The Presumptions of Expertise: The Role of *Ethos* in Risk Analysis

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The civilian nuclear power enterprise in the United States has had a short and not very happy life. There was an initial period of slow development from the late 1940s through the late 1960s, a very brief period of rapid growth that lasted less than ten years, and then an unforeseen rapid decline beginning in the mid-1970s that was only hastened by the Three Mile Island accident in 1979; this decline has been called "one of the most stunning reversals of fortune in the history of American capitalism."[1] A *Forbes* article declared in 1985 that "for the U.S., nuclear power is dead," and the scientist who chaired the National Research Council's 1992 report on nuclear power declared a year later that the future of nuclear power in the U.S. "looks grim."[2] Although some 20 percent of the nation's electric power in 2003 was supplied by 104 nuclear plants, nuclear power has not been prominent on the public agenda: in the 1990s, a slowed growth in the demand for energy, reduced funds for R&D, the deregulation of the power industry in 1992, and reduced prices for fossil fuels made the nuclear option less important.[3] There has been some

1. Mark Hertsgaard, *Nuclear, Inc.: The Men and Money Behind Nuclear Energy* (New York: Pantheon, 1983), p. 67.

2. James Cook, "Nuclear Follies," *Forbes* (February 1985): 83; John F. Ahearne, "The Future of Nuclear Power," *American Scientist* 81:1 (1993): 24.

3. Energy Information Administration, U.S. Department of Energy, *Monthly Energy Review, September*, September 26, 2003, PDF file, U.S. Department of Energy, available at *http://www.eia.doe.gov/mer/* (October 11, 2003); Robert J. Duffy, *Nuclear Politics in America: A History and Theory of Government Regulation*, Studies in Government and Pub-

Configurations, 2003, 11:163–202

talk of a "second nuclear era," with new plant designs that are "inherently safe," new approaches to regulation, and changed energy economics.[4] However, no plants have been ordered since 1978, and all forty-one orders placed since 1973 were canceled or rejected by state governments; of 259 orders ever placed, 124 were canceled, the last two in 1995. The last operating license was issued in 1996, for a plant whose construction permit was originally issued in 1973.[5]

The short, controversial life of the nuclear industry leaves at least three legacies: the problem of decommissioning worn-out plants, the necessity of long-term waste storage, and the practice of risk analysis—this last legacy no less important than the other two for being less material. Risk analysis originated in the efforts of the federal government to sell the nuclear option to both the electric power companies and the public in the 1950s and 1960s, and the nuclear industry contributed much to its advancing methods.[6] The field developed and expanded rapidly with the environmental and consumer legislation of the 1960s and 1970s—more than thirty major federal laws concerning health, safety, and the environment were passed between 1965 and 1985, many of them requiring the regulation of hazards and thus inviting, and often mandating, risk analysis.[7] Although risk analysis began in the nuclear power enterprise

lic Policy (Lawrence: University Press of Kansas, 1997); Matthew L. Wald, "Reactors: Healthy but Dying," *New York Times*, March 7, 1999, sect. 4, p. 16.

4. Bernard I. Spinrad, "U.S. Nuclear Power in the Next Twenty Years," *Science* 239 (1988): 707–708; U.S. National Research Council, *Nuclear Power: Technical and Institutional Options for the Future* (Washington, D.C.: National Academy Press, 1992); Alvin M. Weinberg, *Nuclear Reactions: Science and Trans-Science* (New York: American Institute of Physics, 1992).

5. U.S. Department of Energy, *Commercial Nuclear Power 1991: Prospects for the United States and the World* (Washington, D.C.: Energy Information Administration, 1991); U.S. Nuclear Regulatory Commission, *Information Digest 1998 Edition*, 1998, available at *http://www.nrc.gov/NRC/NUREGS/SR1350/V10/index.html* (September 3, 1999).

6. For the origin of risk analysis in nuclear power, see Allan Mazur, "Bias in Risk–Benefit Analysis," *Technology in Society* 7 (1985): 25–30. H. W. Lewis maintains that "the nuclear energy community is responsible for many of the most impressive advances in risk analysis" (*Technological Risk* [New York: Norton, 1990], p. 21). Starr made a similar claim in congressional testimony after the Three Mile Island accident: "The nuclear industry has pioneered methods for estimating risks (and attempting to determine their acceptability) that are being carefully studied and copied by other major industries" (Chauncey Starr, *The Utility Industry Response to the TMI Accident* [Washington, D.C.: U.S. House Committee on Science and Technology, Subcommittee on Energy Research and Production, 1979], p. 15).

7. Vincent T. Covello and Jeryl Mumpower, "Risk Analysis and Risk Management: An Historical Perspective," *Risk Analysis* 5:2 (1985): 103–120; Dominic Golding, "A Social and Programmatic History of Risk Research," in *Social Theories of Risk*, ed. Sheldon

and drew from safety and reliability engineering, it has become interdisciplinary, drawing from such intellectual traditions as operations research and systems analysis, public policy, actuarial statistics, toxicology, and epidemiology.[8]

Risk analysis acquired much of its disciplinary form and by-now-pervasive influence through governmental support and implementation. The National Science Foundation began a major funding program for risk analysis in 1979, in response to a request by the U.S. House Committee on Science and Technology; this program had significant impact on the development of the field.[9] In 1983, in response to a request by the Food and Drug Administration, the National Research Council published a report on the issues and problems involved in using risk assessment in the regulatory process.[10] This report, informally referred to as the "Red Book," helped create what has become the "standard account" of risk analysis, which maintains that the scientific process of risk *assessment* should be separate from the subsequent political process of risk *management*.[11] The Red Book conceived of risk assessment as having four stages: hazard identification, dose-response assessment, exposure assessment, and risk characterization; and it described risk management as a process that builds on the results of risk assessment but also involves "social,

Krimsky and Dominic Golding (Westport, Conn.: Praeger, 1992), pp. 23–52. One source lists thirty-three federal statutes as of 1984 that "arguably" require some form of risk assessment (although none of them uses the term itself) (Dennis J. Paustenbach, ed., *The Risk Assessment of Environmental and Human Health Hazards: A Textbook of Case Studies* [New York: Wiley, 1989], p. xi).

8. See the discussion in Arie Rip, "The Mutual Dependence of Risk Research and Political Context," *Science and Technology Studies* 4:3/4 (1986): 3–15. The Society for Risk Analysis, which publishes the journal *Risk Analysis*, was formed in 1980 to promote communication among these various traditions.

9. Golding, "Social and Programmatic History" (above, n. 7).

10. U.S. National Research Council, *Risk Assessment in the Federal Government: Managing the Process* (Washington, D.C.: National Academy Press, 1983).

11. Sheila Jasanoff, "Bridging the Two Cultures of Risk Analysis," *Risk Analysis* 13:2 (1993): 123–129; Rip, "Mutual Dependence" (above, n. 8), p. 11. According to Golding, the term "risk analysis" was used by the Society for Risk Analysis as a broad term that encompassed both risk assessment and risk management (Golding, "Social and Programmatic History" [above, n. 7], p. 35), but usage varies. Shrader-Frechette describes the "standard account" of risk assessment as having three stages: risk identification, risk estimation, and risk evaluation (K. S. Shrader-Frechette, *Risk and Rationality: Philosophical Foundations for Populist Reforms* [Berkeley: University of California Press, 1991], p. 55). "Estimation" covers the middle two stages of the other model, dose-response and exposure assessment. She agrees that the standard account separates the process of risk assessment from a subsequent process of risk management.

economic, and political concerns" in order to "weigh policy alternatives and select the most appropriate regulatory action."[12] Continuing congressional concern about the scientific basis for risk analysis led to provisions in the 1990 Clean Air Act Amendments calling for another report, as well as a federal commission to investigate the uses of risk analysis in federal regulatory programs.[13] The commission's final report offered program-by-program recommendations to the EPA, OSHA, USDA, and other agencies, as well as a model of risk management that it hoped would become as influential as the 1983 model of risk assessment.[14]

Risk has become a central condition of our technoscientific culture, possibly a defining one, as Ulrich Beck has claimed. The "risk society," as he calls it, is dominated by the production and distribution of risks—in contrast to industrial society, which is dominated by the production and distribution of wealth. The domination of risk is the result of advanced industrialization and the increasing necessity to deal with problems created by "techno-economic development itself," or "reflexive modernization"; in the risk society, the "positive logic" of acquisition is replaced by a "negative logic of disposition, avoidance, denial, and reinterpretation."[15] If risk is in fact a defining condition of late modernity, the rapid rise and diffusion of risk analysis is not surprising. Although it is usually considered to be a technical methodology, I discuss risk analysis here as a *discourse*, as a way of conceptualizing and communicating about a range of issues at the interface of science, technology, public policy, and social values. Developed originally as a tool of those with an axe to grind, it has been used as a persuasive strategy as well as a method of inquiry and an algorithm for decision making. Risk analysis has been separated from risk communication by those who practice both, but my argument in this essay implies that this is a false distinction, that risk analysis *is* a form of communicating about risk—in other words,

12. National Research Council, *Risk Assessment* (above, n. 10), p. 3.

13. U.S. National Research Council, *Science and Judgment in Risk Assessment* (Washington, D.C.: National Academy Press, 1994); Presidential/Congressional Commission on Risk Assessment and Risk Management, *Final Report,* vol. 1: *Framework for Environmental Health Risk Management* ([Washington, D.C.]: The Commission, 1997).

14. Gilbert S. Omenn, *Report on the Accomplishments of the Commission on Risk Assessment and Risk Management*, September 26, 1997, website, available at *http://www.riskworld.com/Nreports/1997/risk-rpt/miscinfo/nr7mi002.htm* (February 14, 2001).

15. Ulrich Beck, *Risk Society: Towards a New Modernity*, trans. Mark Ritter, Theory, Culture, and Society, ed. Mike Featherstone (London: Sage, 1992), pp. 19, 26. Anthony Giddens has made a similar case about the role of risk in modernity in *The Consequences of Modernity* (Stanford, Calif.: Stanford University Press, 1990).

that it has rhetorical import.[16] In developing my discussion, I draw upon some descriptive and analytical resources from a distinctly preindustrial society: those of classical rhetoric. Aristotle, in particular, has some revealing things to say about the discourse of risk. He was keenly interested in change and contingency, in how human debate deals with uncertainty, or, in his expression, with "things that seem capable of admitting two possibilities."[17]

In particular, it is what Aristotle called deliberative discourse that concerns what we call risk, that is, the possible outcomes of future events (I.iii.4); and, like risk analysis, deliberation concerns the advantageous and the harmful (I.iii.5). Aristotle considered deliberative discourse the noblest form of rhetoric ("deliberative subjects are finer and more important to the state than private transactions," such as forensic litigation: I.i.10). He also noted that because deliberative discourse is concerned with the future, it cannot rely much on enthymemes—that is, on deductive reasoning from givens, since the "givens" of the future are unknown; instead, the deliberative rhetor must use examples from the past, projecting them inductively into the future (III.xvii.5). Finally, Aristotle tells us that in deliberation about the future, the credibility of the speaker is especially important, more than in judgment about the past (II.i.4). His reasoning is that someone who seems fair-minded is readily believed in any situation, and in situations of uncertainty about the future there may be little else to go on (I.ii.4). Indeed, because all forms of rhetoric concern contingent knowledge, the most decisive influence on persuasion is the character of the persuader (I.ii.4): beyond what can be demonstrated factually, we put our trust in people who have good sense (*phronêsis*), good moral values (*aretê*), and goodwill toward us (*eunoia*) (II.i.5–7). These are the constituents of *ethos*, as he defines it: the character of the persuader understood against the character and conventions of the culture. Aristotle thus treats *ethos* as the default appeal, the one we rely on when others are insufficient or unavailable.

In this essay, I explore ways in which rhetorical *ethos* operates in risk analysis, looking specifically at its origins in the nuclear power debate—the rhetorical environment in which it developed, and the specific rhetorical challenges it faced. My focus will be the most

16. Compare Theodore M. Porter's discussion of quantification technologies as "strategies of communication" in *Trust in Numbers: The Pursuit of Objectivity in Science and Public Life* (Princeton, N.J.: Princeton University Press, 1995), p. viii.

17. Aristotle, *On Rhetoric: A Theory of Civic Discourse*, trans. George A. Kennedy (New York: Oxford University Press, 1991), I.ii.12; subsequent parenthetical references are to this work.

influential risk analysis document, the Atomic Energy Commission's 1975 *Reactor Safety Study* (*RSS*), sometimes known as the Rasmussen report (after its director, Norman Rasmussen), or as WASH-1400, its original AEC document number.[18] Although controversial, the *RSS* set the research agenda for subsequent risk analysis of nuclear power plants and became a major precedent for the dissemination of risk analysis to other areas of decision making. The *RSS* introduced the use of expert opinion in arguments about risk, in the form of "subjective probabilities," a form of argument that is now embedded in the widely used Probabilistic Risk Analysis methods. The reliance on expertise is an argument from authority, and thus, in rhetorical terms, a signal that *ethos* is an important mode of appeal.[19]

The Rasmussen report's reliance on expert opinion is particularly interesting in view of the traditional rejection by science of the argument from authority, and it acquires even more significance in view of the change in American public values in the 1970s, which began to reject the long-standing presumption in favor of science and technology. We can thus understand the *RSS*, and the use of subjective probabilities, as part of an ongoing negotiation over the burden of proof in public argument about risk. Risk analysis was born in a very tight rhetorical corner, boxed in by four severe constraints: (1) political pressure to produce a risk analysis friendly to nuclear power by a congressional deadline; (2) the need to use "expert opinion" in lieu of failure data because there were few data from actual reactor failures; (3) long-standing skepticism of engineers and scientists about the value of opinion; and (4) a dramatic reduction in public willingness to defer to technology and its experts. In response to these constraints, risk analysis retreated into claims based on the authority of its own expertise and an insistence that public conceptions of risk were inadmissible; risk was thus monopolized by those expert in technical fields like nuclear engineering or toxicology, or by the increasingly professionalized expert field of risk analysis itself. Risk analysis became a discourse of experts, in which the assumptions, interests, values, and beliefs of experts are deployed to answer public questions about new technologies, government policies, and human behavior. These responses involve transformations of *ethos*—

18. U.S. Nuclear Regulatory Commission, *Reactor Safety Study: An Assessment of Accident Risks in U.S. Commercial Nuclear Power Plants* (Washington, D.C.: 1975) (henceforth, *RSS*). Norman Rasmussen was a nuclear engineer at MIT.

19. Risk analysis also invokes rhetorical *pathos*, appeals to emotion, as I have argued in Carolyn R. Miller, "The Roots of Risk Analysis in Rhetorical *Pathos*" (conference paper presented at the Speech Communication Association, San Diego, 1996).

transformations that conflate *ethos* with *logos* and at the same time narrow the scope of *ethos* considerably from the original Aristotelian conception.

"The First Modern Risk Analysis"

The *Reactor Safety Study* is considered a landmark in risk analysis. It was commissioned in 1972 by the Atomic Energy Commission, cost $4 million, and involved seventy "man-years" of effort, by its own account.[20] The resulting report is 150 pages long, plus eleven appendices and an executive summary, all presented in nine volumes. Its stated purpose was "to assess the risks to the public from potential accidents in nuclear power plants of the type being built in the United States today" and, by producing "a more realistic assessment of those risks than has been provided in earlier work . . . help to dispel some of the existing confusion."[21] It concluded, among other things, that the probability of a core melt was about one in 20,000 per reactor per year, the probability of an accident resulting in ten or more fatalities was one in 3 million per reactor per year, and the probability of one thousand or more fatalities was one in 100 million; for comparison, it also pointed out that the chance of dying from a reactor accident is about the same as the chance of being struck by a meteor.[22] The Rasmussen report has been called "the classic reactor risk analysis study," "the first modern risk analysis," "the most complete hazard analysis ever accomplished," a "pioneering" effort to model uncertainty, and "*the* milestone study."[23]

The Rasmussen report was not the first safety study of civilian nuclear power, however, but the third.[24] All three were undertaken to

20. *RSS*, Executive Summary, p. 5. Daniel Ford provides a detailed behind-the-scenes account of the preparation, release, and reception of this report in *The Cult of the Atom: The Secret Papers of the Atomic Energy Commission* (New York: Simon and Schuster, 1982), pp. 137–173; he led a ten-year investigation of nuclear safety as executive director of the Union of Concerned Scientists, and is considered by some nuclear power proponents to be biased.

21. *RSS*, Executive Summary, p. 1.

22. Ibid., pp. 8, 9.

23. Robert Sugarman, "Nuclear Power and the Public Risk," *IEEE Spectrum* 16:11 (1979): 61; Rip, "Mutual Dependence" (above, n. 8), p. 8; Shrader-Frechette, *Risk and Rationality* (above, n. 11), p. 95; George Apostolakis, "The Concept of Probability in Safety Assessments of Technological Systems," *Science* 250 (1990):1361; M. R. Hayns, "The Evolution of Probabilistic Risk Assessment in the Nuclear Industry," *Process Safety and Environmental Protection* 77.B3 (1999): 120.

24. The previous studies were WASH-740, U.S. Atomic Energy Commission, *Theoretical Possibilities and Consequences of Major Accidents in Large Nuclear Power Plants* (1957), and

provide support for congressional passage or periodic renewal of the Price-Anderson Act, which has been called "one of the most extraordinary subsidies in congressional history."[25] Passed initially in 1957, Price-Anderson limited the liability of reactor operators in the case of a reactor accident to a maximum of $560 million, guaranteed by the federal government beyond whatever amount private insurers were willing to provide (initially $60 million); it thus protected the owners and operators of the plant from any liability at all.[26] This support was deemed necessary because without it electric power companies were unwilling to invest in this untried and expensive technology. The federal government had invested enormous resources in promoting the peaceful uses of nuclear energy in the 1950s—partly in order to circumvent limits on defense spending, to camouflage the influence of the military on nuclear technology development, and to counteract negative opinion about the use of atomic weapons in World War II.[27] Price-Anderson was designed to expire after ten years, by which time it was expected that the safety record and operating experience would be such that it would no longer be needed. In fact, however, it has been renewed regularly, at approximately ten-year intervals. In 2003 there was talk of a permanent renewal, although a twenty-year renewal seems more likely, but the legislation is still pending in early 2004.[28]

WASH 1250, U.S. Atomic Energy Commission, *The Safety of Nuclear Power Reactors (Light Water-Cooled) and Related Facilities* (1973). The latter was not a full, quantified risk study but a descriptive report released in lieu of a secret study begun in 1964 to update WASH 740 but never released. See the discussion in Ford, *Cult of the Atom* (above, n. 20).

25. Hertsgaard, *Nuclear, Inc.* (above, n. 1), p. 33.

26. Steven L. Del Sesto, *Science, Politics, and Controversy: Civilian Nuclear Power in the United States, 1946–1974* (Boulder, Colo.: Westview Press, 1979), pp. 58–59; William C. Wood, *Insuring Nuclear Power: Liability, Safety, and Economic Efficiency* (Greenwich, Conn.: JAI Press, 1982), p. 5.

27. Lee Clarke, "The Origins of Nuclear Power: A Case of Institutional Conflict," *Social Problems* 32:5 (1985): 476, 485.

28. The 1988 reauthorization extended the Act for fifteen years (until 2002), increased the compensation limit to $9.43 billion, and created a Presidential Commission on Catastrophic Nuclear Accidents to decide how the compensation should be distributed: U.S. Department of Energy, *Report to Congress on the Price-Anderson Act*, 1998, website, Office of the General Counsel, available at *http://www.gc.doe.gov/Price-Anderson/public-comments/Nuclear%20Energy%20Agency/PAA-REP.htm* (September 3, 1999). In 2002, Price-Anderson renewal was approved in both the House and Senate but died in energy bill negotiations, so early in 2003 it was renewed through the end of the year. In late 2003, the House authorized a twenty year extension as part of a comprehensive energy bill, but the legislation stalled in the Senate, which is expected to take it back up in early 2004.

The *RSS* was begun in anticipation of the 1975 Price-Anderson renewal because it was clear that there would be greater controversy than before, and the two earlier studies had addressed only the consequences of reactor failures, not the probabilities.[29] The anticipated controversy and the congressional deadline combined to create the first of the rhetorical constraints for the *RSS*. The public concern about nuclear power during this period also led to the Energy Reorganization Act of 1974, which separated two conflicting functions of the AEC, regulation and promotion, by replacing it in January 1975 with two agencies: regulation was assigned to the Nuclear Regulatory Commission (NRC), and promotion to the Energy Research and Development Administration (ERDA), later the Department of Energy (DOE). As it turned out, the Rasmussen analysis took much longer than planned, and because of the Price-Anderson congressional deadline and AEC/NRC anxiety to allay public concern, the final schedule short-changed both internal and external review processes.[30] In spite of the final rush to complete it, copies of the final report were not distributed in time to be part of the record for the Price-Anderson renewal.[31]

The *RSS* was indeed controversial. It was produced during a period of intense public concern about health, safety, and the environment, and of increasing opposition to nuclear power: five years after the first Earth Day, but four years before Three Mile Island. AEC officials had begun releasing statements about the findings in 1974, well before the study was completed in late 1975, and the NRC continued promoting it aggressively after its release, through a publicity campaign and a Q&A Executive Summary, distributed separately.[32] Reaction in the scientific community has been described as "turbulent."[33] The report was reviewed by several scientific groups, which found

29. Ford, *Cult of the Atom* (above, n. 20), pp. 136–137; Wood, *Insuring Nuclear Power* (above, n. 26), p. 6.

30. Ford, *Cult of the Atom* (above, n. 20), pp. 143–169; Subcommittee on Energy and the Environment of the Committee on Interior and Insular Affairs, U.S. House of Representatives, *Observations on the Reactor Safety Study* (Washington, D.C.: U.S. Government Printing Office, 1977), p. 20.

31. Wood, *Insuring Nuclear Power* (above, n. 26), p. 7.

32. Ford, *Cult of the Atom* (above, n. 20), pp. 157–160. See also Morris Udall's comments in Subcommittee on Energy and the Environment of the Committee on Interior and Insular Affairs, U.S. House of Representatives, *Reactor Safety Study Review* (Washington, D.C.: U.S. Government Printing Office, 1979), p. 80.

33. Roger M. Cooke, *Experts in Uncertainty: Opinion and Subjective Probability in Science* (New York: Oxford University Press, 1991), p. 28.

many technical problems.[34] Roger M. Cooke classified the problems into three types: falsification of the report's predictions by experience;[35] challenges to the methodology used to calculate risk; and questions about the objectivity of the report. After scientific criticism heightened the controversy, the House Committee on Interior and Insular Affairs requested a formal external review, which was commissioned in 1977 by the NRC. This review (often called "the Lewis report" after its chairman, physicist H. W. Lewis) agreed with other critics that the Executive Summary was inaccurate and should not be used to represent the report. The review also concluded that the *RSS* suffered from "an inadequate data base, a poor statistical treatment, an inconsistent propagation of uncertainties throughout the calculation," as well as "understated" error bands for the probability of a core melt, although the reviewers were unable to say whether the probability as given was too high or too low.[36] In summarizing its technical criticism, the Lewis report was blunt:

> WASH-1400 is defective in many important ways. Many of the calculations are deficient when subjected to careful and probing analysis, with the result that the accuracy of many of the absolute probabilities calculated therein is not as

34. Reviews were conducted by the Study Group on Light-Water Reactor Safety, "Report to the American Physical Society," *Reviews of Modern Physics* 47, Suppl. 1 (1975); the U.S. Environmental Protection Agency, *Reactor Safety Study (WASH-1400): A Review of the Draft Report* (Washington, D.C.: EPA, 1977); the Ford Foundation (contracting with MITRE Corporation), published as Nuclear Energy Policy Study Group, *Nuclear Power Issues and Choices* (Cambridge, Mass.: Ballinger, 1977); and the Union of Concerned Scientists, *The Risks of Nuclear Reactors: A Review of the NRC Reactor Safety Study* (Cambridge, Mass.: Union of Concerned Scientists, 1977), among others. Summarizing some of these scientific studies in his own attack on the *RSS*, Ralph Nader claimed that "the real world refutation of the Reactor Safety Study is the Price-Anderson Act" ("The Reactor Safety Controversy," *Quality Progress* 8:12 [1975]: 25).

35. The Union of Concerned Scientists noted that several scenarios to which the *RSS* had assigned extremely low probabilities had already happened, some of them more than once: see Roger M. Cooke, "Risk Assessment and Rational Decision Theory," *Dialectica* 36:4 (1982): 334–335; Amory B. Lovins and John H. Price, *Non-Nuclear Futures: The Case for an Ethical Energy Strategy* (San Francisco: Friends of the Earth International, 1975), p. 59.

36. Risk Assessment Review Group, *Report to the U.S. Nuclear Regulatory Commission* (Washington, D.C.: U.S. Nuclear Regulatory Commission 1978), pp. vi, vii. The Lewis report is written in a remarkably direct and personable style, making it one of the most refreshing technical reports I have ever read. For example, about one mathematical assumption, it comments: "The degree of arbitrariness in this procedure boggles the mind" (p. 8). In turn, it calls the *RSS* "inscrutable," saying "it is very difficult to follow the detailed thread of any calculation through the report. This has made peer review very difficult" (p. vii).

> good as claimed. One key deficiency is the use by the study team of some methodological and statistical assumptions that lack credibility. Therefore, the absolute values of the risks presented by the Report should not be used uncritically either in the regulatory process or for public policy purposes.[37]

On the basis of the Lewis report, the NRC distanced itself from the *RSS*. In an unsigned statement issued January 18, 1979, the NRC withdrew its endorsement of the Executive Summary, agreed with criticisms of the peer-review process, and stated that it no longer "regard[ed] as reliable the Reactor Safety Study's numerical estimate of the overall risk of reactor accident."[38]

The Lewis report was by no means entirely negative, however. It praised the *RSS* as a "substantial advance over previous attempts to estimate the risks of the nuclear option,"[39] and declared it "successful in the provision of a logical framework for the discussion of reactor safety, information about the relative probabilities of various accident sequences, and the beginning of an effort to provide absolute probabilities."[40] The "framework" consisted of a pair of methods known as fault-tree analysis and event-tree analysis, which had been devised as reliability engineering methods in the aerospace programs of NASA and the Department of Defense. The two methods are essentially mirror-images of each other: event-tree analysis begins with an event (such as a valve failure) and traces forward in time all the possible causal consequences of that event; fault-tree analysis begins with a failure and traces backward in time all the conditions that could lead to that failure. Both use graphical "tree" forms of representation, with binary branching (the valve fails or does not fail, the core melts or does not melt, the containment holds or does not hold); at each branch point, the probabilities of each alternative are determined. The probability of operator error or maintenance error can also be included, as well as the probabilities of casualties and damages if radioactivity is released. Thus, the probability of a particular chain of events can be calculated by multiplying the probabilities of all contributing alternatives together.[41] The use of the fault-

37. Ibid., p. 3.

38. Reprinted in Subcommittee on Energy and Environment, *Reactor Safety Study Review* (above, n. 32), p. 342.

39. Risk Assessment Review Group, *Report* (above, n. 36), p. viii.

40. Ibid., p. 2.

41. Ford discusses some of the weaknesses of these methods, weaknesses that had been apparent for some time to engineers in the aerospace and weapons programs. He notes

tree and event-tree methods was hailed as a major achievement in nuclear risk analysis, and the Lewis report found the methodology to be "the best available tool with which to quantify [accident] probabilities" associated with nuclear reactors.[42]

These methods were the foundation for Probabilistic Risk Assessment (PRA, sometimes called Probabilistic Safety Assessment, PSA), the approach that dominates nuclear risk assessment today.[43] Although the Lewis report supported this approach and encouraged its use in regulatory decisions, and although the subsequent NRC statement on the *RSS* officially supported "the extended use of probabilistic risk assessment in regulatory decisionmaking,"[44] it took the accident at Three Mile Island to motivate serious NRC attention to developing and applying these techniques. The Three Mile Island accident occurred just two months after the NRC issued its policy statement about the *RSS*, and alert industry and regulatory analysts realized that Rasmussen's team had analyzed a sequence of events very similar to the one that led to the accident; this fulfillment of a hypothetical sequence that was beyond those normally considered

that "approximately 20 percent of the ground-test failures in the Apollo program, and more than 35 percent of the in-flight malfunctions, were failures that had not previously been identified as 'credible' possibilities" (*Cult of the Atom* [above, n. 20], p. 145). One can never be sure that all "significant" pathways have been identified, and one cannot include design errors (if one could, one would change the design); in addition, the method does not allow for "continuous variables" (a valve that leaks slowly, rather than failing completely). Cooke also summarizes criticisms of these methodologies raised by the Union of Concerned Scientists and the Ford MITRE study ("Risk Assessment" [above, n. 35], pp. 338–339). In the judgment of the Lewis Commission, however, "it is incorrect to say that the event-tree/fault-tree analysis is fundamentally flawed, since it is just an implementation of logic" (Risk Assessment Review Group, *Report* [above, n. 36], p. 4).

42. Chauncey Starr, *Oversight Hearing on Reactor Safety Study (Rasmussen Report)* (U.S. House Committee on Interior and Insular Affairs, Subcommittee on Energy and the Environment, 1976), pp. 180–181; Risk Assessment Review Group, *Report,* p. viii.

43. Hayns, "Evolution of Probabilistic Risk Assessment" (above, n. 23).

44. Subcommittee on Energy and the Environment, *Reactor Safety Study Review*, (above, n. 32), p. 342. In spite of this official support, the internal NRC response seems to have been somewhat different. A later NRC report notes that after the Lewis report, "the staff of the NRC was directed to avoid using [WASH-1400] or its methodology in regulatory applications" (U.S. Nuclear Regulatory Commission, *Special Committee Review of the Nuclear Regulatory Commission's Severe Accident Risks Report* [Washington, D.C.: U.S. Nuclear Regulatory Commission, 1990], p. 2). Rodney P. Carlisle provides a detailed account of the development and acceptance of PRA methods in the nuclear industry and regulatory community during the latter half of the twentieth century in "Probabilistic Risk Assessment in Nuclear Reactors: Engineering Success, Public Relations Failure," *Technology and Culture* 38 (1997): 920–941.

in the licensing process restored some credibility to the *RSS*. Furthermore, the two official investigations of the Three Mile Island accident both endorsed the use and development of PRA techniques.[45]

A sequence of major studies and reports in the next decade developed standards for nuclear plant PRA, applied PRA to licensing decisions and plant design, improved the models for core melt progression and containment performance, added external events such as earthquakes and aircraft accidents to the probability calculations, and developed methods to incorporate the effects of human actions both in initiating accidents and in recovering from them.[46] In 1984, Rasmussen, writing with Saul Levine (who had been the AEC staff director of the *RSS*), concluded that subsequent PRA studies had not contradicted the insights gained in the *RSS*, except that the contributions of external events had been underestimated.[47] Later assessments, however, claim that PRA has in fact modified earlier conclusions: the probability of core damage is higher than the industry had previously believed, the risk to the public is lower than previous estimates, and the risks from external events vary significantly from plant to plant.[48] Although the findings of the *Reactor Safety Study* may have been superseded, it survives as an originating conception, both methodologically and substantively. It had a great deal to do with making risk analysis the influential enterprise it is today. It argued, perhaps for the first time, that risk is a technical entity, to be measured and managed by experts. And, as I will claim here, it exemplifies and promotes the presumption that expertise can substitute for *ethos*.

Expert Opinion in the *RSS: Ethos* as *Logos*

As the Lewis report emphasizes several times, the *Reactor Safety Study* attempted an analysis of great complexity, isolating "from an impossibly large number of accident sequences, a relatively small number" for analysis.[49] To conduct these analyses, the Rasmussen group adopted a quantitative definition of risk that has become standard: risk is equal to the probability of an event (or frequency

45. S. Levine and N. C. Rasmussen, "Nuclear Plant PRA: How Far Has It Come?" *Risk Analysis* 4:4 (1984): 247–254; J. S. Wu and G. E. Apostolakis, "Experience with Probabilistic Risk Assessment in the Nuclear Power Industry," *Journal of Hazardous Materials* 29 (1992): 313–345.

46. Hayns, "Evolution of Probabilistic Risk Assessment" (above, n. 23).

47. Levine and Rasmussen, "Nuclear Plant PRA" (above, n. 45), p. 252.

48. Carlisle, "Probabilistic Risk Assessment" (above, n. 44), p. 938; Wu and Apostolakis, "Experience with Probabilistic Risk Assessment" (above, n. 45), pp. 319–321.

49. Risk Assessment Review Group, *Report* (above, n. 36), p. 14.

per unit of time) multiplied by the undesirable consequences of that event.[50] Thus, in their example using data from 1971, the risk of death in a car accident each year is calculated as 15,000,000 accidents per year (frequency) times 1 death per 300 accidents (consequences), or 50,000 deaths per year; expressed as a probability, 50,000 deaths per year divided by the relevant population of 200 million, the risk of death is 0.00025 per person per year.[51] Because risk is quantified as the product of two values, the risk of events with high probabilities but low consequences (such as car accidents, which have low consequences in that only a few people may be killed in a single accident) may be *equal to* the risk of events with low probabilities but high consequences (such as a meteor hitting the earth, which might kill thousands of people at once). The report acknowledges that this equivalence is not intuitively or emotionally satisfactory for many people, and that risks are not always equally or randomly distributed across a population (pp. 11–12). But because analyses of such factors, which the *RSS* calls "risk acceptability," were still under development, they were not considered.[52]

The Rasmussen group divided its overall work into three major tasks: (1) identifying potential accidents and their probabilities and consequences in terms of radioactive releases to the environment; (2) calculating the dispersion of radioactivity in the environment and its effects on health and property; and (3) combining the consequences and probabilities determined from the second task to determine the overall risk from potential accidents, and comparing these to a variety of non-nuclear risks (p. 41). For the first task, which constituted the major part of the work, accident sequences had to be identified and then their probabilities and consequences determined. The identification of possible accident sequences was based on design specifications and the construction of event trees, but the determination of probabilities and consequences was made difficult by the relatively short history of commercial nuclear technology, and in particular by the fact that failures were rare. In general, for

50. This definition was apparently adopted from 1967 work by a British physicist employed by the U.K. Atomic Energy Authority; see F. R. Farmer, "Reactor Safety and Siting: A Proposed Risk Criterion," *Nuclear Safety* 8:6 (1967): 539–548; Wu and Apostolakis, "Experience with Probabilistic Risk Assessment" (above, n. 45), p. 314.

51. *RSS*, p. 9. Subsequent parenthetical references are to this work.

52. In fact, the differences between quantitative, scientific approaches to risk and public acceptance of risks stimulated a great deal of research in the 1980s on what came to be known as "risk perception"; see Paul Slovic, "Perception of Risk," *Science* 236 (1987): 280–285.

high-probability events, historical records can provide actuarial data on both frequency and consequences; even for new systems with short histories, experimental work can generate the data needed. However, for low-probability events, data are more difficult to obtain. Frequencies may sometimes be inferred by breaking a rare event into a series of more likely events, and this approach is the basis of the fault-tree and event-tree techniques used in the *RSS* (p. 13). In other cases, frequencies and consequences may be extrapolated from the historical record of similar but less severe and less rare events, such as using 50-year floods to model 100-year or even 1,000-year floods. Using these principles, claims the *RSS*, "it is possible to make reasonable estimates of the probabilities of very unlikely events" (p. 13).

But the simple fact was that in determining the risk of a core melt, the Rasmussen group did not have much to go on, and this is the second of the major rhetorical constraints that it faced. As the Executive Summary itself proclaimed, "It is significant that in some 200 reactor-years of commercial operation of reactors of the same type considered in the report there have been no fuel melting accidents" (ES, p. 6). And the Lewis report noted:

> Since [core melt] has never occurred in a commercial reactor, there are no direct experimental data on which to base an estimate. The only datum that exists is the observation that there have been no core melts in several hundred reactor-years of light water power reactor operation, and this fact provides at best an upper bound on the probability to be estimated. Therefore it is necessary to resort to a theoretical calculation of the probability. But since the system is so complex, a complete and precise theoretical calculation is impossibly difficult.[53]

In the absence of historical statistics and adequate theoretical models, the Rasmussen risk assessors turned to the best substitute they could find: expert opinion, or engineering judgment based on knowledge and experience. As the Lewis report continued the passage just above: "It is consequently necessary to invoke simplified models, estimates, engineering opinion, and in the last resort, subjective judgments."[54] Such judgments came to be called "subjective probabilities."

Appendix III of the *RSS* explains the types and sources of failure data used to assess the risk of various scenarios and consequences,

53. Risk Assessment Review Group, *Report* (above, n. 36), p. 6.

54. Ibid.

including core melt: “The failure rates and demand probabilities used in the study were derived from handbooks, reports, operating experience, and nuclear power plant experience” (III, p. 3). It is not obvious from this characterization that the data include a great many “subjective probabilities.” Two extensive fold-out tables provide the failure probabilities for 60 component parts as given by 31 sources (not all sources provide data on all components) (III, pp. 7–10). The sources are listed separately, in an extensive references section that indicates that “approximately 50” sources in addition to internal AEC operating experience were used, not all of which are independent because some cite each other (III, pp. 91–97). Some of these sources are clearly compilations of historical data: number 7, for example, contains failure data from test and research reactors, and number 11 contains data on the reliability of instruments in chemical plants. Others are quite different and can probably only be characterized as “expert opinion”: for example, number 34 is listed as a “Letter from W. F. Shopsky to D. F. Paddleford dated October 20, 1972”; number 37 is a lecture by F. M. Davies for the Safety and Reliability Directorate, Risley, U.K.; number 44 is a paper published in the October 1971 issue of the journal *Nuclear Technology* (III, pp. 94, 95). Cooke’s analysis of the use of expert opinion in the *RSS* shows that for one component, a high-quality 7.6 cm diameter steel pipe, thirteen failure-rate estimates were acquired, ranging from 5×10^{-6} to 1×10^{-10} per section per hour, a range of more than three orders of magnitude. Cooke claims that the data overall showed “extreme spreads” and clustering (that is, some experts tended to be either “optimists” or “pessimists,” with the result that their opinions were all either low or high with respect to those of other experts), and that such data cannot easily be reproduced and are not well calibrated with empirical experience.[55]

The Lewis report endorsed the use of expert opinion, however, an endorsement that encouraged its use in the subsequent development of PRA:[56]

> RSS had to use subjective probabilities in many places. Without these, RSS could draw no quantitative conclusions regarding failure probabilities at all. The question is raised whether, since subjective probabilities are just someone’s opinion, this has a substantial impact on validity of the RSS conclusions.
>
> It is our view that use of subjective probabilities is necessary and appropri-

55. Cooke, *Experts in Uncertainty* (above, n. 33), pp. 30–40.

56. Ibid., p. 29

> ate, and provides a reasonable input to the RSS probability calculations. But their use must be clearly identified, and their limits of validity must be defined.
>
> It is true that a subjective probability is just someone's opinion. But . . . some people's opinions can be very accurate, even in a quantitative sense. . . . For many of the steps in which a subjective probability was used it was the output of experienced engineering judgment on the part of people familiar with events of that type. This, of course, does not guarantee the accuracy of the probabilities so generated, but if properly chosen makes them the best available.[57]

Other reactions to the use of expert opinion were less accepting—and they form the third constraining side of the rhetorical box. A commentary in the *Bulletin of the Atomic Scientists* in 1975 noted that the *RSS* results were misleading to the extent that they were presented as "comparable to actuarial data."[58] Another comment in the same journal two years later by one of the participants in both the Lewis Commission and the American Physical Society study of the *RSS* charged that "when crucial numbers were not available, they were simply guessed."[59] And in 1979 congressional testimony Daniel Ford, of the Union of Concerned Scientists, said: "When you hear the Atomic Energy Commission speak of the engineering judgment that they apply, that is the ponderous term that they use with outsiders to tell you the process they go through in rationalizing license and probability judgments."[60] In the same congressional hearing, Norman Rasmussen defended the use of expert judgment by saying:

> For more than two decades the NRC has licensed reactors on the basis of engineering judgement. The safety record of plants indicates that this had been an effective process. . . . However, more and more such judgements are being questioned by those skeptical about reactor safety. Faced with this skepticism, the engineer must justify his judgement. The methods developed in WASH-1400 provide a logical, rational, step-by-step process for supporting these judgements . . . . Sometimes, the judgement of two competent engineers dif-

57. Risk Assessment Review Group, *Report* (above, n. 36), pp. 9–10.

58. Robert K. Weatherwax, "Virtues and Limitations of Risk Analysis," *Bulletin of the Atomic Scientists* 31:7 (1975): 32.

59. Frank von Hippel, "Looking Back on the Rasmussen Report," *Bulletin of the Atomic Scientists* 33:2 (1977): 44.

60. Subcommittee on Energy and the Environment, *Reactor Safety Study Review*, (above, n. 32), p. 56.

fers. The methods of WASH-1400 provide a logical and systematic framework for discussion of these differences.[61]

Because the methods of the *RSS* came under criticism even during the peer-review process of the 1974 draft, the final version of the study included extensive (and occasionally redundant) explanation and justification of the methods for gathering and analyzing the data. The Main Report includes a chapter on "Risk Assessment Methodology" (I, pp. 41–57) as well as an addendum called "Overview of Event Tree and Fault Tree Methodology" (I, pp. 143–198). Ten of the appendices consist in large part of explanations and documentation of the various parts and stages of the analysis, and the eleventh is an analysis of and response to the peer review comments on the 1974 draft. Interestingly, a discussion of the "Adequacy of the Fault Tree Methodology" that appears both in the addendum (I, pp. 149–151) and in appendix XI (XI, 3.2–3.5) responds to a series of challenges to fault-tree analysis primarily by invoking expert opinion in the form of lengthy quotations from supporting letters written by the administrator of NASA, the general manager of the British Systems Reliability Service, the U.S. Environmental Protection Agency, and the Congressional General Accounting Office. Three of these letters are reproduced in both places as attachments, complete with letterhead and signature (I, pp. 184–198; XI, 3.7–3.21). Expert opinion is thus presented to justify the use of expert opinion.

The use and justification of expert opinion in risk assessment indeed became a central point of contention in the subsequent development of PRA, one that still has not been fully resolved. The extensive technical literature—in journals such as the *Annals of Nuclear Energy*, the *Journal of Energy Engineering*, and *Reliability Engineering and System Safety*—documents a continuing uneasiness about expert judgment and subjective probabilities.[62] The skepticism, discomfort,

61. Ibid., p. 137. In the 1974 hearings on the extension of the Price-Anderson Act, engineer Chauncey Starr had given similar testimony: "[F]or all low-risk events of any type, whether it is drugs, chemical accidents, nuclear stations, or large comets coming down wiping out the city of Washington—which is a perfectly feasible thing to happen—the experimental proof of this is an impossible thing. It has to be based on professional judgment, the transfer of knowledge from related experience and scientific analysis" (Joint Committee on Atomic Energy, U.S. Congress, *Possible Modification or Extension of the Price-Anderson Insurance and Indemnity Act* [Washington, D.C.: U.S. Government Printing Office, 1974], p. 625).

62. For example (in chronological order): "analysts resort, almost apologetically, to the so-called engineering judgment" (George Apostolakis, "Probability and Risk Assessment: The Subjectivist Viewpoint and Some Suggestions," *Nuclear Safety* 19:3 [1978]: 305); "subjectivist results lack credibility. . . . [and] engineering judgment . . . may lend

and occasional hostility toward the use of expert opinion in risk analysis reflect more than the engineering preference for "hard" or objective data: these attitudes also derive from a specific disagreement within engineering culture, and from a long-standing ambiguity in the theory of probability. Rodney P. Carlisle's history of PRA points out that part of the disagreement stems from two different design traditions brought together by the early nuclear industry—chemical engineering (from Du Pont Corporation, the first major reactor contractor), and electrical engineering (from General Electric and Westinghouse, the major power companies). The Du Pont chemical engineers used a deterministic method of designing reactors in order to minimize risk, focusing on failure modes and their remedies by building in redundancy and emergency backup systems without attempting to quantify risk, while the Westinghouse electrical engineers introduced probabilistic methods, focusing on the combined effects of possible problems in multiple subsystems to calculate overall risks.[63] These disciplinary preferences reflect fundamental philosophical differences regarding statistical probability. Probability, as many observers have noted, has two sorts of meanings, which produce two corresponding schools of statistical thought. In one sense, probability refers to variations in populations and thus to differences in samples from a population: the events or members of the population are determinate and, in theory, determinable. In the other sense, probability is an epistemic quality referring to the indeterminacy of knowledge about a phenomenon. The statistical approach typically taken by those committed to the first sense is called

a spurious air of reliability to the result" (L. R. Abramson, "The Philosophical Basis for the Use of Probabilities in Safety Assessments," *Reliability Engineering and System Safety* 23 [1988]: 255, 256); "physical scientists and engineers . . . are uncomfortable with the extensive use of judgment that PSAs require" (Apostolakis, "Concept of Probability" [above, n. 23], p. 1363); the use of expert opinions creates "discomfort among decision makers" (Dennis Bley, Stan Kaplan, and David Johnson, "The Strengths and Limits of PSA: Where We Stand," *Reliability Engineering and System Safety* 38:1 [1992]: 3); "there is a fair amount of controversy and skepticism in the scientific community towards the use of expert judgments" (Sumeet Chhibber, George Apostolakis, and David Okrent, "A Taxonomy of Issues Related to the Use of Expert Judgments in Probabilistic Safety Studies," ibid., p. 27); "Expert opinion is often the soft spot in a study, being the easiest aspect to attack and having the l[e]ast credibility among critics" (Stephen C. Hora, "Acquisition of Expert Judgment: Examples from Risk Assessment," *Journal of Energy Engineering* 118:2 [1992]: 146); "there is a lot of scepticism among risk analysts when speaking about subjective probabilities" (Terje Aven and Kurt Pörn, "Expressing and Interpreting the Results of Quantitative Risk Analyses. Review and Discussion," *Reliability Engineering and System Safety* 61:1 [1998]: 3).

63. Carlisle, "Probabilistic Risk Assessment" (above, n. 44).

"frequentism," which requires a database of independent observations; the approach taken to accommodate epistemic probability is known as "Bayesian statistics," which defines probability as degree of belief and can combine frequency observations and physical models with subjective probabilities.[64] The determinist-frequentist approach, more traditional and more widely accepted, is reinforced by the federal regulatory structure, which tends to presume that probability is a measurable physical property.[65] Bayesian methods are necessary in situations when either a paucity of data or a state of epistemic uncertainty requires the use of expert judgment as data; the difficulty with this approach has been the challenge of developing reliable methods for selecting experts and for aggregating their opinions.[66]

The *RSS* did not highlight its use of expert opinion. Indeed, the Lewis report charged that the *RSS* did not systematically identify "just what is a subjective probability, what is an experimental probability, what is a model and so on":[67] the data tables present all sources of data as effectively equivalent to each other. If the terms "subjective probability" or "expert opinion" are used anywhere in the report, they are certainly not easy to find. Perhaps the closest the *RSS* comes to these concepts is in its discussion of the fault-tree analysis: acknowledging that "for complex systems . . . there can be no a priori assurance that a fault tree is complete," it grants that validation and checking of fault trees "still depend upon the analyst's knowledge and understanding of the system" (II, p. 28).[68] George

64. M. Elisabeth Paté-Cornell, "Uncertainties in Risk Analysis: Six Levels of Treatment," *Reliability Engineering and System Safety* 54 (1996): 95–111. There is substantial discussion of these differences in this literature; see, for example, George Apostolakis and Ali Mosleh, "Expert Opinion and Statistical Evidence: An Application to Reactor Core Melt Frequency," *Nuclear Science and Engineering* 70 (1979): 135–149; Stanley Kaplan and B. John Garrick, "On the Quantitative Definition of Risk," *Risk Analysis* 1:1 (1981): 11–27; Norman C. Rasmussen, "The Application of Probabilistic Risk Assessment Techniques to Energy Technologies," *Annual Review of Energy* 6 (1981): 123–138. A useful general discussion of the development of Bayesianism and the recent interest in it is in David Malakoff, "Bayes Offers a 'New' Way to Make Sense of Numbers," *Science* 286 (1999): 1460–1464; apparently, improvements in computation have made Bayesian methods dramatically more feasible to use than they used to be.

65. Stephen R. Watson, "The Meaning of Probability in Probabilistic Safety Analysis," *Reliability Engineering and System Safety* 45 (1994): 261–269.

66. Paté-Cornell, "Uncertainties in Risk Analysis" (above, n. 64).

67. Risk Assessment Review Group, *Report* (above, n. 36), p. 12.

68. In his 1981 discussion of PRA methods, Rasmussen did argue explicitly for the subjectivist approach: Rasmussen, "Application of Probabilistic Risk Assessment Techniques" (above, n. 64).

Apostolakis, one of the first students of the *RSS* and an early advocate of subjective probabilities in risk analysis, notes that "the RSS analysts were reluctant to openly admit that they were implementing the subjectivistic, that is, Bayesian theory."[69] He points to a passage in Appendix III in which Bayesianism is introduced as a kind of hypothetical alternative but not really embraced: "Treating data as random variables [an approach that the report has just argued for] is sometimes associated with the Bayesian approach . . . the data and system characteristics were treated by the study as being simply random variables, however the Bayesian interpretation can also be used" (III, p. 3). In fact, Apostolakis claims elsewhere, "no PSA [PRA] has been performed to date that does not use subjectivistic methods (although very few analysts state explicitly that they are using Bayesian methods)."[70]

The need to rely on expert opinion constituted a serious rhetorical challenge for the Rasmussen team, given the usual preferences and expectations of the engineering community. It violates the preference for the objective over the subjective, for the quantitative over the qualitative, and for frequentist over subjectivist approaches to probability. Even the supporters of subjectivist approaches continually present them as a necessary evil, necessary only because data are not sufficient for classical deterministic statistics: as the Lewis report put it, the *RSS* "had to use" subjective probabilities; or, as Apostolakis and a coauthor noted later, "PRA analysts are compelled to use expert opinions."[71] That the *RSS* justified its methods in great detail may strike us as rhetorical compensation for a methodological liability rather than as standard scientific procedure.[72] M. R. Hayns comments that the continuing need to rely on expert judgment constitutes an "apparent contradiction to what is meant by a fully quantitative methodology," and Apostolakis acknowledges that PRA is not yet a "hard science."[73] Both Rasmussen and Levine, in separate commentaries, highlight the tension between the ideal of quantita-

69. G. E. Apostolakis, "The Interpretation of Probability in Probabilistic Safety Assessments," *Reliability Engineering and System Safety* 23 (1988): 247–248.

70. Apostolakis, "Concept of Probability" (above, n. 23), p. 1363.

71. Wu and Apostolakis, "Experience with Probabilistic Risk Assessment" (above, n. 45), p. 335.

72. See, for example, *RSS*, Addendum I to the Main Report, "An Overview of Methodology" (I, pp. 143–198); Appendix II, sect. 3, "Fault Tree Quantification" (II, pp. 27–76).

73. Hayns, "Evolution of Probabilistic Risk Assessment" (above, n. 23), p. 139; W. E. Vesely and G. E. Apostolakis, "Editorial: Developments in Risk-Informed Decision-Making for Nuclear Power Plants," *Reliability Engineering and System Safety* 63 (1999): 223.

tive methodology and the need to use expert judgment in the *RSS*. Rasmussen begins by promoting PRA as a quantitative method of risk analysis, but also points out that "there is never any proof in the mathematical sense that the analysis is complete. To ensure that no obvious factors have been missed requires analysts with considerable background and experience in the system being analyzed."[74] Levine and his coauthors claim that the construction of event trees is fundamental to the quantitative aspect of PRA, but at the same time they acknowledge that the process requires engineering judgment.[75] Objectivity and quantification were the scientific expectations against which the *RSS* had to make its case, given the limitations of the available data.

Rhetorically, the prominent role of expert opinion in the argument of the *RSS* can be seen as a reliance on *ethos*, and we can understand the engineering community's discomfort with expert opinion as an expression of its long-standing preference for *logos* over *ethos*. A scientist or engineer is expected to support a claim with factual observations and sound reasoning (*logos*), abjuring appeals to emotion (*pathos*) or personal character (*ethos*). Thus, what might in other situations be central to an ethical appeal—affiliation, prior success, masterful expertise—in science and technology must be treated as *logos*, as factual evidence, attributes of the technical situation rather than of an advocate in a rhetorical situation. The *RSS* is consistent with these rhetorical conventions of impersonality, even taking some refuge in a heightened conventionality, reluctant to call attention to the dilemma of having to rely on expert opinion. By focusing our attention on the facts of the case (and on opinions presented as though they were facts), rather than on the character of the community it represents, the *RSS* makes expert opinion acceptable; by treating expert opinion as data and detaching it, to the extent possible, from the character that authorizes it, the *RSS* rhetorically transforms *ethos* into *logos*.

But there is a further dimension to ethos in the *RSS*. One of the primary conclusions of recent work in the rhetoric of science is that the rhetorical style of impersonality, the denial of ethos, is itself an argument that universalizes results originating in particularity: the scientist must seem fungible, so that her results could have been—

74. Rasmussen, "Application of Probabilistic Risk Assessment Techniques" (above, n. 64), p. 131.

75. S. Levine, V. Joksimovich, and F. Stetson, "Probabilistic Risk Assessment in the US," *Reliability Engineering* 6 (1983): 200.

and might be—achieved by anyone.[76] Ideally, the facts speak for themselves and do not need an advocate; ethos should be unnecessary. However, if we understand this style of reasoning as itself a rhetorical choice that helps make an argument credible, we see that it constructs its own ethos, an ethos that denies the importance of ethos. The technical ethos—impartial, authoritative, self-effacing—is all the more powerful for its self-denial. So not only is ethos transformed into logos, but the favoring of logos becomes its own ethos. The two Aristotelian modes of appeal take on a complex, interactive relationship in which each dissolves into the other.[77]

The *RSS* thus expresses an ethos in its style of reasoning and expression. But beyond this conventional technical ethos, the *RSS* acquires additional power from its gallery of expert opinion, off-stage perhaps but still formidable, an absent presence operating as both logos and ethos. As ethos, the expert opinion serves to emphasize that the *RSS* is a collective product, produced by the expert community and thus implicitly by those whose opinions are cited as well as by those who selected and put those opinions to use. As logos, the unconventional reliance on expert opinion is a liability that must be suppressed; as ethos, it is a powerful rhetorical resource.

Authority in the *RSS:* Ethos as Expertise

The reliance on expert opinion in the *RSS* is an experiment with the *argumentum ad verecundiam*, or argument from authority. Douglas Walton has pointed out that argument from authority has a dual history, as both a legitimate strategy and a fallacy. Citing Locke's *Essay Concerning Human Understanding* (1690) and Whately's *Elements of Logic* (1836), Walton notes that appeals to expert opinion can be reasonable in the absence of more "objective" or reproducible knowledge, or what Whately calls arguments *ad rem*, those about the substance of the matter. This is essentially Aristotle's reasoning, mentioned earlier, that ethos is a default appeal. On the other hand, appeals to expert opinion can be abused if they attempt to coerce

76. Alan G. Gross, *The Rhetoric of Science* (Cambridge, Mass.: Harvard University Press, 1990); Evelyn Fox Keller, "The Paradox of Scientific Subjectivity," in *Rethinking Objectivity*, ed. Allan Megill (Durham, N.C.: Duke University Press, 1994), pp. 313–331; Bruno Latour and Steve Woolgar, *Laboratory Life: The Social Construction of Scientific Facts* (Beverly Hills, Calif.: Sage, 1979).

77. Here we seem to have a limit case of Eugene Garver's claim that for Aristotle ethos is not a separate source of conviction, but that it occurs *through logos*, that "reason persuades when it is evidence of character" (Eugene Garver, *Aristotle's Rhetoric: An Art of Character* [Chicago: University of Chicago Press, 1994], p. 154).

agreement solely on the basis of authority; in other words, such arguments can verge on threats (or arguments *ad baculum*).[78] Both Locke and Whately characterize the *ad verecundiam* as inherently subjective, in that it depends upon a personal relationship, that of respect for the authority invoked. Hence, logic textbooks have tended to describe the *ad verecundiam* as a fallacy, pure and simple. Walton notes that the standards of scientific argument have similarly rejected arguments from authority as fallacious, in part because the rise of scientific method in the seventeenth century was accompanied by a concerted rejection of medieval scholasticism's reliance on argument from authority, Galileo's conflict with the church being the paradigm case.[79]

Walton distinguishes two kinds of authority, cognitive and administrative. Fallacious argument may confuse one kind for the other—presuming, for example, that members of a government commission have experiential "engineering judgment" about a subject on which they must provide advice, or that a physicist or nuclear engineer can speak authoritatively about community zoning laws in a waste-disposal siting decision. And indeed these two kinds of authority often overlap, or are so closely intertwined that the type and relevance of the expertise being invoked is difficult to determine.[80] Earlier, I characterized risk analysis as an expert discourse, and the reliance of the *Reactor Safety Study* on its cognitive authority is consistent with this characterization. But because risk analysis is usually commissioned for public purposes and used administratively to make and justify decisions, this characterization is incomplete. Situated on the boundary between expert and public discourse, risk analysis may easily invoke both kinds of authority. It can be seen as part of a broad movement that has expanded the "jurisdiction" of scientific and technical expertise into the realms of public decision.[81]

78. Douglas Walton, *The Place of Emotion in Argument* (University Park: Pennsylvania State University Press, 1992), pp. 7, 147. Literally, *ad verecundiam* means "based on modesty," the idea being that one should, out of modesty, defer to expertise greater than one's own.

79. Douglas Walton, *Appeal to Expert Opinion: Arguments from Authority* (University Park: Pennsylvania State University Press, 1997), pp. 28, 34, 46–48. The motto of the British Royal Society, committed to eye-witness empiricism, was *Nullius in verba*, "on the word of no one." See Steven Shapin's discussion of the role of trust and authority in the rise of modern science in *A Social History of Truth: Civility and Science in Seventeenth-Century England* (Chicago: University of Chicago Press, 1994).

80. Walton, *Place of Emotion in Argument* (above, n. 78), p. 48.

81. Richard H. Gaskins, *Burdens of Proof in Modern Discourse* (New Haven, Conn.: Yale University Press, 1992), p. 142.

A major risk analysis project functions in toto as an *argumentum ad verecundiam* within the public realm: the *RSS* was intended to provide an authoritative technical argument—authoritative *because* of its technicality—in the congressional decision on renewal of the Price-Anderson Act. As a boundary discourse, then, risk analysis must speak *to* political decision makers, industry managers, consumers, citizens, as it speaks *from* a position of technical expertise. But it must also speak persuasively to experts, if it is to carry its technical authority successfully into the public realm. As we saw, the Rasmussen report's difficulties in that realm began with its failure to persuade other technical experts.

The validation of expert opinion by the *RSS* is of particular interest when compared with the frequent excoriation of public opinion about nuclear power during this same period. Proponents of nuclear power accused those who opposed it of "intellectual mischief," "prejudice," and "bigotry"; of promulgating "scare arguments" in "loud voices"; of relying on "myths of fear" rather than "accurate perception of the facts."[82] The most pervasive accusation was that of irrationality, with opponents of nuclear power analyzed in psychological terms, not on the basis of the soundness of their evidence or the validity of their claims. For example, a psychiatrist writing in *Business Week* in 1981 charged that the inconsistencies in individual risk assessments are governed by "irrational" psychological perceptions; he likened these "distortions," or "biases," to clinical phobias, and concluded that "the nuclear power industry has been virtually stopped in the U.S. because of fear."[83] Perhaps the most vivid form this charge took was the comparison to hysteria about witchcraft, an image that was repeated many times: "Everyone's innate fear of the invisible and the unknown is a major impediment to the appropriate development and use of nuclear power. It's comparable, perhaps, to the fear of witchcraft, which was so intense in early American history."[84]

82. These quotations are all from speeches given in the late 1970s, but before Three Mile Island: Llewellyn King, "Nuclear Power in Crisis: The New Class Assault," *Vital Speeches of the Day* 44 (1978): 714, 716 (King was publisher of *The Energy Daily*); Endicott Peabody, "Towards an Energy Vision for America's Future: The Hard Choices Just Ahead," ibid., 42 (1975): 139 (Peabody was with Americans for Energy Independence); Frank G. Zarb, "Nuclear Power: A Time for Decision," ibid., 41 (1975): 672 (Zarb was administrator of the Federal Energy Administration).

83. Robert L. DuPont, "The Nuclear Power Phobia," *Business Week*, September 7, 1981, pp. 14–15.

84. Chauncey Starr, "The Three Mile Island Nuclear Accident: The Other Lesson," in *The Three Mile Island Nuclear Accident: Lessons and Implications*, ed. Thomas H. Moss and David L. Sills, vol. 365, *Annals of the New York Academy of Sciences* (New York: New York

Even risk researchers sympathetic to public concerns characterized the opinions of the public as "risk perceptions" and the opinions of experts as "risk analysis," implying an a priori difference in validity. My point here is not that public opinion and expert opinion about risk should necessarily be taken as equally valid; rather, the comparison allows us to see that an important aspect of the risk debate is a negotiation over the burden of proof: whose opinion should be presumed valid until proven otherwise? Engineering judgment and other forms of expert opinion claim a strong cognitive (and often administrative) authority over public issues, whereas public opinion has little claim to authority in the expert realm and sometimes not much in the public realm. We normally, and reasonably, grant a presumption to expertise, assigning a smaller burden of proof to those with knowledge, those whose evidence and arguments are understood to be more authoritative and thus *should* be more persuasive. That is, expertise is usually understood to enjoy what Whately calls a "preoccupation of the ground," such that it is presumed to prevail "till some sufficient reason is adduced against it."[85] The burden of proof lies with the one who disputes the presumptive position.

Whately's influential discussion of presumption acknowledges that it is both a legal and a psychological construct: thus, though the law may assign the burden of proof (in our system, to the prosecutor or plaintiff), arguers may also claim a presumption upon their audience and may attempt to shift the burden in the course of the argument. Whately notes that presumption usually lies with "every existing institution" and with tradition.[86] Interestingly, he also notes that presumption may rest either with or against "the learned." On the one hand, we presume that for any question "the most eminent men in the department it pertains to" will provide the best judgment. On the other hand, there is a counterpresumption that eminent men may be subject to jealousy of anyone who offers new or noncanonical arguments on an issue in their field; in addition, we may presume that learned elites form a self-interested society who are biased in favor of their own eminence. Whately concludes that the counterpresumptions, those against the learned, have "often as

Academy of Sciences, 1981), p. 295. This image was subsequently used by Alvin Weinberg in "Hazards, Technology and Fairness," *Issues in Science and Technology* 2:1 (1985): 71; and Lewis, *Technological Risk* (above, n. 6), p. 336.

85. Richard Whately, *Elements of Rhetoric*, Landmarks in Rhetoric and Public Address, ed. Douglas Ehninger (Carbondale: Southern Illinois University Press, 1963), p. 112.

86. Ibid., pp. 114, 117.

much weight" as the presumption in their favor, "and sometimes more."[87]

The presumption in favor of learned experts in the public realm has multiple historical roots, both shallow and deep.[88] Perhaps the deepest is in Plato, who argued repeatedly that we should take our advice and our decisions from those who are accomplished in a particular art or *technê*—the physician, the architect, the navigator—rather than from the rhetorician, or from the more generally uninformed public.[89] In the decades following World War II, expertise enjoyed particularly high confidence in the United States; Cooke, for example, calls this a period of "almost unlimited faith in expert opinion."[90] The presumption in favor of expertise in postwar American culture owes much to institutional arrangements resulting from the discussion about reconversion to peacetime relationships among academic science, the military, and the government. The Office of Naval Research, the Atomic Energy Commission, and the National Science Foundation, all major results of this discussion, provided a system in which the allocation of federal resources for research was made not primarily by politicians but by scientists themselves, insulated from political processes and social needs.[91] Daniel Kevles has called this arrangement a "victory for elitism."[92] When our age is characterized as an age of science and technology, what is often meant is simply our deference to expertise in the public realm; as Walton notes, in displacing religion and historical and literary tradition as cultural authorities, science did not abolish the use of authority appeals—it simply became our primary authority.[93]

87. Ibid., pp. 128–129.

88. See Walton's survey of the history of treatments of arguments from authority in *Appeal to Expert Opinion* (above, n. 79), chap. 2.

89. See Bruno Latour's analysis of Plato's *Gorgias* as an assault on the demos of Athens by various forms of expertise: Bruno Latour, "Socrates' and Callicles' Settlement—or, the Invention of the Impossible Body Politic," *Configurations* 5 (1997): 189–240.

90. Cooke, *Experts in Uncertainty* (above, n. 33), p. 4.

91. Daniel Lee Kleinman, *Politics on the Endless Frontier: Postwar Research Policy in the United States* (Durham, N.C.: Duke University Press, 1995), p. 54.

92. Daniel J. Kevles, *The Physicists: The History of a Scientific Community in Modern America* (New York: Knopf, 1978), p. 366.

93. Walton, *Appeal to Expert Opinion* (above, n. 79), p. 35. Gross notes that scientific authority can be as stultifying as religious or political authority when it holds too tenaciously to a received dogma: Gross, *Rhetoric of Science* (above, n. 76), p. 13. On the decline of public discourse and political life that accompanies the rise of expertise to political power, see G. Thomas Goodnight, "The Personal, Technical, and Public

However, the standing presumption in favor of expertise that was so strong in the immediate postwar period began to weaken in the early 1970s, just when the *RSS* was under way. And this is the final rhetorical constraint that boxed in the Rasmussen team: an implicit but dramatic increase in the burden of proof on experts within the public realm. Cooke claims that "a period of unbridled growth and almost unlimited faith in expert opinion came to a close in the United States sometime in the early 1970s"; he notes that the percentage of Americans with "great confidence" in the leaders of a variety of institutions (medicine, education, religion, industry, and others) declined sharply between the mid-1960s and the early 1970s, and points to the war in Vietnam as a significant factor in this change.[94] Attributing this shift in the rhetorical environment to a series of changes in the law and its interpretation, Richard Gaskins claims that in the late twentieth century, Whately's conservative presumption in favor of existing institutions was replaced by "the increasingly radical presumption of institutional failure."[95]

Concurrent with the increased willingness to question expertise was a disenchantment with technology as a guarantor of progress. One sign of this shift was the congressional decision in 1971 to cancel support for the SST, Boeing's heavily subsidized supersonic passenger plane, a decision that has been described by Randall Bytwerk as "a turning point: . . . one of the few times . . . the United States chose not to accept a major new technology."[96] He calls the decision a sign of a "significant change in the American rhetorical climate."[97] During this same period, between 1965 and 1975, there was a rever-

Spheres of Argument: A Speculative Inquiry into the Art of Public Deliberation," *Journal of the American Forensic Association* 18 (1982): 214–227; Magali Sarfatti Larson, "The Production of Expertise and the Constitution of Expert Power," in *The Authority of Experts: Studies in History and Theory*, ed. Thomas L. Haskell, Interdisciplinary Studies in History (Bloomington: Indiana University Press, 1984), pp. 28–80.

94. Cooke, *Experts in Uncertainty* (above, n. 33), pp. 4–5.

95. Gaskins, *Burdens of Proof* (above, n. 81), p. 46. In the early 1950s, the Warren Court dropped the operative presumption of constitutionality that had guided the Supreme Court since the 1930s. A series of decisions extending the application of the due-process clause placed new burdens of proof on public agencies and officials, shifting legal presumption further toward individuals and challengers of the status quo (p. 7). The 1958 Delaney clause to the Food, Drug, and Cosmetic Act forbidding food additives with any carcinogenic effects at all is seen as a major and influential arrogation by the legislature of decision authority usually left to expert agencies (p. 150).

96. Randall L. Bytwerk, "The SST Controversy: A Case Study in the Rhetoric of Technology," *Central States Speech Journal* 30 (1979): 188.

97. Ibid., p. 198.

sal in public opinion about nuclear energy, a shift that was "dramatic and unexpected," according to a 1977 discussion in *Science* of the "distrust of nuclear power"; the sudden and unmistakable nature of this reversal helps explain the sometimes shrill and "rancorous" tone of the nuclear power proponents as they negotiated a rhetorical environment that now imposed an unaccustomed burden of proof.[98] It probably also explains the Executive Summary of the *RSS*, both its very existence and its misrepresentations of the report's conclusions noted earlier. When elites sense that they are losing their privileged status, they adopt certain predictable rhetorical strategies, according to Andrew King; that we can see three of the four strategies King describes (ridicule, threats of anarchy, and setting impossible standards) in the public rhetoric of the nuclear power elite during this period is an interesting corroboration of his thesis, as well as suggestive evidence that nuclear experts did in fact understand their position in exactly this way.[99] The rhetorical task of the *RSS*, at least in the public arena, was to regain some presumption, to "preoccupy" more ground, in Whately's phrase.

It did this, somewhat perversely, by relying on its expertise, the very element that was under increasing suspicion in the public arena and would never suffice in the expert arena. In the previous section, I discussed the stance of the *RSS* with respect to the expert community, as demonstrated primarily in the main sections of the report and the appendices, where the Rasmussen group struggled with the need to use subjective probabilities. Here, I examine its stance with respect to the public by looking for more conventional manifestations of ethos, primarily within the Executive Summary, the part of the report intended for public consumption. The strategies we find are those predicted by Steven Shapin, who notes that the "credibility-economy" obtaining between experts and the public relies upon such formal warrants as institutional affiliation, the explicit framing of methodology, and the display of consensus across multiple experts.[100]

The Executive Summary has two sections: a three-page summary of results, and an eight-page set of "Questions and Answers About This Study." In many ways it is similar to the technical part of the re-

98. Christoph Hohenemser, Roger Kasperson, and Robert Kates, "The Distrust of Nuclear Power," *Science* 196 (1977): 33.

99. Andrew King, "The Rhetoric of Power Maintenance: Elites at the Precipice," *Quarterly Journal of Speech* 62 (1976): 127–134.

100. Steven Shapin, "Cordelia's Love: Credibility and the Social Studies of Science," *Perspectives on Science* 3 (1995): 270.

port. For one thing, it is not typographically or visually distinct from the main report in any way, with double columns, single-spacing, right-justification, decimal-numbered headings, line graphs (on log-log scales!), and typed tabular material. In the first paragraph, the report associates itself with technical institutions and agencies: with the AEC, MIT (Norman Rasmussen's institution), the Department of Defense, and NASA (the source of the methods) (ES, p. 1). The group responsible for the report is described directly as "scientists and engineers who had the skills needed" (ES, p. 5), and the competence and diligence with which they conducted the study are mentioned or implied in several places: in section 2.1, we are told that "60 people, various consultants, 70 man-years of effort, and about four million dollars were involved" (p. 5); in section 2.15, that the study calculated the probabilities and health consequences of 140,000 possible combinations of occurrences "with the aid of a large digital computer" (p. 9); and in section 2.20, that because the approach used was "systematic," oversights are unlikely (p. 12). The methods are described in more detail in section 2.21 and again attributed to DoD and NASA and associated with "engineering and mathematical logic" (p. 12). The similarity to the Main Report is reinforced by the fact that the introductory chapter of the Main Report provides extensive additional detail on all these points to the expert audience.

But there are also ways in which the Executive Summary distinguishes itself from the rest of the report, indicating that the Rasmussen team did understand its two audiences differently. The Summary has the voice of a highly knowledgeable but empathetic expert: it speaks authoritatively but with reassurance, repetition, and apparent understanding of the audience's concerns. As though to demonstrate the impartiality of the researchers, as well as their comprehensive grasp of facts, it presents the results of the study almost exclusively as comparisons with other sources of risk (figures 1.1, 1.2, 1.3, table 1.1, and three of the four unnumbered tables in section 2).[101] These comparisons allow the authors to be reassuring, almost every time a specific risk is mentioned: "The risks to the public

101. These graphs were the objects of some of the most severe criticism of the Executive Summary. The graphs that compare nuclear plant fatalities to other "man-caused" events and to natural events do not include delayed cancer deaths, but only immediate deaths; estimates of delayed fatalities are included in the appendices and alluded to in the text of the Executive Summary (Ford, *Cult of the Atom* [above, n. 20], pp. 169–170), but the graphs that omit these data are able to show impressive gaps between the fatality curves for nuclear power plants and every other event shown, except for meteor strikes.

from potential accidents in nuclear power plants are comparatively small" (p. 1); "Even for a large accident, the small increases in these diseases would be difficult to detect from the normal incidence rate" (p. 2); "The number [of thyroid nodules] that might be produced in very unlikely accidents would be about equal to their normal occurrence in the exposed population" (p. 3). The Q&A format of section 2 constructs a somewhat anxious public whose every possible question ("Can a nuclear power plant explode like a nuclear bomb?" "How might a core melt accident occur?" etc.) has already been anticipated and then answered attentively and thoroughly by a knowledgeable team.[102] The reader seems to participate in a rational dialogue with a wise and trusted parent-figure. Finally, the report explicitly claims that it has made "no judgment on the acceptability of nuclear risks," which, it notes, "should be made by a broader segment of society than that involved in this study"; rather, it has merely "presented the estimated risks and compared them with other risks that exist in our society" (p. 3). Thus, the report tells us, it has been objective rather than judgmental, technical rather than political. It plays the role of the self-effacing technician standing on the sidelines, providing information when requested, but not entering the debate.

The report's address to the public, then, combines a traditionally impartial or objective scientific ethos with a paternalistic authority. And the Executive Summary differs from the rest of the *RSS* in its overt marking of expertise and in its somewhat solicitous address. Both elements emphasize the technical knowledge in which the report is grounded, the expertise that authorizes its claims. As a response to the rhetorical problem—that is, the substantial and unaccustomed burden of proof the *RSS* was required to meet—these strategies might have succeeded with the public audience, particularly the press and the Congress, had it not been for the scrutiny of other experts, who would not be influenced by these overt ethical appeals and who resisted the covert ethos-as-logos proofs in the body of the report. Achieving credibility with another expert group, Shapin notes, requires rhetorical resources different from those that succeed with the public. Both Shapin and Theodore Porter make the point that quantification is the strategy best suited to enhance credibility between expert groups, because mathematical methods are, if not universal, at least widely shared and sharable, and thus are un-

102. One of the editors of this issue (J. S.) pointed out that the Q&A format is "by design, an organized prolepsis," a figure of anticipation that treats the future or the potential as though it already exists.

derstood to ensure rigor and to prevent bias.[103] And as we saw above, the Lewis report faulted the *RSS* on exactly this point, that its quantitative methods were defective.

The public dimension of the *RSS* brings a somewhat different pattern of ethos to our attention than did the expert dimension. What we see in the Executive Summary is the narrowing of ethos to expertise, again a pattern characteristic not only of the *RSS*, or only of risk analysis, but of scientific and technical discourse generally as it has developed in the modern era. This narrow form of ethos grounds its appeal primarily in the first of Aristotle's three constituents, at the same time transforming it from Aristotelian *phronesis*, a knowledge focused on prudent action in the social world, to *episteme*, a knowledge focused on objects and ideas abstracted from a social context, a knowledge that is close to what we call expertise. The other two constituents of ethos, moral qualities (*arete*) and goodwill (*eunoia*), are not absent, but they are grounded, to the extent possible, in the rhetor's expertise. We can see this when we notice that the three constituents of ethos reflect the three Aristotelian appeals; in effect, they channel the three appeals through the rhetor, such that *phronesis* is a reflection of logos (the way the rhetor handles the facts and reasoning of a case), *eunoia* is a reflection of pathos (the way the rhetor addresses the audience), and *arete* is a reflection of ethos itself (the way the rhetor exhibits excellence of character).[104] Thus, in the Executive Summary of the *RSS*, *eunoia* takes the form of the solicitousness with which the expert rhetor addresses our anxieties, and *arete* takes the form of knowledge exhibited as a virtue. Looked at this way, the reduction of ethos to expertise may simply be another way of describing the transformation of ethos into logos that we saw in the previous section. However, these two perspectives show us two distinct aspects of this transformation: in the first, ethos is de-

103. Porter, *Trust in Numbers* (above, n. 16), p. ix; Shapin, "Cordelia's Love" (above, n. 100), p. 270.

104. There has been some empirical confirmation that audience response to a rhetor is grounded in these three Aristotelian constituents. See James C. McCroskey and Jason J. Teven, "Goodwill: A Reexamination of the Construct and Its Measurement," *Communication Monographs* 66 (1999): 90–103; James C. McCroskey and Thomas J. Young, "Ethos and Credibility: The Construct and Its Measurement after Three Decades," *Central States Speech Journal* 23 (1981): 24–34. Aristotle's conceptualization continues to guide recent empirical work on trust in risk situations: see, for example, Richard G. Peters, Vincent T. Covello, and David B. McCallum, "The Determinants of Trust and Credibility in Environmental Risk Communication: An Empirical Study," *Risk Analysis* 17 (1997): 43-54; Ortwin Renn and Debra Levine, "Credibility and Trust in Risk Communication," in *Communicating Risks to the Public*, ed. Roger E. Kasperson and Pieter Jan M. Stallen (Dordrecht: Kluwer, 1990), pp. 175–218.

nied—transformed or displaced into logos, another mode of proof altogether; in the second, ethos is constricted, focusing its considerable power on only those resources that derive from specialized knowledge. In this second form, ethos becomes merely a shadow of logos.

The Technicizing of Ethos

In 1980 a Supreme Court decision held that a benzene exposure standard announced by OSHA in 1977 should have been justified by a quantitative assessment of risk rather than by expert judgment and discretion within the agency. This decision is now understood as a judicial turning point that increased the burden of proof on expert opinion, shifting the presumption not toward the public but toward quantitative methods.[105] After this decision, risk analysis methods became increasingly quantified, and the development of PRA involved the creation of methods to quantify expert opinion. A good illustration of these developments is the NRC's next major risk analysis, NUREG-1150. A study of five power plants of different design, this document was intended as a follow-up to the *RSS* that would provide guidance to NRC staff in carrying out risk assessments and in meeting safety goals.[106] Like the *RSS*, NUREG-1150 relied heavily on expert opinion, but after extensive criticism of its 1987 draft, major attention was given to developing an explicit and defensible method for the gathering and use of expert opinion: "The revised process was designed to obtain subjective estimates of uncertain physical quantities and frequencies in a manner that best utilizes the available expertise and accurately reflects the collective uncertainty about these values."[107] An elaborate ten-step process was developed, with special attention to the selection of both issues and experts, the formalization of an "elicitation process," and the training of the experts "to help [them] become better able to encode their knowledge and beliefs into a form that can be incorporated into computer models."[108]

105. Sheila Jasanoff, "The Misrule of Law at OSHA," in *The Language of Risk*, ed. Dorothy Nelkin (Beverly Hills, Calif.: Sage, 1985), pp. 155–178.

106. U.S. Nuclear Regulatory Commission, *Severe Accident Risks: An Assessment for Five U.S. Nuclear Power Plants* (Washington, D.C.: U.S. Nuclear Regulatory Commission, 1990).

107. S. C. Hora and R. L. Iman, "Expert Opinion in Risk Analysis: The NUREG-1150 Methodology," *Nuclear Science and Engineering* 102 (1989): 324.

108. Ibid., p. 325. Thus, the opinions elicited were in the form of quantified probabilities—e.g., "a likelihood of 10%," rather than "a small chance" (N. R. Ortiz, T. A. Wheeler, M. A. Meyer, and R. L. Keeney, "Use of Expert Judgment in NUREG-1150," *Sixteenth Water Reactor Safety Information Meeting* [Nuclear Regulatory Commission, 1988], *Proceedings of the U.S. Nuclear Regulatory Commission*, V, p. 3).

Porter has noted that there are two forms of expertise at work in situations like this: a recent one that emphasizes the mastery of a set of formal methods, and a more traditional one that emphasizes the judgment that derives from long experience rather than from method or rules. He documents the increasing replacement of expert judgment with quantified or formal decision methods over the nineteenth and twentieth centuries in both Europe and the United States, a trend that he characterizes as a political response to political pressures, a bureaucratic "strategy of impersonality" adopted in "conditions of distrust" when decision makers have neither the power nor the presumption to ensure that decisions will not be challenged.[109] In fact, Porter notes, quantified decision making is associated with democratic government, and not with forms of government based on heredity or force, because it "is one of the most convincing ways by which a democracy can reach an effective decision in cases of potential controversy, while simultaneously avoiding coercion and minimizing the disorderly effects of vigorous public involvement."[110] Decisions based on formal method, on numbers and algorithms, are perceived to be both fairer and truer than those based on experiential judgment because their impersonality is interpreted as objectivity: Porter epitomizes the move toward methodized decision-making as our "trust in numbers." And if the preference for quantification over expert judgment signifies adherence to democratic values, the OSHA benzene decision illustrates the loss of authority by expert elites. Generally, Porter notes, elites tend to resist having their discretion limited by being forced to use calculative models, as the OSHA decision did. Thus, the NRC's efforts in NUREG-1150 seem to reflect an attempt to have it both ways: to accede to the need for egalitarian impersonality, and at the same time to turn expert judgment *into* formal method and thus retain expert privilege. Quantification can be seen as a form of expertise that is independent of experts; as such, it assists ethos in its concealment as logos.

Quantification is an essential aspect of the contemporary deployment of expertise. It not only legitimizes expert judgment, it also makes problems tractable and computable, allowing the aggregation of multiple expert judgments. But it has the additional rhetorical effect

109. Porter, *Trust in Numbers* (above, n. 16), p. xi.

110. Theodore M. Porter, "Objectivity as Standardization: The Rhetoric of Impersonality in Measurement, Statistics, and Cost-Benefit Analysis," in Megill, *Rethinking Objectivity* (above, n. 76), p. 206. Porter's argument should be contrasted with that of Latour, which pits expertise in the service of objective truth, in Plato's *Gorgias*, *against* the interests of the demos of Athens (Latour, "Socrates' and Callicles' Settlement" [above, n. 89]).

of "technicizing" problems—that is, of changing the central question (in risk analysis, "how safe is safe enough?") from an evaluative one into a technical one. In rhetorical terms this is a shift of *stasis*, a shift of what is centrally at issue. Traditional rhetoric recognizes four stases: existence, definition (including classification), value, and policy, each of which requires different sorts of premises.[111] Evaluative questions, which occupy the last two stases, require answers based on values—they are best answered by public deliberation and not by quantification; in contrast, technical questions, occupying the first two stases, require answers based on specialized knowledge—they are best answered by expertise. As Cooke has noted, quantitative risk assessment makes "the question "'how dangerous?' . . . no longer a matter for democratic adjudication or for executive reflection . . . [but] in principle a matter for technical review"[112] Thus, an evaluative treatment of the question "how safe is safe enough?" would focus on "safe enough," as a judgment reached by a community; it would presume agreement by the relevant community on both facts and values. A technical treatment would focus on "how safe," as a definition reached through measurement, and would presume that values and actions are not at stake. Although the technical treatment lacks the assent of the community, it acquires the epistemic authority of science and the presumption accorded to such authority.

In risk analysis there is a constant tension between these two types of question, a tension grounded in what Walton called the confusion of cognitive and administrative authority. The National Research Council's 1983 "Red Book" on risk assessment in the regulatory process defined the distinction between risk assessment and risk management as just this difference in stasis, establishing the expectation that technical and policy issues should and could be kept separate: according to the report, "A frequent deficiency of agency risk assessments is the failure to distinguish between scientific and policy considerations in risk assessment"; the report goes on to point out the consequences of this failure: "Critics contend that the results of risk assessment are often seen as scientific findings by regulators and the public, whereas in fact they are based in part on other considerations."[113] Here, then, is one way that presumption works for expertise: anything that seems rigorously expert or technical, or

111. Recent discussions often include cause as a fifth stasis or as part of the second stasis. See Jeanne Fahnestock and Marie Secor, "The Stases in Scientific and Literary Argument," *Written Communication* 5 (1988): 427–443.

112. Cooke, "Risk Assessment" (above, n. 35), p. 330.

113. U.S. National Research Council, *Risk Assessment* (above, n. 10), p. 164.

seems authorized by expertise, is taken to be operating in the stasis of existence or definition, rather than of value or policy. Technical claims (or technical-looking claims) are thus presumed to be matters of fact rather than of judgment. Especially in the public arena, the evaluative is often presumed to be technical, a move that may allow the silent importation of values into presumably neutral territory. Barry Barnes and David Edge note the widespread tendency to "'convert' value issues into technical discussions," seeing it as a form of "scientism."[114] K. S. Shrader-Frechette is particularly critical of this point: she instances the many value judgments made in the course of the supposedly objective analysis in the *RSS*. Because of the value judgments inherent in scientific risk assessment, she holds that "the distinction between expert/objective and lay/subjective determination of environmental risks will not hold up."[115] And since the burden of proof is greater for risk experts in value or policy questions than in technical questions, the recurring shift to the technical is yet another example of expertise working to gain (or regain) the presumption.

The stasis confusion works in the other direction as well, when the technical is presumed to be evaluative. The *RSS*, as noted above, disavowed the value questions and claimed a concern with technical questions only. In addition to the passage from the Executive Summary quoted earlier, there is this disclaimer at the end of the Main Report:

> The question of what level of risk from nuclear accidents should be accepted by society has not been addressed in this study. It will take consideration by a broader segment of society than that involved in this study to determine what level of nuclear power plant risks should be acceptable. This study should be of some help in these considerations. (p. 140)[116]

But the situation—and the rhetoric—were more complex than this, given the impending congressional decision on the Price-Anderson Act, the history of AEC promotion of civilian nuclear power, and the strengthening public distrust of government and corporate authority. Congressional oversight hearings raised this issue more than once. In July 1976, Representative Jonathan Bingham wrote to NRC

114. Barry Barnes and David Edge, "Science as Expertise," in *Science in Context: Readings in the Sociology of Science*, ed. Barry Barnes and David Edge (Milton Keynes: Open University Press, 1982), p. 244.

115. Shrader-Frechette, *Risk and Rationality* (above, n. 11), p. 97.

116. There is a similar disclaimer at the conclusion of chap. 1, p. 7.

Chairman Marcus A. Rowden about concerns expressed in a June hearing that the *RSS* "contained an implied conclusion that the hazards associated with operation of light water reactors were so small as to be 'acceptable.' While I am aware that the Study states that such value judgments are not its intent, the manner in which the executive summary is laid out can readily convey a contrary impression."[117] A January 1977 report by the House Subcommittee on Energy and the Environment concluded that "while the NRC maintains the Study was not intended to address the question of the 'acceptability' of the nuclear risk, its method of presentation lent itself readily to this end. Its conclusions have, in fact, been widely used to indicate that the risk of nuclear electric generation is 'acceptable.'"[118]

The "implied conclusion" and "method of presentation" noted by Congress alert us to the presumption that expertise gains from the stasis shift from technical to evaluative: once an expert answer for a technical question is given, there is a strong inclination to take it as an answer to any related policy questions as well, presuming public adherence to the values invoked by experts. Thus, the *RSS* ostensibly made first-stasis existence claims about the probabilities of various kinds of failures, radiation releases, fatalities, and health effects, as well as second-stasis classification claims about how these probabilities compare with other risks, such as fires, dam failures, air travel, hurricanes, and toxic chemicals. Yet the conclusions press subtly into the fourth-stasis policy arena, both in the Main Report and in the Executive Summary: "The operation of 100 reactors will not contribute measurably to the overall risks due to acute fatalities and property damage from either man-made or natural causes" (I, p. 132); "The number of cases of genetic effects and long-term cancer fatalities is predicted to be smaller than the normal incidence rate of these diseases. Even for a large accident, the small increases . . . would be difficult to detect from the normal incidence rate" (Executive Summary, p. 2). Although the Executive Summary ends with the disclaimer about policy, it also includes the following widely quoted statement: for 100 similar nuclear plants, "the chance of an accident causing . . . 1,000 or more fatalities . . . is 1 in 1,000,000 per year. Interestingly, this value coincides with the probability that a meteor would strike a U.S. population center and cause 1,000 fatalities" (ES,

117. Subcommittee on Energy and the Environment, *Observations on the Reactor Safety Study* (above, n. 30), p. 30.

118. Ibid., pp. 19–20. The House Subcommittee was chaired by Rep. Morris K. Udall, a central figure in congressional criticism of the *RSS* and its connection to the Price-Anderson Act.

p. 9). The policy implications in this comparison are nearly inescapable: the risk is so low that it would be foolish to let it deter the use of nuclear energy to generate electricity. The disclaimers, then, are overt indications of the technicizing of the *RSS*: the claim to a technical stasis is also a claim to the presumptions of the technical arena rather than the public arena—that is, to the authority and values of expertise.

The boundary between technical and policy issues is a rhetorically porous one, which is just to say that stasis is a rhetorical phenomenon, not a scientific one, a dimension of argument that will be shaped to the exigence of the moment. In a study of expert advice to Congress in the SST controversy, Ian Clark found that the distinction between technical and policy questions was rarely maintained, claiming that such confusion can lead to exploitation and propagandizing rather than good policy.[119] Daniel Bell noted similar confusions in the ABM debate in the late 1960s, concluding that "technical issues cannot easily be separated from political ones, and scientists who come into the policy arena will necessarily be advocates as well as technical advisors. But one facet cannot be a shield for another."[120] Critics of risk assessment focus on this point, contending that no phase of risk assessment can be as value-free as the Red Book requires, and that consequently the demarcation between policy and science cannot be as simple as assumed.[121] And the ambiguities of stasis, along with the confusions of cognitive and administrative authority, allow technical expertise to stand in for moral and political authority, that is—they allow the reduced one-dimensional ethos described earlier to suffice for a full three-dimensional Aristotelian ethos.

As noted earlier, Plato emphasized that we should take our advice from experts, and Aristotle agreed that those expert in various arts and disciplines could reason and communicate about them better than a skilled orator having just a passing acquaintance with a subject—but, he added, they would be better only among other experts (I.i.12). For Aristotle, however, such expert talk is not rhetoric; expertise removes one from the public forum to the more restricted

119. Ian D. Clark, "Expert Advice in the Controversy about the Supersonic Transport in the United States," *Minerva* 12 (1974): 416–432.

120. Daniel Bell, *The Coming of Post-Industrial Society* (New York: Basic Books, 1976), p. 402.

121. Jasanoff, "Bridging the Two Cultures" (above, n. 11); Harry Otway and Brian Wynne, "Risk Communication: Paradigm and Paradox," *Risk Analysis* 9 (1989): 141–145; Shrader-Frechette, *Risk and Rationality* (above, n. 11)

company of the wise, and from the realm of rhetoric to that of dialectic or scientific demonstration (I.ii.21). In such company, where expertise prevails, ethos is unnecessary. Demonstration reasons from true and universal premises, and dialectic reasons syllogistically from generally accepted opinions that "seem right to all people or most people or the wise—and in the latter case all the wise or most of them or those best known and generally accepted."[122] The intellectual quality needed by the dialectician or the wise person is not *phronesis*, *arete*, or *eunoia*, but *sophia* (wisdom), and it is needed not in order to persuade others, but simply in order to know the premises and conclusions. In dialectic, as Eugene Garver says, logos drives out ethos.[123] Thus dialectic is impersonal—unlike rhetoric, which is a relational art, because, as Aristotle put it, "the persuasive is persuasive *to someone*" (I.ii.11, emphasis added). Risk analysis, like most expert discourse, presents itself not as rhetoric but as dialectic. And if we think of dialectic as an ideal communicative situation, in which there is agreement on ends and values and a perfectly knowledgeable and rational audience, then expertise is the only quality of character needed to prevail. In Aristotelian rhetoric, then, *ethos stands in for expertise*, because it occurs in situations where either complete knowledge is unavailable or the audience is not adequately knowledgeable or competent: *arete* and *eunoia* make up for the lack of knowledge. In contrast, because I take the relational complexities of rhetoric rather than the idealizations of dialectic as the grounds for theorizing, I have been arguing that in a technical discourse like risk assessment *expertise stands in for ethos.*

The substitution of expertise for ethos is not unique to risk analysis, but as a case example, risk analysis has the advantage of making the consequences of that substitution manifest. An ethos of expertise—that is, an ethos grounded not in moral values or goodwill, or even in practical judgment, but rather in a narrow technical knowledge—addresses its audience only in terms of what it knows or does not know. The diminution of *arete* and *eunoia* in an ethos of expertise has a specifically rhetorical effect, because these qualities are relational in a way that expertise is not; similarly, the transformation of *phronesis* to *episteme* diminishes the practical, or relational, dimensions of knowledge. Without *arete* and *eunoia*, there is no basis for agreement on values or for belief in the good intentions of a rhetorical agent; the rhetorical relationship becomes impersonal.

122. Aristotle, *Topica*, trans. E. S. Forster, Loeb Classical Library (Cambridge, Mass.: Harvard University Press, 1960), I.i.100b18.

123. Garver, *Aristotle's Rhetoric* (above, n. 77), p. 182.

The consequence is, as Garver claims, that "when ethos disappears, so does trust."[124] We trust those in whom we sense goodwill (*eunoia*), those with moral qualities (*arete*), and those whose knowledge can be applied to our practical problems (*phronesis*). Garver notes that "speakers lose our trust when they act as though the problems we face are not practical but theoretical,"[125] a situation identical to the stasis confusion discussed earlier. The impersonality of an ethos of expertise runs the risk of being persuasive to no one.

And in fact, in the voluminous literature on risk communication—the field that developed as risk analysis failed to win over the public on its own terms—trust has become a central issue of research. Lack of trust in experts and in government agencies on the part of the public is said to explain much of the data on public resistance to official risk analysis.[126] The National Research Council's recent contribution to the risk literature also notes that mistrust is often at the root of conflicts over risk.[127] In its commitment to expertise, risk analysis sacrificed its claim to public trust, and the resulting political difficulties have been entirely predictable from a rhetorical point of view. Does lack of trust affect dialectic, that is, the success of risk analysis with an expert audience? The Aristotelian would say no, that here only reason and evidence will make the difference. However, as Shapin has argued, trust is essential to the constitution and maintenance of bodies of knowledge, just as it is essential to maintaining social order.[128] In other words, even among the wise, pure dialectic is not possible—a conclusion supported by the recent decades of work in the rhetoric of science. The epistemic order requires moral virtues and a truthful and trusting intention toward others. Risk analysis and other expert discourses have had great rhetorical success, in part due to the presumptions that expertise has been able to gain. But their success is limited by the loss of trust—that is, precisely by the poverty of their ethos.

124. Ibid., p. 190.

125. Ibid., p. 154.

126. William Leiss, "Risk Communication and Public Knowledge," in *Communication Theory Today*, ed. David Crowley and David Mitchell (Stanford, Calif.: Stanford University Press, 1994), pp. 127–139; Peters, Covello, and McCallum, "Determinants of Trust and Credibility" (above, n. 104); Paul Slovic, "Risk Perception and Trust," in *Fundamentals of Risk Analysis and Risk Management*, ed. Vlasta Molak (Boca Raton, Fla.: Lewis, 1997), pp. 233–245.

127. U.S. National Research Council, *Understanding Risk: Informing Decisions in a Democratic Society* (Washington, D.C.: National Academy Press, 1996).

128. Shapin, *Social History of Truth* (above, n. 79), pp. xxvi, 8–17.