REVISITING POST-NORMAL SCIENCE IN POST-NORMAL TIMES & IDENTIFYING CRANKS

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INTRODUCTION

Although the concept of Post-Normal Science (PNS) was a major source of inspiration for The Post-Normal Times (PNT), I was as surprised and baffled as anyone else at turns taken by Jerry Ravetz ever since he posted an essay back in February 2010 at the climate "skeptic" blog WattsUpWithThat (WUWT). The essay, which has long since been published as a journal article, appears to accept what has become the "Climategate" myth at face value. It was followed by a workshop on *Reconciliation in the Climate Change Debate*, held January 2011 in Lisbon, for which Jerry was the lead organizer. The agenda of that workshop was to discuss points of agreement and disagreement on some scientific issues, such as the Hockey Stick, regarding which there is little if any actual disagreement within the scientific community and which are well supported by peer reviewed literature, but that are often contested from outside of the normal scientific process, following a very different set of rules.

As if all of the above were not confusing enough, there have also been some non-sensical interpretations of PNS, made not only by the Heartland Institute at its 2011 Sixth International Conference on Climate Change (ICCC6), which have little to do with the concept as it was defined by Funtowicz and Ravetz in 1991. These essentially blame PNS for the "abandonment of the scientific method" which presumably led to "Climategate."

Another incident was the Civil Investigative Demand filed by Virginia Attorney General, Ken Cuccinelli, who went so far as to allege that Michael Mann committed fraud because he did not disclose the post-normal nature of climate science in a grant application. That case was ultimately dismissed by the court on a technicality, with prejudice, without ruling on whether this claim might be considered a valid cause of action.

For anyone just tuning in, PNS has come a long way since the term was coined in 1991, and is now recognized even in the journal Nature, where a recent editorial about a workshop in Hamburg states: "Science becomes 'post-normal' when facts are uncertain, stakes high, values in dispute and decisions urgent; in such cases, societal needs must be taken into account to avoid costly mistakes." As I started graduate school in 1993, after having worked at both the National Academy of Sciences and the former Congressional Office of Technology Assessment, I found that the concept explained a lot with respect to what has been a dysfunctional interface between science and policy, and still does. However, without some common understanding of the term, it could become a meaningless one.

When I started the PNT in 2005, Jerry became an advisor and occasional contributor - and although I have discussed this subject with him, he has not contributed to PNT since he changed course. As for the future, we will just have to see where this discussion goes.

For reasons of practicality, I decided to focus this blog on post-normal "times", I.e., the context or situations, rather than on what can easily become obtuse discussions of science philosophy that don't lend themselves very well to the blog format. However, In this very long and long overdue post, I am going to revisit the basic definition of PNS, at least as I understand it, and the role of "extended peer review" as a basis for public participation in science-based decision-making as. In the process, I will address a few questions that were raised in the course of these events, that I refrained from commenting on because I did not have pithy answers:

- Is PNS "tailor made for the denialist crowd because it speaks of science in negative terms"? (as was suggested by the Policy Lass) or
- Has PNS simply been hijacked? (as was suggested by Deep Climate).

Adopting Ravetz' (2006) criteria of "negotiation in good faith" as a basis for evaluating the quality of an extended peer review process, and using illustrative cases, I will then want to address the question of how one can:

- distinguish a "good faith negotiation" from a sham – in the context of science for policy,
- identify cranks, and
- evaluate the quality of scientific information when cranks are at the table.

I had a chance to talk with Jerry and other leading PNS practitioners at another Lisbon workshop held in May 2011 in honor of the retirement of Silvio Funtowicz. I also had an opportunity to ask Joseph Bast, the president and CEO of the Heartland Institute, just what he understands "post-normal science" to be. Since ICCC6 was held in Washington DC last year, and they welcomed bloggers as press, I attended it as a correspondent for *The Post Normal Times,* free of charge.

While at the conference, I happened to sit next to a very pleasant woman from the Ayn Rand Institute, who gave me a book entitled *The Logical Leap* - which seemed a fitting description of the entire affair. Although PNS does speak about certain kinds of science in negative terms, my overall argument is that the tale of corruption in climate science, as told by cranks and contrarians of various persuasions, only appears to fit this negative narrative if one takes a flying leap over crucial distinctions between the kinds of science that have led to unintended consequences, in which risks tend to be downplayed, and the kinds of science used to understand and address those consequences. It is also important to consider distinctions between different types of knowledge, uncertainty, and peer review - all distinctions that Jerry himself has observed.

That tale of corruption is only believable because of unrealistic public images and expectations of science, e.g., that it provides "proof", or that it is some sort of a crystal ball. Although skepticism is inherent in the practice of actual science, for reasons that should be obvious, I also argue that many of those who call themselves "skeptics" are actually cranks and contrarians who are performing something like a parody of science. Missing is the crucial wink/nod to indicate it as such – thereby crossing the line from parody to outright deception (see Nachmanovitch 2009), as the act gets mistaken for the real thing by those least informed,

and/or cannot tell the difference. The paradox is that parody only sticks when it has some element of truthiness, which means there are lessons in all of this for the practice of science as it enters the policy arena.

Since I still don't have pithy answers, and it seemed useful to keep it all in one piece as a reference document, I have posted these reflections as a pdf. In response to comments, I may follow-up with shorter posts on some of the sub-topics.

A REVIEW OF BASIC CONCEPTS

PNS concerns itself with the implications for science and society of situations in which "facts are uncertain, values are in dispute, stakes are high, and decisions are urgent" (Funtowicz and Ravetz 1991). At the highest levels of stakes and uncertainty, i.e., when conditions are post-normal, and with all of human well-being and existence potentially at stake, the implications of PNS are that, whether they want to or not, scientists will find themselves playing a more social role in informing the broader process of political decision-making, as well as being informed by it. However, given that trade-offs are typically linked to the diverse interests and values of those affected, the decision process is likely to become one of conflict resolution and of seeking to broaden the range of options, rather than simply finding *the right answer* - as there may not be one.

This is in contrast with *Normal Science* (NS), described by Kuhn (1962) as a process of puzzle-solving within an unquestioned paradigm, which can easily become a blinder to novelty, or anything outside that paradigm, which then comes as a surprise. In other words, NS is essentially an extension of laboratory approaches in which the environment is held or is assumed to be constant, the aim of which is to produce generalizable knowledge. However the applicability of this knowledge, and therefore also it's quality, is conditional on the highly variable and ever-changing context to which it is applied.

In a post-normal situation, NS is not sufficient because uncertainty and changes in context are not tractable from within a narrow technical definition of the problem at hand. This failure to consider changes in context produces all sorts of unintended consequences which are analogous to the more familiar concepts of "externalities" in economics, or "downstream" impacts, or simply, "someone else's problem."

Framing a problem narrowly, as merely a technical one, may be sufficient when there is an uncontested goal, like going to the moon, which could then be treated as merely a matter of rocket science. Agreement is less likely when it comes to contested matters, like building a dam or a nuclear power plant in a particular place, or carrying out a "large scale geophysical experiment" through the extraction and combustion of fossil fuels - not to mention changes in land use and agricultural practices. An exclusive focus on technicalities then becomes a blinder that may even be used quite deliberately to cover up value conflicts as well as to justify decisions already made. In this context, technical scientific information remains a key consideration, but whether or not it is relevant will depend on what the goal is.

Conversely, the increased complexity of many contemporary problems is in part the result of a broader framing of problems to include the social context, rather than the loss of some imagined idyllic past or purity of the scientific method. In part it is because many of the problems confronted in what have now become

post-normal times, from battling combined earthquake- and tsunami-induced meltdowns at nuclear power plants to coping with Frankenstorms, are the unintended consequences of the normal practice of science with blinders on, or what could also be called *A New Species of Trouble*.

Given problems that cannot be 'solved', in which uncertainty is inherent and unavoidable, emphasis in PNS is on evaluating the quality of information needed to understand trade-offs between different courses of action and to inform policy decisions. This broader framing of the problem leads to an extension of the peer review process both internally, to engage multiple disciplinary perspectives among experts, and externally, to engage those likely to be affected by such policy decisions. This democratization of the scientific process may also improve the quality of information, as these extended peers may bring greater knowledge of the context to the table - or may not, depending on whether participants are acting or negotiating in good faith - a point I will come back to.

However, as Jerry also pointed out in his essay, *extension* of the peer review process is not meant to imply *replacement* of the traditional peer review process – only that the latter is not sufficient for addressing problems with post-normal characteristics.

What seems to get overlooked in the context of controversies concerning PNS and climate science is that there are different kinds of knowledge and uncertainty, and that scientists have always managed technical uncertainties within their field. However, to go beyond laboratory conditions it is necessary to understand the context in which technical information is to be used and applied. In addition, it requires the engagement of those affected in deciding what are ethical rather than scientific questions as to whether the trade-offs are acceptable.

For example, double-blind studies conducted in a laboratory are typically done to determine the effectiveness of a drug for an "average" patient. But as my doctor once said, those aren't the patients she ever sees. To determine if a drug is appropriate for an actual patient, a doctor has to actually see and engage that patient in a "professional consultancy" mode. And the higher the stakes, i.e., the more serious the disease, and/or the greater the side effects of a drug, the more likely it is that the patient will want more information, will become more engaged in evaluating trade-offs among various treatment options, and will seek out second or third opinions. And then there was The Patient from Hell, aka, Stephen Schneider, who among other things, wrote a book that chronicled his successful battle against cancer, by working in close collaboration with his doctor.

Doctors are also held to a higher standard than laboratory scientists, through licensing requirements and codes of ethics, as are engineers who need to understand operating conditions to determine the reliability of a dam or a nuclear power plant. In addition, whether the risks or trade-offs of building a dam in a particular place are acceptable will require judgments not only about the reliability of the information, but also ethical ones about whether or not the benefits outweigh the costs of habitat destruction and the relocation of entire communities from areas that will be submerged. This mode also brings with it greater expectations of transparency regarding uncertainties, and accountability of experts to those affected.

KNOWLEDGE QUALITY ASSESSMENT

Extension of peer review raises the question of how to evaluate the quality of information in this broader context. In their 1990 book on Uncertainty and Quality in Science for Policy, Funtowicz and Ravetz presented a "research-pedigree" matrix (Table 1) with a set of criteria for evaluating knowledge quality. One of these criteria is the degree of consensus among colleagues, which may range from "no opinion" to acceptance by "all but cranks". It begs the question of how to identify cranks, and how to evaluate information quality where there are cranks at the table, who have no interest in finding areas of agreement, and who are not playing by the rules of science.

Table 1: Research Pedigree Matrix

Theoretical structures	Data-input	Peer acceptance	Colleague consensus
Established theory	Experimental data	Total	All but cranks
Theoretically based model	Historic/field data	High	All but rebels
Computational model	Calculated data	Medium	Competing schools
Statistical processing	Educated guesses	Low	Embryonic field
Definitions	Uneducated guesses	None	No opinion

Source: Funtowicz and Ravetz 1990

An additional challenge is that, given the need to engage experts from diverse disciplines, there is the danger of forcing knowledge from one area into the conceptual boxes of another. As pointed out by van der Sluijs et al (1998), "when quantitative information is produced in one disciplinary context and used in another, important caveats tend to be ignored, uncertainties compressed, numbers used at face value."

Ideally, this process becomes a forum for mutual learning rather than for combating wilful ignorance - as when the US House of Representatives actually passed legislation that would have overturned a scientific finding, that greenhouse gases endanger human health and the environment. However, when it comes to engagement of those affected, who have diverse goals and values, and who may all also bring diverse rationalities, not everyone will be interested in finding solutions, or even agree that there is a problem. As Sardar (2010) explains, in another *Welcome to Post-Normal Times*:

> …there is no natural law that states that activism will should or ought to be, dedicated solely to the common good. Nor is there any rule that they should take a balanced view and think through the risks and benefits of their agenda. Indeed it is in the nature of many of the self-organizing networks that have emerged to confound the times by offering simplistic, single issue, one-dimensional prescriptions and thereby increase the toxicity, animosity and dissatisfaction of society as a whole.

Lastly, while there may be multiple legitimate perspectives, it is important to keep in mind that *not all perspectives are legitimate*. At least in science, for a perspective to be legitimate, one needs to be able to justify it with supporting evidence.

However, the supporting evidence for climate change will not be as certain and clear cut as many seem to expect. It should be obvious that given such broadly defined problems and uncontrolled conditions, it will rarely be possible to actually quantify risk probabilities, that uncertainty is simply a fact of life, and that science involves a great deal of informed and professional judgment. Conversely, that probabilities should only be quantified to the extent that there is defensible data to support such an exercise with confidence, and that care needs to be taken to properly communicate uncertainties, as well as to be transparent about value judgments (Risbey and Kandlikar 2007). That does not mean that conclusions should not be drawn regarding aspects for which there is good data, such as global mean temperature, or when there is agreement among multiple lines of evidence.

The catch, as pointed out by van der Sluijs (2012), is that this "demands a culture that is open to uncertainty and that recognizes there are many things for which science cannot provide an answer." The infamous 2002 Luntz global warming memo, which informed the Bush administration that "there is still a window of opportunity to challenge the science", actually urged the Bush administration to take advantage of this lack of openness by emphasizing the lack of scientific certainty. In other words, climate science is caught in that infamous *double ethical bind* that Stephen Schneider once referred to, where nuance and uncertainty are impossible to communicate in sound-bites, but any appearance of overstating certainty is likely to backfire.

DIFFERENT KINDS OF SCIENCE

PNS does speak of certain *kinds* of science in negative terms, but Funtowicz and Ravetz (1990) also note that "the sciences which are required to *solve* the problem are systematically different from those that *created* the problem in the first place". Others have drawn similar distinctions between production science, which has been implicated in the "creation of many chemical, technological and ecological risks" – which it tends to downplay, and impact science, which has generally "challenged the assumptions that production science inevitably leads to progress" (McCright and Dunlap 2010).

PNS was originally conceived in the context of probabilistic risk assessment. Funtowicz and Ravetz (1990) took special aim at nuclear power, for which there is little if any basis for assigning numerical probabilities of risk that was somehow deemed to be "acceptable". In it they describe the "Ch-Ch Syndrome" which refers to "the experiences of Chernobyl and Challenger, both resulting from lapses of quality-control" and described as

> the catastrophic collapse of mega-technologies resulting from political pressure, incompetence and cover-ups (Ravetz et al., 1986). The destructive impact of our industrial system on the natural environment is another manifestation of the syndrome. Here the phenomena are less dramatic but more pervasive. The pathologies of the industrial system are transferred out, so that it degrades its environment while running 'normally'. This contradiction affects more than particular high technologies; the very place of science in our civilization is called into question.

Other work by F&R has taken aim at conventional economics - for example, in *The Worth of a Songbird* (1994), they show that in estimates of the economic impact of climate change, an analysis by Nordhaus (1991) is based on arbitrary guestimates that have a high degree of uncertainty. But this is not consistent with conclusions found in the same paper, that there is "no strong presumption of substantial economic impact" – a conclusion that is "justified entirely by hunches [that are] buried in a mass of hyper-precise arithmetical data".

A more recent piece by Jerry on *Faith and Reason in the Mathematics of the Credit Crunch* illustrates the way that mathematical science legitimized gambling and enabled the colossal failure of financial institutions. The mathematical equations underlying the AAA rated "toxic derivatives" were flawless except for the fact that they ignored the real world consequences of their application, beyond the immediate production of wealth for those who applied them.

With respect to climate science, in a *The No-Nonsense Guide to Science* published in 2006, Jerry stated:

> ...Certainly, no-one intended that fossil fuels would produce a 'the greenhouse effect' in the atmosphere.
>
> That is a classic case of Muddle that leads to 'unintended consequences' or Murphy. For some years after the problem was first announced, there was a lively debate among the scientists. The skeptics - always a minority - were appreciated for their usefulness in finding weak points in the arguments. As evidence of climate change from diverse sources accumulated, the remaining 'skepticism' became 'denial'.
>
> However, the fossil-fuel lobby continued its campaign unabated. At this point the issue changes from 'muddle' to 'malevolence', as those vested interests are only pretending to engage in a real scientific debate. In response to this new class of challenges, many leading scientists and scientific institutions change their orientation - on this sort of issue - from 'normal science' to SHEE [Safety, Health and Environment plus Ethics], warning of the dangers and invoking lifestyle, politics and ethics in their arguments.

In 2006, Jerry also published a paper in which he reconsiders *PNS in the context of transitions towards sustainability* and survival rather than the assessment of technological risks, and ponders whether the original PNS concept has become obsolete. In this new context, the key concern of PNS is with potential failure in extremely complex systems rather than with the original focus on uncertainty.

These complex system failures are associated with contradictions, which are defined as "a set of problems or tasks that cannot be resolved within the terms of reference (or 'paradigm') in which they are conceived." Jerry provides examples which suggest that, as contradictions mature, they are resolved either destructively, through wars and revolutions - as in the case of slavery in a country that was conceived in liberty, or creatively, through non-violence.

Among the noted contradictions of our time are that "governments are caught in the contradiction between guaranteeing safety to citizens and violating it in the cause of economic innovation and growth." Another is the "unsustainability of a lifestyle that most of the planet's people cannot ever enjoy, and the physical unsustainability of that lifestyle even for the (temporarily) fortunate minority." Thus his new focus on how science can support transitions to sustainability, and on moving *Towards a non-violent discourse in science* (Ravetz 2006c), and away from the scientific legacy of creating and downplaying risk, divorced from ethical considerations over its use and abuse.

It is in the transitions paper that Ravetz also suggests "negotiation in good faith" as the "essence of PNS dialogue", and as "a good criterion for distinguishing a real PNS [extended peer-review] process from a sham."

"CLIMATEGATE" IN CONTEXT

Sometime after that transitions paper, Jerry himself made a transition that leaves me baffled. In his infamous 2010 essay, he provided few details and no references or other evidence in support of his repetition of the often heard allegations of corruption in climate science. As if these were simply well-established facts, he equated "Climategate" with the credit crunch, the quickly corrected Himalayan glacier error in the IPCC report with Tony Blair's 'dodgy dossier' on Saddam, and the statement that "the debate is over" with "WMD beyond a doubt." He characterized climate science as "evangelical" because it leaves "little room for uncertainty", states AGW as a proven fact, and "relies on hockey stick behavior of all global temperature indicators". He also credits the blogosphere with empowering the Extended Peer Community (EPC) and exercising quality control. However, he also concedes that:

> keeping up with the blogosphere requires different skills from keeping up with traditional literature; it is most useful to find a summarising blog that fits one's special interests, as well as a loyal correspondent, as (in my case) Roger 'tallbloke' Tattersall.

Given that he has filtered his information through one particular person, I'm not sure how he can actually make a judgment about the quality of the Extended Peer Review process as carried out in the blogosphere.

The allegations he made are somewhat of a Gish Gallop, which would require a book to unpack – fortunately Michael Mann, who can speak from direct experience, has already written one that puts these kind of statements into context. Therefore, just a few brief comments before putting it into the broader context of what have been concerted and misleading attacks on climate science. First, I have never heard climate scientists refer to AGW as a "proven fact" although I have heard cranks misleadingly state that it is "not

proven", as if science ever could provide absolute proof. And given that science explores things we don't already know, there is plenty of uncertainty in particular aspects of climate science, although given the consistency among different lines of evidence, there is a well-established consensus that AGW is happening.

As for the "climategate" emails, one can hardly blame scientists for being misrepresented by others. For example, the reference to "hide the decline" appears to have involved some oversimplification in the merging of different datasets for purposes of presentation, to avoid confusing anyone with data from a particular area that was not thought to reflect actual recent temperatures, and therefore considered to be misleading (Mann 2012). Although this problem was well covered in the literature, one could argue that more detail should have accompanied the graph - but it hardly equates with the credit crunch.

I'm not convinced Ravetz has even read the responses of the scientists themselves, explaining the content of the emails that were taken grossly out of context and distorted. But if he could provide something more than vague allegations that only reference unsubstantiated claims, that take into account those responses, we could have a conversation. I do share his concern with the critical issue of trust in science, but the responsibility rests with those out to deliberately destroy it through fakery and deception.

At the very least, I would have expected Ravetz to take into account the conclusions of what are now eight reports of investigations by reputable bodies into various aspects of "climategate" plus two that examined IPCC errors and procedures. Overall, these found some practices that could be improved but no improprieties, deviations from standard practices or errors that undermined the conclusions of the IPCC. My own interpretation is that, although climate science faces some institutional challenges as it has moved from the world of basic research to informing policy, about which it has been learning the hard way, and some standard procedures may need to be revisited in light of increased demands for accountability, there is nothing to suggest that climate scientists have not been acting in good faith. Mistakes happen, but were admitted to and corrected.

If, as Jerry suggests, the essence of PNS dialogue is "negotiation in good faith," and that this constitutes a "good criterion for distinguishing a real PNS process from a sham", the next question is whether the allegations made by the so-called climate "skeptics" were made in good faith. If not, should we call them fake skeptics, contrarians, denialists, or simply cranks? And how should they be dealt with in a participatory process?

CONCERTED AND MISLEADING ATTACKS ON THE HOCKEY STICK

To address the question of good faith, we can also look at the concerted set of attacks on the "hockey stick" that preceded "climategate" which seem to provide one of the more clear-cut examples of bad faith. The iconic hockey stick, which shows that average temperatures in the 20th century were warmer than at least the past 1000 years, and that the most recent decades have been the warmest, is based on work by Mann, Bradley and Hughes (1998 and 1999) (or "MBH"), which has been replicated and confirmed by others in the peer reviewed literature, and also in a review by the National Academy of Sciences. Although some errors were found by McIntyre and McKitrick (2003 and 2005) (or "M&M"), both the peer-reviewed papers and the NAS

panel found that these did not affect the MBH conclusions. They were also addressed in subsequent papers by MBH.

This post only mentions selected highlights of a more complex story. For more complete accounts I recommend Michael Mann's book, these two posts by Deep Climate, and a very detailed report on Strange Scholarship by John Mashey (2010). The following tale draws on all of these.

Anyone not paying close attention can be forgiven for confusing the Wegman Report (pdf) with a report of the National Academy of Sciences. Edward Wegman is a statistics professor at George Mason University who, at the request of the House Committee on Energy and Commerce and its subcommittee on Oversight and Investigations, assembled an ad hoc committee to conduct an "independent" assessment of the work of MBH. Committee Chairmen, 'Smokey' Joe Barton (R TX, better known for his apology to BP) and Ed Whitfield (R KY), introduced Wegman at a Congressional hearing as "Chair of the National Academy of Sciences' (NAS) Committee on Applied and Theoretical Statistics" - although for purposes of this "assessment" he was operating independently of the Academy, though not from the Congressional Committee, from which Wegman's group received their source material. Whitfield also added: "and I can tell you right now that his document has been peer reviewed". Wegman himself claimed the report had been reviewed using "basically the same mechanism that was used at the National Academy".

According to Mashey, this ad hoc committee consisted of Wegman, some of his students, and a long-time associate who was not very involved. Thirty five of 91 pages in the report were found to contain mostly plagiarized text, with enough alteration to render it misleading and raise questions of academic misconduct. A journal article based on the report was published in the journal Computational Statistics and Data Analysis (CSDA) and then retracted. Ironically, the article was a network analysis which alleged that climate scientists were engaged in "pal review", i.e., friends reviewing one another's studies, but the paper appears to have been quickly reviewed only by the editor of the journal, who was also a friend of Wegman.

It is not clear in what way the "peer review" might have resembled the rigorous process used at the National Academy. As Wegman himself conceded at the hearing, there was no higher level review and approval of a proposed list of reviewers. Nor was there a higher level review by independent report review monitors to determine whether the reviewer comments were adequately responded to. As a former NAS staffer, I have actually had to prepare documents listing how each and every review comment in a draft report was responded to by the committee, providing justifications for any points on which the committee may have disagreed with the reviewer. That doesn't even count the cross-check of all references, also done by someone who had not worked directly on the report.

At the congressional hearing, Wegman indicated there were nine reviewers, of which two requested anonymity. Mashey's review summarizes the review process as one in which review was requested at the last minute, reviewers were not given adequate time, and feedback that was received was not addressed in the report. One review, submitted by Enders Robinson after the report was finalized, and entered into the hearing record, consisted of one paragraph that, as Mashey points out, confuses models and methods. Another reviewer was Grace Wahba. In a seminar presentation (mp4), speaking about the referee job done on the Wegman report, Gerald North paraphrases an email from her: "Hey they used my name and they said I

was a referee. He sent it to me about 3 days beforehand and I sent him a bunch of criticisms which they didn't take into account."

Finally, there is this comment from Michael Mann, in replies to questions raised at the hearing that were submitted for the record:

> There is another element of this question which raises a deeply troubling matter with regard to Dr. Wegman's failure to subject his work to peer review, and Wegman's apparent refusal to let other scientists try to replicate his work. Professor David Ritson, Emeritus Professor of Physics, Stanford University, has found error in the way that Dr. Wegman models the "persistence" of climate proxy data. Interestingly, this is the same error Steven McIntyre committed in his work, which was recently refuted in the paper by Wahl and Ammann, which was in turn vetted by Dr. Douglass Nychka, an eminent statistician. Dr. Ritson has determined that that the calculations that underlie the conclusions that Dr. Wegman advanced in his report are likely flawed. Although Dr. Ritson has been unable to reproduce, even qualitatively, the results claimed by Dr. Wegman, he has been able to isolate the likely source of Wegman's errors. What is so troubling is that Dr. Wegman and his co-authors have ignored repeated collegial inquiries by Dr. Ritson and apparently are refusing to provide any basic details about the calculations for the report (see Attachments 3 and 4 to this Response). It would appear that Dr. Wegman has completely failed to live up to the very standards he has publicly demanded of others. Moreover, the errors that Dr. Ritson has identified in Dr. Wegman's calculations appear so basic that they would almost certainly have been detected in a standard peer review. In other words, had Dr. Wegman's report been properly peer-reviewed in a rigorous process where peer reviewers were selected anonymously, it likely would not have seen the light of day.

In addition, Wegman never released his own computer code, which he had said he would do, in response to a 2006 request from Rep. Henry Waxman, the ranking minority leader of the Congressional Committee. He also gave misleading reasons for this. But Deep Climate found other reasons he may have been reluctant to release it. A dive into the M&M code revealed that Wegman took a carefully selected "sample" of 10,000 pseudo-proxy principal components, i.e., the top 100 with the most pronounced upward blade – rather than a random selection (further analysis here).

Dr. Judith Curry has no excuse for confusing the process used in the Wegman report with that used by the NAS/NRC. According to her CV, she has served on both the NRC Climate Research Committee and Space Studies Board, and might have at least asked what process was used before making the following unsubstantiated comment (#114) on another blog:

> Specifically with regards to the Wegman Report, this was a paper that had been commissioned by a Committee of the US House of Representatives, peer-reviewed in exactly the same way as NRC 2006, and was read into the House record on 17 July 2006.

Curry is another scientist and blogger who has seemingly embraced PNS and, like Ravetz, has engaged contrarians as an Extended Peer Community – and seems to have become one herself. She also participated in

the Lisbon Reconciliation workshop. There are a number of posts on her blog on the topic that I may or may not come back to in a future post. I have not been able to take her blog seriously or spend inordinate amounts of time on it since she wrote a post back in January 2011 being dismissive of climate science for doing attribution studies - because they potentially provide fodder for "the blame game" (H/t The Policy Lass). Curry's writings on climate uncertainty generally read like an unsynthesized literature review by someone who has just *discovered* the social sciences. But I digress…

Barton and Whitfield had been offered an actual review by the National Academy of Sciences. Noting that "a Congressional investigation is probably not the best way to resolve a scientific issue" the president of the National Academy of Sciences, Ralph Cicerone, offered to "create an independent expert panel… to assess the state of knowledge in this area."

The scientific issue to which Cicerone was referring being the errors found by M&M that had led to what can best be described as an inquisition. Following the M&M publications - the first in 2003, in the non-peer reviewed publication Energy and Environment, and a second one in the AGU's Geophysical Research Letters in February 2005, and a talk at the George Marshall Institute in Washington DC (May 11, 2005), Senator Inhofe began to proclaim that the hockey stick had been discredited, as did an editorial in the Wall Street Journal, June 21 2005.

Interestingly, the WSJ editorial uses the same obsolete, modified, and not properly attributed figure,7.1(c), from the IPCC 1990 report as was used in the talk given by M&M, further modified in the Wegman report, and later used also by Inhofe in a 2012 book. To be fair, the figure, which illustrates that there was a Medieval Warm Period, had also been mislabeled in the 1990 IPCC report itself as representing global temperature trends rather than temperatures only from Central England, and appeared with caveats in the text about the lack of historical data at the time. Although it had not been used in any subsequent IPCC reports, which were issued in 1992 and 1995, William Connolley , recently found another alteration of it, which labeled the 1990 data as representing "IPCC 1990-2001", as if there had been scientific agreement on the trend up until the hockey stick diagram was published.

Two days later, Congressional Representatives Barton and Whitfield, Chairmen of the House Committee on Energy and Commerce, and the Subcommittee on Oversight and Investigations, sent intimidating letters to Mann, Bradley, and Hughes requesting information on all studies related to climate change on which they were authors or co-authors, and their funding sources, all financial support received related to research, all agreements pertaining to those research grants, locations of all data archives for all published studies including supporting documentation, computer code used to generate results, responses to requests for data not publicly archived, along with detailed responses to the alleged errors, and detailed explanations of work for IPCC and IPCC procedures.

In addition to an 11 page response from Michael Mann, there were responses from over 30 scientific organizations including the European Geosciences Union, AAAS, in addition to an editorial in Nature, several US scientists, and other politicians - all expressing concern about the nature of the request and use of the political process to investigate scientific matters. (some of them are posted here)

Another Congressman, Sherwood Boehlert, chairman of the House Science Committee, did request an NAS review (Nov 2005) and even invited Barton to join with him. The letter from Boehlert questioned Barton's jurisdiction over scientific matters, and pointed out that Barton was well aware that the National Science Foundation (NSF) "soundly rejected" allegation that MBH had refused to share data used in studies".

Instead, Barton commissioned the investigation by Edward Wegman, and both reports were published in the summer of 2006. The NAS report reaffirmed conclusions of MBH, while noting higher uncertainty for earlier periods. Wahl and Ammann (2005) had already found that the errors found by M&M did not make any difference in the results.

REPEATING OBVIOUS URBAN LEGENDS, FOR YEARS ON END…

In a recent paper, Van der Sluijs (2012), another PNS practitioner, recognizes the difficulty of including dissenting scientific views on climate change, given that these run the gamut from "criticism to alternative interpretations of data that are scientifically sound to obvious urban legends" that are "sometimes repeated for years on end, even when the errors have been pinpointed and sometimes even recognized by the corresponding skeptic." He concluded that "the high standards that climate skeptics rightly demand from the IPCC apply to them too."

So I was surprised that, in the very same paper, he uncritically repeated a claim by Benny Peiser (2005), that Naomi Oreskes (2004) had exaggerated the degree of scientific consensus on climate change found in the scientific community. First, note that the Oreskes paper appeared in the peer-reviewed journal Science, which rejected the Peiser critique - for a good reason. That critique was instead published in the National Post, a Canadian newspaper, on a page that no longer appears on that site but that can be found using the wayback machine.

Oreskes' finding of consensus was based on a review of abstracts of 928 articles in peer-reviewed literature that came up in a search of the terms "global climate change". Using slightly different terms, Peiser found additional abstracts, of which he claimed that 34 rejected the consensus. These 34 abstracts were posted and reviewed on Tim Lambert's Deltoid blog - where it was found that the only item that actually questioned the consensus was a non-peer-reviewed article published by the Association of Petroleum Geologists. At the time, I looked through those 34 abstracts and generally found that Peiser had failed to distinguish uncertainty about whether or not global warming is human induced, from uncertainty about the possible impacts. He also confused studies of the climate itself with studies of climate policies and conflated uncertainty with lack of consensus. In short, he appears to have just looked for the term "uncertainty" – without looking at the context in which it was used. Although Peiser conceded he was wrong, and said he had withdrawn his criticism as of March 2006, he continued to maintain his belief that "the majority consensus is far from unanimous".

Nevertheless, Peiser's withdrawn claims were recycled in 2007 in papers by Christopher Walter, The Third Viscount Lord Monkton of Brenchley and also by one Dr. Klaus-Martin Schulte that also claimed to find 32 additional post-2004 abstracts that rejected the consensus either implicitly or explicitly. A pre-publication version of the latter was reported on in a Daily Tech article, and was then very well publicized in the blogosphere. Although initially turned down for publication by the journal Energy and Environment, to which

it had been submitted, it was later published – but is behind a paywall. Analyses by Tim Lambert and John Cook of the 7 abstracts that were claimed to explicitly reject the consensus found the same kinds of mistakes as had been made by Peiser , e.g., one was about perception of climate risk, another about uncertainties in current understanding of the carbon cycle that did not reject the consensus, and another about uncertainties with respect to impacts of sea level rise. Although there were two that did reject the consensus, these relied on arguments that don't hold up, and another was a review paper.

At the time, Oreskes also commented, pointing out that, at least according to news reports about the pre-publication version of the Schulte paper, Schulte had misrepresented her research question, the results obtained, and her interpretation of the severity climate question, as well as the positions of the scientific societies whose positions she had compiled. In response, Schulte alleged that she had inaccurately represented his paper, but refused to share with her an actual copy of it. He also threatened to take "appropriate measures" if her allegations were not withdrawn within 14 days. Oreskes comments were updated to clarify that they were based on blog descriptions of the Schulte paper rather than the paper itself, and asks "Why did [Schulte] not make clear, when the story broke, that Daily Tech and the EPW committee were spreading an incorrect story" and direct his demand for an apology to them? In a further reply, no longer found online, Schulte lists 5 additional papers from the original Oreskes review, that he claims reject the consensus – Lambert found that these not only repeat the Peiser mistakes, but were cut and pasted from the Monkton paper, without citing either Monkton or Peiser , who was the original source.

Peiser's work was cited again by Richard Lindzen in a 2008 article, where he claims that Peiser only acknowledged that one of the papers in his sample was probably inappropriate. He must have not read the direct quote from Peiser, in his response to MediaWatch, which was that: "some of the abstracts that I included in the 34 "reject or doubt "category are very ambiguous and should not have been included." He also overlooked the above analysis which found only one of those 34 that rejected the consensus position, but was not a peer-reviewed article. The whole story has been chronicled in full detail by John Mashey.

ABANDONING THE SCIENTIFIC METHOD?

As I mentioned, I also had an opportunity to ask Joseph Bast, president and CEO of the Heartland Institute, what he thinks PNS is, and how he thinks it led to the "abandonment of the scientific method." And to "Climategate." His main argument was that, in the absence of certainty, PNS relies on consensus instead of data as justification for conclusions, and is value laden. To which he added that, Anthropogenic Global Warming (AGW) is not science because it is not falsifiable, that alarmists keep moving the goalposts – *even colder weather is cited as evidence of climate disruption*, that the IPCC is a political organization, that scientists are political hacks, and that the IPCC executive summary has no bearing on the content of the report.

As a source for his interpretation of PNS, Bast cited Mike Hulme's book, *Why we disagree about climate change*, which does have a nice discussion of the trade-offs inherent in using a consensus approach. Hulme summed it up as: "the worst of all possible ways of assessing knowledge about climate change... apart from all the others" and as at least "better than a random solicitation of views." Although seemingly at odds with the notion of science as "an objective adjudicator between truth and error", at some point, "where stakes are high and decisions urgent, some way has to be found of bringing scientific evidence into the policy-making process

even when uncertainties remain endemic and even when there is not unanimity among all scientific experts." The key issue for Hulme is "the processes of governance, through which consensus is consolidated, and by which disputes… are handled." Providing that "the process of consensus building is open, transparent and well governed, if it seeks to be true to the many uncertainties which persist, then consensus may offer policy makers the best that science has to offer."

The drawback of consensus is that it tends to exclude unexpected events and poorly understood processes, for which there is insufficient information upon which to agree, as discussed further by Oppenheimer (2007) and mentioned also by van der Sluijs (2012). In other words, one of the main limitations of consensus knowledge is that it is conservative – as is confirmed by a new study (reported on in the Daily Climate), which compared "past IPCC predictions against 22 years of weather data and the latest climate science" and found that "the IPCC has consistently underplayed the intensity of global warming in each of its four major reports released since 1990." For example, the Arctic was not expected to lose summer ice before 2070 but after the dramatic decline, and a series of new record lows, researchers now say it could happen within the next 20 years. And measured sea level rise from 1993-2006 was 3.3 mm/year, which is over 50% higher than the 2 mm average projected in the 2001 IPCC report. In an interview with the Daily Climate, Naomi Oreskes, one of the co-authors said "the pattern is under – rather than over-projection…. These data simply do not support the allegations by skeptics that scientists have been alarmists."

The basic argument of PNS is simply that, in a policy context with high stakes and high uncertainty, and given the increasing role of science in daily life, it is no longer sufficient to seek consensus only from peers within a particular discipline. As discussed above, this process has to be extended across disciplines, and among those affected. But within the boundaries set by the principle of good faith negotiation. Science is ultimately a way of validating knowledge, which implies acceptance by peers, so it could be argued that consensus is actually part of the scientific method. Given that policy making typically requires that an accommodation be found among conflicting perspectives, uncertainties and dissenting views are therefore also relevant for policy purposes, providing that they pass the smell test.

Bast gave no indication of what alternative he might propose to consensus as a basis for making policy decisions. Furthermore, his view of what the scientific method is does not even appear to include the ability to replicate a study. As an example of an actual peer-reviewed study that refutes AGW, he talked about Richard Lindzen's notion that there is a "tropical heat vent over the tropics" or an "iris effect" in which warmer temperatures reduce the upper level cirrus clouds, which leads to more radiation of heat, thus, to a negative feedback or cooling effect. In other words, the earth has a natural thermostat, which he says can be tracked.

I assume he was referring to the Lindzen et al 2001 paper on the "adaptive infrared iris" hypothesis published in the AMS bulletin. Although peer-reviewed, so far no one appears to have been able to replicate it. Skeptical Science puts this paper into the context of Lindzen's main argument, that climate sensitivity is low. Later, in Lindzen and Choi 2009, Lindzen claimed that satellite observations demonstrate this increase in radiation loss in conjunction with warmer temperatures. But even Roy Spencer, who agrees with Lindzen and Choi that climate sensitivity is low, does not think "their results actually demonstrate this." As pointed out at Skeptical Science, others have soundly refuted them.

Like Ravetz, Bast was on a Gish Gallop but to respond briefly to his other comments:

Regarding the "falsifiability of AGW" – the consensus on AGW is one based on multiple lines of evidence from many different kinds of studies – does he not know the difference between individual studies that tests a falsifiable hypothesis, models, and knowledge assessments? As for "moving the goalposts" perhaps he is confusing colder weather with higher snowfall, which has been attributed to higher atmospheric moisture - which is associated with AGW.

As for his remarks about the IPCC: it was established to provide knowledge to support policy decisions with respect to climate change, so opposition to it is to be expected from anyone not convinced that climate change is even a problem. Judging from statements made at ICCC6, which were loudly cheered by conference attendees, that group does not just disagree with climate change science, but also opposes any government funding of science. In other words, if they had their way, climate science would never have been funded, and we would know even less about it.

As an organization, the IPCC appears to have learned the hard way that the boundary between science and policy is a fuzzy one, and as discussed in a review by the InterAcademy Council (2010), there are always ways it could be improved. But I have not seen a convincing case that any of the errors affected the overall conclusions, or that its participants have acted in bad faith. Nor could one expect good faith participation from those who vehemently disagree with its entire purpose. However, given the limitations of consensus, I actually agree with Bast, that it would be a good thing if the IPCC executive summary provided more insight on areas of uncertainty and disagreement, but only to the extent that these are *legitimate*, i.e., justifiable.

CONCLUSION

Going back to the questions posed in the introduction - PNS speaks negatively about the kind of science that has had unintended consequences, or "production science", in which risks tend to be systematically downplayed. Unfortunately, the science used to understand and address those consequences, or "impact science", remains rooted in the same scientific traditions, in which it is more important to avoid "Type I" errors - of accepting a hypothesis that is wrong, with a 95-99% confidence level, than "Type II" errors - of rejecting a hypothesis that is true. The first is entirely appropriate for basic research, but in the context of evaluating risk, it leads one to wrongly assume risk is low or that a technology is safe (Freudenberg et al 2008). Scientific consensus leads to a similar bias in that it will be limited to well understood processes and expected events for which there is sufficient information upon which to agree – thereby increasing vulnerability to unexpected events that are poorly understood.

PNS is also critical about the practice of normal science in a post-normal situation, i.e., within a narrow technical framework, devoid of context, which can lead to "Type III" errors, of producing information that is entirely irrelevant (Funtowicz and Ravetz 2003). An example is the inappropriate use of science to make value judgments as to what is an acceptable level of risk, or simply using technical information to cover up value judgments, which are embedded in the way a problem is technically framed.

If PNS appears "tailor made for the denialist crowd" it is because these differences are mostly overlooked. I would argue instead that it is the failure to recognize the implications for science of a post-normal situation that makes science vulnerable to what some have called "Scientific Certainty Argumentation Methods", or SCAMs (Freudenberg et al 2008). Thus we find science in a double-bind, where failure to fully communicate uncertainties reinforces public expectations of certainty, which in turn allows the credibility of science to be undermined by exposing those uncertainties. But uncertainty is inherent in all science, and is not limited to technical considerations.

Broader framing of problems to consider contextual and ethical concerns leads to an extension of the peer review process to involve experts in other disciplines, as well as stakeholders – who bring knowledge of context, and who are directly affected by any trade-offs being made. Farrell (2010) makes the case that this is an empirical observation rather than some sort of normative prescription. Given the extent to which science is intertwined with our daily lives, and enabled by the blogosphere, this extension is happening, regardless of whether anyone thinks it is a good idea. However, for the same reason one does not go to a dentist or to neighbors for heart surgery, it does not replace traditional peer review of technical information by qualified experts.

What seems to be missing from PNS is a discussion of how to evaluate quality of information in this extended process, particularly in the presence of cranks who are only participating to disrupt it by challenging technical information rather than being open about value differences. In this post, I have used selected examples to make the case that these self-proclaimed "skeptics" are not playing by the rules of science or acting in good faith. These example show instances of:

- False and unsubstantiated claims,
- Failure to adhere to high standards demanded of climate scientists,
- Continued repetition of allegations, without acknowledging responses to them, in one case even after they were withdrawn by the person who first made them,
- Extensively plagiarized text, altered to be misleading,
- Biased "random" sampling,
- Alteration and mislabeling of a particular IPCC graph, and
- Making claims based on studies that no one has been able to confirm or replicate.

In addition, as I mentioned in the introduction, starting with the act of calling themselves "skeptics", they are actually performing a parody of science, but without any winks or nods that would indicate it is such, thereby, deceiving the literal-minded, and those least informed, who cannot tell the difference. Or as Erving Goffman (1974) once described it (referring to earlier work of Gregory Bateson):

> "a bit of serious activity can be used as a model for putting together unserious versions of the same activity, and… on occasion, we may not know whether it is play or the real thing that is occurring…. individuals can intentionally produce framing confusion in those with whom they are dealing… "

This is evident in the release of reports that continue to recycle debunked claims such as those of the NIPCC, i.e., the "Nongovernmental International Panel on Climate Change" or "Not the IPCC", a so-called "Addendum" to the report on Global Climate Change Impacts in the United States by the US Committee for Global Change

Research (USGCRP), or "Not the USGCRP", and a fake PNAS paper. It is also evident in "science conferences" aimed at generating publicity, rather than advancing scientific understanding, and the insinuation that the Wegman report had some link to the National Academy of Sciences, and/or followed the same procedures, as discussed in this post.

Perhaps, in confusing others, Heartland Institute has itself become confused. The HI website claims it is "the world's most prominent think tank promoting skepticism about man-made climate change". However, in a recent letter to the Washington Post, to correct an error in reporting about Heartland's finances, Joseph Bast claimed that:

> The Heartland Institute is not "skeptical of climate change science." We are one of its leading supporters, having hosted seven international conferences… and published a comprehensive survey of the scientific literature in two volumes...

Questions have also been raised as to whether some of these "skeptic" organizations are following the rule of law, given their focus on influencing public policy, while having the status of tax-exempt public charities. The Heartland Institute for example, denies any lobbying activity, while also touting their ability to reach US elected officials – just one of several questionable activities documented by Mashey.

In sum, now that the blogosphere has indeed enabled the extension of the process of peer review to enable participation by stakeholders, more consideration needs to be given to evaluating the quality of information in this context, and to protecting the integrity of the process.

Just as rules have developed over time for participation in the scientific endeavor, there is now a need for consideration of more explicit rules or norms that might be developed for participation in Extended Peer Communities. If we think of scientific or other knowledge as a commons, we might draw inspiration from Elinor Ostrom's principles for managing the commons – one of which is the need for explicit boundaries that limit access to those entitled to use a resource.

For purposes of managing knowledge (vs physical resources), I have suggested boundaries be established that limit access to those negotiating or acting in good faith. Taking advantage of much heavy lifting that has been done by a handful of climate blogs, I have also made the case that the self-proclaimed climate skeptics are doing no such thing, and that they are merely using pseudo-technical arguments to block discussion of political disagreements and value differences.

I can only speculate whether Jerry Ravetz has been hijacked by those "pretending to engage in a real scientific debate" or has reasons for engaging them that I have not been able to fathom. My suggestion for him is that he remove the 'tallbloke' filter and start reading some of the blogs aggregated at www.planet3.org. Or better yet, Michael Mann's book, *The Hockey Stick and the Climate Wars*, which puts the full "climategate" story into context, and provides links to many of the most relevant related blog posts. Or even some of the posts on this blog.

Lastly - Ravetz also expressed concern about climate change being addressed in isolation, as a technical issue, while neglecting sustainability and equity concerns. However, I fail to see how climate change can be addressed without taking these broader considerations into account. Although found in different silos, there is also no lack of research and policy attention to such questions. As an issue, understanding climate change merely provides an entry point for learning about these interconnections – there is nothing like droughts in key growing areas for understanding the implications of rising food prices…

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