

WHAT ROLE FOR SCIENTISTS?

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I. Introduction

Scientists in the contemporary world have increasingly become “sentinels”,¹ alerting the world to matters – such as stratospheric ozone depletion, anthropogenic climate change, and biodiversity loss – that threaten both human well-being and the continued existence of the diverse life with which we share our planet. Although these threats are not purely matters of natural science – being rich with social, political, economic and moral dimensions – they were first identified by natural scientists, and they cannot be solved without a robust scientific understanding of their causes, character, and extent. Thus, scientists, it might seem, have an obvious role in discussing both the problems and their solutions.

Yet most contemporary scientists shy away from becoming involved in articulating solutions, fearing to trespass into territory that seems to belong to others. In some cases, scientists hesitate even to explain the implications of their work in human terms – hesitating, for example, to explain why a 2-degree climate change *matters* – insofar as that might also lead into non-scientific territory.

A major location where this tension expresses itself is in scientific assessments for policy. In an on-going research study of such assessments, we have found that participating scientists believe strongly in the existence and importance of a clear and distinct boundary between “science” and “policy.” In interviews with participants, the question of the relationship between science and policy persistently arises, and is viewed as an extremely important matter.²

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¹ Oreskes, Naomi, 2013. “The Scientist as Sentinel”, *Limn* 3: 69-71.

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Scientists often claim that it is essential for them to honor the boundary between science and policy because their credibility as neutral, objective experts depends upon it. In making this argument, they are implicitly making an argument about *politics* – that they should not become involved in them. Politics, they say or suggest, is not a matter of objective knowledge and therefore a realm where scientists do not belong. Scientific facts, they argue, should inform political decision-making, but scientists, *qua* scientists, should not stray into politics. In formal interviews and informal conversations, many scientists involved in assessment work stress the imperative of preventing the “infiltration” of political considerations into their technical reports, insisting that it is essential that their work remain firmly on the “science” side of the science-policy border. If asked why it is essential, a common response is that the credibility of the assessment depends upon it.

Yet, at the same time, there are considerable differences of opinion among scientists as to where the posited boundary sits. Assessments exist for the purpose of providing scientific information to support potential policy decisions, a dimension that distinguishes them from “ordinary” scientific work, but in the practice (as opposed to the theory) of assessment, there is no absolute (or even consistent relative) standard for the relationship between “the science” and “the policy”. Some issues that may look to an outsider as policy matters may be considered by scientists to be amenable to technical analysis. Conversely, matters that some scientists wish to avoid as “political” might seem to a layperson to be highly technical. Moreover, many scholars in the social sciences would argue that the very act of making an assessment for a political purpose necessarily makes the assessment itself an instrument of politics.³

If we step back from current discussions, we find that scientists’ views on this matter have not been stable over time, but have changed considerably over the course of the 20th century. In theory – and from the perspective of expertise (as well as democracy) – an observer might agree that it makes sense for scientific experts to focus on science, leaving the social, political, and economic dimensions to other experts, to governments, and the public. In practice, this proves to be shifting, contested, and murky ground.

³ Sheila Jasanoff, *Science and Public Reason* (2013); idem. *Designs on Nature: Science and Democracy in Europe and the United States* (Princeton, 2011); idem., editor. *States of Knowledge: The Co-Production of Science and the Social Order* (Routledge, 2004); Bruno Latour, *Politics of Nature: How to Bring the Sciences into Democracy* (Harvard, 2004); idem., *We Have Never Been Modern* (Cambridge: Harvard, 1993); Clark A. Miller and Paul Edwards, editors, *Changing the Atmosphere: Expert Knowledge and Environmental Governance* (Cambridge: The MIT Press, 2001).

Some scientists believe that making policy recommendations *is* appropriate. In the U.S. National Acid Precipitation Assessment Program (NAPAP), for example, many participants suggested that the assessment would be incomplete without recommendations as to how much acid rain should be controlled. This meant calculating the degree of emissions reduction needed to protect lakes and forests. Other NAPAP scientists argued the opposite: that as scientists they should stand steadfastly in the domain of “science” and not tread in policy waters. Similar arguments were made about ozone depletion, and scientists assessing ozone in the 1970s and early 1980s similarly divided on the issue. Early assessments included discussions of how much and how rapidly chlorinated fluorocarbons – the chemicals that were causing ozone depletion – needed to be controlled. Over time, however, leaders in the ozone research community retreated from that position, as they came to believe it necessary to demarcate technical findings from policy recommendations, developing the rubric that an assessment should offer “policy-relevant but not policy-prescriptive” information. In hindsight most scientists who were involved in ozone now argue strongly against making policy recommendations. Scientists involved in climate change assessment generally take the same view, which is officially endorsed by the IPCC. In its statement of principles and procedures, the Intergovernmental Panel on Climate Change explicitly states that its role is to provide “policy-relevant but not policy-prescriptive information” (IPCC 2010).⁴

Scientists active in the IPCC argue that it is important, even essential, for them to stay on the “science” side of the science-policy divide, holding (sometimes with great force) that scientists should tell the government what obtains in the world, but not presume to tell the governments what they should do. (Or, to paraphrase Galileo, to tell how the heavens go but not how to go to heaven).⁵ To put it in David Hume and Max Weber’s famous *is/ought* terms, they should describe what is, but not presume to say what ought to be done about it.⁶ But it was not always this way; scientists’ views of their appropriate role in addressing policy questions have been neither uniform nor static.

⁴ <http://www.ipcc.ch/pdf/press/ipcc-statement-principles-procedures-02-2010.pdf>

⁵ Galileo Galileo: Letter to the Grand Duchess Christina of Tuscany, 1615, on line at *Modern History Sourcebook*, www.fordham.edu/halsall/mod/galileo-tuscany.asp

⁶ Robert Proctor, *Value-free Science? Purity and Power in Modern Knowledge* (Harvard, 1991).

II. Science and Policy: A Historical Trajectory

If we step back from recent assessments and the views of scientists now living, we can discern significant differences in approaches to this issue. Indeed, we can recognize a trajectory from a period immediately after World War II, when many physicists considered it not only appropriate but urgent that they speak out on political matters related to their science, through an intermediate period during the Cold War when scientists gave policy advice but in a more measured way, to our current situation wherein scientists insist that what they do is policy-relevant, but not political. In short, we can discern a trajectory of retreat from the political.

The Scientist as Wise Man and Public Intellectual

Consider Neils Bohr's famous interventions in the matter of nuclear arms control. Both during World War II and the years immediately following it, Bohr spoke passionately and publicly to the urgent need, created by nuclear weapons, for international cooperation to control their spread. But Bohr's intervention was not uniformly welcomed.⁷ After his meeting with U.S. President Franklin Roosevelt in 1944, the U.S. government questioned Bohr's loyalties, limited his participation in the Manhattan project, and placed him under FBI surveillance. It was not until 1995 that the U.S. government officially cleared Bohr – along with Robert Oppenheimer, Leo Szilard, and Enrico Fermi – of accusations that he had acted as agents for the Soviet Union. The FBI stated that their decision was based on their own “classified information” – in other words, files that the FBI (and perhaps the CIA) had collected on Bohr and the others, having at the time viewed scientists – particularly, but not only foreign-born ones – as uncertain allies.⁸

Bohr was joined by Albert Einstein, who spoke out strongly during the war against the Nazi threat, after the war for arms control, and in later years as an advocate for Zionism, pacifism, socialism, and civil rights. During the Cold War, Einstein also spoke strongly against McCarthyism in the United States. Both Bohr and Einstein can be seen as embracing the role of public intellectual, speaking on diverse cultural and political questions, some related to their expertise in matters nuclear but many others not.

⁷ Richard Rhodes, *The Making of the Atomic Bomb* (New York: Simon and Schuster, 1995).

⁸ http://articles.latimes.com/1995-05-02/news/mn-61373_1_atomic-bomb On scientists as uncertain allies, see also Oreskes, Naomi and Ronald Rainger, 2000. “Science and security before the atomic bomb: The loyalty case of Harald U. Sverdrup”, *Studies in the History and Philosophy of Modern Physics* 31B: 309–369.

Einstein and Bohr spoke as individuals, but as individuals whose views were taken to be of more than ordinary value in light of their exceptional brilliance and insights into the workings of nature. They were also taken, in some unarticulated but still evident way, to reflect the insights and wisdom of science. Einstein spoke as Einstein, but many viewed his as a voice of science and therefore as a voice of *reason*.⁹

The environment of their interventions was of course unique: the looming existential threat created by nuclear weapons. The atomic bomb owed its existence in part to the intervention of scientists, including Einstein, who signed (although did not actually write) the letter that first alerted U.S. President Franklin Roosevelt to the possibility that an atomic bomb could be built. Other scientists, notably Americans Vannevar Bush and James Conant, played a critical role in persuading the President that it should be built.¹⁰ Thus one might argue that, to a significant degree, the atomic bomb existed because scientists had waded into political (and military) waters. Given this, it was not entirely surprising that, having done so once, they would do so again.

Once the Manhattan project was underway, most American scientists who knew about it supported it, but as the war in Europe came to a close, the prospect loomed that the bomb would be used in a manner that scientists had not anticipated and did not necessarily support. Leo Szilard, the Hungarian-born scientist who actually wrote the famous “Einstein letter”, began a petition drive at Los Alamos to collect signatures against the use of the bomb against Japan.¹¹ Scientists at the University of Chicago, led by physicist James Franck, also began to organize opposition, advocating at least for a test demonstration before any possible use.¹²

Oppenheimer opposed the Szilard petition, discouraging scientists at Los Alamos from signing on grounds that the bomb’s use was outside their domain of expertise. Yet after the war, he spoke to many issues outside the narrowly

⁹ Silvan S. Schweber, *In the Shadow of the Bomb: Oppenheimer, Bethe, and the Moral Responsibility of the Scientist* (Princeton: Princeton University Press, 2013).

¹⁰ Stanley Goldberg, “Creating a climate of opinion: Vannevar Bush and the Decision to Build the Bomb”, 83: 429-452, 1992.

¹¹ Rhodes, *Atomic Bomb*, 697, 749-750, Charles Thorpe, *Oppenheimer: The Tragic Intellect* (Chicago: The University of Chicago Press, 2008), 7, 123, 156. See also Martin Sherwin, *A World Destroyed: The Atomic Bomb and the Grand Alliance* (New York: Vintage Books, 1977), 217-219; Gus Alperovits, *The Decision to Use the Atomic Bomb* (New York: Random House, 1995) 190, 604-607.

¹² Rhodes, *Atomic Bomb*, Sherwin *World Destroyed*, Alperovits, *Decision*.

technical, including, famously, arguing that physicists, having built the bomb, now knew “sin”. Later he would claim that the sin he referred to was the sin of pride, but that was not how most people interpreted it at the time. Most saw it as suggesting that scientists bore some responsibility for what they had done, and thus for thinking as well about its future and control.¹³

President Harry Truman, along with his military and political advisors, ignored the scientific opposition and used the atomic bomb against civilian targets in Hiroshima and Nagasaki. But the fact that their interventions were not necessarily welcomed (much less heeded) did not prevent men like Franck and Szilard from believing they were justified in taking the positions they did. Franck in particular argued that scientists’ intimate involvement in the question of atomic weaponry, including their “prolonged preoccupation with its world-wide political implications”, not only justified but indeed imposed upon them the obligation to offer their views.¹⁴

While at Los Alamos, Robert Oppenheimer disagreed, suggesting that scientists had no special competency in the social, political or military aspects of atomic weapons. A young Robert Feynman went further, claiming to practice “active irresponsibility” as a matter of principle.¹⁵ But soon after the war, many scientists began to argue something closer to Szilard and Franck’s position: in building the bomb they might not have sinned, but they did have an active responsibility to engage in discussions of its future use by virtue not only of their role in building it, but also by virtue of their intimate knowledge of and proximity to the problem. The so-called Scientists’ Movement – initially an informal assortment of voices but later organized into the Federation of Atomic Scientists and then re-named the Federation of American Scientists – stressed the point earlier made by Franck that their familiarity with atomic weapons gave them a particular, specific, and immediate responsibility to engage in public discussion of them. After the war, Oppenheimer allowed that it was “true that we are among the few citizens who have had occasion to give thoughtful consideration to these problems...”¹⁶

Oppenheimer was inconsistent – perhaps conflicted – on this point. During the war he argued against the scientists’ role in the decision on how or whether to use the bomb, but soon after it he argued for the imperative

¹³ Thorpe, *Tragic Intellect*, 191, 286.

¹⁴ Zuoyue Wang, *In Sputnik’s Shadow*. (New Brunswick: Rutgers University Press, 2009) 19–20.

¹⁵ Wang *In Sputnik’s Shadow*, 22.

¹⁶ Wang, *In Sputnik’s Shadow*, 20.

of policies to control it.¹⁷ In 1946, Oppenheimer would be a co-author, along with other Manhattan Project luminaries such as Hans Bethe, Arthur Compton, Walter Alvarez, and Glenn Seaborg, of the Report on the International Control of Atomic Weapons – known as the Acheson-Lilienthal report for the chairs of the committee – which advocated international control of fissile materials.¹⁸ The justification that these men offered for their foray beyond the technical and into the political was the same one Szilard and Franck had offered – that their intimate scientific knowledge of nuclear weapons gave them a particular – even unique – appreciation of the political and existential threat that they represented. Scientists also understood acutely that the notion of a “secret of the atomic bomb” was a fantasy; Soviet scientists would soon catch up if they had not done so already.¹⁹

When the time came just a few years later to discuss the hydrogen bomb, Oppenheimer and his colleagues dove deeply into a deep that was not merely, or even centrally, about its technical aspects. As historians Barton Bernstein and Peter Galison have shown, leading physicists initially opposed the H-bomb on moral grounds. Asked in 1949 whether the H-bomb should be built, a majority of the General Advisory Committee said no. The magnitude of the destruction it would wreak meant that the H-bomb could not be directed solely at military targets, but would necessarily kill civilians in copious numbers. A minority of the committee – including Enrico Fermi – went further, arguing that, as a weapon of mass destruction – a genocide weapon – it was “necessarily evil in any light”.²⁰ After the American decision to build the bomb was made – and Robert Oppenheimer humiliated, stripped of his security clearance in part because of his initial hesitation – prominent scientists, including Einstein and Bohr, nevertheless continued to speak against it, intermittently joined by others including Hans Bethe, Frederic Joliot-Curie, George and (later) Vera Kistiakowsky.²¹

¹⁷ Wang, *In Sputnik's Shadow*, 26.

¹⁸ <https://history.state.gov/milestones/1945-1952/baruch-plans>

¹⁹ Richard G. Hewlett and Francis Duncan, *Atomic Shield: 1947-1952*, Volume II of *A History of the U.S. Atomic Energy Commission* (Washington DC: US Department of Energy 1970); Herbert York, *The Advisors: Oppenheimer, Teller and the Superbomb* (Palo Alto: Stanford University press, 1989); Zuoyue Wang, 2014, Scientists and Arms Control: The U.S.-China Case and Comparisons with Climate Change, paper presented at “Arms Control and Climate Change Conference”, UT Austin, January 16-17, 2014.

²⁰ Barton Bernstein and Peter Galison, In any light: Scientists and the decision to build the superbomb, 1952-1954, *Historical Studies in the Physical Sciences* 19(267-347).

²¹ On the role of the Joliot-Curies in France, see Spencer Weart, *Scientists in Power*: (Cambridge: Harvard University Press, 1979). On Hans Bethe, see Silvan S. Schweber,

What role did Oppenheimer's initial opposition to the H-bomb play in his loss of his security clearance? Historians do not agree on the answer to that question, but we do know that the hearing board that took that action cited, as one justification, the fact that Oppenheimer had inappropriately strayed beyond the technical and into the moral and political realm.²² While most leading scientists – including the conservative stalwart Vannevar Bush – defended Oppenheimer, for scientists unsure of where the ship of science ended and the ship of state began, this was a clear shot across the bow. Oppenheimer's "candor" had seemingly contributed to his downfall. Historians Martin Sherwin and Kai Bird suggest that American scientists took this to heart, and now believed that they could serve the state "only as experts on narrow scientific issues".²³ That is an overstatement, but certainly scientists saw that reticence on policy questions was a safer strategy.

The rise of elite committees

Oppenheimer's downfall, and the broader context of the governments' need of scientific advice on diverse technical questions related to the prosecution of the Cold War, including, especially, Sputnik, led policy-makers increasingly to recognize science advice as a formal problem, and scientists to press for a formal mechanism to supply it. The need was answered by the creation of new institutional structures, most notably in the United States the President's Science Advisory Committee (PSAC, created in 1957), and the JASONs (established in 1960), a group of reclusive scientists, originally all physicists, who advised the U.S. Department of Defense and the Atomic Energy Commission (later the Department of Energy) throughout the Cold War (and continue to do so today, albeit with a more diverse disciplinary and gender distribution).

Despite Oppenheimer's downfall, the scientists involved in these committees defined their role expansively. Historian Zuoyue Wang has described how PSAC members were mindful of the need not to over-step their authority – the famous question of whether scientists should be "on top" or "on tap" – particularly as their remit was explicitly to advise the President of the United States. Yet the argument was also made – most notably by a President,

Nuclear Forces: The Making of the Physicist Hans Bethe (Cambridge: Harvard University Press, 2012) and idem.

²² Wang, *In Sputnik's Shadow*, 46.

²³ Kai Bird and Martin Sherwin, *American Prometheus: The Triumph and Tragedy of J. Robert Oppenheimer* (New York: Vintage Press, 2005) on p. 549.

Dwight Eisenhower – that the distinction between science and policy was too crudely wrought. Eisenhower argued that there was an *expertise-policy continuum* from technical considerations through policy evaluations and into political decision-making. Scientists, Eisenhower felt, would be more useful if they could “liberate themselves from their “exact” mind-set to see beyond the logic of technological determinism, and to take the broader political factors into considerations in the policy realm. This did not mean that he would want political considerations to distort technical evaluations, but it did mean that the technical arguments should be balanced with those derived from other justified sources”.²⁴ Many did: one sees in PSAC analyses considerations of diverse issues, from arms control to DDT, in which social, economic, and environmental aspects are not ignored. (PSAC under George Kistiakowsky also wanted to examine birth control, but Eisenhower rejected the topic as too divisive).²⁵

PSAC served as persuasive advocates for civilian control of the U.S. space program – and not only on technical grounds – despite pressure from the Pentagon to control it, and for the Limited Test Ban Treaty. They also served as persuasive critics of misguided military projects such as aircraft nuclear propulsion, and gave extensive advice about weapons systems, especially missile systems and the infamous “missile gap”. In all these areas, PSAC scientists argued that the technical and political considerations were closely linked, if not inseparable. Discussing a review of the 1961 DOD budget, for example, PSAC wrote “We have not found it possible to limit our review to purely technical considerations in view of the complex interaction between weapons technology and non-technical factors”.²⁶ Meanwhile, scientists like Herbert York, first Director of Defense Research and Engineering at the Pentagon, joined with PSAC in arguing against “technological palliatives to cover over serious persistent underlying political and social problems”.²⁷ York was not alone in becoming an advocate for arms control in light of the futility of trying to solve the problems posed by nuclear weapons by building more of them. And the President, according to Wang, consistently supported this approach.

While PSAC scientists may have restrained from overt political statements and were mindful of honoring the prerogatives of the President they served, PSAC in its day made many recommendations that, by the standards of our

²⁴ Wang, *In Sputnik’s Shadow*, 64.

²⁵ Wang *In Sputnik’s Shadow*, 107.

²⁶ Quoted in Wang, *In Sputniks’ Shadow*, 110.

²⁷ Quoted in Wang, *In Sputnik’s Shadow*, 104.

contemporary informants, would be viewed as over-stepping. Wang concludes that the overarching philosophy of PSAC in the Eisenhower administration was that experts needed to consider technical issues in their full context, that “technical issues could never be neatly and completely separated from social, economic, and political factors, and what was technically feasible was not always desirable”.²⁸ More than that, PSAC’s impact derived from this recognition and the willingness that followed from it *not* to restrict their analyses to the narrowly technical. “Crucially” Wang concludes, “Eisenhower agreed with PSAC on the need for science advising to integrate technical evaluations and policy considerations”.²⁹

PSAC scientists played a major role in supporting arms control because they believed it obvious that an uncontrolled arms race would decrease national security, no matter how sophisticated those arms were. What they brought to these discussions, Wang suggests, was a form of “technological rationality” that applied equally to the technical and the political.³⁰ In the words of the nation’s first Science Advisor, James Killian, the “scientific” issues they addressed “involve political, ethical, and scientific considerations in a way that ... cannot be wholly disentangled”.³¹ In short, Wang argues, PSAC scientists “wanted to exercise their social responsibility and [to consider how] their technical investigations fit into the broader social and political context”, and President Eisenhower encouraged them to do so. This overall philosophy continued into the Johnson administration, which, although more focused on domestic policy than its predecessors, wanted “scientists to help make life better for ‘grandma’”.³²

This is not to say that PSAC scientists never attempted to draw lines between science and policy – or, more specifically, between the scientific and the political – at times they clearly did. But it is to point out that in various ways these scientific advisors understood their role to be both technical and political. They believed that artificial distinctions between these realms would lead to flawed analyses and costly errors. And they rejected instrumental rationality by insisting instead that the ends of scientific and technological programs were as important to consider as the means.³³

²⁸ Wang, *In Sputnik’s Shadow*, 2.

²⁹ Wang, *In Sputnik’s Shadow*, 3.

³⁰ Wang, *In Sputnik’s Shadow*, 5.

³¹ Quoted in Wang *In Sputnik’s Shadow* 14.

³² Wang *In Sputnik’s Shadow*, 244.

³³ Wang *In Sputnik’s Shadow*, 9.

The same point may be made about the JASONs. The original JASONs were all physicists, but that did not stop them giving advice when asked in the 1960s and '70s about military policy in Vietnam, the desirability of building the SST or negotiating an anti-ballistic missile treaty, and whether climate change was something to worry about.³⁴ In the Vietnam case, scientists argued against carpet bombing on *moral* grounds, something which our current IPCC members would find at least discomfiting if not inappropriately over-stepping.³⁵

Before long PSAC *would* be accused of overstepping, as members of PSAC publicly opposed President Richard Nixon on the deployment of anti-ballistic missile defense.³⁶ PSAC had also opposed ABM under Johnson – on political as well as technical grounds – a position that was adamantly rejected by Johnson's Joint Chiefs of Staff, but supported by Defense Secretary Robert McNamara. Johnson did not have strong views on ABM, but President Nixon did, and the public stance against it taken by some PSAC members, as well as their publicly expressed opposition to the Vietnam War, deeply angered the President. His anger was compounded when PSAC member Richard Garwin testified in Congress against the super-sonic transport program, which Nixon also strongly favored and shortly after his re-election, Nixon dissolved PSAC.³⁷

Despite the demise of PSAC, scientists continued to play a role in diverse domains where the technical and political met. Throughout the 1960s, '70s, and '80s scientists such as Barry Commoner and Paul Ehrlich spoke publicly about environmental threats; Roger Revelle advocated actively for population control; and Frederick Seitz, a former President of the U.S. National Academy of Sciences, became a public advocate against tobacco control.³⁸ Physicists

³⁴ Wang, *In Sputnik's Shadow*, 259–260, Ann Finkbeiner, *The Jasons: The Secret History of Science's Post-war Elite* (New York: Penguin, 2006); Marvin ("Murph") Goldberger, personal communications.

³⁵ Finkbeiner, *The Jasons*, 63–71, 76–78, 87–89, 103–117; Goldberger, pers. comm.

³⁶ PSAC members, including science advisor Donald Hornig, had opposed President Johnson on the war in Vietnam (and before that President Kennedy on manned space flight) but this opposition had mostly occurred in private. See discussions in Wang, *In Sputnik's Shadow*.

³⁷ Wang suggests that Johnson's decision to develop the Sentinel "thin ABM" system over scientists' opposition, and particularly Robert McNamara's false suggestions in public that scientists had endorsed this approach, led PSAC scientists for the first time to begin to dissent publicly. Thus the ABM "split" began in the Johnson years, but culminated under Nixon. Wang, *In Sputnik's Shadow*, 280.

³⁸ Barry Commoner, *The Closing Circle: Nature, Man and Technology* (New York: Bantam Books, 1974); Paul Ehrlich, *The Population Bomb* (New York: Sierra Club-Ballantine

also continued to advocate for and even participate in arms control negotiations and agreements. Turning again to Wang, he has documented how in the late 1980s, Wolfgang Panofsky, Director Emeritus of the Stanford Linear Accelerator Center and member of the U.S. National Academy of Sciences Committee on International Security and Arms Control, helped to open a back channel to the Chinese government through his contacts in the Chinese physics community.³⁹ Personal connections seem to have played an important role in building relations in trust in the fraught domain of arms control; elsewhere however, elite committees and informal personal approaches were being overshadowed by the rise of organized assessments of science for policy.

From Elite Committees to Organized Assessments

The history of PSAC illustrates an intrinsic tension between objectivity and loyalty that may arise when scientists serve as advisors inside government. Members of PSAC served at the discretion of the President who appointed them, and the same is true of scientists who have served the US (or other) governments on the various panels that predated and post-dated PSAC. In hindsight, moreover, one might also agree at least in part with critics who noted that PSAC and JASON scientists commented on many areas well beyond their formal training and expertise.

It is therefore not surprising to find that one reason given by scientific participants for the need for international assessments was that they came to believe that international assessments would be viewed as more objective and therefore carry more authority than assessments tied to the policy aims of national governments.⁴⁰ One might conclude, in parallel, that national assessments might be viewed as more objective than the advice of individual scientists serving at the discretion of a particular President. Strikingly, the rise of formalized assessments does not quite coincide with the fall of PSAC, but it comes close: NAPAP was authorized by the U.S. Congress in 1980; the International Ozone Trends Panel issued its first report in 1981; the IPCC was created in 1988.

While scientific panels and committees within governments persist, and celebrated individuals at times still offer their opinions on diverse subjects,

Books, 1970); see also, Paul Sabin, *The Bet: Paul Ehrlich, Julian Simon, and Our Gamble over Earth's Future* (New Haven: Yale University press, 2013) and Naomi Oreskes and Erik M. Conway, 2010. *Merchants of Doubt: How a Handful of Scientists Obscured the Truth on Issues from Tobacco Smoke to Global Warming* (New York: Bloomsbury Press).

³⁹ Wang, 2014.

⁴⁰ Keynyn Brysse, *Assessing Ozone*, forthcoming in *Assessing Assessments*.

since the late 1970s scientific advice has been increasingly sought by governments and offered by scientists in the form of large, formal assessments. Unlike the elite advisory committees that prevailed in the Cold War, these assessments are made by large groups (hundreds or even thousands) of scientists, few of whom are known to the public and most of whom may be fairly described as “rank and file” scientists. That is to say, whereas scientific advice for policy was previously offered by famous individuals, and then for a time by select committees of hand-picked mostly famous men, today it is offered by diverse scientists (many of whom are not particularly famous and certainly not known to the general public), often working in international contexts, and speaking in a collective voice.

III. Criticism, Pushback, and Shifting Epistemologies

For historians, it is hardly surprising to find that “science” and “policy” are not fixed categories, nor that the perceived relation between them has been involved shifting standards. However, many scientists *do* find it surprising, and, surprising or not, these observations lead to two questions: What drove the trajectory from individuals freely making policy recommendations, to smaller groups continuing to do so but in a more circumspect manner, to our current situation where the IPCC formalizes the concept of a high wall of separation? And if scientists have not always thought it wrong to make policy recommendations, why do so many of them think so now?

One obvious answer is political pushback. FDR’s response to Bohr is one illustration of the obvious fact that advice is not always wanted or heeded. Bohr was correct in his prediction that nuclear arms without arms control would lead to an arms race, but that did not make his inventions welcome in the political domain. One might argue that, almost inevitably, scientific interventions in the public sphere are going to be critical ones, insofar as scientists who agree with their nation’s policies or the general state of world affairs are unlikely to feel the need to speak out about them. Yet, by and large World War II and the early Cold war were periods when scientific advice was often heeded, and during which scientists were generally held in high cultural regard.⁴¹

But, as scholars have noted, the cultural heyday of science as a general model for knowledge and of scientists as the embodiment of epistemic au-

⁴¹ Daniel J. Kevles, *The Physicists: The History of a Scientific Community in America*, (Cambridge: Harvard University Press, 1995); Paul Boyer, *By the Bomb’s Early Light: American Thought and Culture at the Dawn of the Atomic Age* (Rayleigh-Durham: The University of North Carolina Press, 1994); Wang, *In Sputnik’s Shadow*.

thority had a relatively short half-life after World War II. One might place the beginning of the end in 1960, when *Time* magazine chose “U.S. Scientists” for its man (sic) of the year. From the apogee, the cultural status of science began to drift downward, first as the political left began to doubt that we could expect “better living through chemistry”, and later as the political right became dis-enamored with a science that seemed to be challenging its core values and presumptions.⁴²

These larger cultural trends may help to explain the change in how scientists viewed their role. As the cultural position of science became less secure, scientists’ became less confident about offering advice to a world that was not so keen to take it, and began to retreat into the more circumscribed technical realm that PSAC, in the 1950s, had so consciously rejected. As society became more skeptical – and at times even hostile – to science, scientists further retreated to the safety of technical territory. Some went further, rhetorically draping themselves in institutional finery of “pure science”, treading carefully so as not to tear the fabric and insisting that they only intended to wear their own clothes and no one else’s. In short, when society was welcoming to scientists’ views on diverse subjects, scientists were happy to offer them and only occasionally doubted that they should. But when society became more critical of science, scientists increasingly hesitated to offer their opinions, and began to develop arguments to justify their hesitation.

This interplay between scientific attitudes towards cultural engagement and the cultural attitude towards science suggests that to a significant extent, what scientists think they *should* do depends on both the perception and the reality of what they actually *can* do, given the prevailing political and social context. The history recounted here suggests that, when it comes to the relation between science and policy, scientists have transformed necessity into virtue, and transmogrified political reality into epistemology.

Some evidence to support this interpretation may be found in the history of attempts to analyze the threat of stratospheric ozone depletion.

IV. Failed Boundary Work: The 1979 Ozone Assessment

Sociologist Thomas Gieryn has long argued that scientists do “boundary work” to preserve and protect their authority against those who might usurp

⁴² Oreskes and Conway, *Merchants of Doubt*. See also Peter J. Jacques and Riley E. Dunlap, *The Organisation of Denial: Conservative think tanks and environmental skepticism*, *Environmental Politics*, 17(3): 349-385, 2008; Thomas Medvetz, *Think tanks in America* (Chicago: The University of Chicago Press, 2012).

or undermine it.⁴³ His classic paper on this subject gave a sociological gloss to a problem that philosophers had previously defined as epistemic: the problem of demarcating science from non-science. Gieryn held that by establishing social borders between scientific research and other activities, scientists helped to establish and maintain the epistemic (and thus cultural) authority of their work. It follows that scientists who feel themselves to be in an insecure position may strive to establish clear and firm boundaries around it. And it also follows that as the cultural position of science became less secure in the 1960s and 70s than it had been in the period immediately following World War II, scientists would increasingly engage in boundary work.

In the early history of ozone assessment we observe this manifested in scientists' attempts to define and honor a boundary between scientific findings and policy recommendations.

In 1979, scientists at the U.S. National Research Council (NRC), the research arm of the U.S. National Academy of Sciences, were asked to assess the threat of stratospheric ozone depletion. Attempting to separate the "science" from the "policy", they wrote two separate reports, one that dealt with technical matters – the science or "is" part of the problem – and one that considered whether the manufacture and use of ozone-depleting chemicals should be restricted – the policy or "ought" part. However, they ran into troubled waters, and were criticized both at the time and by some later commentators for making policy recommendations.

In the most detailed account of the history of ozone science and policy, UCLA Law Professor Edward Parson criticizes the NRC scientists for wading into policy waters. Parson judges the scientists harshly for attempting to make policy recommendations, arguing that the reports "established a harmful model for scientific assessments and weakened the credibility of subsequent Academy reports on the issue", because, he argues, scientists were perceived as taking sides. He concludes that the reports did more harm than good in the international arena, where "those [governments] who were initially skeptical [of the need for regulation] viewed the Academy reports simply as scientific supporting documents for the US government position".⁴⁴

One might conclude that the scientists involved made a mistake by wading into the waters of policy, and indeed, influential scientists drew that

⁴³ Gieryn, Thomas F. (1983). "Boundary-work and the demarcation of science from non-science: strains and interests in professional ideologies of scientists". *American Sociological Review* 48(6): 781-795. doi:10.2307/2095325.

⁴⁴ Parson, Edward A. 2003. *Protecting the Ozone Layer: Science and Strategy*. Oxford: Oxford University Press, on 41.

conclusion. International ozone experts Robert Watson and Dan Albritton developed the rubric of “policy-relevant but not policy-prescriptive”, later instantiated at the IPCC, in light of their experiences of ozone assessments.

But Parson neglects to ask a key question: *why* did the scientists do what they did? The answer is: In response to a *request* from the White House. The charge for the report came from the White House Office of Science and Technology Policy, and it specifically asked for *both* a technical analysis of the problem and a set of recommendation as to what should be done in light of that analysis.

The National Academy of Sciences and the National Research Council were created to answer questions posed to them by the U.S. government. By definition, this means questions of policy import, so policy is always implicit in the issues being address. (As already suggested, the act of commissioning an assessment is a political act). But the Academy is not an arm of the government, so it necessarily faces the matter of how to give advice judiciously. Nowadays the NRC generally negotiates with its sponsoring agencies, and frequently works to adjust or alter questions that it finds to be poorly posed, but rarely does decline an offer to proffer advice. It is certainly difficult to imagine the NRC refusing or even seriously questioning a request from the White House.

This raises a further question: if the issues at stake were a question of desirable policy (rather than of technical information) and outside the realm of scientific expertise, then why did the White House ask the scientific community for its views? Without further research in Executive Office archives, we cannot answer this question definitively, but, as already noted, during the 1950s, 1960s, and even into the 1970s, it was common for the President, his staff, or other Executive Branch officials to ask the President’s Science Advisory Committee for their views on diverse issues, such as the use of pesticides, the prosecution of the war in Vietnam, or the appropriate response to the Santa Barbara oil spill.⁴⁵ There is no intrinsic reason why the White House should not ask any highly educated and intelligent expert or group of experts for their views on any matter. In this context, it seems unsurprising that the White House in the 1970s would have asked the NRC not just about the facts of ozone depletion, but also for advice about what to do about it.

Yet, just as the NRC scientists in 1979 were grappling with the questions placed in front of them, the relationships between scientific experts and the U.S. federal government were changing. The fact that we view the matter

⁴⁵ Wang, *In Sputnik’s Shadow*; Goldberger, personal communications.

differently now – that it now seems unproblematic to criticize scientists for doing what they were asked to do – clearly reflects changes in the character of science–society relations. One way to view this history is to suggest that the scientists involved got caught in shifting societal standards and expectations. Another is to suggest that the terms “science” and “policy” are too blunt to adequately capture the subtleties of the issues at stake.

The policy question that the NRC scientists were asked to answer – whether the probable extent of ozone reduction warranted restrictions on the use of CFCs – was a matter of assessing the *severity* of the problem, and to do this required specialized technical expertise. In this sense, they were being asked to address the necessity of a proposed policy goal, and they responded to a request to judge the extent of what appeared to be a serious problem but whose exact degree of severity was impossible for a layperson to judge. It was neither unreasonable that they were asked the question, nor that they answered it. *At that juncture, they were the only people in a position to answer.*

The scientists recognized the challenge that they faced, and tried, as we have seen, to maintain a clear distinction between the two questions by writing two separate reports. But their attempt at demarcation was not entirely successful, in part because although the questions may have been distinct the people involved were not. Essentially the same group of people who had summarized the technical information in the first report – the “is” question – were responsible for answering the “ought” in the second (perhaps affirming the point that no one else was really capable of doing so).

One way to read this episode, then, is as illustrating the difficulty that scientists have in finding (or building) and navigating (or patrolling) a clear boundary between science and policy/politics. If one wanted to criticize scientists for wading into policy waters, the fact that essentially the same group was involved in both reports certainly invited such criticism. But if no one else was able to answer the question posed, then what was the alternative? Scientists were asked the question of what should be done because their expertise placed them in a position to answer.

There is also a larger historical point that the scientists involved understood: that no matter what they did, they would almost certainly be criticized. As committee member Harold Schiff put it, they were not unaware that they were “fooling with a fairly major industry”.⁴⁶ In the highly contested domain of ozone depletion, where the financial, political, human, and environmental

⁴⁶ Quoted in Sharon Roan, *Ozone Crisis: The 15-year Evolution of a Sudden Global Emergency* (New York, Wiley: 1990), 80.

stakes were high, the idea that scientists could protect themselves from criticism through boundary work was probably unrealistic. At that time, the industry was still committed to defending its product, and therefore to challenging evidence that indicated its potential for harm. Sticking to the facts would not have shielded scientists from attempts to undermine those facts.⁴⁷

Scientists were striving to demonstrate their objectivity and avoid accusations of bias or inappropriate excursions into policy domains, but the reality was that many groups were standing ready to attack, no matter what the scientists had done. Given the stakes, as well as the hovering industrial sector to which Schiff refers, it seems reasonable to suppose that no matter what the scientists had recommended, and no matter how carefully those recommendations had been framed, they would have been criticized by those who did not like the results. Because ozone depletion had serious consequences – because it was a *problem* – the technical and the social – the *is* and the *ought* – overlapped.⁴⁸

Consider another example. In the 1975 Climate Impacts Assessment Program (CIAP), sponsored by the U.S. Department of Transportation, scientists came to the conclusion that the exhaust produced by a proposed large fleet of super-sonic transport planes (SST) would pose a serious threat to the ozone layer. This had the obvious policy implication that the proposed fleet should not be built. But bureaucrats in the sponsoring agency (whose leadership supported the SST) altered the scientists' message, writing an Executive Summary that dwelled mostly on the effects of a small and near-term projected SST fleet (30 or so aircraft), which were essentially negligible, and downplaying the possible effects that scientists predicted of the large long-term projected fleet (200 or so aircraft – although this number was not mentioned in the Executive Summary). In addition, any potential adverse effects were cast as preventable through future, unspecified and as yet undeveloped, technology. The overall effect of the tone and wording of the Executive Summary was to suggest that the scientists had dismissed, rather than confirmed, the worry that the SST could damage ozone. This suggestion that was erroneous. As Parson notes, “a wire service report of the press conference made this misinterpretation explicit and was widely repeated, in some cases with scathing attacks on the scientists who had raised the alarm”.⁴⁹ When scientists realized what had happened, they tried but found themselves

⁴⁷ Oreskes and Conway, *Merchants of Doubt*, Chapter 4.

⁴⁸ Oreskes and Conway, *Merchants of Doubt*, see also Wagner and McGarity, *Bending Science*; Michaels, *Doubt is their Product*; Proctor, *Golden Holocaust*; Brandt, *Cigarette Century*.

⁴⁹ Parson, *Ozone*, 28–29.

unable to undo the impact of the Department's intervention.⁵⁰ Why did the Department of Transportation misrepresent the scientists' conclusions? Presumably because they did not like their policy implications. Whether or not those implications were stated explicitly or were left implicit mattered not to the officials who wrote the misleading summary.

These episodes, and others in the histories we have documented, suggest that much as scientists may strive to define and respect a boundary between science and policy – and as much as they may strive to be fair, neutral, and objective – the intrinsically political character of assessments makes it almost inevitable that there will be pushback against scientific results from those who dislike their implications. It may have also be predictable – if not inevitable – that such pushback would cause at least some scientists to want to retreat from the contested borderlands into safer territory. One might conclude that that is what subsequently occurred.

The lesson that ozone scientists took from their experiences was that they needed to articulate a bright line between science and policy and “never to be prescriptive”. Dan Albritton expressed pride in the formulation he developed with Bob Watson, citing the example of the finding that “Unless there is a 100% elimination... of all long-lived chlorine- and bromine-containing compounds... the Antarctic ozone hole will be with us forever”. This, he argues, was not a prescription because it did not tell the governments what to do: “it was totally non-prescriptive”.⁵¹ Perhaps. But as Erik Conway and I have documented in our book, *Merchants of Doubt*, that did not stop opponents of regulation from criticizing them, nor has a comparable strategy protected climate scientists.

Moreover, albeit unstated, the policy implication of Albritton's statement is by no means unclear: it suggests that society should move towards 100% elimination of all long-lived chlorine- and bromine-containing compounds. Semantics matter, and Albritton may be correct that governments prefer an implicit approach, perhaps because it seems less arrogant and more respectful of governmental authority and prerogatives. The implicit rather than explicit approach may be a useful and defensible *rhetorical* strategy. It may even be understood as a form of good manners. But epistemologically, the policy implication was certainly clear to the industries who opposed the finding, framed prescriptively or not, which is, of course, why they objected to it.

⁵⁰ A similar story is told about an Acid Rain peer review panel, in 1983, see Oreskes and Conway, *Merchants of Doubt*, Chapter 3.

⁵¹ Brysse, *Assessing Ozone*.

To avoid both prescription and the pushback they believed it provoked – to make it clear that they were “rendering unto Caesar...” – ozone scientists moved into the mode of “scenario development”, a model that is now extensively used in climate assessments. By this they meant outlining what-if (or what-if-not) options. But, as several of our informants have noted, this still implicates them in choices that are not purely technical. When climate scientist Jonathan Shanklin suggested in an interview the benefits of letting politicians “chose from a menu” of policy options, his colleague Michael MacIntyre revised that to say that scientists should *not* present policy options, they should rather say, “if you do this, then we think the range of possible [outcomes] is that”.⁵² Yet, whether it is a menu of options or a set of scenarios, scientists decide what is on the menu and which set of scenarios is reasonable and appropriate to analyze and offer.

Albritton’s “choices” for policy-makers and Shanklin’s “menu of policy options” also introduce an intriguing ambiguity. On the one hand, ozone assessors now generally agree that it is not their place to make explicit policy recommendations; the international ozone assessments since the Montreal Protocol have adhered to this ideal. On the other hand, ozone assessors also agree that the assessments should present a clear set of options, menu selections, or choices. This would seem, in general, to be sending the message that policy action is needed, and this is one reason why assessments come under attack as “politicized” by those who think that doing nothing is not only acceptable but preferable. The very fact of having an assessment suggests that the issue being assessed is at minimum at potential problem about which something (at least probably) needs to be done. (The IPCC “business as usual” scenario is not presented as a reasonable choice, but as a mean to demonstrate the adverse implications of continuing to act as we have been acting). In principle, business as usual may be one of the options, but in practice there is an implicit message that it would be highly undesirable, if not unconscionable.

Assessors are not telling policy-makers what choices to make, but they are deciding what choices to *present* and guiding policy-makers to interpret those choices in certain ways. Watson credits the fact that the ozone assessments did just this with a large measure of their success: “most critically, we had developed a set of plausible futures that highlighted the implications of inaction as well as the implications of different policy actions”.⁵³

⁵² Brysse, *Assessing Ozone*.

⁵³ Watson 2005, 476.

How do assessment authors decide which scenarios should or should not be included in an assessment? Should this process of inclusion/exclusion be viewed as a political decision? Interestingly, few of our informants raised this issue. While Watson argues that scientists should not tell policy-makers what to do (i.e., which policy option to choose), he still argues that that ozone assessments *should* present them with a selection of clear policy options among which to choose; many of his colleagues agree. One could argue that this weakens the wall of separation that Watson and others have worked hard to build. After all, what does it matter if the Antarctic ozone hole lasts forever, if you are not suggesting that that is a bad thing? Scientists speak of doing the thing that is “least worst”, but this is least worst is from their perspective, and that may be different from that of stakeholders outside the scientific community. As Albritton notes, you are not presenting every option under the sun, you are presenting a set of options that seem reasonable to you. Scientists routinely leave out options that others might consider reasonable – prayer, for example. So while the conceptual virtue of scenarios is clear, the strategy does not expunge judgment, and perhaps in part for that reason it has not succeeded in expunging political pushback, either.

Both NAPAP and early ozone assessments included both science and policy. In some cases this was by design: scientists were asked to make policy recommendations relevant to the question of urgency, something which they, as experts, were in a position to understand. In other cases it was by desire: scientists felt themselves qualified to make recommendations of a certain sort. But scientists were criticized – by industry representatives, by government officials, and by later commentators, and even explicitly blamed for delaying regulatory action by blurring the boundary between science and policy. Parson, perhaps the most well informed of the critics, argues that the incursions into policy in the UK ozone reports of the late 1970s undermined their scientific contribution.

Although these reports [i.e., UK DoE 1976 and 1979] provided cogent reviews of scientific knowledge and recent results, the attempt to combine objective scientific review and partisan advocacy in one volume rendered their credibility suspect and their purpose obscure. ... The substantial scientific effort that went into these assessments was wasted as contribution to international policy debate, because the resultant report was tainted by its association with the UK government position.⁵⁴

⁵⁴ Parson, *Ozone*, 97-98.

Parson is arguing that scientific credibility rests on its objectivity and objectivity rests on neutrality. If scientists are seen as aligned with their government's stated or desired policies, their contributions may be suspect. Many scientists would agree. But Parson provides scant concrete evidence to support the claim of "taint" – and leaves it unclear as to who considered the report tainted. Many parties had reason to want to delay regulatory action on ozone, and it seems reasonable to suppose they would have found reasons to justify that position whether or not the UK reports had cleanly and clearly separated the "science" from the "policy". After all, the US NRC had done what Parson suggests the UK scientists should have done – separated the science and the policy into separate volumes – but both industry and Parson criticized them as well.

What does seem to be the case is that scientists took from these episodes the lesson that Parson suggests they should have taken: to build and keep a high wall of separation between science and policy. Another response to this was to make the assessments international, so that they could not be accused of representing the views of any particular national government. Yet a third was to make them larger.

V. Institutional Expansion and Standpoint Epistemology

By making their ozone assessments international, Bob Watson and colleagues tried to address the complaint that they were biased in favor of their own governments' views. They also began to expand the size of their assessments, to include as many relevant experts as possible. In essence, they adopted a standpoint epistemology, attempting to demonstrate and achieve objectivity by including the broadest possible range of perspectives. This is the approach that prevails in the IPCC today, where inclusivity is a guiding principle: it is a matter of course that chapter authors must encompass men and women, include representatives from many countries, and not be dominated by scientists from the US or Western Europe. It is also now viewed as important that, to the degree possible, anyone who has significant expertise should be included in the process, if not as a lead author, then as a contributing author or at least a peer reviewer. Objectivity is constructed as a group accomplishment; accusations of bias are remedied through inclusionary processes. The intellectual presumption is that so long as sufficient diverse voices are heard, no one bias could prevail.

The expansion of the IPCC to be as inclusive as possible may be viewed as a defensive measure to protect the organization from accusations of bias. It may also be viewed as reflecting a contemporary vision of objectivity as a group achievement: it appears that scientists have come to the conclusion

that to achieve credibility and influence, assessments must demonstrate objectivity through inclusivity. (One may note the irony that scientists are embracing a version of objectivity that only a few years ago was considered radical, and a threat).⁵⁵ In this context, it is important to note that the impetus for the creation of the IPCC came from scientists, and scientists have largely presided over its growth. Scientists have not been forced to participate in assessments nor to make them as large as possible; they have done this by their own volition.⁵⁶ The “scientific voice” is no longer the voice of the sage individual, or small group of sages, but the collective voice of the essentially the entire community of relevant experts. The growth of the institutionalized assessment both reflects and reinforces this view.

Consensus then emerges as an important element, because it signals agreement and permits scientists to speak with a collective voice. The consensus of the scientific community marks the recommendations of the assessment as not merely the views – the *opinion* – of a man or even a group of men and women. It marks the results as scientific *knowledge*. Through the assessment, expert opinion is transmuted into knowledge.

This contrasts markedly with earlier traditions, and with the epistemologies that prevailed in early modern science generally, wherein the reliability of the scientific knowledge was assumed to arise from the stature and reliability of the individual or individuals involved. As Steven Shapin and others have emphasized, early modern traditions placed the source of epistemic credibility in the virtues of the individual scientist.⁵⁷ This view persisted into the 1960s, as small groups of “wise men” were called upon to offer up expertise on diverse subjects ranging far from their disciplinary expertise. The intellectual presumption seems to have been that if the correct experts were chosen – men of both relevant knowledge and good reputation – then correct answers could be expected to follow.

The modern assessment both reflects and creates a different epistemological standard, one that implies that no matter how “good” any particular expert, he or she may be accused of bias. Thus, we see a practice we may label “bal-

⁵⁵ Cf. Sandra Harding, *The Science Question in Feminism*, Ithaca: Cornell University Press, 1986 and idem., editor, *The Feminist Standpoint Theory Reader: Intellectual and Political Controversies* (Routledge, 2003); Helen E. Longino, *Science as Social Knowledge* (Princeton: Princeton University Press, 1990).

⁵⁶ Bert Bolin, *A History of the Science and Politics of Climate Change: The Role of the Intergovernmental Panel on Climate Change* (Cambridge: Cambridge University Press, 2008).

⁵⁷ Steven Shapin, *A Social History of Truth: Civility and Science in Seventeenth Century England* (Chicago: The University of Chicago Press, 1995).

ancing of bias” – that of including as many voices as possible in the belief that this ensures that any possible biases are cancelled out.⁵⁸ Objectivity is not achieved by finding the right (unbiased) individuals, but by finding a capacious and comprehensive mix of differently biased ones. Bias is viewed as a form of error that may be cancelled by opposing error. This is an epistemic shift from locating the source of scientific objectivity and reliability in the individual to the institution.

The balancing bias approach gives the scientific community an argument with which to respond to accusations of bias; whether or not it actually produces an epistemically robust result is another matter. Whether it helps to prevent political stalling is also unclear. Recent experience suggests that those who wish to delay action will find ways to do so irrespective of how scientists present the evidence that might warrant such action. Over the course of the 20th century, we have seen a shift from assessments that were primarily nation-based to assessments that are predominantly international, a shift to more clearly delineate and separate technical from political considerations, and a shift to larger numbers of included experts. Yet these changes have not led to speedier political response.

Moreover, despite the evident shift in prevailing epistemology within the scientific community, it appears that important cultural strands, at least in the US and Europe, still cling to the older model. Thus opponents of action on climate change have embarked on significant efforts to discredit particular individuals whose work has played a major role in IPCC conclusions. After the IPCC concluded in its Second Assessment Report that the “balance of evidence suggests a discernible human impact” on global climate, the co-lead author of the key chapter on attribution was the target of a sustained and hostile attack on his virtue, accused of doctoring the conclusions and making unauthorized changes in the report (claims that were later shown to be unsubstantiated). In the wake of the Third Assessment, climate scientist Michael Mann, a co-author of the “hockey stick graph” documenting the rapid uptick of global mean temperatures as measured by instrumental records and proxies, was also the target of personal attacks and Congressional investigations suggesting personal misbehavior. And after the IPCC released its Fourth Assessment and shortly before the 2009 Copenhagen COP 15

⁵⁸ On “balance as bias” in the media, see Maxwell T. Boykoff and Jules M. Boykoff, *Balance as Bias: Global Warming and the U.S.* (Prestige Press, 2004), 125-136; idem., *Climate Change and Journalistic Norms: A case-study of US mass-media coverage*, *Geoforum*, 38: 1190-1204, 2007.

meeting, the personal emails of British climate scientists Phil Jones, evidently stolen some time before, were released to the public and the media, accompanied by allegations that Jones and his colleagues had attempted to fudge the data and to distort the peer review process.

If the IPCC strategy of objectivity and reliability through scale and diversity had been effective, these attacks on individual scientists would have lacked resonance. Observers might have simply argued that, even if an individual had done something inappropriate, it would have been detected and corrected by the others involved. Indeed, some defenders of Santer did make exactly that point – that Santer could not have done what he was accused of doing without others noticing.⁵⁹ Defenders of Phil Jones similarly argued that he was just a human being, and any dark thoughts he may have expressed in private was irrelevant to the larger (public) IPCC process. But arguments of these sorts had little impact on the media or in the blogosphere, which were greatly take with the idea that individual malfeasance was a major story.⁶⁰

In short, scientists have adjusted their practices to vest reliability, credibility, and authority in the organizational structure of the assessment, rather than in the virtues of the individuals involved, but if the motivation for doing so is to persuade larger publics of their virtue, or to address the concerns raised by critics and accelerate the uptake of scientific conclusions into policy making, a critical observer might conclude that those efforts have not had the desired effect. The same may be said about the strenuous efforts that have been made to segregate scientific conclusions from policy recommendations.

VI. Conclusion: Normative Considerations and the Role of Consensus

Let us return to Neils Bohr. As historian Paul Boyer has noted, Bohr was not only criticized by government officials who suspected his motives, but also by civilian commentators who questioned his authority to expound on matters of international diplomacy. Was Bohr not speaking out of court, some asked, when he attempted to tell world leaders how they should pursue their affairs? Arms control was not, after all, a *scientific* matter; it was a *social and political* one. And was it not ironic, even hypocritical, for the scientists who made weapons of mass destruction possible now to instruct the world on the necessity of peace?⁶¹

⁵⁹ <http://connection.ebscohost.com/c/articles/9703113538/open-letter-ben-santer> see also <http://www.ucar.edu/communications/quarterly/summer96/insert.html>

⁶⁰ Boykoff and Boykoff, *Balance as Bias*.

⁶¹ Boyer, *Bomb's Early Light*.

These were reasonable questions in 1950 and they remain reasonable today. What right do scientists have to speak on social and political solutions beyond the domain of their technical expertise? If they do, what obligations do they incur? Certainly scientists have the same right as ordinary citizens to speak up on issues of import. Beyond that, scientists have the right – and some would say the obligation – to speak out, to alert the world to threats, challenges, and opportunities of which they, by virtue of their scientific expertise, are especially or even uniquely aware. Is it possible to make sense of these competing considerations and make a normative recommendation? Is there a recognizable line between useful interventions and unhelpful stepping out of bounds?

Policies and Instruments

One way to begin to answer these questions is by differentiating between policies and instruments. While scientists like Watson and Albritton came to an unequivocal conclusion that they should not make policy recommendations, others came to a different conclusion. Sherwood Rowland, who first recognized the threat that CFCs represented to stratospheric ozone (and later won the chemistry Nobel Prize for it) thought it was important that ozone scientists speak out because they understood the character of the problem in a way that no non-expert could. In fact, because they (alone) understood the threat that ozone depletion represented, they had an obligation to speak out. That obligation went beyond simply describing the problem to becoming advocates for action to prevent further irreversible damage to life on Earth.

The key point here is that their expertise put them in a unique position: no politician, no layperson, and not even a scientist who was not an ozone expert could accurately articulate the threat. But it went further than this. One could argue that ozone scientists were right to raise the alarm, but still should have left the policy decisions to the government – in effect what Parsons does argue. This is also what Hans Bethe argued in the wake of Hiroshima and Nagasaki: that scientist should speak up, yes, about the threat of atomic weapons, but they would refrain from advocating a means of arms control – such as world government – lest they lose prestige by speaking too far outside their realm of expertise, prestige they might need in the future when their expertise was again needed.⁶² Bethe's argument reminds one of the most recent arguments of climate scientists, stressing that honoring

⁶² Wang, *In Sputnik's Shadow*, 24.

the science/ policy divide preserves their credibility. Yet, the boundary where technical expertise ends and politics begins is not so easy to draw.

Returning to ozone, it took a certain level of expertise to understand *what* level of reduction in the use of CFCs would protect the ozone layer, and how soon that level of reduction needed to be achieved. These were technical matters, yes, but they were also matters of policy as well. How much and how fast were questions that were both scientific and technical. Therefore, it was not only appropriate but necessary for experts to be heard, not just on the fact of ozone depletion, but also on the degree of action needed to prevent it.

In effect, what Rowland was saying was that “policy” is too capacious a word to address what needed to be done. One aspect of policy was the demand for the rapid decrease or phase out in the use of CFCs. Was it enough to reduce them a little? Or did they need to be phased out entirely? And how rapidly did that need to occur? These were policy issues, but they could only be answered through technical expertise. In essence, they were questions about *what to do*. In the case of both acid rain and ozone the answer was: reduce emissions of the pollutants that were the driving forces of the problem. This was a policy question, but it was also a scientific question, because it was a matter of science to identify the driving forces. Once you knew what the driving forces were, it was a logical – indeed a deductive – consequence that they had to be controlled, and it took a scientist to determine what levels and rates of reductions were needed. A second aspect of policy was the choice of instruments to achieve that control. Those questions required different sorts of expertise. We could call this the second sense of policy the *how to do it*: with taxes, treaties, emissions trading regimes, or other policy instruments. Ozone was controlled through an International convention, acid rain through an emissions trading regime. It took other forms of expertise to answer the question of the choice of instruments to do the job. (Although as we have already noted, some nuclear physicists became advocates for the particular instrument of international control of atomic weapons).

Rowland’s position implicated him in an implied value premise: the value of life on Earth as we know it. If one wanted to protect life on Earth, then it was necessary to prevent ozone depletion. For Rowland, the value of life was so obvious as not to need stating, so the implication – that ozone needed to be protected – was equally obvious. And no one, in fact, ever seriously argued otherwise. Yet, many of his colleagues did feel that he went too far, and some felt for that reason that he would not be an asset in the assessment process, and should not be asked to serve on ozone assessments. But how and where did scientists make that judgment? Why was it acceptable to imply – even extremely strongly – that ozone-depleting chemicals needed

to be controlled but not acceptable to say so explicitly?

For our purposes here, the significant question is how and why certain kinds of claims that go beyond the “purely scientific” are judged to be appropriately included in “scientific” judgments. Parson attributes the qualities of “modesty” and “common-sense” to the conclusion offered by some scientists that if CFC production continued unabated, stratospheric ozone would be substantially reduced. But one man’s common sense is another man’s gauntlet; today most climate scientists would say the same thing about anthropogenic climate change: that if greenhouse gas production continues unabated, dangerous anthropogenic climate change will accelerate. They might also say that given the harmful impacts of increased greenhouse gases in the atmosphere, it is common sense that greenhouse gas emissions need to be curtailed. The argument is logically parallel to the claim about CFCs, but that has not led to general acceptance or prevented attempts to challenge the epistemic authority of the IPCC.

Science/Policy v Facts/ Values

The scientific effort to distinguish between science and policy closely mirrors the traditional demarcation between facts and values. Scientists striving to remain on the “science side” of science/policy divide are striving to remain on the “fact” side of the facts/values divide. While there has been an enormous amount of ink spilled over the facts/values distinction, particularly on the matter whether or not it exists, most scientists have no doubt that it does and they believe that it is part of their job is to keep their science clear of values. Whether this is an ideal towards which one should rightly strive or a fantasy that obscures the intrinsic subjectivity of scientific work is not the question here. Rather, the questions here is to understand why scientists have taken the position they have.

It seems clear, on historical analysis, that under the increasing external political pressure of the mid-late 20th century, scientists concluded that their best way to protect themselves from criticism and attack would be to retreat from policy, and therefore, implicitly, politics. This meant developing both rhetorical and epistemic strategies that articulate and reinforce the presumed boundary that they promised not to transgress. This accounts for the observed historical trajectory from a period in which leading scientists, secure in their cultural position, spoke freely as to what they believed society needed to do, to the current situation in which scientists, insecure in their cultural position, insist that they do not and must not tell anyone what to do.

This historical trajectory mirrors Pierre Bourdieu’s distinction between the total intellectual and the specific expert. Bourdieu criticized what he

called intellectual prophet, or “total intellectual, the man (usually a man) who, by virtue of his position, may comment on any aspect of intellectual, political, or social life”.⁶³ His type case was Jean-Paul Sartre, but we might argue that Bohr and Einstein fit that role as well, speaking broadly on diverse issues far from the expertise that originally warranted their fame and presumably undergirded their credibility.

When scientists attempt to build demarcating boundaries, they are rejecting the ideal of the total intellectual preferring to be a specific expert, a man (or now a woman) who hews to his (or her) specific knowledge. Thus for example, when interviewed by the New York Times on the occasion of the release of the IPCC Fourth Assessment Report, an IPCC leader reiterated the IPCC conclusion that “warming was unequivocal”, but when asked what we should do about it, replied, “It’s not my role to try to communicate what should be done”.⁶⁴ When asked about this comment, former IPCC chair Robert Watson summarized the tension felt by many in the scientific community, saying on the one hand that, “Ducking the question of what is needed did weaken the impact of the report to many observers”, but on the other that one “could argue that her neutrality on the policy question provides her greater credibility as an unbiased scientist and chair”.⁶⁵

There are obvious reasons why specific experts should not stray beyond their specificity. Outside their domain of expertise, scientists often know little more than lay people and sometimes knowing less, as a consequence of their long years of specialized training and acutely focused work. (A thrust of my work with Erik Conway on the history of doubt-mongering and the construction of Potemkin science is to suggest that we should be troubled when scientists speak assertively on questions outside their specific expertise, as when a physicist makes claims about tobacco control or a climate modeler recommends nuclear energy policy).⁶⁶ When the IPCC leader declined to

⁶³ David Swartz, From critical sociology to public intellectual: Pierre Bourdieu and politics, *Theory and Society*, 32: 791–823, 2003. Swartz notes the irony that Bourdieu himself became a public intellectual, arguably of the sort he had earlier criticized.

⁶⁴ http://www.nytimes.com/2007/02/06/science/earth/06profile.html?module=Search&mabReward=relbias%3Ar%2C%5B%22RI%3A11%22%2C%22RI%3A14%22%5D&_r=0

⁶⁵ Disinterestedness here is interpreted as policy-neutrality. Before World War II it was largely interpreted as having no financial interest; see Wang, In Sputnik’s Shadow, 24.

⁶⁶ See Oreskes *Room for Debate* <http://www.nytimes.com/roomfordebate/2013/11/14/is-nuclear-power-the-answer-to-climate-change/we-need-a-new-manhattan-project-to-deal-with-climate-change>.

comment on the policy dimensions of global warming, she pre-empted (or attempted to pre-empt) the claim that her science might be biased by her political preferences, an understandable choice in the context in which climate scientists operate.

Yet our discussion should also make clear the limits of the specific expert, particularly insofar as the challenge of climate change – and many pressing issues of our day – cannot be solved by specific expertise alone. As diverse actors from Dwight Eisenhower to Bob Watson have noted, policy choices involve a good deal more than technical considerations, and the technical and the political are not always easily, or even appropriately, separated. Scientists' recommendations on such matters are not necessarily *inappropriate*, but they are often – one might even argue almost inevitably – incomplete. And some scientists, notably including those of the earlier generation who *did* give policy advice, felt that that the IPCC leader was too reticent. To give one example: former Caltech President Murph Goldberger, member of PSAC during the 1960s, and long-time member of the JASONs, felt that the IPCC had missed an important opportunity.⁶⁷

Proximate Expertise

From Neils Bohr to Sherry Rowland, scientists who defended taking a position on policy matters did so from a position we may label “epistemic proximity”, or “proximate expertise”. They argued that their particular, intimate knowledge of a problem – like nuclear weaponry or ozone depletion – qualified them to speak to the issue in a way that justified a public, cultural, intervention. This argument, I argue, gives us a basis for thinking about what the right role for scientists may be.

I wish to argue that scientists *should* generally refrain from making recommendations in areas far from their expertise, but they should not refrain from commenting on areas within their proximate expertise. In these domains, scientists, by virtue of their knowledge, are among those qualified to judge, and sometimes the most qualified to judge, what actions may be called for. Consider once again Sherry Rowland.

Sherwood Rowland was criticized by some colleagues for publicly stating that CFCs needed to be controlled. Rowland did not advocate a specific policy instrument, but some colleagues nevertheless felt that by calling for any action Rowland was over-stepping the science/policy divide. But

⁶⁷ Murph Goldberger, Discussion in SIO 286, February 6, 2007, Scripps Institution of Oceanography, University of California, San Diego.

consider this thought experiment. Imagine that Rowland and his colleagues had published their research demonstrating that chlorinated fluorocarbons (CFCs) had the potential to destroy stratospheric ozone. Imagine as well, that they had published this work as articles in peer-reviewed journals, but that like most scientific work it had been largely ignored. 30, 40 or 50 years later, dermatologists and oncologists began to notice an apparent but unexplained increase in rates of skin cancer. Epidemiologists analyzed the available data, and concluded that there was in fact an epidemic of skin cancers around the globe, especially severe in Australia, southern Chile, and among white Africans. Meanwhile, plant pathologists noticed increased UV-damage in agricultural crops; veterinarians noted increased rates of cataracts in farm animals. Scientists would have begun to search for an explanation for this strange association of human, animal, and plant pathology, and, in time, someone would have come across Rowland's work, connected the dots, and understood what was happening. Programs would then have been quickly put in place to measure stratospheric ozone, which would have demonstrated that the ozone layer had been massively depleted. But by that point, it would have been too late to do anything about it.

This scenario, while counter-factual, is not fantastic. It is essentially what did occur with asbestos and tobacco; it could easily have been the case with CFCs. Rowland and his colleagues had to be the ones to alert the world to the threat of ozone depletion – they had to be the sentinels – because there was no one else who could, for the simple reason that *there was no one else with the specific knowledge to understand the general threat*. By virtue of their epistemic proximity to the problem, these scientists were the only ones who could see it and explain it. They were the only ones who could sound an alarm. And they were the only ones who could accurately judge how urgent the problem was, and therefore how quickly society needed to adopt a solution. Their expertise was specific, but they needed to speak in a general way. By virtue of their epistemic proximity, they became the relevant public intellectuals. Perhaps we could call them “specific public intellectuals”.

One might argue that it is one thing to say, “CFCs can destroy the ozone layer that protects life on Earth from damaging UV light” (a statement of scientific fact) and “Therefore we must take steps to control CFCs” (a policy recommendation). This demarcation would fit the IPCC's current notion of policy relevant (this can happen) and policy-prescriptive (we need to control CFCs). But the fact is, the second statement is a direct consequence of the scientific information, a consequence that requires scientific understanding to deduce. Drawing on the traditional deductive-nomological model, we might say that the need to control CFCs is a deductive consequence

of the general conclusion that CFCs destroy ozone. We might put it this way: CFCs destroy ozone. Ozone protects us. Therefore, if we want to continue to benefit from the protection ozone offers us, we must control CFCs. This, of course is what Rowland did say. So we might go further: If we know the rate at which CFCs destroy ozone, then we might also say that CFCs need to be controlled within a certain time frame. This is what the NRC committees and the ozone trends panels were grappling with. They were using their scientific expertise to understand causes and consequences – and the rates at which those causes operate – something that scientists do every day. It is something that is very much part of science as traditionally understood.

One might draw a line between conclusions from the science versus recommendations as to how to achieve social and political goals. This is not a question of value-neutrality; any claim that the ozone layer should be protected – or that dangerous climate change should be stopped – is inherently value-based. It is rather a question of epistemic proximity: that scientists are epistemically proximate to certain questions and by virtue of that proximity in a position to judge the consequences of certain forms of actions or inaction. It does not mean that their views are necessarily correct, nor that they have adequately understood, much less incorporated, the complex social, political, economic, ethical, religious or aesthetic considerations that may be involved. But it does mean that their views are relevant, and it is not necessarily wrong for them to be articulated.

To return to the IPCC leader, here is what a group of graduate students, having discussed the issue in class, concluded that she might have said that day to *The New York Times*:

Well, I'm a scientist, so it's not for me to recommend specific policy instruments. But I can tell you this. We know what is causing global warming: it's increased greenhouse gases in the atmosphere. So whatever we do, we need to control greenhouse gases. And that's not just my opinion, it's the conclusion of the IPCC.⁶⁸

This final point brings us back to the question of consensus. U.S. National Research Council assessments of scientific evidence are called “consensus” reports, and consensus is an implied (if not explicit) goal of most (if not virtually all) assessments. The reason for this is now evident. Articulating conclusions as the consensus of an inclusive community of experts marks

⁶⁸ Graduate students in SIO 286, February 6, 2007, Scripps Institution of Oceanography, University of California, San Diego.

those conclusions as knowledge, rather than opinion. Bohr and Einstein spoke as men, the IPCC speaks for science.⁶⁹

But speaking for science necessarily also means that the moral and ethical considerations of the issue at stake have been expunged. It remains a challenge for scientists to find an appropriate way to communicate the moral implications of their technical work.

⁶⁹ And if consensus cannot be reached, it means that the science is not settled and we don't yet quite know what is going on, and more research really is needed.