

HOW ROBUST IS “SOCIAL- LY ROBUST KNOWLEDGE”?

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THE ACADEMIC DEBATE ON THE DEMOCRATIZATION of expertise has reached the level of public politics. The U.S. National Research Council, in its study *Understanding Risk* (Stern and Fineberg 1996), has suggested “collaborative analysis” as a method adding deliberation to risk analysis and risk evaluation, thus opening advisory processes to broader participation. The British House of Lords Select Committee on Science and Technology, reacting to the devastating loss of credibility of expertise after the BSE (mad cow disease) crisis, published the report *Science and Society* (2000). Finally, and perhaps most prominently, the European Union, in a white paper on democratic governance (2001) produced in connection with a working group on “Democratizing Expertise,” announced guidelines “on the collection and use of expert advice in the Commission to provide for the accountability, plurality and integrity of the expertise used” (5). It is justified to speak of a discourse that extends from academic discussions on the challenges of postnormal science and a new mode of knowledge production to public debates and declarations on public engagement with science and technology. A host of concepts reflect the change in perception that is taking place. Some of these have diffused into the public realm,

while others are still confined to their academic origins. They all communicate the need to somehow bridge the lines between politics, power, and science, and truth. And they all proclaim the new regime of accountability. “Accountability” suggests that scientific experts are held responsible to political practitioners, and beyond, to their constituencies, that is, the public. “Quality” and “transparency” of scientific advice echo the same expectation voiced, in this case, by the European Union (1997). “Participatory technology assessment” (pTA) has become a movement that has led to a variety of experiments with consensus conferences, round tables, and similar devices of deliberation that bring together scientific experts and laymen (Abels and Bora 2004; Joss and Durant 1995).

All these terms allude to organizational devices that are intended to achieve accountability of scientific knowledge production, and perhaps even the democratic shaping of knowledge and technologies as they develop. In fact, the entire discourse is focused on the basic dilemma between democratic legitimacy by representation (vote) and the legitimacy conveyed by rationality on the basis of certified knowledge. The dilemma, of course, is old, but the discourse produces ever new answers, both terminological and institutional. The new term “socially robust knowledge” (SRK) refers to the underlying epistemological issue: how to accommodate democratic procedures of representation and decision by compromise, on the one hand, and the credibility, reliability, and quality of scientific knowledge claims, on the other (Nowotny, Scott, and Gibbons 2001). It is a cornerstone in the discourse insofar as it points to the nature of scientific knowledge proper rather than to institutional mechanisms with which to solve the dilemma.

The concept of socially robust knowledge and its discursive career are well suited to reveal the underlying motives and conditions of the discourse on the democratization of expertise. Rather than adding yet another think piece on how democratization can be achieved and what the obstacles are, I intend to take a metaperspective and focus on the discourse itself.

Socially Robust Knowledge as a Rhetorical Device

“Socially robust knowledge” is a central element in Nowotny, Scott, and Gibbons’s notion of Mode 2 science (2001). It is left open whether it is a descriptive term or a normative one, but it is clear that it is something we do not yet have. We are not told what exactly socially robust knowledge is. The first mention of the term in the book comes as an aside: “The reliability of scientific knowledge needs to be complemented and strengthened by becoming also socially

robust. Hence, context-sensitivity must be heightened and its awareness must be spread. *The necessary changes pertain to the ways in which problems are perceived, defined, and prioritized*, which has implications for the ways in which scientific activities are organized” (Nowotny, Scott, and Gibbons 2001, 117, my italics). One can infer from this that socially robust knowledge has to do with problem perception, definition, and prioritizing and is also a part of contextualization.

Since there is an entire chapter called “The Context Speaks Back,” one can hope to find more information on the nature of that process. The authors describe how science has shaped society (“spoken to it”) and how now society “speaks back”: “As a result, basic science has been *de facto* reconfigured in the context of the knowledge-based economy. The other half of the story concerns what we mean by contextualization of science” (53).

Unfortunately, the answer to this important question is hard to come by. Next we read that “a Mode 2 society generates the conditions in which society is able to ‘speak back’ to science. Contextualization is invading the private world of science, *penetrating to its epistemological roots* as well as its everyday practices, because it influences the conditions under which ‘objectivity’ arises and how its reliability is assessed” (55, my italics). Although the meaning of contextualization remains vague to this point, the authors rightly feel compelled to state that “it is necessary to demonstrate that contextualized knowledge is at least as objective as uncontextualized knowledge—*albeit in a different sense*” (55, my italics).

Next we are given two meanings of contextualization: “by pointing to shifts in research agendas and how research priorities are set, and describing how the policies of research councils and other funding agencies are articulated and directed towards certain objectives”; and “a second and deeper meaning which relates to our conceptions of how science ‘really’ works” (56). The authors want to scrutinize the second, “socio-epistemological meaning of contextualization” in order to understand “how the first affects the second” (56). After a discussion of shifting boundaries, we are told once again that “it has become necessary to explore the extent to which and in what ways these processes are affecting the core of scientific knowledge production. Is there a hard epistemological core underlying scientific knowledge production which cannot be changed without destroying what makes science work?” (58).

A few pages further on, following the discussion of the problems of peer review, the authors return to asking a question: “Why is there such resistance to admitting that the result—the commonly accepted reality as defined by sci-

ence—is also open to social shaping, to cultural meanings, to integration into a life-world which science makes little attempt to explain?” (62). At the end of the chapter we are still without an answer, but we get the same normative claim again: “If scientists would openly acknowledge these perceived threats, it might be possible to develop another model of knowledge production, in which knowledge becomes *socially more robust*” (64, my italics). In addition, we are presented with another vacuous meaning of “contextualization,” but while a few paragraphs earlier it was still an open question what effects contextualization would have *if* it happened, now the authors claim that it *has* already happened: “In historical terms it is clear that contextualization has surreptitiously crept into what was once held to be the inner core of science, while science’s more outwards-oriented parts have actively and openly embraced contextualization” (64).

Alas, what we have just learned already exists is yet but a dream! Depending on a “political decision based on cultural considerations” yet to be made, “the actual practice of science . . . might be set free to explore different contexts and perhaps evolve in different directions. The research process would . . . be seen . . . more as a comprehensive, socially embedded process. . . . It is in this sense that we talk of the contextualization of science, as an enlargement of its scope and enrichment of its potential” (65).

Even though we still do not know precisely what contextualization (and therefore socially robust knowledge) means and whether it exists already or not, we are led to the next question: “How does it happen?” (chap. 7), that is, the question of “how contextualized knowledge is produced and which form it takes” (96). The answer has to do with the move of science from a socially segregated activity to an integrated one. This diagnosis is based on the unquestioned fact that universities educate for a broad labor market way beyond the reproduction of academia, and that scientific knowledge is in broad demand. A “second answer” is the “increasing prevalence and importance of uncertainty” (113). A little further in the text the authors have another insight: “Control from within,” more elaborate peer review, formal quality control, and so on are “tiny cracks in the fabric of knowledge production through which contextualization enters” (115). However, the authors do insist that autonomy and independence of science have to be preserved.

Although at the end of that chapter we are still left in the dark about what contextualization means (remember, we want to know because it is an important ingredient of socially robust knowledge), we are at least given another

example of what beautiful prose can achieve: “Contextualization . . . suggests a spectrum of complex interactions between potential and use, constraints and stimulants” (120). Not surprisingly, where there is a “spectrum” there is weakly and strongly contextualized knowledge, as well as examples of “middle-ground contextualization” (120).

Up to this point we know the following about socially robust knowledge and contextualization: It has to do with the priority setting and funding programs of research councils and other funding agencies. Scientists are apparently reluctant to acknowledge that these have changed, but if they did, science could develop its potential more fully. How far the contextualization of science has progressed is unclear, but while it is considered a good thing, it is not supposed to adversely affect the so-called epistemological core or the autonomy of science.

Chapter 11, entitled “From Reliable Knowledge to SRK,” promises to give a direct answer as to the nature of socially robust knowledge (166). Here we learn:

the more strongly contextualized a scientific field . . . the more socially robust is the knowledge it is likely to produce. What does that mean in practice? First, social robustness is a relational, not a relativistic or (still less) absolute idea. . . . Next, social robustness describes a process that, in due course, may reach a certain stability. Third, there is a fine but important distinction to be drawn between the robustness (of the knowledge) and its acceptability. . . . Of course, the two are connected—but social robustness . . . is prospective. . . . Fourth, robustness is produced when research has been infiltrated and improved by social knowledge. Fifth, and last, socially robust knowledge has a strongly empirical dimension. (167)

First, the authors turn to “reliability” as a major epistemic value. In their view, it should not be defined exclusively in terms of replicability but as validity outside “sterile spaces” such as laboratories (168). Then they enter into a lengthy discussion about the scope of consensus in view of specialization and the relationship between consensibility and consensuality pointing to different potential “experts” and practitioners. Given the expansion of communities of “stakeholders,” “is it still possible to produce reliable knowledge?” (174).¹ This question is so important to them that they repeat it three times and then insist that “there is no suggestion that consensuality, like consensibility cannot be practised in such a context” (176; see also 173–75).

Having come this far, two issues still remain on the authors’ agenda: “the potential for replication and the impact of secrecy” (176). Why these issues are

selected is unclear, but the gist of the argument is that secrecy is counterproductive even in the Mode 2 setting, and replicability is no problem because it is not achieved anyway, either in Mode 1 experimental science or in Mode 2 (176). Finally, concluding this development, the authors have convinced themselves that “reliable knowledge, as validated in its disciplinary context, is no longer sufficient or self-referential. Instead it is endlessly challenged, and often fiercely contested, by a much larger potential community” (177). Then it is added, in good circular fashion, that “reliable knowledge—to remain reliable—has also to be socially robust knowledge” (178).

Toward the end of the book we are taken to the sanctum, the “epistemological core.” While we still do not know what socially robust knowledge is, the authors insist that it does not violate the epistemological core, that autonomy and independence of science remain intact, and that scientific knowledge remains reliable. The plot thickens; suspense becomes almost unbearable. Here we expect to encounter the crucial argument. How is the contradiction between socially robust knowledge and the epistemological core resolved? “The most radical part of our argument, and therefore the most difficult to accept, is that the co-evolutionary changes . . . bundled together under the convenient label ‘Mode-2 society’ . . . have made it necessary not only to re-conceptualize the reliability of knowledge but also to question its epistemological foundations” (178). The solution to the riddle is simpler than anyone would have guessed: “the epistemological core is empty—or, alternatively and perhaps more accurately, crowded and heterogeneous.” Their argument “therefore is that a more nuanced, and sociologically sensitive, account of epistemology is needed” (179).

To make a long story short about the “ambivalence of novelty” and the “decline of cognitive authority,” this badly needed “sociologically sensitive epistemology” should “incorporate the ‘soft’ individual, social and cultural visions of science as well as the ‘hard’ body of its knowledge” (198). Thus the problem that seemed so insurmountable at the beginning has been successfully restated. Its dissolution into a harmonious accord of scientific method, new knowledge, and its social embedding is already caressing our senses: “the process of contextualization moves science beyond merely reliable knowledge to the production of socially more robust knowledge” (246).

To summarize, social robustness is, above all, a property that scientific knowledge should achieve. This could happen if science were to open itself to the social context. The nature of “context” remains vague, but it can be inferred

that it means social and political concerns, the values and interests of lay publics that are directly or indirectly affected by scientific knowledge. Their voices are supposed to be heard, at least in democratic regimes. The authors of SRK are more ambitious than many others in that they take the dilemma of power and truth to the epistemological level. What they call the “epistemological core” is never clearly defined, either, but it is associated with the autonomy of science and the reliability of scientific knowledge (58). At the end, however, they state their “radical answer” to the dilemma: the epistemological core “is empty” (179). (It is an interesting side issue that the social sciences are deemed able to provide the “sociologically sensitive epistemology” that the natural sciences are lacking.) The term “socially robust knowledge,” then, encompasses the dilemma and suggests, in programmatic fashion, its solution. To be convincing, the epistemological basis of science must be taken seriously at the outset, only to be cast aside in the end. It should not escape attention that the very vagueness of the terms involved explain their popularity among scholars of Science, Technology, and Society (STS) and practitioners of science policy because such vagueness creates the illusion that the dilemma can be solved. If the concepts were more concrete and if they had a better empirical grounding, the message would be more disappointing.

Socially Robust Knowledge and the Participatory Turn

The concept of socially robust knowledge has received some attention from other scholars. Thus it is worthwhile to look at interpretations and uses of the term in writings addressing the same dilemma in order to see which role they attribute to it and if and how they configure it. One appropriate place, among others, for such an exercise is a special issue of *Minerva* in which several authors respond to the precursor of *Re-Thinking Science* (Gibbons et al. 1994). Among them is Sheila Jasanoff, whose work has been focused for a long time on the special features of “regulatory science,” and who supports her views with rich empirical observations. In her reaction she regards the concept of socially robust knowledge as the authors’ “solution to problems of conflict and uncertainty” as they are endemic to the “pipeline model” of the relationship between science and society (Jasanoff 2003a, 235). However, she continues, the problem is “how to institutionalize polycentric, interactive, and multipartite processes of knowledge-making within institutions that have worked for decades at keeping expert knowledge away from the vagaries of populism and politics. The question confronting the governance of science is how to bring knowledgeable

publics into the front-end of scientific and technological production—a place from which they have historically been strictly excluded” (Jasanoff 2003a, 235).

The answers to this crucial question are varied. One indisputable observation is to point to the “participatory turn,” examples of which were cited above, such as the initiatives of the U.S. Congress to “concede unchecked autonomy to the scientific community in the collection and interpretation of data,” the European Union’s commitment to “involving public in technically grounded decisions,” or the various “experiments . . . such as citizen juries, consensus conferences and referenda” (quoted in Jasanoff 2003a, 236–37).

Jasanoff herself observes the practical problems connected with these procedural and organizational innovations, not least the danger that participation may become “an instrument to challenge scientific points on political grounds.” She states that “participation alone . . . does not answer the problem of how to democratize technological societies,” and that the issue is “*how to promote more meaningful interaction among policy-makers, scientific experts, corporate producers, and the public*” (237–38, my italics). Her suggestion is to complement the existing “technologies of hubris” as they are embodied in the predictive approaches “(e.g., risk assessment, cost benefit analysis, climate modeling)” with new “technologies of humility” (238, 240). These are considered to be “*social technologies*” that “would give combined attention to substance and process, and stress deliberation as well as analysis,” or, in other words, would “seek to integrate the ‘can do’ orientation of science and engineering with the ‘should do’ questions of ethical and political analysis” (243, my italics).

A sympathetic reading of Jasanoff’s response to Gibbons et al. will readily acknowledge her analytical precision as well as the succinctness of some new key concepts. She terms the amalgamation and disciplining of scattered and private knowledge “civic epistemology.” As a crucial component of the “technology of humility,” the term conveys a meaning somewhat similar to socially robust knowledge. (Incidentally, she, too, sees the social sciences in the role of providing the substantive basis for these “technologies.”) Also, she probes further into the meaning of socially robust knowledge by identifying “focal points” around which technologies of humility are to be developed: framing, vulnerability, distribution, and learning (240).

The difficulty, if not impossibility, of achieving any real progress (as opposed to mere conceptual innovation) is illustrated by another variant of bridging the expert/lay divide. Looking at the sources of legitimacy of experts

in different political cultures (in the United States, the United Kingdom, and Germany), Jasanoff sees the reason for the foreclosure of “continuous dialogue between expert and critical lay judgment” in the imperfect framing of the problem of expertise in democratic societies (Jasanoff 2005). She therefore suggests it would be better “to recast the role of experts in terms that better lend themselves to political critique,” that is, to subject expert decision making to “notions of delegation and representation.” In such a framework, experts will act “not only in furtherance of technical rationality, but also on behalf of their public constituencies” (Jasanoff 2005, 222). However, she insists that “*equally*, citizens need to recognize that governmental experts are there to make judgments on behalf of the common good rather than as spokespersons for the impersonal and unquestionable authority of science.” The resulting expectation is “that a fullfledged political accountability—looking not only inward to specialist peers but also outward to engaged publics—*must* become integral to the practices of expert deliberation” (Jasanoff 2005, 222, my italics; cf. also Jasanoff 2003b).

These examples are sufficient. They are from one of the most sophisticated observers of expert advice to policy making, and thus there are no hidden secrets or surprises of institutional innovations to be expected. Two conclusions can be drawn from the above. First, the discourse on the democratization of expertise is clearly a normative one. Second, a look at close range shows that for all their conceptual ingenuity, the analyses end up in a continuous restating of the dilemma. Why is that so? What keeps the discourse going despite the impasse?

Discursive Strategies and the Limits to Transgression

The dilemma of power and truth cannot be resolved and can therefore only be processed by way of discourse (Maasen and Weingart 2005). This is the common cause of the discussion’s impasse and its normative nature. Both sides of the dilemma are deeply ingrained in our culture and have been theoretically conceptualized as their functional differentiation (Luhmann 1990). This means that as long as modern societies are organized on the principle of functional differentiation, there will be no room for a blurring of boundaries. The very fact that a sizable array of metaphors continue to be invented that suggest management or mediation of the boundary (“trading zones” giving rise to “contact languages” and “transaction spaces,” “transgressivity,” etc. [Nowotny, Scott, and Gibbons 2001, 145]) points toward the ironclad existence of that boundary.

Does this mean that the empirical descriptions of the changes conveyed with these metaphors are wrong, and does it mean that nothing can be done about the institutional arrangements of expert advice to policy making? The answer is no to both questions! The issue is rather what can be expected.

In terms of discursive strategy, two options are employed that seem to suggest that the dilemma can somehow be resolved. One is to argue that scientific knowledge is not truth-bound as much as it is supposed and believed to be. Keeping to the symmetry of the dilemma, the other option is to argue that political decision making is not entirely power-oriented but is rationality-oriented as well. Curiously, the first option receives much more attention than the latter from analysts of the interface between experts and policy makers. This imbalance of attention may itself be taken as an indicator of the preoccupation with scientific expertise and the privileged role of experts and their knowledge as somehow foreign to the ideals of democratic representation. The primary concern in the discourse is the invasion of “truth” (that is, knowledge held by few) into democratic decision making. The opposite option, that is, the invasion of political considerations into the search for knowledge, receives comparatively less attention. This has not always been the case. Concerned by instrumentalizations of science by authoritarian fascist and socialist governments, writers such as Robert Merton, Karl Popper, and Michael Polanyi were primarily concerned with the politicization of science. Thus, it matters who observes, and when.

The former part of the discourse is determined by sociologists and science studies scholars who, given their professional preoccupation with the highly specialized study of science, argue against the view of science as universal, certified, and unequivocal knowledge, and experts as neutral, politically disinterested arbiters of that knowledge. A plethora of publications is devoted to demonstrating, either by theoretical speculation or by empirical study, that experts play a much more ambiguous role, that they are not neutral, that they do not always restrict their advice to the realm of their expertise, and likewise that their knowledge is rarely certain, nor is it consensual, and that it is selective with respect to the questions asked, and so forth (Jasanoff 1990; Weingart 2001; Maasen and Weingart 2005). In addition, there is an equally elaborate literature on democratic, participatory technology assessment based in part on actual experiences with round tables, consensus conferences, open forums, and similar devices intended to involve the lay public in decisions on the design and

implementation of new technologies (Joss and Durant 1995; Renn, Webler, and Wiedemann 1995; Abels and Bora 2004).

The gist of this part of the discourse seems to be that the apparent dilemma of power and truth is softened on the knowledge side: If the knowledge conveyed by experts is not the inimitable truth that is only accessible to a small elite, but rather is closer to negotiable opinion, then it is easier for the lay public to be successfully involved. Furthermore, if the role of experts is inherently political in the very context of giving advice, it is not legitimate for them to have privileged access to power. The demand that they be accountable is irrefutable.

The persistence of the dilemma in spite of this dismantling of the epistemic core of scientific expertise is perhaps best demonstrated by calling upon a witness from the "sociopolitical" side to comment on the other side of the discourse. Jasanoff diagnoses "the technocratic turn in US policy" characterized by its reference "back to an outmoded view of expertise as certified, elite knowledge and judgment" (Jasanoff 2003b, 161). At the same time, however, she observes the "slanting of expertise toward particular ends" happening in the (politically mandated) restructuring of the scientific advisory committee of the U.S. Department of Health and Human Services (HHS) and considers this a "different but equally thorny puzzle" (Jasanoff 2003b, 161). In effect, the George W. Bush administration kept the advisory committee intact but replaced members with handpicked choices. This may be read either way: as a technocratic turn of politics insofar as the role of experts as conveyors of certified knowledge is upheld, and as a political instrumentalization of scientific expertise insofar as the experts are chosen according to political criteria (such as the experts' ideological allegiance). This is a "thorny puzzle" only if primary attention is given to the administration's use of experts as sources of reliable knowledge. The other, neglected side is that experts are used as sources of legitimation, but only if their views correspond with those of the politicians that ask for their advice.

Curiously enough, policy makers seem to have no qualms about holding on to an "outmoded view of expertise," asking for sound and reliable advice, and instrumentalizing that advice for political ends at the same time. The supposed technocratic turn, as exemplified in the call for sound science, quality control of expert advice, and rosters of recognized experts (as intended by the EU), may in part be a reaction to the deterioration of scientific advice by politicization and the realization that politically instrumentalized experts do not deliver the cred-

ibility and, thus, legitimacy that is expected from their advice. The strength of this line of reasoning should not be underestimated, because it reflects the dominant view that policy makers and even the public have of science and experts, as any reading of media reports on incidents of political instrumentalization of scientific advice will readily show (Weingart 2005, chap. 5).

The gist of this less prominent part of the discourse seems to be that the dilemma is softened on the policy-making side. If policy makers cannot rely on democratic delegation of power alone because they are faced with problems emanating from science and technology, they are likely to attempt to gain political control of the expertise they have to enlist without losing its functionality both in terms of its problem solving and its legitimating capacities.

One incident during Gerhard Schröder's reign as chancellor of Germany may illustrate this. The minister for Consumer Protection, Nutrition and Agriculture, a member of the Green Party, was unhappy with the Central Commission on Biological Safety (ZKBS) regarding its friendly position towards genetically modified plants and organisms. Instead of attacking the scientific advisors directly or dissolving the commission, the ministry changed its mission and structure. It added criteria such as "ethical nature" and the protection of consumers and conventional farming to the evaluation of risks of genetic engineering. In order to implement these criteria the commission was divided into two panels, one that retained the commission's previous competence for work on genetically modified products, the second responsible for their distribution. The latter has to apply the new criteria. Thus, on the one hand, the expertise was to be adapted to the values of the Greens, on the other hand, the scientific reputation of the old commission was not given up.

This softening of the dilemma of power and truth has carried the discourse far beyond its original rigid state as it was represented in Max Weber's classic decisionistic juxtaposition of politics and science. It has led to novel institutional arrangements that appear to be hybrid, and to new alignments of experts and laypeople in which knowledge, values, and interests are combined in transgressive modes. But does this mean that the dilemma has truly been resolved?

The irreducible concern about a completely technocratic politics, just as much as about a complete reduction of science to politics, suggests the contrary. The more far-reaching visions of the accountability of experts remain programmatic. The most advanced arrangements of public participation in science- and technology-related decision making remain carefully staged exercises, special events with no institutionalized connections to existing forms of democratic

representation and decision making. They are looked upon with great suspicion by policy makers, who jealously guard their legitimate claims to power. There is no escape from the logic underlying the dilemma: Strengthening accountability and public participation in expert judgments ultimately compromises the reliability and legitimating power of the experts' specialized knowledge, and likewise, putting experts in the position of determining political agendas and decisions conflicts with democratic representation and legitimation of power. The expectations invested in the emergence of a socially robust knowledge cannot realistically reach beyond these limitations.

Are There Alternatives to Robust Knowledge?

The notion of socially robust knowledge adds yet another twist to the classic problem of how to control science and technology in a modern democratic society which is more dependent on science and technology than ever before and, thus, also more susceptible to their risks. This debate began in the 1960s, when damage to the environment resulting from technological progress first became an issue. The subsequent debate can be reconstructed as the successive design of mechanisms whereby the control is gradually moved upwards from intervening at the stage of implementation of knowledge to that of the production of new knowledge. Or, as it may be phrased, the controls are moved from an *ex post* to an *ex ante* stage of crucial knowledge production. The rationale for this is the often-lamented fact that once knowledge is available, little can be done to stop its diffusion and implementation. Thus one would have to control its production in the first place. However, the logic of this progression is flawed for at least two reasons. One is the implicit crossing of the boundary between the implementation and the production of knowledge. Whereas the former can be subjected to democratic control, that is not as self-evident with respect to the latter. At best, one can think of a (democratic) control of the political decisions that determine (temporary) priorities of public funding of research. Here, values to be respected and risks to be avoided can be taken into consideration, as in fact happens already. Even these decisions are subject to repeal in the light of new knowledge. What would come closest to generating another kind of knowledge would be to strengthen mechanisms that would give a voice in implementing new knowledge to those who are affected by it, that is, to make their acceptance a criterion of implementation. But that points to a second reason why the concept is flawed: the recourse to "society." Marilyn Strathern has pointed to the problems of such an abstract notion of society,

which clearly are not intended by the promoters of socially robust knowledge: “It produces the concept of ‘science’ (or technology, or academia) in contradiction to itself; this de-socializes ‘science and technology’ as somehow less part of society than arts and humanities. . . . But above all, the invocation of ‘society’ summons the fragility of measurement: What will count as ‘society?’” (Strathern 2005, 476). Indeed, this question is not posed, and the implication is disquieting. Either a diffuse, unspecified public is invoked (strikingly similar to that of the campaigns for “public understanding of science”), or lobby interests that are not legitimated by popular vote will prevail in this unstructured public because their backers have the capital and the manpower.

The only escape from the difficulties of imagining institutional mechanisms of representing society vis-à-vis scientists would seem to be the implantation of a “socially responsible” conscience in the individual scientist’s mind. Since that option is also not available, notwithstanding the relegation of many biomedical problems to the responsibility of scientists and to ethics committees, the concept of socially robust knowledge actually marks a turning point in the debate over the control of science and technology. Rather than carrying controls, however democratic, into the realm of knowledge production, the focus must be on institutional mechanisms that process new knowledge and values and interests at the same time. Round tables, consensus conferences, ethics committees, and similar stagings are only a beginning, and it is important not to overburden them with misplaced expectations. Their function can only be to serve as exemplars of the discursive processing of science- and technology-related issues in the broader public. They cannot control the production of knowledge, nor can they be representative of social values and interests. Rather, they can contribute to a better understanding of the scientific issues on the side of the lay public, and a better understanding of the lay public’s concerns with regard to scientific knowledge and its social implications. Thus, the issue is to provide for institutional frameworks that allow for an unrestricted and unbiased deliberation of new knowledge and new technological options in the light of existing values and interests, because in this process, all of them will change.

NOTE

1. “‘Consensibility’ is supposed to mean that each message should not be so obscure or ambiguous that the recipient is unable to give it whole-hearted assent or to offer well founded objections” (Nowotny, Scott, and Gibbons 2001, 170).

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