia nor the former Soviet states. Brazil submitted comments on three chapters and Chile commented on one chapter; other than that there were no comments from any South American countries. None of the small island states in the Pacific submitted comments. In eastern Europe, the Czech Republic commented on one chapter, and Hungary commented on three chapters; other than that there were no comments from any government in Eastern Europe.

On the whole, the evidence shows that, except for Australia and the US, government review was cursory or non-existent. Yet the fact that all the member states “accepted” the conclusions is sometimes invoked as evidence of the report’s authority. It is hard to see why the Government Review process even exists, and I will recommend further on that it be removed as part of a larger reform.

As to the expert review process itself, as shown by McLean (fn 8), one of the odd features of the IPCC drafts was the extent to which IPCC authors commented on their own chapters. For example, for WGI Chapter 9, there were 56 contributing authors and 62 reviewers. But of the reviewers, 7 were also authors, three were editors of the IPCC Report, one was an employee of the IPCC Technical Support Unit, and 26 were authors or coauthors of papers discussed in the chapter. Ten of the reviewers advocated on behalf of their own papers in their review comments. Only 31 reviewers could be identified as truly independent. So there were fewer reviewers than authors. This was the case for 8 of the 11 chapters.

Moreover, of the 62 reviewers, more than half contributed only one or two comments, suggesting they did not even read the whole chapter. This does not necessarily show that a lot of scientists disagree with Chapter 9 or with the IPCC Report as a whole. But it does show that the core of the report received relatively scant review, and there is no indication that large numbers of experts, or governments, studied the material prior to its publication.

My own experience is that the Review Process is ineffective at constraining a Lead Author who is determined to put his or her view forward. An example concerns the statistical issue of Long Term Persistence (LTP). There is a large literature showing that LTP affects climatic data and if it is not properly handled, trend significance is likely to be overstated. After the first IPCC draft I put in expert comments objecting to the simplistic method being used to estimate error bars around the temperature trends. I was apparently not alone, as revealed by a subsequent email from LA David Parker to Phil Jones:<sup>9</sup>

Maybe the biggest problem is Ross McKittrick and David Stephenson’s remarks on trends; we used only an AR-1 and they may be correct in advocating a more complex model. Our software for restricted maximum likelihood does not cope with ARMA(1,1) and may have to get John Kennedy to investigate new software using

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<sup>8</sup> See McLean, J. (2009) “Peer Review? What Peer Review? Failures of Scrutiny in the UN’s Fourth Assessment Report.” Science and Public Policy Institute, Washington DC.

<sup>9</sup> Obtained as part of a FOIA disclosure to David Holland, UK.

the cited references. This may be a big job but could be done after the LA3 meeting if we agree there what to do. Alternatively - as we have considered already - we could consider not citing linear trends, just overall changes of level from the smooth curves. This would save some space.

The first striking thing about this email is that the IPCC LA's in charge of estimating temperature trends admit they do not have any software that can handle ARMA(1,1), which is one of the simplest time series specifications and is handled by any standard modern statistical package. This puts an obvious question mark around the idea that the IPCC represents the work of the world's leading experts on all the topics it writes about. The reality is that no one can be an expert on such a large range of topics, and there needs to be better supervision of cases where LA's need outside assistance. I will make a recommendation on this later.

As for the review process, the First Draft of the IPCC report Chapter 3 contained no discussion of the LTP topic yet made some strong claims about trend significance based on unpublished calculations done at the CRU. I was one of the reviewers who requested insertion of some cautionary text dealing with the statistical issue. Chapter 3 was revised by adding the following paragraph on page 3-9 of the Second Order Draft:

Determining the statistical significance of a trend line in geophysical data is difficult, and many oversimplified techniques will tend to overstate the significance. Zheng and Basher (1999), Cohn and Lins (2005) and others have used time series methods to show that failure to properly treat the pervasive forms of long-term persistence and autocorrelation in trend residuals can make erroneous detection of trends a typical outcome in climatic data analysis.

Similar text was also included in the Chapter 3 Appendix, but was supplemented with a disputatious and incorrect claim that persistence models lacked physical realism. I criticized the addition of that gloss, but other than that there were no second round review comments opposing the insertion of the new text.

Then, after the close of Expert Review, the above paragraph was deleted and does not appear in the published IPCC Report, yet the disputatious text in the Appendix was retained. There was no legitimate basis for deleting cautionary evidence regarding the significance of warming trends. The science in question was in good quality peer-reviewed journals, the chapter authors had agreed to its inclusion during the review process and there were no reviewer objections to its inclusion. But, evidently, at some point, one or more of the LA's decided they did not want to include it anymore, and the IPCC rules do not prevent arbitrary deletion of material even after it has been inserted as a result of the peer review process. Also, the authors proceeded with an incorrect method of evaluating trend significance, rather than obtaining expert advice.<sup>10</sup>

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<sup>10</sup> They used an AR-1 model, then checked the residuals using a Durbin-Watson statistic, despite the fact that the dw statistic is not valid in an autoregressive model.

In 2008, a UK citizen named David Holland sought information about how one of the Chapter 6 Review Editors (John Mitchell of the UK Met Office) had handled the controversies over the Mann et al. hockey stick. As part of his inquiries, Holland submitted a Freedom of Information Act request to the Met Office. The documents released in reply contained an email from IPCC Chair Susan Solomon advising Mitchell on the limitations to his responsibilities to provide explanations for editorial decisions. The email was dated March 14, 2008, and stated, in part, the following.

The review editors do not determine the content of the chapters. The authors are responsible for the content of their chapters and responding to comments, not REs. Further explanations, elaboration, or re-interpretations of the comments or the author responses, would not be appropriate.<sup>11</sup>

In practice, RE's have neither the responsibility nor the authority to stop Lead Authors who are determined to make arbitrary decisions about chapter content. For this reason the IPCC review process is fundamentally unlike the academic peer review process, in which the editor has the right to accept or reject a paper and its contents based on review comments. To the extent the IPCC gives Lead Authors the sole right to determine content and accept or dismiss comments, it is more like a weblog than an academic report.

*f. Preparation of the Synthesis report, including the Summary for Policy Makers*

I chose not to comment on the draft Summary for Policy Makers since I knew it would be subject to complete re-write many months after the close of scientific review in an IPCC plenary session. So it is pointless to put it out for review. As for the Synthesis report, it is produced long after the close of the review of the Assessment Report, and so once again, expert review is pretty pointless.

*g. Adoption of report by the IPCC plenary*

As I explained above, most governments did not provide meaningful or rigorous input during the government review phase, so their "acceptance" and "adoption" of the report is not evidence of its soundness. And in practice, elected governments are not bound in their actions by anything in the report. The only purpose that seems to be served by having the report "accepted" is that environment bureaucracies are then relieved of the responsibility of responding to questions from their own citizens or conducting their own assessments of the science, since they can repeatedly defer to the IPCC report as the last word on the subject. I think this ill-serves the public, and stifles legitimate discussion.

*h. Preparation of any special reports*

3. *What is your opinion on the way in which the full range of scientific views is handled?*

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<sup>11</sup> David Holland, pers. comm.

As should be clear, my experience is that there are formidable mechanisms in the IPCC process that prevent certain points of view from being expressed in the report. The behind-closed-doors selection of Lead Authors, the use of a toothless review process, and the full, backroom re-write after the close of expert review, ensures that the IPCC doctrine will dominate the final report. I think this was not a problem for the first IPCC report, and was less of an issue in the 2<sup>nd</sup> AR, but as of the TAR and the AR4, the problem had become acute.

It not only means that people who try to inject alternative perspectives into the IPCC process encounter great frustration and end up feeling their efforts were wasted, but it also has led to an attrition over the years in which people whose expertise should be brought to bear on the climate problem simply give up participating in the IPCC.

*4. Given the intergovernmental nature of IPCC, what are your views on the role of governments in the entire process?*

Governments have failed to provide oversight and accountability. The question to ask yourselves is this. If I found evidence of data fabrication and fraud in a set of corporate financial statements, I know what phone number to call: 1-877-785-1555, which will put me in touch with the Ontario Securities Commission, to whom I can make a complaint. But suppose I find evidence of data fabrication or fraud in the IPCC report. What is the phone number of the agency that I can contact, that has the authority to investigate and prosecute such conduct? As far as I am aware, there is no such phone number, and no such agency. My only recourse seems to be to contact the IPCC Bureau itself, which is not a satisfactory situation.

*5. Given that IPCC assessments consider a vast amount of literature, what are your views and suggestions for improvement on the sources of data and the comprehensiveness of the literature used, including non-peer-reviewed literature?*

Several people have proposed a wiki-style process, but I think this is unlikely to work satisfactorily. However, the review process needs to be fixed. Ultimately, if the IPCC is going to have a review process at all, it has to delegate some actual authority to reviewers rather than treating them as chumps and discarding their input at will. But if reviewers are to have real authority, then they have to be chosen in a more formal way, and they have to be assigned sections to review, rather than leaving it up to chance whether some sections get reviewed. I propose the following structural reforms.

Lead Authors, Coordinating Lead Authors and Review Editors would be selected by the IPCC as is currently done.

In addition, a 21-member Editorial Advisory Board would be created independently of the IPCC. No more than 7 members of this Board would be from within the fields of climate, meteorology or earth sciences. The remainder would

be drawn from the fields of mathematics, statistics, physics, engineering, chemistry, economics, biology, medicine, computing, and other areas. Individuals could apply or be nominated, and those that agree to stand would submit a CV to a web site for public review, and then representatives of each field would be selected by a vote restricted to, for example, Academy members, or members of each discipline's major academic societies. Membership on the EAB would be for a fixed 7-year term, staggered so that 3 members per year would be replaced.

The Report-writing process would then be done as follows.

- a) CLA's would divide up the chapter into the major sections and assign two or more LA's to solicit contributions and produce a preliminary text.
- b) Review Editors would recruit at least 3-5 referees for each section. The names of the referees would be published. At least one referee would be someone completely outside the field of climate and climate-related sciences. Review Editors would be responsible for ensuring that the referees encompass a full range of views on the topic.
- c) If the list of referees is deemed to have arbitrarily excluded an important perspective, observers can submit a request to the EAB asking for a review of the section referee list, making a case why another person or persons should be added.
- d) Once the list is complete, the CLA's would submit the section text to the referee group. Upon receiving responses, the LA's would have the responsibility of preparing a revision and a reply to the referee comments. This process could iterate many times during the report preparation process. There is no reason to limit it to the current FOD/SOD approach which effectively only allows for only one iteration of comments. However, LA's and referees would be required to work to a reasonable time frame, such as 180 days. Also, there would be no reason to maintain the current onerous page limits. Far too much time is wasted trying to achieve arbitrary word count limits. Since the final report would be published on the internet, word limits are not important, as long as the discussions are as concise and clear as possible.
- e) If LA's are confronted with technical issues which they deem themselves unable to address, they would not be permitted to go offline and ask colleagues for assistance. Instead they would contact the EAB and describe the issue they need assistance on. The EAB would then provide them with a list of individuals to contact for advice. This is to prevent LA's from recruiting partisans to their own cause for coaching on how to rebut reviewer comments they don't want to concede.
- f) The iterative process would continue until all LA's and referees agree to the text. In cases of serious dispute the expectation is that there would be a

box or footnote inserted recording a reviewer's dissent and citation of evidence for the dissent.

- g) If a deep disagreement arose whereby an author or referee was not satisfied with the text or proposed resolution, the matter would be referred to the EAB for guidance. The EAB would first make a non-binding recommendation and ask the LA's and referees to come to a voluntary agreement. If they are unable to do so, the EAB would have the option to impose a binding resolution in the form of final text.
- h) Because referees would have some rights over the final content, the process of selection would have to be more formal than is currently the case, which is the reason for steps b) and c) above.
- i) The agreed-upon text would then go to the CLAs, who would read through the entire chapter and ensure the sections are coherent. The CLA's would make a report to section LA's advising them of any conflicts in conclusions or material and the LA's would work together to resolve them with minimal disruption to the text.
- j) Any changes would then go back to the section referees for final approval. Disputes would be resolved as above.
- k) When all chapters are complete the CLA's would read through them to ensure there are no contradictions or inconsistencies. If changes are required they would be iterated as in i) and j).
- l) Upon receipt of the final text by the CLA's, the report text would then be frozen, and no further changes could be made. The complete report would be immediately published on the Internet without a summary.
- m) Preparation of a Summary for Policy Makers would be done afterwards by a Summary-writing group that would include selected government delegates as well as LA's and referees. An iterative process would operate as above. The major changes would be: there is no global plenary session where hundreds of delegates pretend to have read the report and then vote on whether they agree with each line; and the Summary would have to concur with the Assessment since there will be no revisions allowed to the Assessment Report to bring it into line with the Summary for Policy Makers.

*6. What are your views and suggestions regarding the characterization and handling of uncertainty in each of the working group reports and the synthesis report?*

The discussion of uncertainty, and the assignment of ratings like 66-90% certainty etc., lends more precision to the discussion than I believe is warranted given the contents of the reports and the ways in which uncertainty over one issue (e.g. cloud feedbacks) propagates into other areas.

There is a diagram in the Working Group I Summary for Policy Makers that sums up the estimated contributions of different planetary variables to the so-called “radiative forcing” of climate. Accompanying each one is an assessment of the “Level of Scientific Understanding” or LOSU. All the listed entries are “Low” or higher. In the review comments on the Second Order Draft,<sup>12</sup> comment number 2-1273 read:

It is notable (surprising?) that the level of scientific understanding for pre-satellite-era solar forcing which is based on proxies and models has jumped from “Very Low” in the TAR, to “Medium” in the AR4 figure. This should either be explained and highlighted here, or corrected including in this Figure which appears 3 times. In addition, this contradicts Chapter 2, page 6, lines 27-28!

The author response was:

Changed to “low”. Accepted

The difference between ‘Very Low’ and ‘Medium’ for a category as important as solar influence on climate implies quite a substantial difference in scientific understanding, yet in this case was decided by what amounts to haggling between a reviewer and an author. In other words, Lead Authors don’t even claim enough scientific understanding to decide what the level of scientific understanding is. Had the reviewer not drawn attention to this item, the LOSU would have been listed as Medium; because of one objection it was scaled down to ‘Low’, suggesting that the authors had no basis for scaling it up so far in the first place.

It is also interesting to look at the way the LOSU ratings were inflated between the second and final drafts of the AR4. In the first draft, 6 out of 15 climate forcing categories were rated as Very Low scientific understanding. In response to reviewer comments, the second draft scaled down its certainty ratings so that 7½ out of 15 were Very Low (contrails includes two sub-categories, one Low and one Very Low). In other words, half the categories of major climatic forcings were subject to the lowest possible rating for scientific certainty. I did not find any review comments on the second draft saying this overstated the uncertainty, yet in the final, published report only 4 of 15 Very Low ratings are shown (with two categories deleted). And in the Summary for Policy Makers Figure SPM-2, none of the forcings in the Very Low categories appear, creating the impression of greater certainty than was indicated in Table 2.11 at the close of scientific review.

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<sup>12</sup> <http://ipcc-wg1.ucar.edu/wg1/Comments/wg1-commentFrameset.html>

Forcing Category	Level of Scientific Understanding (LOSU)			
	1st Draft	2nd Draft	Final Draft	SPM
Greenhouse gases	H	H	H	H
Stratospheric & Tropospheric ozone	M	M	M	M
Stratospheric water vapour from methane	L	L	L	L
Stratospheric water vapour from other	V. L	V. L	V. L	-
Tropospheric water vapour from irrigation	V. L	V. L	V. L	-
Aerosol scattering and absorbing	L-M	L	L-M	M-L
Cloud albedo effect	L	V. L	L	L
Cloud lifetime effect	V. L	V. L	-	-
Cloud semi-direct effect	V. L	V. L	-	-
Contrails and aviation cirrus	M	L-V.L	L	L
Solar	M	L	L	L
Cosmic Rays	V. L	V. L	V. L	-
Surface Albedo	L	L	M-L	M-L
Non-Albedo Surface	V. L	V. L	V. L	-
Volcanic	M	L	L	L
<b>Proportion Listed as Very Low</b>	<b>6 / 15</b>	<b>7½ / 15</b>	<b>4 / 15</b>	<b>0 / 8</b>

Table 1: Evolution of LOSU ratings in IPCC AR4 Table 2.11, and in the SPM Figure 2. ‘-’ denotes not shown.

7. *What is your view of how IPCC handles data quality assurance and quality control and identification and rectification of errors, including those discovered after publication?*

There are multi-layered problems here, beginning with the fact that journals do not typically impose or enforce data archiving requirements on authors. I would like to see the following process.

- Any author who submits an article for consideration by the IPCC, or whose article is to be cited by the IPCC, is asked to sign a form certifying that data and code sufficient to support independent replication are available, supplying an FTP or HTTP link to the appropriate site.
- If an author cannot make such a supporting claim, the paper could still be used, but would be marked with an asterisk \* and it would be noted throughout the report that studies so noted cannot be independently replicated.
- Studies denoted with an \* cannot be the basis of statements in the Summary.

8. *What is your view of how IPCC communicates with the media and general public, and suggestions for improving it?*

The IPCC does not have a communication problem. It has a substance problem. I think that if the report-writing process I described above were followed, the reports would have more reliable substance and the IPCC would not find itself under assault from critics the way it is now.

9. *Comment on the sustainability of the IPCC assessment model. Do you have any suggestions for an alternative process?*

I have outlined an alternative process above. Those suggestions are focused on WGI. As for WGII and WGIII I see little need for them since the sponsoring governments appear to make very little use of their reports. I would suggest WGIII simply be abolished and WGII be reformed along the lines I suggested for WGI.

I think you should also recognize that the IPCC began before the Internet did, and its structure is now obsolete. It adopted a rigid bureaucratic structure that had some relevance in the days before the internet imposed deep transparency on public organizations. But times have changed, and public expectations have evolved. Henceforth, from the start of the chapter review process, the attention of international bloggers will be intense, and every aspect of the report-writing process will now be done in a fishbowl. Without major reforms to the process, the next Assessment Report will simply explode on impact. All it will take is for one error to be found, or one email to be leaked, or one graph to be manipulated, and the entire report will be discredited.

This is not because there are armies of nasty bloggers out there who are being unreasonable (although even if there are armies of nasty bloggers out there, they are not going away so you need to find a process that can still function in their presence). It is because the IPCC has become one-sided and brittle, and has no real ability to cope with legitimate differences of opinion. That makes it inevitable that there will be growing numbers of critics who see it as biased and insular. The choice is whether simply to press onwards with the hope the IPCC will somehow regain its former glory, or to consider whether the critics actually have a point, in which case the process is in need of correction.

*10. Do you have any suggestions for improvements in the IPCC management, secretariat, and/or funding structure to support an assessment of this scale?*

Only insofar as such improvements would be connected with implementing the revised process I outlined above.

*11. Any other comments*

Good luck in your deliberations. If you need any further comments or input please contact me directly at [rmckitri@uoguelph.ca](mailto:rmckitri@uoguelph.ca).