1 Risk Sciences: Expertise for Decision-Making and Dispute

Risk assessment emerged in the 1970s as a regular component of the dominant paradigm of systems analysis, alongside "cost-benefit analysis, technology assessment, social forecasting and the like" (Hoos 1979, 192).[1] From the moment it first took shape, risk assessment was considered to be a symptom of the emergence of "new forms of technology management, the most visible of which are detailed analyses of the anticipated impact of proposed developments" (Fischhoff 1977). Economic cost-benefit analysis, general systems analysis, operations research, decision-theory way of thought, and risk assessment all are "attempts at policy science" (Wynne 1975, 118). They comprise a "family of techniques … conceived as ways of improving decision-making by broadening the role of logic and empirical inquiry" (Tribe 1972, 75). Rip (1986) later labeled this set of sciences "strategic" sciences, to convey the fact that they shared a similar interest in aiding decision-making.[2]

By shaping and embracing the quantitative assessment of health risks or the comparative economic analysis of their reduction, the EPA has placed itself in the ambit of these sciences, and of this particular way of understanding the administration of the environment and health, as a way of making rational decisions. Sociologists and philosophers, very often critical of these policy sciences, tend to argue that they are representative of an expanding technoscientific or technocratic ideology. This narrative, however, obscures the contextual and historical constitution of these sciences and of their techniques. They were born in the context of public controversies surrounding technologies and their hazards, as well as policies for managing them. Risk research, it appears, is knowledge formed to respond to public controversies about environmental and health hazards, with a view toward solving them.

The Sociological Critique of Policy Sciences

Risk assessment has emerged as the discipline of quantifying the negative events in technical systems. As such, it is intimately linked to systems analysis. Originally, systems analysis was the name that was given to the civic, administrative, and corporate transposition of the particular discipline known as "operations research" in the United States or "operational research" in the United Kingdom (Thomas 2015). *Operational research* was the name that various departments of military and defense ministries in these two countries gave to services in charge of the analysis and optimization of military operations, equipment, and weapons. Operations research sought to apply a theory to an almost infinite set of contexts of action. It was the stuff of physicists and mathematicians, recruited to work closely with military personnel, to review and analyze operations in the field and to find ways of optimizing them by applying a "scientific method."[3] It aimed to provide the means for choosing among various solutions in a situation with multiple options and high levels of uncertainty as to the capacity to attain the defined objectives.

Systems analysis evolved after World War II as an attempt to generalize the philosophy of operations research (i.e., optimization) beyond military issues and organizations. The RAND Corporation, an organization for research and development founded by the US Air Force, became the hub of that intellectual enterprise (Jardini 2000). The concept was theorized within its walls and applied to more and more cases, particularly under the intellectual drive of Charles J. Hitch, a British economist. Hitch theorized the extension of operations research into an approach for guiding choices and rational decisions, becoming a "generic form of policy analysis" (Thomas 2015, 268), a more widely applicable heuristics aimed at articulating and exploring ranges of scenarios and decision options, rather than at identifying the only optimal decision.[4]

Systems analysis started to be applied within government and gained public visibility in the 1960s when Robert McNamara became secretary of the US Defense Department, and Hitch his general comptroller. Together with other "whiz kids" recruited from RAND and the Ford Corporation (where McNamara worked before joining President John F. Kennedy's cabinet), they applied the Planning Programming Budgeting System (PPBS) method that Hitch had developed at the RAND. In 1965, President Lyndon

B. Johnson asked for the PPBS method to be generalized across the federal government. This meant that systems analysis, and specifically the kind of cost-effectiveness and cost-benefit analysis branded by RAND professionals to improve economic efficiency, started to be applied to a variety of new federal programs composing Johnson's "Great Society" vision, from poverty to mass education to urban renewal and health care (Enthoven 1980).

Systems analysis and risk analysis share several important programmatic dimensions. The first concerns the possibility and desirability of rationalizing human actions: from game theory and the concept of utility, what these sciences maintained was the belief that human actions could indeed be directed by optimal decisions, or decisions that would achieve the highest levels of utility for all players in a system. That notion was also apparent in cost-benefit and risk-benefit analyses, which sought to balance risks and costs, or risks and benefits, as if they could be measured on the same plane and their relation or ratio optimized. A second, cross-cutting claim was that mathematical and statistical analysis could indeed be used, as in the prisoner's dilemma, to compute these utility functions. From systems analysis to technology assessment, the motto was: "Data, quantification, calculation."[5] Data gaps or uncertainties were not irredeemable: all of these sciences built on probabilistic thinking and believed in the power of probabilistic analysis, whether objective or subjective, to identity optimal outcomes (Fortun and Schweber 1993). Third, policy science was marked by a belief in the power of a generically conceived scientific method (Mirowski 1999, 2002; Thomas 2015). Systems analysis, technology assessment, and, later, risk assessment all built on a similar belief in the power of fundamental thought processes and modes of reasoning to solve multiple kinds of problems, as well as a distinctive ethos of abstraction, whereby problems are captured through standard, quantitative parameters rather than studied empirically.

Rationalization/optimization, quantification of probabilities, and scientific method were the pride of practitioners of operations research, and hence of systems analysis and policy sciences as well. All three traits were shared by the practitioners of the emerging practice of risk assessment. Furthermore, all of these sister sciences were the object of the same scathing critique by sociologists, such as Ida Hoos.[6] Hoos took up all three traits of policy science, but then derided each of them. First, where analysts saw optimization, she discerned marketing and a lack of accountability. She claimed that systems analysis was an oversold, booming business—an artificial market energized by professionals (not only engineers and economists, but also so-called soft scientists such as environmental designers, urban planners, political scientists, and sociologists) in search of professional prestige and contracts. Likewise, engineers and economists involved in risk assessment were contract-seeking advisers who drew from massive public budgets the funds to perform studies that never got reviewed and had no substantial impact on decisions (Hoos 1983). Systems analysts were everywhere in government, where they claimed to render decisions and policies optimal. Yet, in practice, any actual systems studies came down to an endless, goalless collection and processing of data, which produced obscure results, if any at all.

Second, in Hoos's work, quantification becomes "quantomania": "What cannot be counted simply doesn't count, and so we systematically ignore large and important areas of concern" (Hoos 1979, 193). Quantomania was equated with the drunkard's search for his keys under the lamppost because there is more light there, even if it is nowhere near where he dropped them. Uncertainties and data gaps were pervasive, yet completely ignored by risk analysts, who replaced them by strings of intuitive guesses and assumptions (and all the while accusing citizens and laypeople of irrationality). Actual risks and disasters were firmly denied by engineers, who stuck to their fictitious calculations to show that risks were very low and concrete consequences minor. Risk-benefit analyses and risk comparisons served essentially to force acceptance of ongoing technological developments (notably nuclear energy) on the public.

Third, Hoos reduced the love of the scientific method to an inability to embrace the complexity of social problems. These scientistic exercises, she claimed, shared a recognizable methodological tropism, or "subtle shift from problem or substance focus to methodology or technique focus" (ibid., 193; see also Tribe [1972] on "process reduction" in policy sciences, and Kramer [1975]). Engineers', economists', and natural scientists' faith that science makes complex problems tractable and amenable to decisions was deceiving. It actually stifles social debate on the criteria and values to apply in the selection of what mattered to the population. Hoos held that "society" was not a system like a weapons system (there was no objective, valid definition of its limits; one did not "solve" the problems in that space definitively, and there were no right or wrong solutions concerning health or urban renewal). Systems analysis perpetuated the illusion that grave problems facing society and social groups could be measured, managed, and even solved in an optimal manner, with no involvement by the people concerned with those very problems.

Policy Science and Political Domination

Like Ida Hoos, whom he cited, Wynne came to risk assessment after considering the institutionalization of technology assessment.[7] He questioned "the political model" underlying the conceptualization of the latter (Wynne 1975, 125), claiming that with technology assessment, politics took on a new, scientific, and decisionistic form that distracted the public from fundamentally political questions. Wynne argued that there were clear "commitments and premises which frame the analysis and action of the [technology assessment]-policy scientific movement: first, man is an exclusively materialistic, uniformly rational economic calculator; his society—the only conceivable society—is a utopian version of leisure-class American. The natural environment is to be replaced by an artificial one created by corporate commerce. Mammoth and sophisticated technology administers man's every need; political conflict is emasculated to 'technical' questions over the acquisition of scarce resources; other societies don't exist except in so far as they strive to attain this utopian—or is it dystopian—ideal" (ibid., 115). This paradigm discouraged public participation and assumed that quantitative analysis of impacts and risks was a medium for managing (and limiting) political confrontations and producing supposedly harmonious decisions. The expansion of this "common political-technical language" (ibid., 126) worked, in practical terms, in favor of corporate interests. Technology assessment, as a language of objectivity, cohered with a politics of consensus and an economic model rooted in corporate monopolies and an unbridled quest for capitalistic growth.

Inspired by Jürgen Habermas (1971), Wynne explained that the scientistic language of technology assessment was one medium of legitimation. Under the rhetoric of consensus, and because of its unicity and lack of competing paradigms, it effectuated a dire, almost totalitarian, social control: "A second and perhaps more appropriate model in our case is the one which he [Habermas] terms 'decisionistic'. In this model, the political process still has a place, but its terms are entirely circumscribed by technical predicates, definitions and means.… It is also a more pervasive form of scientism just because it obscures the resultant distortion of the nature of politics and authority under the fallacious impression that politics is still alive and well" (ibid., 123).

Wynne approached risk assessment later, as he set out to study the Windscale inquiry,[8] using the emerging sociological methods of laboratory studies to study "scientific knowledge in important public arenas" (Wynne 2013, xvii). Through numerous publications (Wynne 1982, 1996, 1995, 2002, 2005, 2013), Wynne developed a "sociological analysis of scientific rationality" (Wynne 1982, 127), through five recurring, often critical themes. The first is that risk is a particular, reductive way of framing concerns that exist in the public, and which relate not strictly to the measure of the frequency and severity of predefined events, but also to the value of technological developments for variegated social projects and worldviews. Risk assessment involves a shift: The issue of the presence of technology in our societies is no longer raised as a question of collective choices to be made, but purely as a problem of social acceptance and management of the negative impacts of technology. The political choices that have supported technologies are rendered invisible, and the debates on the common worlds brought about by them are concealed (Wynne 2002; see also Rayner 2003, 2007). There are hidden commitments—a favorite term of Wynne—behind risk assessment. However, by framing the technological problem in strict scientific and metrological terms, it effectively shuts off all possibilities for open deliberation around the ends and values of technology.

Second, risk sciences operate with a standardized model of the citizen as interested only in the utilitarian measure of the consequences of technologies, as only concerned with safety; this model presents the citizen as being incapable of dealing with uncertainty and constructing independent social meanings through autonomous relationships, outside institutions (Wynne 2005,72). The third theme is the claim that reliance on these analytical exercises and on its practitioners severely restricts institutions' ability to learn and compounds a general lack of reflexivity. Risk assessment, being insensitive to the diversity of meanings and commitments toward technologies or the environment, actually fuels the frustrations of the public and communities with regard to institutional policy mechanisms. Fundamentally, it partakes of the declining trust in institutions. The fourth theme is the construal of the public as a figure of error and ignorance. Finally, the fifth theme is that risk analysts or risk assessors are the cadres of the rationalization of society and of its technoscientization. Risk assessment is an instrumental science, pushed by an elite, that is part and parcel of the policy of technological development.

Wynne's arguments provide the beginning of a theorized answer to the problem of why and how risk assessment acquired some sort of currency in the administration and governance of technology-related issues: It is because of its capacity to depoliticize public opinion and to conceal power and domination issues, naturalizing the monopoly of technical experts and bureaucracies in tackling issues at the expense of the wider public. He saw it as a reinvention of the language, whereby technocracy naturalized ongoing technological developments, colonized the life-world, and removed itself from public consciousness and critique. For Wynne, risk assessment and its professional cadres are not assistants to making decisions about technologies; rather, they are part and parcel of a ready-made decision to develop technologies and diffuse them. They are the technology themselves.

Risk: Professional Domination

In 1981, the risk profession became visible with the creation of the Society for Risk Analysis (SRA) in the United States. *Risk assessment,* by then, had become an official label for a new group of professionals close to government and major petrochemical corporations and energy utilities (Hoos 1983). In 1984, James Short Jr., then-president of the American Sociological Association, focused his annual address on the fact that "an entire industry has grown up around risk analysis, complete with professional trappings, governmental agencies, and personnel drawn from a variety of disciplines and professions" (Short 1984). As an organizational sociologist interested in accidents and disasters, on the path toward the development of a theory of "normal accidents" that confronted engineers' faith in the reliability of large engineered systems, Charles Perrow had more reasons than any other scholar to cross the path of risk analysts. His curt and incisive paper (Perrow 1982)[9] was motivated by his attending a symposium in 1981, the proceedings of which were published as *Societal Risk Assessment: How Safe Is Safe Enough?* (Schwing and Albers Jr. 1980). In it, he reviewed the various papers delivered at the conference by leading figures in the field, from Howard

Raiffa to Lester Lave. Perrow was quick to detect the particular worldview (and political inclinations)[10] underpinning the conversation among the new risk professionals. According to Perrow, the speakers had in common a misrepresentation of the public as a mass of ill-informed, irrational individuals who were so frequently wrong about the real safety issues that they should not be allowed to make a contribution to the issue. Perrow also pointed out the biased agenda of risk professionals: "If the public is misrepresented, the issues are more so. The cases that are most frequently discussed are nuclear power, toxic chemicals, genetic engineering, and various forms of transportation. In each of these cases, the risk-acceptance position is the one that favors private business and industry, and the risk-aversive position would harm it" (Perrow 1982, 299).

Risk and Culture (Douglas and Wildawsky 1983), written at about the same time, proposed an equally incisive critique of risk analysis. This book inaugurated an original theory to understand why and how certain collective hazards or harms come to matter more in a social group, given the diversity of such hazards—a question that risk assessors typically overlook, focusing as they do on a limited set of risks, and one that risk perception specialists often misrepresent. These authors argued that what people choose to worry about depends on their worldviews and their belonging to a given social group. From this perspective, anyone is entitled to worry about whatever she or he chooses to worry about; it is just a matter of cultural selection of hazards out of a broad range of hazards and uncertainties that exist in the wider world.

The book is a clear attack on the view, common among engineers, toxicologists, economists, and statisticians, that people worry about the wrong risks. It confronts these experts, and their claim to know risks better, by making the simple point that, as there are so many dangers threatening us individually or collectively, we cannot know them all, or know them fully; hence, uncertainty matters. In quantifying the costs and benefits associated with particular damages (ibid., 69), risk assessors overlook uncertainty, assuming that "what is true for the past will remain true for the future" (i.e., data about past frequency of events is extrapolated as a measure of what will occur). They further claim that the costs and benefits of technologies can be measured in the marketplace, and that people have sufficient information to make intelligent choices. However, none of these assumptions can be verified: relations to risk in a social context are moral and political and depend on changing, variable values. There are, moreover, massive uncertainties in the calculation of probabilities.

So risk assessors' claim of measuring risks objectively is misleading. In the harsh words of Douglas and Wildavsky, "it is a travesty of rational thought," a kind of "naked objectivity" that applies standard methods without consideration and relevance for the social contexts of risk, and one that comes down to "educated guesses." Disengaged from value debates, working at a distance from the public through numbers only, assuming no role in public decision-making (only in giving "advice"), risk assessors stop where responsibility starts (ibid., 80–81).

Mary Douglas remained interested in the emerging profession of risk analysis, its worldview, and its social role,[11] and continued to write about the profession from her own, anthropological perspective in order to show that risk assessment was the locus of a particular political morality. Industrial hazards, the new taboos of industrial societies, were appropriated by a new class of professional public figures: the specialists of risk analysis and risk perception. The kinds of things that were being called risks in the 1970s and 1980s, typically, were pollutions. Douglas saw pollutions as objects that threatened the cohesion and solidarity of a group; they were things that got designated as bad and foreign to us, and were made invisible to protect the integrity of our communities. The quasi-science of risk assessment was essentially a central institution of judgment and blame attribution in modern, stratified societies.

Risk analysts are precisely in the business of finding causes—or faults—for all the hazards that affect us as individuals. They fulfill the social function of blame, and risk assessment is a blaming technology: "its forensic uses fit the tool to the task of building a culture that supports a modern industrial society" (Douglas 1992, 15). It is the sign of an adversarial, blame-your-neighbor type of society. This institutional role is reflected in the particular epistemology and values that define risk professionals: a defense of objectivity and ideological neutrality, the belief in the rationality of the individual, a preference for methodological individualism, and a depoliticized perspective on problems and situations (Douglas 1990). This forensic role combines with an ideological discourse of social domination over the rest of social beings, which are considered irrational (Douglas 1992, 13).

Secondary Scientization and the Reinvention of Expertise in the Risk Society

In the ideological analysis advanced by the critics of risk assessment, risk assessment (and policy sciences more generally) are the reincarnation of a perpetual technocratic project of rationalization, which is seemingly unaffected by the context of conflicts and controversies surrounding science and technologies. It is curiously decontextualized, and variations and historical changes in the actual practice of risk assessment are mostly overlooked.[12]

Risk assessment appeared in a context in which threats to our safety, environment, and health had become primary issues of concern—which was precisely the vantage point from which critics of policy science and risk assessment could claim that risk assessors ignored publics, knowledges, and meanings. Beck's argument in *Risikogesellshaft* (Beck 1986), his opus on the risk society, is that hazards are inherent to the nature of contemporary societies and manufactured by them. The scale of manufactured risks is bigger than any of the safety issues societies have dealt with thus far. They are often irreversible and extend across time, space, and social groups. They are fundamentally difficult to analyze and apprehend collectively. In the English translation of his book, Beck (1992) makes no explicit mention of "risk assessment" or "risk analysis," and names no professional risk group.[13] But Beck does put forward an original argument about the kinds of techniques and expertise that were being reframed at the time as elements of risk research.

The rise of manufactured risks magnifies one of the greatest tensions in the politics and government of contemporary societies: their relationship to specialized, scientific knowledge as a means of dealing with collective issues. One of Beck's main theses is that science has lost part of its credibility and authority to describe the risks that we face and to prescribe what to do about them. In his lapidary terms, "there is no expert on risk" (Beck 1992, 29). The experience of disasters, risks, and uncertainties is dispersed into societies, and thus everyone is an expert on risk and may participate in the social definition of threats. Scientists' typically rational, calculative understanding of these risks is only one perspective on risks. The authority of science is challenged by the difficulty to calculate risks and establish their causes: "ultimately no one can know about risks, so long as to know means to have consciously experienced" (ibid., 72). Scientists have lost their monopoly over the production of validated knowledge. Their social authority is breached and makes space for explicit distrust. The autonomy and professionalization of science are severely restricted, as citizens question the knowledge and proofs that come forward from this space (ibid., 157).

Beck's thesis about the loss of authority of science is actually intertwined with a second thesis called *secondary scientization*. He discerns a historical shift toward a new mode of scientization in the postwar period. The first phase of scientization, during the Industrial Revolution, was an era when disagreements among scientists and engineers were managed in confined spaces, and where the truth-bearing products of science were transferred to society without question. In the risk society, internal errors and criticism among scientists are exposed because the public has come to doubt technoscientific products. Yet science does not recede entirely when challenged. Scientization continues because the social controversies over technosciences and associated risks are themselves couched in the language of science. All the parties engaged in the definition of risk employ scientific language, notably what Beck calls the "scientized ecology movement" (ibid., 162). Science expands under the form of a language of doubt and critique that fills the public sphere. Scientization thus increases while the authority of scientists diminishes. In the risk society, science emerges as a medium of controversy. It partakes in social contexts of dispute made of data comparisons, interpretive wars, and mutual deconstruction of methods.

The secondary scientization thesis can potentially transform the critique of policy science. Some of Beck's writing echoes the critical arguments of Hoos, Wynne, and others. He points to the fact that scientists tend to legitimize and force the acceptance of risks. He agrees with them that beneath the cloak of objectivity and scientific demonstrations, science justifies private interests and commercial values. Science has become "self-service shops for financially endowed customers in need of arguments" (Beck 1992, 173). Self-appointed scientific experts on risks advance convenient assessments of low or negligible risks of continuing to develop and diffuse technologies (Beck 1992, 173).

But Beck also notes that risk controversies affect scientists who act as experts on risk, and thus they become a driver of transformation of scientific expertise, its content, and its modes of expression in public. He does not fold risk research and risk assessment strictly into science, but rather sees them as the product of an expansion of science in a society marked by controversy. They are the scientific disciplines' response to the new conditions of uncertainty, interpretive flexibility (Collins 1981), and controversial scientific statements on risk. *Secondary scientization* means that more people and groups wield scientific arguments in generalized disputes about the definition of risks and secondary adverse effects. A game of expertise and counterexpertise sets in, in which science is prompted to move toward new competences and territories as it is embroiled in controversy. In Beck's words:

> New public-oriented scientific experts emerge, the dubious aspects of the foundations of scientific argumentation are exposed with counter-scientific thoroughness, and many sciences are subjected through their applied practices to a "politicization test" of a previously unknown extent. In this way, science not only experiences a rapid diminution of its public credibility, but also opens new fields of activity and application for itself.… Here the self-contradiction that scientific development has got into in its reflexive phase becomes tangible: the publicly transmitted criticism of the previous development becomes the motor of expansion. (Beck 1992, 161)

Risk assessment must be seen as characteristic of the era of secondary scientization, of an altogether different contract between policy and science premised on the impossibility of speaking truth to power or of conveying knowledge directly from science to decision-makers—premised, furthermore, on the fact that science has been harnessed by publics and interests.

Thus, this process is politicized and can serve to translate disputes, not avert them altogether. Risk assessment, from this perspective of secondary scientization, is the product of the transformation of scientific rationality under the effect of generalized doubts and interpretive flexibility of scientific claims in controversial contexts. It is less the reincarnation of a technoscientific, instrumental rationality that ignores and suppresses social meanings and relationships to technologies and their risks. Instead, it emerges as a product of these controversies: a reaction of scientific disciplines and of professions that have a stake in the development and diffusion of technologies, to the disputes that their products generate in the public sphere. The science of the assessment of technologies and risks is the product of the encounter between science and social conflict.

Risk Research and the Political Context

Risk research came together as an interdisciplinary field in the 1970s (Rip 1986; Krimsky and Golding 1992; Thompson et al. 2005; Bales 2009; Boudia 2014). The field was structured through the SRA. Risk research lay at the intersection of five separate fields that embraced a probabilistic perspective on the diverse kinds of hazards they were studying (Rip 1986). The first was risk analysis itself, derived from the application of systems analysis to nuclear safety issues. The second was safety and reliability engineering—a far less visible tradition, but equally linked to the search by governments and corporations for optimized, standardized safety in engineered systems such as transportation or industrial production. The third root was research about catastrophic events and disasters, particularly in the discipline of geography. Actuarial statistics constituted the fourth root, and medical research the fifth (i.e., toxicology and epidemiology specifically, aiming to define comparative rates of mortality and of untoward effects). Research on risk perception forms an emergent, sixth branch as well. The various strands of research converged toward the use of the word *risk*, rather than *hazard*, out of an interest in probabilistic calculation, judgment, and mitigation. It also stemmed from their search for new audiences and sources of funding.

The creation of the SRA did not merge these groups into a single new disciplinary entity.[14] The areas of risk research focus on very different sorts of hazards, use different methodologies of calculation, and speak to different audiences. The term *risk*, which successive working groups of the society have tried to define, is quite elusive (Thompson et al. 2005). The first leaders of the SRA ensured that it would become a home for three such groups in particular: safety and reliability engineers (the smallest of the three), health and toxicity people, and social scientists (Bales 2009). For several of these (particularly risk analysts, engineers, and toxicologists), it was a way to move away from a peripheral or subordinate position in their originating discipline and toward a home where their identity would be respected more. Risk research, even after the launch of the SRA, remains a disciplinary archipelago (Hood and Jones 1996; Bales 2009).

One reason for the internal differences in the "interdiscipline" (Klein 1996) is that risk research evolves in response to institutional demands for restoring capacities to decide and choose in the face of social protest (Rip 1986). The very first articulation of assessing risks of nuclear accidents was by an engineer reacting to the problem of deciding on the siting of nuclear facilities (Farmer 1967). Civil use of nuclear technology has always been controversial, ever since the project of adapting this military technology to the production of electricity was formulated (Nelkin 1971).

It soon became the object of national disputes waged in the media, as well as local conflicts surrounding the siting of plants and later of nuclear waste facilities.

At a meeting of the International Atomic Energy Agency in Vienna in 1967, F. R. Farmer suggested converting safety into a measurable index of risk, implying that numerical levels of acceptability and unacceptability could be established precisely. In 1969, Chauncey Starr, an engineer, physicist, and then-dean of the School of Engineering and Applied Science at the University of California, Los Angeles (UCLA), felt the need to engage in the mounting controversies surrounding the development of the civil use of nuclear energy and to demonstrate that public concerns about the safety of that energy were unfounded. Starr's paper was developed as part of his chairing the National Academy of Engineering's Committee on Public Engineering Policy (Boudia 2014).[15] In a paper published in *Science*, he computed a risk-benefit ratio for a set of technologies (car, civil aviation, nuclear reactors), implicitly taking on the context of contested technological developments. Comparative measurement of risks was a manner of demonstrating to the public that focusing on that particular hazard was pointless. It was a way of telling groups and communities mobilized against that technology to shift their focus to other concerns. The paper was one of the most visible and powerful articulations of the notion, repeated ever after by everyone in this field, that involuntary or uncontrolled risks are always more negatively and acutely perceived than voluntary, controlled risks, and that the public directs its attention to these risks, however less probable or damaging they may be.[16]

This link between comparative risk assessment and the handling of public controversy also verifies with legally mandated exercises of cost, risk, or impact assessment. Environmental impact assessment burgeoned after a requirement to perform such analysis was included in the US National Environmental Policy Act of 1969, which provided the impetus for federal agencies to hire or consult thousands of environmental analysts (Clark 1997). Toxicology accelerated when assessments of the safety of food additives, pesticides, and chemicals found in air and water were made mandatory by successive laws that shaped a new social and environmental regulatory landscape. Successive executive orders for the quantification of costs and benefits of regulations (starting with Executive Order 12291, passed in 1981, mandating the performance of cost-benefit analysis for all rules, with an anticipated impact on the economy above $100 million) were the most direct reason for the federal government hiring economists. A study of risk professionals in the United States showed that a third of them were working for federal organizations (Dietz and Rycroft 1988).[17]

The point is that, with “policy sciences” broadly speaking, scientific norms and procedures were being imported “into a political setting of intense conflict” (Taylor 1984, 8), where public decisions had been formidably difficult to shape and implement. The idea that political contexts structure risk research was evidenced in the birth of the SRA. Two of the three disciplinary groups inside the SRA, in fact, were dealing with the most intense public disputes of the time: Safety and reliability engineers were directly involved in the controversy over risks relating to the expanding civil use of nuclear technology; health scientists were involved in the controversy over the risks of chemicals in food and in the environment, and they also were decisive in forging ways of rendering the chemical problem controllable (Sellers 1999; Frickel 2004; Shostak 2013). The third area of risk perception consisted of precisely the field addressing—and translating into tractable terms—the problem of the public and the emergence and expression of its collective preferences, as well as the difficulty of applying decisions where they remained controversial.

This embedding in a political context has implications for the kinds of knowledge produced by risk research (Rip 1986). First, the engagement with controversial contexts means that the field had adopted ways of conceptualizing social controversies and their causes, and of demarcating the scientific contribution to decision-making from the realm of politics. Notions of risk evaluation and risk acceptability featured centrally among the concepts that helped the field take form. In his book *Of Acceptable Risk*, William Lowrance, an engineer by training, distinguished between a factual, scientific measurement or assessment of risk and a normative judgment of safety. He recommended distinguishing these two planes of discussion explicitly, so that value conflicts would not disrupt the agreement on facts (Lowrance 1976).

The fact/value distinction was not always easy to apply, but William D. Rowe’s contribution improved the concepts further. In his *Anatomy of Risk* (Rowe 1977)—originally a report to the EPA, dating from 1975, to review the attempts to settle the nuclear dispute by means of probabilistic risk analysis (Rowe 1975)—he did away with the objective/subjective, fact/value dichotomies. He argued that the main distinction to make was not between science and policy, but between technical value judgments, those "made by experts in the absence of hard technical information when faced with the difficulties in obtaining further information" (Rowe 1975, 3) and "societal" or "managerial" judgments of appropriate balance between risks, costs, and benefits. This codification of the politics of risk helped the field assert its legitimate role in decision-making, as shown by the themes that the founders of the SRA selected for the society, notably the "application of assessments to such things as cost-benefit evaluations, administrative actions, political factors, etc." (Thompson et al. 2005, 1336). The center of gravity of the interdiscipline is not simply calculating risk with ever more sophisticated methods and data, but contributing to the intellectual process of forming judgments and public decisions.[18]

The close relation with institutions and with contexts of political uncertainty surrounding acceptance of hazardous economic activities meant that there was a tight connection between the political context in which problems of risk are defined and the actual orientations of risk research. As Rip (1986, 6) put it: "The problem-definitions used by relevant actors in different contexts have shaped the direction and content of risk research directly. In turn, the expertise shaped by such problem-definitions will produce particular kinds of policy advice." Risk analysis of nuclear installations illustrates this point—namely, that risk assessment is the field of inventing technologies to frame controversy and is attached to the context of such controversy.

The probabilistic risk analysis of nuclear reactors in the 1970s stemmed from the problematic implementation of the Price-Anderson Nuclear Industries Indemnity Act of 1957 (or Price-Anderson Act). The Act was adopted to create a pool of funds to insure against the costs of nuclear accidents and to clear a major hurdle for private investment in that energy. It stipulated that private companies were to pay up to around $10 billion of damages, while further actions (e.g., appropriation of federal funds) could be decided by the US Congress in the case of damages exceeding that sum. In that context, the definition of possible effects of a nuclear accident on populations, and the quantification of the probability and severity of these damages, were instrumental in defining insurance policies in monetary terms. In effect, this was a quest for low probabilities (Fuller 1975), which led the Atomic Energy Commission, on the occasion of the renewal of the Price-Anderson

Act in 1967, to dedicate $3 million to new probability research, resulting in the Reactor Safety Study (WASH-1400 or NUREG 75/014) by Norman Rasmussen, a professor at the Massachusetts Institute of Technology (MIT).

Rasmussen's study is remembered as the first large-scale application of probabilistic risk assessment (e.g., Bedford and Cooke 2001), considering a wide variety of possible negative outcomes related to the use of a nuclear reactor, ranging from events affecting the reactor itself to thyroid problems in surrounding populations following accidental radioactive emissions. The power of his study, a fault-tree analysis, rests as much in the identification of the initial 130,000 sequences of events, and the selection of seventy-eight more important sequences, as in the actual computation of probabilities for each of these sequences. This is where the value of the method lies, according to Rasmussen. In his conception of risk assessment, a larger error band than in a strict reliability study can be tolerated because the heart of the exercise is the consideration of the results "to see if they are meaningful" (Rasmussen 1975, 5). He articulated a logical method to identify such meaningful events and assign a number to each of them.

It is not possible to separate the report from the controversy. The authors (and sponsors) consciously engaged with the public dispute about nuclear risks. Rasmussen referred in several places in the report to the public discussions of nuclear risks, and he also set an objective of ending the confusion surrounding those risks. He also included in his study a comparison between nuclear and nonnuclear risks, to stress (as he did throughout the report) that the risks concerning nuclear reactors were low probability (Rasmussen 1975, 103). The Reactor Safety Study was heavily and publicly criticized, instantly after its publication. Rasmussen appeared to have too much sympathy for the development of nuclear energy and minimized the risks in a way that was much too evident. In light of his positive assumptions about nuclear energy, the actual computations presented in the report appeared suspect (Hippel 1977). This applied either to health damages resulting from a nuclear accident, such as genetic defects or water pollution (Primack 1975), or to the failure of reactor components (Weatherwax 1975).

Thus, this study neither resolved the controversy about the risks of nuclear accidents nor laid the foundations for a political compromise on the development of nuclear energy in the United States. But it had an impact in the longer run, as the incarnation of a procedure to organize and enable decision-making on disputed issues. "Shortly thereafter [after Rasmussen's study] a new generation of [Probabilistic Risk Assessments] appeared in which some of the methodological defects of the Reactor Safety Study were avoided. The US NRC released the Fault Tree Handbook … in 1981 and the PRA Procedures Guide … in 1983 which shored up and standardized much of the risk assessment methodology" (Bedford and Cooke 2001, 7).

From a longer-term perspective, the development of uncertainty analysis methods, both qualitative and quantitative, was risk assessors' coded response to the criticism of a technical and political nature (Hayns 1999). Several years after its publication, what was remembered from Rasmussen's study was not the actual estimations, but the heuristics that had been applied. The method was tried again several times after that, in successive reactor safety studies, which means that the report still had a legacy—it provided a way of reasoning about safety issues. Risk assessment, in the aftermath of the report, appeared less as a given numerical figure for the risk of nuclear accidents than as a general method and set of criteria (voluntary/involuntary, natural/artificial, high/low probability, acceptable/not acceptable) to be able to identify, classify, and select safety issues—in short, a socially legitimate way to talk about risk. Finally, by hiding pro-nuclear energy bias within an arcane methodology, Rasmussen paradoxically showed the value of isolating technical considerations from value discussions in a structured decision-making process. The study inspired people like Paul Slovic to study the inherently sociopolitical nature of risks (Slovic 1999). It was also a key lesson for Rowe's thinking on acceptability, societal judgments, and a decision-making structure that could accommodate both the scientific inputs and the consideration of values (Rowe 1975).

Conclusion

Risk assessment can thus be envisioned as a science that participates in the development of rational rules for decision-making in the context of the disputes that those who administer these problems face, as much as one that carries instrumental rationality into public policy. Controversies can be considered to be a primary, causal context for the development of policy sciences, and not simply scientific and industrial interests to avert deliberation and contestation by the public (Cambrosio and Limoges 1991). This has clearly been the case with technology assessment, which evolved a constructive technology assessment branch (Schot and Rip 1997). It has also been the case of systems analysis: the generic systems approach devised by Hitch and his colleagues in the 1950s was a reaction to the political contexts in which analysts were placed, as well as an adjustment to become more effective in these contexts. This has also been the case with risk analysis. Risk research and risk assessment are not inattentive to the possibility of dispute. Risk assessors do not design decisions by themselves; rather, they design opportunities for administrations to make decisions. They do not ignore public controversies. Instead, they develop methods and produce calculations to limit and avert them, in conjunction with public administrations and regulatory agencies. There is a political dimension inherent in risk assessment and in its rise—and methodologically, there is a way to do a political analysis of risk analysis—if we understand it as a science that is constrained by, and responsive to, contexts of controversy.

The ways in which risk assessors conceive of new kinds of data, analytical methods, and structures for decision-making and governance, pinpointing where the public, experts, and decision-makers stand, show that design is just such a response. Risk sciences, and policy sciences more broadly, are design sciences: sciences that construct the formal elements of a mode of decision-making to frame controversies and form decisions. They do so differently in the various subareas of risk research, with a different notion of where the controversy comes from and the kinds of uncertainty that cause them. Probabilistic risk analysis, as developed by nuclear engineers, is certainly different from the calculative-protective vision of medical scientists, who adapted probabilistic approaches to the assessment of cancer risks. This is where the mixed histories of the EPA and of risk decision-making begin. Cancer assessment was gradually formalized and put in place at the EPA in the second half of the 1970s in order to decide the dispute surrounding cancer-causing chemicals—the first of many episodes of the invention of the EPA in response to the public emergence of risk.

The Science of Bureaucracy

Risk Decision-Making and the US Environmental Protection Agency

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