ISSA Proceedings 2006 – Scientific Demarcation And Metascience: The National Academy Of Sciences On The Greenhouse Effect And Neo-Darwinism

Scholars who have followed up on Thomas Gieryn's work (1983) on scientific boundary – work have often seen rhetorical behavior of this kind as an informal alternative to the kind of demarcation undertaken by philosophers of science. The functionality of informal demarcation was fleshed out in Charles Alan Taylor's (1996) application of this model to various controversies in American science. Like Gieryn, Taylor regards boundary – work as a positive alternative to formal philosophizing on the nature of science. I do agree that the articulation of such dividing lines as arise from institutional challenges to science may achieve practical resolutions to problems that philosophers of science have never been able to resolve, but this exclusive focus overlooks some of the complexities arising from demarcation of this kind.
Certainly it is as important for scientists as it is for philosophers to develop what I will here call "metascience," answers to the question: what is science? And so the informal argumentative work that achieves this may be as vital as Gieryn and Taylor suggest – especially if it succeeds where more academic exercises of scientific demarcation do not. But in this essay I will consider the complicating fact that the motives that inspire boundary-work are not strictly regulated by intellectual concerns. Because of this informal demarcation could easily misfire, causing scientists to define their own intellectual labors in ways that could weaken or perhaps even undermine public deliberations that bear upon scientific questions.

This problem is suggested by Gieryn's own analysis of the three ideological pressures that inspire boundary-work (pp. 785-791):
(1) outside encroachments upon science such as might come from religious

interests,
(2) challenges to the ethicality of science, and
(3) the need to protect scientific patronage by excluding pseudo-science.

Of course these efforts may have something to do with science as practice, but more often they have to do with the secondary concerns of science as an institutional body. This is shown in one of the cases that interested Gieryn, the informal demarcation undertaken in the energetic public campaign for science that was advanced in Victorian England by such figures as Thomas Huxley, Herbert Spencer, and John Tyndall. Focusing specifically on Tyndall, Gieryn (pp. 785-786) observed that the Irish physicist constructed these boundaries differently when he was working two different fronts of this campaign. The emerging scientific professions at this time felt threatened by the deeply entrenched power of the Anglican Church, which continued even in the face of science's rising fortunes to wield considerable influence over faculty positions and curricular decision-making in English universities. But on another front (pp. 786-787) scientists like Tyndall were also wary of the growing power of the technical professions, since these competed with science for patronage and for a hold on the public imagination.

Gieryn observes that Tyndall demarcated science differently on each of these fronts. To show science's epistemic superiority over technology, the physicist highlighted its purely theoretical powers, but to show its superiority to theology he was disposed to play up its concrete character and applicability. Science was superior to theology because it solved real problems, but it was superior to engineering precisely because it did not. While the pragmatic reasons why this influential scientist would have taken these contradictory stances are evident, Gieryn does not consider the rhetorical costs that demarcation of this kind might have accrued. In fact he does not regard this inconsistency as a problem at all. Tyndall, Gieryn tells us, was not "disingenuous" when he described science differently in various contexts. "It would be reductionistic, "he insists, "to explain these inconsistent parts of a professional ideology merely as fictions conjured up to serve scientists' interests" (p. 787). This was a "genuine ambivalence" reflecting "an unyielding tension between basic and applied research, and between the empirical and theoretical aspects of inquiry" (p. 787). Of course Gieryn is right about this, but this explanation overlooks the obvious fact that Tyndall communicated these half truths with the intention of deceiving his listeners by masking this very ambivalence. Had the physicist explained this as forthrightly as Gieryn does, he would not have been able to achieve these boundary-work effects, for to acknowledge that science is both theoretical and applied, would be to admit that it cannot be utterly demarcated either from theology or engineering. In wanting to forgive Tyndall's equivocation, in other words, Gieryn seems to suggest that it is okay to mislead the public, provided that one remains true to science.

While this work of informal demarcation may have helped to achieve the institutional goals that were at issue in Victorian positivism, there is some danger that demarcation of this kind, were it to really succeed, could interfere with scientific inquiry. The same positivist demarcation that enforced a separation between science and theology by insisting that science is based in fact and theology in mere speculation, has sometimes blinded scientists, for instance, by making them unable to recognize that their own thinking also has an important speculative aspect. A famous instance hinting of such a barrier was the general reluctance of physicists to embrace big bang cosmology in the last century. Having convinced themselves that scientific thinking was not governed by speculative concerns, they were disinclined to recognize that it had been their naturalistic predispositions that made them cling to the steady state view. Without this kind of critical reflexivity, scientists did not recognize the implications of the expanding universe suggested by Albert Einstein's general theory of relativity and Edwin Hubble's discovery of a pervasive red-shift (Farrell, 2005, pp.73-120).

It is perhaps revealing that the scientist who ultimately did recognize the larger implications of general relativity and red shift, the Belgian physicist Georges Lemaître, also happened to be a Catholic priest. It was undoubtedly the theological perspective that he brought to his science that exempted him from the positivistic preconceptions that had prevented such eminent contemporaries as Eddington, Hoyle and Einstein from seeing this solution (Jastrow, 1978). Although Lemaître had deduced his theory of the "primeval atom" from general relativity, even the typically fair-minded Einstein had initially ridiculed his proposal and suggested, as did Eddington, that the priest's judgment was clouded by his religious convictions (Farrell, p. 100).

My point here is not to say that theology actively assisted scientific discovery in this instance – omething Lemaître certainly would have denied (Farrell, pp.

192-198). Religious metaphysics, even within a relatively homogeneous faith such as Catholicism, are quire diverse, and they could just as easily be a deterrent. My point is only that, contrary to what Tyndall and countless of his successors have argued, speculative thinking such as is found in theology also figures in science. Both fields are concerned, for better or for worse, with basic metaphysical questions – in this particular instance the age-old question of whether the universe is eternal or temporal.

My concern here is with another side of this problem, the extent to which the positivism sustained by such boundary-work may interfere with scientists' responsibilities as public actors. In exploring this suggestion I would like to show how some of the boundary-work occurring in scientific responses to religious anti-evolutionism may affect public thinking about another controversial subject, the environmental effects of greenhouse gas emissions. My argument will be that the boundaries set up by the first debate are potentially deleterious to the scientific interests at stake in the second one. To put this simply, in the boundary-work transpiring in official efforts to combat religious anti-evolutionism, experts appeal to the traditional positivist topos of certainty. They affirm the verifiability of scientific claims as a rationale for dismissing what they regard as unwarranted skepticism. But in the debate over greenhouse emissions, as in many areas within evolutionary science as well, such an affirmation is not possible. Greenhouse theory makes considerable conceptual sense as an explanation for global warming, but if held up to the rigid standards of epistemic certification proposed to demarcate science in debates about evolution and religion it will fail.

If the demarcation achieved by contrasting science against religion persists in public thinking about global warming, it should not surprise us that many policy makers regard the greenhouse gas theory as an insufficient warrant for the decisive regulation of CO2 emissions. This danger arises from a rhetorical feature of public science that Gieryn did not consider. His analysis seems to assume that the rhetorical effects arising from informal demarcation are contained within their immediate rhetorical situations. When Thomas Huxley championed the applicability of science in the popular "working men's" lectures he gave to London's cloth caps, Gieryn seems to suppose that he did not need to worry that Parliament would take these messages to heart and cut off funding for theoretical research that seemed to lack this promise. But is this a safe assumption? Are the situated acts of public demarcation truly situated, or do they have a more general effect?

My reason for supposing that certain definitions of science may be generalized for all contexts comes from what Chaim Perelman (1982, pp. 35-36) called effective presence. This is the recognition that arguments designed to achieve immediate persuasive goals may also have presence in other contexts for which they were not intended. Thus while the boundary-work that is executed to demarcate science from theology may be intended for the pragmatic work of silencing religious criticism by affirming scientific certainty, this constitutive effect may also come into play in other situations where a scientific standard based on probability would better serve the public interest.

In consideration of this interpretation, I will examine how the constitutive effects of boundary-work arising from one scientific publication intended for broad distribution might affect public judgment of other scientific messages that demand greater discernment. This publication is a small book issued by the National Academy of Sciences entitled "Teaching about Evolution and the Nature of Science" (1998). It was explicitly designed to influence how educators throughout the United States teach evolutionary biology. The main purpose for its publication (p. viii) was to remedy the fact that many American "students receive little or no exposure to the most important concept in modern biology, a concept essential to understanding key aspects of all living things – biological evolution." But since the authors attribute this deficit to religious belief, they actively undertake boundary-work as a pedagogical measure that may help to counteract its influence. Two factors are likely to give the arguments advanced in this book effective presence in other contexts. First, the NAS which has sponsored it is the most elite scientific association in the U.S., and thus the voice of scientific opinion leadership in this country. Second, as a publication specifically designed to guide educators in secondary schools, it is likely to reflect how most Americans come to understand the nature of science.
The last part of this analysis will consider what would result if the understanding of science developed in the first publication had effective presence for those reading a second NAS publication on global warming. This report, "Climate Change Science: An Analysis of Some Key Questions" (2001), was commissioned by the Clinton administration to brief policy makers on current scientific opinion in this area. Unlike the evolution publication, this report is not concerned with boundary-work. Its authors seem to assume that the constitutive features of scientific knowledge are uncontested for their readers. But what if the scientific judgment of these readers had been shaped by the sort of metascientific

discourse we find in the evolution book? How would this equip them to interpret the current state of climate science? I believe that public understandings of science are shaped by the kind of scientific demarcation at work in the evolution book and that metascience of this kind will be effectively present for those reading the publication on global warming. Since public discourses on global warming occur in a metascientific vacuum, salient understandings of science originating elsewhere, such as in the science classrooms for which the NAS publication on evolution is intended, will move in to fill this conceptual gap.

1. *The NAS and the Nature of Science*

In the preface to the evolution publication, the authors (a committee of thirteen scientists) indicate that demarcation is one of their chief purposes and that it occurs here as an effort to combat religious skepticism. They acknowledge that "most religious communities do not hold that the concept of evolution is at odds with their descriptions of creation and human origins" (NAS, 1998, pp. viii-ix), but they then go on to add that because religious faith and scientific knowledge are "different," this publication "is designed to help ensure that students receive an education in the sciences that reflects this distinction." The writers reiterate their intention of demarcating these two realms a few pages later (p. 4) by adding that because "some people see evolution as conflicting with widely help beliefs, the teaching of evolution offers educators a superb opportunity to illuminate the nature of science and to differentiate science from other forms of human endeavor."

It is in the context of this discussion that the authors treat what they regard as an attendant subject, the religious skepticism that is expressed in the popular notion that a theory such as Darwin's is merely a "guess or hunch." The authors counter this by insisting that in science theory "refers to an overarching explanation that has been well substantiated."

Science has many other powerful theories besides evolution. Cell theory says that all living things are composed of cells. The heliocentric theory says that the earth revolves around the sun rather than vice versa. Such concepts are supported by such abundant observational and experimental evidence that they are no longer questioned in science (p. 4).

In an effort to help teachers wishing to combat religious skepticism about evolution, it makes *prima facie* rhetorical sense to assert that it is certitude that sets scientific theories apart from other categories of speculation. Once it is supposed that scientific theories are constructs that have been so thoroughly substantiated as to be "no longer questioned," resistance of this kind would seem silly or irrational at best. But this rhetorical achievement comes at the price of historical and metascientific infidelity. Even a moment's reflection will show that demarcation based on certitude excludes all manner of theoretical constructs that practitioners now regard or once regarded as scientific. First, it excludes those theories that are seriously discussed and researched by scientists but which remain controversial and often speculative – such as the Gould-Eldredge theory of punctuated equilibrium, theories of abiogenesis, or the theory that birds evolved from dinosaurs. Second, this definition would exclude even the well substantiated theories mentioned here, if we were to consider their scientific status at some earlier point of development. Scientific theories are never "well substantiated" positions in their inception, and achieve such standing typically only after decades or centuries of study. Cell theory and heliocentrism once *were* more like hunches or guesses, and only found extensive support after a long and arduous examination. Were we to take the above definition at face value it would mean that they only *became* "scientific" when they had reached an advanced level of maturity. String theory by this standard would be excluded, even though it is currently at the forefront of theoretical physics, and so would Ludwig Boltzmann's pioneering work on atomic theory, at least during his life time when it was generally dismissed. Third, this description fails to recognize that even theories supported by an abundance of evidence may subsequently fail. A theory can be compelling in its power to "save the phenomena" and still turn out to be wrong once additional data is taken into consideration. In every instance theories of this kind, (e.g. geocentrism, ether theory, phlogiston theory, and steady state cosmology), could at one time have been said to be "no longer questioned."

A characterization of scientific theory as unrealistic as this would be difficult to sustain without selectively omitting or distorting vital elements of scientific history. This perhaps explains why this book's effort to illustrate how theories achieve this certainty, its discussion of the Copernican revolution in a chapter called "Evolution and the Nature of Science," relies on a traditional or "folk" narrative that shapes this historical episode to fit prearranged didactic purposes (Lessl, 1999). Desiring to certify that scientific theories are cognitive frameworks that are "no longer questioned," the authors fail to mention that the Copernican view was more hotly contested by the scientific community than by religionists (Santillana, 1955, pp. 197-238; Finocchiaro, 1980, pp. 10-15).

Wanting to make straight the path that leads from heliocentrism's modern inception in Copernicus' mind to its supposed certification by Galileo, and to depict this road as one paved entirely with fact, they give no role to the kind of intellectual discord that Thomas Kuhn's (1962) recognized as an inevitable attendant of scientific revolutions. Instead it was merely an accumulation of data that "complicated the hypotheses" formerly used to account for planetary movements," that led "astronomers of the 16th and 17th centuries" to make "even more precise observations of the movements of the heavenly bodies" (NAS, 1998, p. 29).

Astronomers used these measurements to demonstrate that the age-old human explanations of the heavens were incomplete. In the process they replaced a complex and confusing explanation with a simple one: the sun, rather than the earth, is at the center of a "solar system," and the earth revolves around it. That simple step – a bold departure from past thinking due mainly to the insights of Copernicus (1473-1543) – dramatically changed the picture of the then known universe.

This dramatization of how theories develop might be called "Baconian." It is not entirely incorrect, but in fancying that this revolution advanced by simple steps of measurement it draws attention away from the hard thinking and vigorous debate that was crucial to this advancement. The result is a picture of this revolution quite unlike what has been given by such philosophers and historians of science as Koyré (1978), Finocchiaro (1980) and Pera (1994). For Koyré, Galileo's contribution to this revolution came from daring rationalism, a kind of applied Platonism, not dogged empiricism. The Italian astronomer's great innovation was to construct through thought experiments, abstract mathematical idealizations of physical laws and then to demonstrate how they could be accounted for by the phenomena.

The empiricist conception of science that the NAS authors project onto this episode is, ironically enough, more similar to the Aristotelian view of science that Galileo was trying to reform. The Platonic corrective to scholasticism that Koyré discerned in Galileo's philosophy of science was needed to overcome the limits of commonsense empiricism that sustained the Ptolemaic view. But this battle of scientific philosophies has no place in the NAS account. To recognize that the Copernican revolution was the outcome of competition between two grand metascientific perspectives would be to acknowledge a speculative and subjective side to science that would undermine their narrative's powers of demarcation.

Wanting to keep speculation and subjectivity out of science, so as not to give any foothold to religious objections to evolution, the NAS authors are not interested in such complexities.

The NAS authors would have needed to acknowledge a similar subjectivity had they mentioned anywhere in this account that the struggle leading toward the triumph of the Copernican view pitted scientists against scientists. Indeed, the uninformed reader of this account would scarcely understand there even was a scientific alternative to what Copernicus proposed – so thoroughly have the authors depicted Ptolemaic cosmology as a theological position. There are only two vague references to the geocentric model. The authors mention "ancient observers" of the heavens and the "theories of the cosmos then prevailing" (NAS, 1998, p. 29), but we hear nothing about Aristotle and Ptolemy or the complex architecture of scholastic philosophy in which the old cosmology was embedded. As they approach the denouement of their story the reason for this becomes evident. They have wished to construct this episode of scientific history as a debate between religion and science rather than a contest of scientific paradigms.

As a result of the steady accumulation of evidence, the theological interpretation of celestial movements gave way to the naturalistic explanation, and it is now accepted that night and day are the consequences of the rotation of the earth on its axis. Today, we can see for ourselves the rotation of the earth from satellites orbiting the planet (p. 29).

An obvious advantage of depicting the triumph the Copernican view as a victory over "theological interpretation," is that this episode can then serve as a warning for religionists who would challenge other naturalistic explanations such as evolution. But it also decisively demarcates science from religion. Science is ultimately about what "we can see for ourselves," and religious explanations, since they have not this basis, cannot stand up for long.

Like the definition of theory advanced by the NAS, this characterization of scientific revolutions plays down the rationalistic side of science for the sake of its empirical side. This may explain Isaac Newton's near invisibility in this discussion. In a summation of the Copernican revolution that runs for twelve paragraphs, (1070 words), the contributions of its most important theorist are summed up in a single sentence. The authors follow their treatment of Galileo by saying that "[c]ontinued study and ever more careful measurements of the movements of the planets and sun continued to support the heliocentric hypothesis."

Then, in the latter half of the 17th century, Isaac Newton (1642-1727) showed that the force of gravity – as measured on earth – could account for the movements of the planets given the laws of motion that Newton derived (NAS, 1998, p. 29).

Having invoked the notion of measurement in both of these sentences as the driving force leading to heliocentrism's victory, the writers continue to sustain the Baconian notion that science is entirely an observation-driven enterprise. Even those Newtonian contributions that were unmistakably idea-driven are nuanced to sound like products of observation alone. We are told only that Newton "measured" the force of gravity on earth but nothing about where the idea of gravity came from, and when the authors say that Newton's laws were "derived," they give no hint of the source of their derivation. The naïve reader is left to suppose that the measurements mentioned in the first part of the sentence were their source, but this was clearly not the case (Kuhn, p. 78).

An explanation of the Copernican revolution that centered on the generative powers of the scientific mind might have been attractive to the NAS in a different rhetorical situation, but the goal here is to make theoretical constructs indubitable. To focus on the rationalistic side of science, no matter how powerful or vital it may have been, would draw attention to the vulnerability of Newton's work to correction by relativity and quantum theories. If classical mechanics could be corrected in such a major way as this, so also might neo-Darwinism. The authors of the NAS book do acknowledge that scientific theories are subject to such change, but it is the half-full glass of scientific certitude that contributes the most to their immediate rhetorical purposes. Skepticism about the neo-Darwinian paradigm might grow even larger if the American public was taught that theoretical constructs, no matter how powerful, always retain a precarious subjectivity as abstract mental representations of physical realities. A simplistic Baconian model which views them as springing up spontaneously from data is preferred, in spite of its clear inability to genuinely "save the phenomena" of scientific history.

The interpretation of the Copernican revolution given by both Finocchiaro (1980) and Pera (1994) and based on their close readings of Galileo's *Dialogue Concerning the Two World Systems*, would do even more damage to the NAS narrative. Although they assign less weight than Koyré to the influence of Renaissance neo-Platonism upon thinkers like Galileo and Newton, both agree that the Copernican theory did not win out on the basis of an inductive proof.

Galileo surpassed his scholastic competitors not by showing that the evidence pointed irrefutably to a sun-centered cosmology but only by marshaling better arguments. But even then, the case was not compelling. Galileo's case for heliocentrism, Pera shows (pp. 2-28), did not derive exclusively from something like "scientific method." It was an argument that marshaled all the available means of persuasion, hard evidence as well as soft speculation. Even the experimental tests described by Galileo served as illustrations rather than demonstrations. They were thought models designed to clarify mechanistic principles rather than to prove physical laws. Galileo himself (Pera, p. 28) rejected the notion that any *experimentum crucis* should be allowed to settle the debate.

2. *Climate Change Science in a Metascientific Vacuum*
I have chosen this treatment of evolution because it was specifically designed to influence how the nature of science is depicted in U.S. classrooms, and in the process to combat widespread doubts about evolutionary science. Because schools are the main source of public information about the nature of science we may also assume that the metascientific thinking of both the American citizens and the policy makers who represent them is born here. Outside the educational contexts for which the arguments of the NAS publication were intended, the scientific culture has few other opportunities to construct metascience – general conceptions of the nature of science such as are produced in this book. Even in the basic science education that most Americans get, very little discourse of this kind will be found. Metascience is typically only the stuff of the introductory sections of the introductory text books used in introductory courses. Apart from such cursory treatments, there is little opportunity for nonscientists to think about what science is in the abstract.

The brief analysis undertaken here would also suggest that these occasional moments of work on the nature of science are likely to be limited, if not distorted, by the salient issues of demarcation that inform them. The NAS initiative looked at here was specifically undertaken to combat the perceived threat of creationism, and so it is concerned with persuasive outcomes that do not seem to inspire a realistic portrayal of scientific practices.
But what happens when the public is involved with scientific controversies that more rigorous understanding of the nature of science? The answer I will propose here is that because such debates frequently occur in a metascientific vacuum,

public actors will draw notions of science into this emptiness that they have appropriated from elsewhere. In such rhetorical situations metascientific work such as we have seen in the NAS book on evolution will have effective presence, even though it gives an unrealistic picture of scientific controversy. Because these conceptual resources are unreliable, they could easily undermine meaningful deliberation.

In the final pages of this analysis, I will consider one such vacuum found at the center of scientific rhetoric endeavoring to shape public opinion on global warming. The second NAS publication introduced earlier, "Climate Change Science: An Analysis of Some Key Questions" (2001), is one such message. Since its readers are not provided with any criteria for assessing the scientific status of the climate theories it discusses, they are left to bring to their judgment of this discourse whatever metascientific criteria they will have absorbed from other messages. In this regard my interest in this message has as much to do with what it does not say as with its material arguments. Specifically I wish to consider the degree of persuasive force this message might have for readers who operate upon the notion that theoretical certitude arises spontaneously from the accumulation of empirical data.

Were readers to take seriously the notions of scientific theory that are found in the NAS book on evolution, they would be justified in disregarding the epistemic merits of the climate theories summarized in the second publication. The evolution book presents scientific theories as constructs made compelling by an accumulation of data that, once available, leaves no room for doubt. But the greenhouse gas theory described in the climate change publication does not appear to have this quality. The climate science publication, as a briefing prepared for policy makers in the executive branch of the U.S. government, is a study in epistemic modesty. It is easy to see why this would be the case. The authors are in some sense writing for their employers, the government that is the main source of scientific funding in the U.S. Reputations and public support are at stake, and so professional caution is in order.

This prudent tone is set in the book's foreword by NAS president Bruce Alberts, who (p. viii) seems to go out of his way to emphasize the tentative character of its findings. He opens by acknowledging several limits of the report, that "tradeoffs were made in order to accommodate the rapid schedule," that various "references to the scientific literature" are not provided," and that "detailed evidence" was not offered for the answers it gives to the questions the Clinton administration asked it to address. The conclusions of the report Alberts calls "'answers,'" using scare quotes as if to accentuate the definitude they lack.

The modest tone of this book is quite unlike what will be found in the evolution publication. Its authors follow Alberts not only in endeavoring to qualify the certainty of their conclusions but also in freely referencing the subjectivity that bears upon scientific reasoning. This is a feature that occasionally manifests in the evolution publication, but the authors of that effort only offer up such qualifiers en route to conclusions that pointedly accentuate the ultimate certainty of theoretical consensus. The evolution publication acknowledges that "the statements of science should never be accepted as 'final truth,'" but in the same breath it then cautions that nevertheless "in the case of heliocentrism as in evolution the data are so convincing that the accuracy of the theory is no longer questioned in science" (1998, p. 30). No similarly bold or emphatic language appears in the climate science report. Its response to the overarching question of whether global warming can be explained by greenhouse gas theory is sprinkled with qualifiers and disclaimers. The conclusion of these writers that "the observed warming of the last 50 years is likely to have been due to the increase in greenhouse gas concentrations accurately reflects the current thinking of the scientific community on this issue" (2001, p. 3). In this instance it is the collective judgment of a community of scientists rather than indubitable fact upon which the theory's truth value stands. Moreover, the reader will soon learn that this conclusion is open to all manner of acknowledged doubts.

The stated degree of confidence in the IPCC assessment is higher today than it was 10, or even 5 years ago, but uncertainty remains because of
(1) the level of natural variability inherent in the climate system on time scales of decades to centuries,
(2) the questionable ability of models to accurately simulate natural variability on those long time scales, and
(3) the degree of confidence that can be placed on reconstructions of global mean temperature over the past millennium based on proxy evidence. Despite the uncertainties, there is general agreement that the observed warming is real and particularly strong within the past 20 years. Whether it is consistent with the change that would be expected in response to human activities is dependent upon what assumptions one makes about the time history of atmospheric concentrations of the various forcing agents, particularly aerosols (NAS, 2001, p.

3).

There is nothing particularly surprising about this summation. Its nuanced language is characteristic of the professional communication of scientists. But the fact that this was written for lay representatives of the American public creates a complication. These readers need to decide to what extent the language of scientific uncertainty reflected in this technical report should affect policy making on this issue. Is the scientific consensus on the causes and future of global warming strong enough to warrant decisive action? The authors of this book say that it is, but they do not explain how that determination takes into account the pervasive uncertainty that is described throughout its pages.

In this regard readers of this report find themselves looking into the metascientific vacuum I described earlier. Without any criteria by which to directly answer this question, these non-specialists are likely to fall back upon more conventional modes of judgment – their own sense of the coherence and evidentiary merits of arguments for greenhouse global warming, their take on the ethos of these scientific messengers, or perhaps their sense of how their own constituents might wish them to judge this matter. But they would be just as likely to fill this empty conceptual space by bringing to this message conceptions of the nature of scientific knowledge that come from sources like the NAS book on evolution. Were they to do so, they would likely judge that the case for greenhouse gas emissions as the factor responsible for rising global temperatures is weak.

Skepticism of this kind is typically put down to political prejudice, and certainly the ideological leanings of the public actors who must to interpret such findings may dispose some to have greater doubts than others. But this does not change the fact that it is scientists who have had the responsibility of teaching the rest of us how to best judge their findings. If scientists engage in such instruction under the pressures of informal demarcation, we should likewise expect that the metascientific tools with which they equip the American public will not be up to the task of discerning complex issues like global warming. Preoccupied as it is by the ongoing challenges of creationism and intelligent design, the scientific culture is unwilling to pull back from a demarcationist strategy that has served it well for several centuries. But in the complex world of the present, in which the worth of various scientific theories must be weighed in public deliberation, this approach to shaping public conceptions of science poses new dangers.

For some time the issue of scientific literacy has occupied the attention of science educators in the U.S., and for good reason. Those living in a world increasing shaped by science, must also find their way by science. Usually these concerns center on literacy as it pertains to the content of science rather than the ways of science, but in reality it may be the latter concern that has the greater importance. Even the highly educated and interested lay person could never hope to attain more than a superficial command of what scientists know – even in several life times. Some parts of scientific learning need to be generally understood, such as those having bearing upon issues of health and nutrition, but most do not. For lay persons who must deliberate on scientific questions, a realistic knowledge of what I have here called metascience would be more useful. As an overarching understanding of scientific inquiry, sound metascience would provide public actors with a more reliable framework for assessing the merits of particular knowledge claims.

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