

# The Significance of the Hamburg Workshop: Post-Normal Science and the Maturing of Science

*Jerome Ravetz*



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## ABSTRACT

In this essay I review my own involvement in climate science, and attempt to draw some useful lessons. I start with a critique of the theory of post-normal science (PNS). This is derived from the experience of the effective criticisms of PNS that were made on the blogosphere. I proceed to a critique of climate science itself, which might be described as the attempt to solve a post-normal problem by “normal science” methods. Since quality, in a variety of aspects, became crucial in the Climategate debates, I analyze that concept in the fraught context of a politicized, contested science. Such sciences have the seeds of tragedy for those who innocently engage with them believing that their task is simply to speak truth to power. Finally, out of my personal history I suggest that we keep in mind the personal investment of anyone holding a contested view, and respect their struggles to maintain integrity when their core beliefs are under attack. This motivates the fostering of non-violence in debates on policy science issues.

## KEYWORDS

climate science, non-violence, post-normal science, quality

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## Introduction

The Hamburg workshop held in May 2011, “Post-Normal Science: The Case of Climate Science,” has been historically significant in several ways. With the inclusion of Post-Normal Science (PNS) in the title it did more than announce that there are still uncertainties in climate science. For PNS it implies that the combination of uncertainties and value-commitments requires the participation of an “extended peer community.” The extension of democracy from political processes involving science, to the work of science itself, is still not universally accepted among scientists. This is the challenge of PNS, and its adoption by the distinguished scientists at Hamburg is a step forward for PNS and, more important, for the principles it represents. Moreover, the



workshop provided the occasion for an editorial in *Nature* (Anonymous 2011) where the issue of PNS and climate science was raised quite directly. We should recall that up to that point and beyond, the official position of the English scientific establishment, as embodied in the Royal Society, has been that “the debate is over” and, “the science is settled.” The political significance of an editorial in *Nature* is not to be underestimated. There is evidence that on this issue the scientific establishment had already lost the confidence of much of the scientific media (“No Candour” 2010). But *Nature* is more than just another journal; it has moral authority as the leading British and maybe the world’s journal of research and comment. For *Nature* to assert that climate science is post-normal, is to defy the scientific establishment on this very crucial policy issue. But that is just what happened in May after the workshop. It was a vindication of Hans von Storch’s courageous and principled stand over the years, and the politics of science will never be the same again.

All these consequences are additional to the constructive and vigorous discussion at Hamburg of the problems of the scientific endeavor in the post-normal age. In the precursor to the Hamburg workshop, the “Reconciliation in the Climate Debate” workshop in Lisbon in January 2011, it was enough to get people from opposed sides to recognize and accept each other’s integrity. In Hamburg in May, although it was less polarized, there was a good spread of positions, and discussion was lively throughout. And the broad spread of participants, including scholars from various fields and also students, gave the occasion a genuinely post-normal atmosphere. New insights from a great variety of perspectives made this workshop not merely a great experience, but also a forum worth revisiting.

## **The New Situation of PNS**

When Silvio Funtowicz and I first sketched the idea of PNS, we knew that its realization would take time. We could not anticipate how that would happen, or what would bring it about. Now we know: it is the blogosphere (see introduction to this volume). As it becomes established, it confronts new challenges. I have experienced these in my own forays in the blogosphere, notably on the very important blog [www.wattsupwiththat.com](http://www.wattsupwiththat.com), organized by Anthony Watts. Some have met with more success, some with less. These are worth reviewing here. To some extent they are a consequence of the history of PNS it-



self, created and developed outside the academic world, and so deprived of the criticism and stimulus provided by colleagues and students. Hence many obscurities in its original formulation have remained. Recent studies by colleagues, organized by John Turnpenny (2011), and criticisms on the blogosphere, notably by the bloggers “scientistfortruth” (anonymous) and Willis Eschenbach, have stimulated me to serious reflection (Ravetz 2011).

First, I should note that the organizers of the Hamburg workshop have distinguished between post-normal “situations” and post-normal “practice.” This distinction improves on the original, where the term post-normal “science” (itself somewhat provocative) covers both meanings. One reason for this damaging ambiguity is that in our writings we said very little about post-normal practice, even though there were already examples (mainly in community-based research on health and safety issues) available to be cited and analyzed.

The most salient problems with contemporary post-normal theory have to do with its interpretation in policy issues. It has proved to be all too easy for the “extended peer community” to be interpreted as a shallow “political correctness” or even as “political determination” along the lines of Lysenko. Supporters of PNS have on occasion strayed into the former, and hostile critics on the blogosphere have seen the latter. (For a criticism of Mike Hulme along these lines, see Lewin 2010.) Again, by failing to anticipate these misinterpretations (or better, alternative interpretations), we did not erect warnings. Worse, when colleagues were making these interpretations, we did not make public clarifications. As a result, it was natural for critics on the blogosphere to make PNS doctrine responsible for what they saw as the dishonesty and corruption of “carbon-based anthropogenic global warming” (CAGW) science (see Ravetz 2011, section by scientistfortruth).

One reason for the absence of clarity on this issue was our failure to offer a serious discussion of quality. If quality does not relate to truth, then what can it be, other than popularity? It is not easy to give a simple definition of quality other than “fitness for function,” which itself presupposes far more than it explains. When challenged, I failed to respond appropriately. A proper essay on quality would demonstrate its complexity, with ethical and recursive properties, and then one should relate its practice to integrity. Elsewhere (Ravetz 2006), I discussed quality in post-normal situations, invoking “negotiation in good faith” as a principle.

Another serious error was in my casual jettisoning of truth. I was aware of a century of philosophical discussion of science in which

that idea has been sidelined, but my correspondents in the blogosphere were not. Also, there is a personal interpretation of truth, which might be called “truthfulness” (personal communication with Elijah Millgram in 2010) which is very relevant here, and also relates to Integrity. By invoking these associations, I could have managed many criticisms.

In the context of the debates on the blogosphere I could not even begin to explore the deeper philosophical issues raised by “quality” and “truth.” The paradox of quality is matched by a paradox of truth. As philosophers have known since the days of Socrates, to destroy the simplistic belief systems on which most people rely, is to risk destroying their personal foundations of morality and to leave them open to corruption and collapse. As Thomas Kuhn (1962) made very clear, most “normal” scientists operate within a very simplistic belief system—the “paradigm”—where the deeper values are implicit or even tacit or unrecognized. If the simplistic faith of working scientists in the truth of their paradigm is destroyed, then they are at risk of cynical disillusion. This danger was recognized in the “science wars” by the critics of Feyerabend and the reductionist sociologists. It could not be handled in a sophisticated way on an excited blogosphere.

Shorn of the traditional epistemological foundations for ethics in science, my account of PNS lacked defenses against the question “Why not cheat?” Indeed, I have been seen by some as the corrupter-in-chief of the Climategate scientists. Here my defending argument would have been along the lines of science as one of the “fiduciary professions,” where the maintenance of trust is essential, although not always present. In my book *Scientific Knowledge and Its Social Problems* (Ravetz [1971] 1996) I argued that in the absence of morality, quality assurance would suffer, and in the absence of quality, research could easily become vacuous. Thus the existence of our objective knowledge depends on the subjective commitments of the researchers. A nice paradox!

Then there was a parallel misunderstanding about “normal.” For most people it has the connotations of being the right sort of thing. For me, however, it partly refers to Kuhn’s image of a dogmatic, myopic practice. Could it be that the myopic puzzle-solving practice of “normal” science is actually perceived by many as the realization of the search for truth? If so, our task in explaining PNS is onerous; but then, why shouldn’t it be like that?

In summary, it is hardly surprising that some intelligent and perceptive people took PNS to be a thoroughly confused and pernicious



doctrine! Of them, I have said (paraphrasing the Jewish motto) with such enemies who needs friends? Seeing it all in historical context, now that such issues are becoming clear for me, there is a chance that I could belatedly build a philosophically interesting theory of PNS out of that original practical insight.

## **The Post-Normal Critique of CAGW Science**

Something has gone very wrong with the science of Carbon-Based Anthropogenic Global Warming (CAGW). Only a few years ago it was the orthodoxy for all but some isolated rather extreme tendencies in science and in society. Nobel prizes were awarded to leading individuals and organizations. Even major fossil fuel corporations joined the campaign; we recall “Beyond Petroleum.” Then it all began to fall apart, most dramatically with Climategate. Just now climate change has been reduced in America to a divisive “touchstone” issue along with evolution, and the market in carbon credits is near to or at collapse. What went wrong? Proponents will blame it all on bad people in politics and science; opponents say that the science was all along flawed at best or corrupted at worst. If it is still too early to explain the significance of the French Revolution (as Chou En-Lai famously said), we should feel no obligation to explain the rise and fall of CAGW science. Still, there remain some useful things to be said, particularly from the post-normal perspective.

I have already dealt with the politics of climate science from a post-normal perspective, emphasizing the role of the blogosphere in enabling the work of an extended peer community in the way discussed during the Hamburg workshop and here in the introduction to this special issue. There is another aspect of the situation, for which our perspective can be useful—whether the whole climate scientific venture was misconceived. It was loaded with policy implications that it could not possibly satisfy. We know from the work of Hans von Storch and Nico Stehr (2010) that “climate” is very much culturally conditioned, and “change of climate” would require a breadth of data and a length of time span that defy ordinary scientific investigation for its determination. So the case for CAGW needed several sorts of evidence, which even together could not be conclusive. There were Al Gore’s anecdotes, including the ambiguous polar bear and the melting glaciers of Mount Kilimanjaro. And there were the models, with impressively precise outputs that begged the question of their under-

lying accuracy. Finally, there was the history, epitomized in the “hockey stick.” And in the last resort there was the “climate sensitivity,” the crucial parameter with a hole of ignorance in the middle of its possible values that effectively spans the gap between complacency and panic.

There have been discussions of uncertainty throughout the history of the endeavor. Steven Schneider tried unsuccessfully to bring some coherence to them. Even when it proposed a sensible scale of uncertainties, the one offered by Risbey and Kandlikar (2007), the IPCC deleted the category of “ignorance”—in CAGW science, that was taboo (see also Risbey and O’Kane 2011). Even in the most classical laboratory sciences, the management of uncertainty varies from the unreflective routine to the passionately contested. What I have not seen in all the discussions of the climate science, is a consideration of the evidential strength and accuracy of the science that would be requisite for establishing the policy implications. Obviously there is no simple answer to such a question, but its discussion would at least set the framework that could be useful, rather than unending, arcane analyses of the different sorts of uncertainty.

Accepting all the complications of politics, personalities, and dubious practices (on all sides), I believe that in this issue of uncertainty we have another important key to the history of climate-change science. Briefly, all the participants have assumed that this is a case of “normal science,” where the facts are discovered by research, and then truth would speak to power. After all, what could be simpler than the questions, “Is the climate changing? And if so, what causes it?” For scientific background there is the classic simple model of Arrhenius, providing a first-order approximation to the quantitative analysis. But, in the actual history of the enterprise, complications and uncertainties intruded, and they were never adequately controlled. This is partly because the original Arrhenius model, not accounting for feedbacks, yielded a “climate sensitivity” that indicated no danger. Up to the very end, three-digit precision was used for the targets for CO<sub>2</sub> concentration, even when there was an uncertainty of a factor-of-three in the key parameter. Even now, Judith Curry (2011a) finds the NUSAP (notional system for uncertain information, see Funtowicz and Ravetz 1990) formalism useless because it is “too complicated.” In respect of uncertainty, climate change science had the same sort of weakness that was fatal in the case of finance science. There it was assumed that all the arcane, frequently incomprehensible speculative financial products had the same “Gaussian” distribution of uncertainty as the



heights of Army recruits. For climate science, the distributions were less ridiculous, but the depth and variety of uncertainties, even in the most mathematical areas, could never be effectively controlled. In the event, finance science collapsed with a big bang, while climate science seems to be going down with a whimper.

The conceptual weakness here is worse than the application of “normal science” methodologies to post-normal conditions. We have a case where the assumptions of simplicity, of both causation and evidence, were appropriate for a controlled, reproducible laboratory situation rather than the long-term behavior of the total global climate system. This is partly a result of the prejudice, among scientists and philosophers alike, to consider the mathematical-experimental sciences as the “real” ones, while the sciences of complex systems, defined by extent, duration, variety, or multiple scales, are denigrated and neglected. Sometimes this bias is effective, as with the triumphs of molecular biology. But even there it can be over-extended, as in the case of “genetic medicine,” which absorbed nearly a hundred billion dollars over several decades (Latham 2011), and achieved little beyond what was known at the beginning. In the case of climate-change science, the failure to appreciate the complexities of knowledge and uncertainty could well have ensured its futility. In *Scientific Knowledge and Its Social Problems* ([1971] 1996), I devoted a chapter to a discussion of “Immature and Ineffective Sciences.” I recall that it was very imperfect, but it did raise an issue that has been insufficiently discussed.

## Quality

The issue of quality is even more vexed. This includes far more than the evidential strength of particular items of information in relation to their policy implications. Of greater importance are the judgments of quality related to the scientific work. These extend over the scientific procedures themselves (as debated in connection with the circulation models and tree-ring analyses), through the probity of the scientists in their management of information and of criticism, and even extending to accusations of corruption of the whole enterprise. This latter point, made repeatedly by severe critics and implicating post-normal science, is the most troubling. If it were to be true, then one of the pillars of our secular, democratic society would be seriously, perhaps fatally weakened. We depend on science in so many ways that its loss

would have untold consequences for our civilization, particularly in times of societal instability and religious counter-attack. Further, as we know from examples in other areas of society, the epistemology of corruption is highly complex and fraught. Until such time as a particular case of systemic corruption is accepted by the broad public, accusations of particular instances are considered to be reprehensible or even criminal. Even if they are eventually proven correct, they are the most dangerous form of whistle-blowing for the activist.

The imperfections in the working ethics of scientific practice are well known; occasional malpractices in research, and its causes in competitive pressures within research communities, are familiar. But each such case could, up until now, be considered as a deviation from the accepted, “normal” good practice of science. There are some important exceptions to this rule, as in the case of biomedical research where profits, or even survival, of firms depends on favorable test results. There the manipulation of scientific practice, in all its phases and to all degrees, is recognized, and some remedies are applied. When that happens it can be understood in terms of the application of external commercial pressures to researchers; the weaker among them succumb to temptation.

The accusations in the case of climate science are therefore far more serious. At their most extreme they include conspiracy theories, about the use of some convenient environmental threat to establish a technocratic world government. But even in the absence of such a background, the charge remains that scientists of all ranks and degrees have allowed themselves to be suborned by a determined and well-placed group that exercised control over the whole agenda of science, including funding, publications, prestige, and rewards. To the long-standing and well-founded accusation that some prominent early critics had been generously funded by industrial interests, and that present skeptics range from serious scientists to cranks and conspiracy theorists, the recent reply is that the climate-science community has been riding on a huge gravy train estimated in the billions of dollars (Anonymous 2011). As I have indicated, even to mention such considerations, especially in a case that is so intensely politicized and where personal investments are so intense, is fraught. But regardless of their truth, they are politically effective in some milieus, and so it would be short-sighted to refuse to consider them simply on account of their unspeakable character.

The evidence in the Climategate story opens further questions, which will eventually need to be explored. It can be put this way: in





Kuhnian “normal science,” what barriers are there to malpractice and corruption? In his model of science, Kuhn made no normative judgments. But his description of normal science, as “the strenuous and devoted attempt to force nature into the conceptual boxes supplied by professional education” (Kuhn 1962: 5) makes it a cross between an intellectual property business and a cult, or perhaps an intellectual property business run by a cult. As to the classic ethical “norms” of science as enunciated by Popper and Merton, they are supremely irrelevant for Kuhn. When he states without an explanatory comment that scientists will attempt to “suppress” and “evade” troubling anomalies (Kuhn 1962: 5–6), he is consigning scientific ethics to the dustbin of history. Reading this, one asks whether those who blamed PNS for the supposed corruptions of research had the wrong targets. All the motivations are right there in Kuhn’s classic description. All this is background to a question, how “normal” in practice has this disillusioned vision of “normal” science become? In the Climategate emails, it is possible to explain away the term “the *Nature* trick,” but “to hide the decline” presents quite severe difficulties to the apologist. And then there is the scientist in Belfast, whose university assisted him in a prolonged struggle to keep his data secret from critics. When he was finally forced to release them, he was quite outraged, since after all he had collected them and they were his property to dispose of as he saw fit. Issues like the openness or reproducibility of his research, or his responsibilities as a recipient of public funds, were foreign to him (Keenan 2011).

The troubling thing about such examples is not so much the behavior as the consciousness. When people see nothing wrong in casually violating the publicly proclaimed norms of behavior of their field, the situation is really quite dangerous. To explain, I will quote the familiar: “hypocrisy is a tribute that vice pays to virtue.” But suppose that there is no hypocrisy? Then there is a situation where vice is not seen as vicious. For another parallel, there is Monk Tetzl. He really came to believe that the believer’s payment of hard cash for a certificate of indulgence (a promise of reduction of the time to be served in purgatory) was itself the meritorious act for which the indulgence was granted. If someone had told him that this was the classic sin of simony, he would have been bewildered; everyone, from popes on down, was doing it; how could it be wrong? Martin Luther thought otherwise, and the rest was history. I once imagined a situation of “Monk Tetzl Science,” where testing agencies would publicly advertise their ability to produce any result the client desired. This would

be a step down from a situation where the client would approach testing agencies that were known to produce results with a predictable bias. In my imagined example, even hypocrisy had been lost.

It was to be expected that the various official inquiries into the Climategate scientists would conclude that they are all honorable men. That conclusion led to disquiet about the inquiries themselves, as expressed in the *New Scientist* editorial (Anonymous 2010) that in the absence of candor by the authorities they would not regain trust. So we are left with the question, how “normal” is this a-moral Kuhnian “normal science,” which some of the climate scientists seem to have been practicing? Such a question cannot be answered; it is constrained by the epistemological paradoxes of corruption. Even to suggest that the scientific community is tainted by widespread malpractice of this sort, is to utter the unspeakable. In a very encouraging response to this dangerous situation, even mentioning the UEA (University of East Anglia) emails and public trust in its announcement, the Royal Society has launched an inquiry (11 May 2011) into openness in science.

## **The Politics of Science in the Post-Normal Age**

The interaction of technologies with social and political life is well known. In the field of communication, the most famous example is the way that the invention of printing with cheap movable type was an enabling force in the Reformation. Access to the text of the Bible, and to subversive pamphlet literature, was suddenly broadened. It became practical to imagine every man being his own priest, as Martin Luther did with such momentous effect. Now we have the Internet, Wiki, and the social media. Various authoritarian regimes, from the Roman Catholic Church to Muslim autocracies, no longer have the protection of the impotence of the masses through their ignorance. One might think that such developments are irrelevant to science, which is after all governed by the Mertonian norms of openness and criticism. Leaving aside Kuhn’s picture of “normal science,” we can accept that the isolated pursuit of truth might well enjoy such immunity, but as soon as knowledge is involved with power—either internal or external to science—it becomes complex and vulnerable to being compromised and even corrupted. And in that new situation, science—like any other social institution—especially one of such size and importance, needs its critics and whistle-blowers.



It could be said that the implicit message of PNS was an advocacy of just such activism. When the doctrine was created in the later 1980s, this sort of situation was quite unimaginable to almost everyone connected with science. Hence the explicit message of PNS needed to be rather anodyne, politically, lest it be shunned by all. So the focus was on the positive contribution that the extended peer community could make to problem solving when uncertainties or decision stakes were so severe as to render even professional consultancy inadequate. Even the term “applied science,” a rendering of “normal science” for the problem situation under study, dealt only with the assumptions of manageability of the qualitative aspects of the knowledge, and not with the mind-set of the scientists. But I would assert that, given all its limitations and conceptual weaknesses PNS is the only vehicle for conveying the message that criticism is more important than ever to the life of science, as the pressures toward conformity are increasing all the time.

It is therefore a very dangerous, and perhaps ultimately tragic, situation for science that in the course of the Climategate fracas, the official scientific community has increasingly adopted a bunker mentality. The most significant manifestation of this was a lecture delivered by Sir John Beddington, in which he recommended that scientists should become “grossly intolerant” of those who abuse scientific methods in their partisan political attacks, comparing them to racialsists and homophobes (Dwyer and Hood 2011; for Beddington’s explanation, see Curry 2011b). He did not specify individuals or issues, but it is clear that in addition to the creationists, he had in mind those who abuse the precautionary principle. Of course, when challenged he affirmed that he has no wish to stifle genuine criticism in science. However, his original words remain on the record, a warning from the chief scientist to critics whom he deems to be illegitimate.

If that official British scientific establishment were still enjoying its traditional authority, then its rulings on the legitimacy of criticism would be more easily accepted. But, as we have seen from the 2011 *Nature* editorial, its assessment of climate science is now being challenged directly. Either the science is “settled” sufficiently for policy purposes, or it is still open, perhaps in the traditional way, or perhaps even post-normal. The debate is clearly very far from over. Since so much has been invested in CAGW, including all the institutions for what I have called “the war on carbon,” it will not be at all easy for the establishment to achieve a graceful and relatively harmless retreat. It is impossible for me to predict the course of the coming debates;

one can only hope that wiser counsels will prevail, and that the defeat will not become a catastrophe for science.

Since the climate issue has become so politicized, the tragedy of Climategate extends beyond the scientists to those other public figures who have become invested in it. The situation has become particularly severe in the United States, where denial of climate change is part of a syndrome including the denial of evolution and modern economics. The battle lines of the culture-war there are between “coastal” and “middle” America, and its outcome can be fateful for the country and the rest of the world. I understand this split as a manifestation of the incompleteness of the process of modernization of American life.<sup>1</sup> This incompleteness has been masked by the success of the American empire up until now. But the hegemony (economic and cultural) of the sophisticated coastal elite has never been total and it was always resented and resisted by the descendants of lower-class immigrants in the regions where those elites were thin on the ground. Those of the “middle”—with all their internal divisions, of course—feel excluded and denigrated by the elites. Their intellectual weapons are always inferior, by the criteria of the elites; their chosen sciences are more in the nature of “folk-sciences,” giving security and guidance, rather than abstracted positive disciplines. Of course, from the perspective of the elites they are reactionary, even irrational; but their distress is real, however confused and impotent. And it could be said that the “coastal” elite financiers who caused the collapse were no less fantasized in their faith in science than their “middle” backwoods opponents are in their faiths based in religion (Ravetz 2008).

Internal critics of a dominant ideology always run the risk that their arguments will be picked up by external enemies. This was the fate of the “constructivist” sociologists of science, who unwittingly gave comfort to the manufacturers of doubt. Now the internal critics of mainstream science find themselves in the same conflicted situation. Although believers in science would deny the reality of the parallel, it has happened repeatedly when the ideological realm was governed by religion and theology. The humanist reformers of the Church, as Erasmus, were outflanked by the crude Martin Luther. He in turn saw his populist message picked up by the radicals of the Peasants’ Revolt, and so he needed to side with the rulers in advocating their brutal repression. As we have seen, PNS has been adopted as a weapon in these new culture wars; there is little that can be done about it, except to know that this is the way of the world.



## Some Personal History

In the climate change issue, it has been impossible to separate the science from the politics; this is, after all, what PNS is all about. And since the issue quickly became a moralistic campaign, one might even say crusade, it has become quite difficult to say where the politics stopped and the science began. My colleague Angela Wilkinson, who had an early experience of the unit at East Anglia, has described it as “evangelical science.” Long ago, when describing the social conditions of industrialized science, I wrote that the average scientist embarking on his career has no more idea of the moral issues he will face than an Irish girl arriving at Euston Station in London. It could be that many things that went wrong with climate science can be ascribed to innocence, as well-intentioned scientists were led into situations where they were out of their depth in their understanding of what were the stakes and also of their place in the debate.

Two bits of wisdom seem to be lacking from the worldview of the “normal” scientist, one epistemological and the other ethical. The first is the “law of unintended consequences,” otherwise known as Murphy’s law, also known in military science as the principle that in battle the first casualty is the plan. To some extent engineers learn about things going wrong, but for scientists it is an alien concept. They see no reason to believe, or even to imagine, that a science based on quantitative data and mathematical models can produce answers that are out of contact with reality, or just plain wrong. We have seen how this simple faith, taken over undiluted from the prophetic visions of Descartes and Galileo, led to the catastrophe on Wall Street. Its effects are less obvious in the case of the climate models, as the retreat from naive triumphalism has been protracted and messy in every way.

The other bit of wisdom has various titles, including “the corruption of the good,” or “the road to hell is paved with good intentions.” Again, to the extent that a “normal” scientist is involved in debates, based on his puzzle-solving experience he will naturally assume that all the good people are on the right side and all the bad people are on the wrong side. So, if we are dedicated to saving the planet, and those others are criticizing our science, then they must be denialists, and thereby equivalent (as for Sir John Beddington) to homophobes or racialists (Dwyer and Hood 2011). For their part, the skeptics see either foul play or conspiracies, and so demonize the insiders equally stridently.

Faced with such a polarization, I have tried to do my bit with “reconciliation” and non-violence. But these well-intentioned ideas could also benefit from some theory. I will offer this now, sketching some ideas about “disillusion” and “investment.” This train of thought was triggered by an exchange I had at the public meeting following the Lisbon workshop. Someone in the audience made a passionate speech about the perils of the planet and how we skeptics were betraying the struggle. In response I told her how I had grown up as a child believing that all the terrible problems of poverty, depression, war, and racism had been solved in “The Socialist Sixth of the World.” Then in adolescence I began to see things otherwise, and went through a sometimes painful retreat. Reflecting in public, I wondered whether Al Gore will be seen to have done as much harm to environmentalism as Joe Stalin did to Socialism.

That exchange reminded me of other idealists whom I knew, who had not got off so lightly as myself. They had dedicated their “lives, fortunes, and sacred honor” to that cause, and then went through a prolonged agony, sometimes lasting for decades, as it became corrupted and eventually fell apart. Of course, they were not the first; how many had suffered in their souls as the bright dawn of the French Revolution became clouded over with the rapid succession of the Reign of Terror, counter-terror, military dictatorship, empire, and finally the ultimate betrayal of the Hundred Days. What, then, was that all about?

Such reflections enabled me to see clearly how important it is, when attempting reconciliation, to appreciate and respect “investment.” I have known and loved some who had to cope with their disillusion with Stalin. I watched them lacerate themselves with questions like, “How could I have been so stupid? I, who prided myself on being a critical, independent thinker as well as an idealist! Do I lack common sense? Can I trust myself in future? Do I have the right to pronounce on anything, after this?” The threat to the self as worthy of respect can be severe, perhaps overwhelming. Small wonder that all sorts of defensive maneuvers will be attempted in the course of such debates. My own rule is that when someone is shouting really loudly at me, perhaps he is actually shouting back at the still small voice within.

With such an appreciation, the non-violent approach to debate gains strength as the most practical and effective. To help someone reconcile those inner conflicts, in their own way at their own pace, and to arrive at their own new synthesis, is the only way forward. This



is why the literature of disillusion can be so important; one sees that it is really all right to have believed, provided that eventually one has the courage and integrity to face up to painful truths. For climate change science, the most powerful example I know is that by Lucy Skywalker (2011). She started off as a conventional environmentalist, then was inspired by Al Gore's messages, but soon began to suspect, and finally produced an excellent critical analysis of the received doctrines.

My own journey may be worth recounting here, if for nothing else than the record. I was aware of the issue of carbon-based global warming, but to me it seemed all too similar to earlier simplistic alarms, like the "population bomb." I was unimpressed by Al Gore, and scarcely noticed his campaign. I was aware that some scholars, including Tuomo Saloranta and Dennis Bray, had mentioned the issue in connection with PNS, but I never developed the connection in my own thinking. Then the skeptics' TV program on Channel 4 (<http://www.channel4.com>) reinforced my distance from the whole issue. I could see that their argument was weak, and shortly thereafter its science was torn apart. As Greenpeace said, if that's the best they can do, we don't have any worries. I did hear a convincing account of the run-around that Steve McIntyre was getting, but for me that was just another one of the stories of suppression in science which, though quite possibly true and scandalous, are as yet too scattered to be worth following up except as a major personal investment.

Then it all changed. I still remember seeing the article at the bottom of the first page of the *International Herald Tribune*, following it through to the long story on an inside page, and thinking, "Here's a social problem of scientific knowledge—I can't let it go by." Very shortly afterward Mike Hulme was in Oxford, and we had a long talk followed by a joint piece. At that point we agreed that it was mainly an issue of openness and courtesy in scientific debate. But very quickly I was in the thick of it, discovering Roger "tallbloke" Tattersall—and posting on [wattsupwiththat.com](http://wattsupwiththat.com).

Of course I couldn't keep up with it all as that would have been more than a full-time job. But I exercised my principles of assessment, coming from the philosopher Imre Lakatos and the historian Hugh Trevor-Roper (later Lord Dacre). Lakatos distinguished between "progressive" and "degenerative" research program, based on how they handled challenges and novelties. Trevor-Roper looked at historical arguments, and said that if, every time you press on the structure your thumb goes through, you are justified in withholding assent. (He had

applied this to the “lone crazed killer” theory of the Kennedy assassination.) In retrospect, I realized that I erred in not appreciating the feelings of persecution of the climate scientists from earlier years, when the fossil fuel interests were really fighting dirty and, as their opponents saw it, relying on manufactured doubt in the time-honored fashion.

It could be that the process of creative disillusion is even more difficult in science than in politics, for those trained in science are given no intimation of the possibility of honest error. After all, if every problem has just one correct solution, which is attainable by any person endowed with sufficient intelligence, then error must be the product of some defect, intellectual or moral. For many generations the history of science told simple morality tales of how superior men achieved truth over the opposition of the stupid or recalcitrant. Thomas Kuhn sensed this in the savage first sentence of his *Scientific Revolutions*: “History, if viewed as a repository for more than anecdote or chronology, could produce a decisive transformation in the image of science by which we are now possessed” (Kuhn 1962: 1). Notice the counterfactual “if ... could,” and notice also the final “possessed.” He later compares the image of science purveyed by textbooks to “an image of a national culture drawn from a tourist brochure or a language text,” and claims that “we have been misled by them in fundamental ways” (ibid.). Kuhn never analyzed how that false consciousness of scientists damages the conduct of science itself, but the repeated reference to “arbitrary” gives a clue to another ironic insight. Although the discipline of the history of science is far more sophisticated now, a half-century on, I have detected no awareness of how the history of scientific errors could illuminate and enrich our understanding of the scientific process. Such a reformed history could also help in the appreciation of scientific dissent and its resolution.

For a concluding speculation, it is possible that Climategate might represent the maturing of science as a culture. So long as one believes that good intentions bring good outcomes, or at the very least justify bad outcomes, then one is still not equipped to cope with the world as it is. Of course, one may nonetheless become famous and powerful, and achieve great things. But the immaturity is still there at the core. The self-awareness of science in knowing that it had touched evil seems to have started with poison gas in World War I, but that was managed and soon expunged. Oppenheimer’s perverted Hindu mysticism at the sight of nuclear weapons explosions made a big difference; but even then that was during a desperate war. The problem





of evil within science could not be handled until a new generation arrived.

Over recent decades, as science has become bigger, more powerful, and ever more closely tied in with industry and the state, the innocence of the pre-war years seems quite antique. But only with Climategate has the possibility been appreciated that scientists could, with the best of intentions, be sucked into a political morass. It could be called “evangelical science” or—following Pielke (2007)—“stealth advocacy” (where the concealment is partly from the advocate himself). It is not that activist science is necessarily bad or dangerous; but that it presents pitfalls for the unwary and innocent. I dealt with this danger in my book ([1971] 1996), in connection with Ibsen’s play *Enemy of the People* and Arthur Miller’s politically correct revision. Steve Schneider was aware of all this, but was misunderstood on all sides, and eventually he paid the heaviest price. Like any other cause, policy-related science takes on a life of its own, and utilizes its recruits in ways that they might not have anticipated or even imagined. Even with the awareness of PNS the hazards are there; how much more dangerous for those who are still “normal”? In these ways, PNS can assist in the maturing of science in the light of the Climategate experience.




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Jerome Ravetz is currently associated with the Institute for Science, Innovation, and Society at Oxford University, UK. His main work is still on the management of uncertainty in science-related policy. He is developing a software app, “Decision-portraits,” using graphical and crowdsourcing methods to improve practice in that area. Address: 48, St Bernard’s Road, Oxford OX2 6EH, UK, E-mail: jerome.ravetz@gmail.com, www.jerryravetz.co.uk.

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## Note

1. Original source of this insight was an undergraduate essay from Daria Tanner (née Fedyk), Leeds University.

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