RISK : PROBLEMS AND PERSPECTIVES

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INTRODUCTION

Let us start from the following rather broad, and somewhat naive definition of risk : 'a risk is an uncertain, harmful event'. Then we can truly say that risk can be encountered virtually everywhere.
Diseases, earthquakes, floods, cyclones are posing a constant though unpredictable threat to people all over this earth, mostly beyond their own control.
At the other end of the spectrum, some people seem to expose themselves more or less deliberately to considerable risks. They engage for instance in skiing, mountain climbing, motor racing, and other dangerous sports; or a young entrepreneur in a capitalist society takes the risk of starting a new business.
Many phenomena that we like to call 'risk', however, are somewhere in between of these two extremes : on the one hand they are at least to a considerable degree man-made; on the other hand it would not be fair to say that their occurrence is completely calculated and accepted by all those who will be affected; and this ambivalence creates framing problems.
Some people are addicted to smoking, alcohol, or hard drugs. To what degree are they to be seen as voluntary choosing, and to which degree as victims?
Some people accept dangerous work for a higher payment. Are they freely choosing agents, captives of their employers, or victims of the economic system?
Computer hackers view it as a sport to gain unauthorized entrance to computer systems. Some people regard hackers as a welcome help to expose failing or lacking security measures, but for others they are just criminals who should be severely punished. According to the first perspective hackers are indirectly reducing risks, according to the second they form a source of risk. Actually, behaviour and labelling are not independent: treating them as criminals could reinforce criminal behaviour.

At a more aggregate level, the welfare of a nation may be put at risk by its socio-economical system. Complex multi-actor processes can push the social system into a direction that is actually desired by no one, e.g. by generating large unemployment. The same goes for issues like acid rain, the depletion of the ozone layer, and the CO_2-problem, where the future of the whole ecosystem is at stake.

It is particularly the large and broad category of 'in-between-risks', man-made and yet for some reason not unconditionally acceptable, that presents us with conceptual difficulties. It is hard to develop an entirely consistent perspective on such risks, or to find ways to deal with them that we are all prepared to call rational or acceptable. This is not to say that we can draw a clear cut boundary between what counts as 'in-between' or entirely voluntary. Even the categorisation of the few examples above could be subject to dispute. Some might want to hold that smoking must be seen as a relatively voluntary activity. Others might ask whether motor racing should not be conceived as a form of addiction. The point is, that which counts as a risk, and why, often depends upon the specific context of the situation, upon the interdependence of actors, and most of all upon how we interpret these situations, contexts and interdependencies. In other words, questions of problem construction become of major importance.
The first part of this paper is an attempt to disentangle some of the most important aspects of risk problems, and to show what makes them so difficult to deal with. The second part singles out a few of these aspects to be explored more deeply.

I. DEALING WITH RISK.
THE CONSTRUCTION OF PROBLEMS AND SOLUTIONS : AN OVERVIEW

In this brief overview, I will not so much try to survey all major approaches available, but rather to give an exposition of the main difficulties that are encountered when dealing with problems of risk. To provide some overall structure, the overview will be split into four parts :
(1) Construction of problems
(2) Construction of solutions
(3) Implementation of solutions
(4) Meta-perspectives
However, as we shall see, and as could already be suspected from the previous examples, the distinction is rather artificial, and no part can be adequately discussed while leaving the other three entirely out.

1. RISK : CONSTRUCTION OF PROBLEMS

A good point to start the discussion is the rather reductionistic approach to risk that was developed in the sixties for technical risks, since it was especially the formulation of this approach that gave rise to a much greater awareness and insight in the complexity of risk issues.

In the sixties and seventies, attempts started to quantify the risks of technical installations, like nuclear power plants, with methods borrowed from natural science. The idea was to construct 'event trees' that specified every conceivable path of events leading into an accident, and to calculate, by estimating the probability of events and paths, the probability of possible accidents. The 'Rasmussen Report' on the safety of nuclear reactors, published in 1974, was one of the first articulations of this approach to technical risk that drew major public attention.[1] It compared the estimated accident probability of a nuclear reactor to those accident probabilities people expose themselves to in their daily lives, for instance when they are driving a car. The conclusion was that the latter were far higher than the former, and that therefore opposition against nuclear reactors should be dismissed as irrational. The crucial background assumption here was that risk could simply be equated to the product

[1] US Atomic Energy Commission : An assessment of accident risks in US commercial nuclear power plants, 1974, Washington

of probability and consequence, where the latter was usually taken as the number of people killed. Risk, so the argument could be summarized, can simply be converted into a single number, and discussions about the acceptability of risks need only to take into account this number.

The Rasmussen report and its conclusions met with severe opposition from the beginning, and its approach gave rise to a stream of publications in various research areas to expose the deficiencies of its line of reasoning. These deficiencies can be divided into two main categories.

(1) Uncertainties in the probabilities and class of possible events

The probabilities of events that may lead to an accident are extremely hard to assess. Such events do not happen every day, which is fortunate for most of us, but unfortunate for those who want statistical data for a well-based probability estimate. In addition, when an accident occurs, it often involves events, or combinations of events, which experts had assumed to be impossible. So the list of possible events is likely to be unreliable too. Common cause failures are another source of serious miscalculations : when two separate events, which could lead to an accident, are treated as independent (which implies that their probabilities can be multiplied), while in fact they can be caused by one common cause, their probability will generally be underestimated.[2]

These ambiguities leave a lot of room for sloppy reasoning. When an accident with a nuclear reactor could no longer be kept secret, the authorities of the plant usually were quick to explain that there had never been any real danger, and that from now on there was no safer place to be. Another frequently delivered message was that this was in fact an older type of reactor, and the incident would never have occurred in a more modern one. But if accidents cannot be compared, it would be fairer to say that it is impossible to estimate the probability of accidents. There is in fact considerable evidence that nuclear energy technology is so complex, and its operation so tightly coupled (i.e. an unexpected event will probably trigger a fast chain of further events), that it is unlikely that within the foreseeable future it can be operated on a large scale with acceptable safety.[3]

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[2] See e.g. Frank P. Lees : Loss prevention in the process industry, 1986 (second impression), Butterworths, London

[3] Charles Perrow : Normal accidents. Living with high-risk technologies, 1984, Basic, New York

(2) Effect and probability are not the only parameters to judge risks

A second, and even more structural deficiency was that there are more dimensions to risk then just consequences and their probabilities. One major dimension that had been completely neglected was that often those who enjoy the benefits of a certain activity are not the same as those who are exposed to the risks created. Those who benefit from the building of a nuclear plant are mainly the company and perhaps its employees. The risks are inflicted upon people living in the neighbourhood, most of whom may have nothing to do with the plant. Similarly, the production of CFK's is in the benefit of a few, but the risk of destroying the ozone layer is shared by mankind, including future generations, and by the ecosystem as a whole.[4] This suggests that the degree of voluntariness must be an important variable in the perception and judgement of risk. Psychological research has shown that this is indeed a major factor in how people actually judge the seriousness of a certain risk. In fact, it turned out that probability is not a major parameter in judging risks with very low probability. This should not surprise us, since it is very difficult to give concrete meaning to chances like one in a million in our daily lives.

Another, related factor in the perception of risk is whether one has the feeling that one can decide oneself what to do when something unexpected happens. A car driver can brake, or turn the wheel. When something goes wrong with a nuclear reactor in the neighbourhood, one cannot influence the line of events. Again related to this is the importance of the parameter of (ir)reversibility, especially for global environmental issues like the depletion of the ozone layer, or the CO_2-problem.

These examples show that it is not just the probabilities of consequences that are subject of dispute. There can even be dispute about who is actually posing a risk to whom. Are only politicians to blame for not taking enough measures to preserve the environment? Are hackers increasing or decreasing the risk of computer systems?

We can see now that risk is not something objectively given, but that it refers to the fact that someone is defining a certain situation as a risk problem, and to (in)acceptability claims raised with respect to that situation.

These difficulties with specifying risk problems must lead to the conclusion that risk problems in the 'intermediate area' - that is, excluding risks which one either cannot control at all, or completely voluntary imposes on him(her)self, with no consequences for others - can in fact only be adequately understood as *distribution* problems (i.e.:

[4] This type of situation is often called 'tragedy of the commons' : one person or a few persons collect all the benefits of a certain activity, while the costs are distributed over a much larger group. In such situations there is little intrinsic incentive for the beneficiaries to stop the activity, they will have to be persuaded by an authority representing the interests of all.

which distribution of costs and benefits is acceptable?); and this, in turn, makes them *procedural* problems as well (i.e.: what decision procedure will lead to an acceptable distribution?).
Most situations with potential harm can be framed in at least several different risk perspectives.[5] Given this ambiguity, and the room it leaves for biases, it is hardly surprising that the construction of risk problems is highly dependent upon anticipated solutions. The complexity and ambiguity of most risk issues provide ample opportunity for actors to anticipate on their own costs and benefits of possible solutions, and construct the problem according to their own desires and perceived interests. Let us consider the construction of solutions in some more detail now.

2. RISK : CONSTRUCTION OF SOLUTIONS

What makes a certain risk (distribution of costs and benefits) acceptable? By which standard shall we judge whether a certain inflictment of risk is acceptable or not?
As the English Council for Science and Society put it in 1977 : there is only one kind of risk that is truly "acceptable" in the ethical sense : the risk that is judged worthwhile (in some estimation of costs and benefits), and is incurred by a deliberate choice made by its potential victims in preference to reasonable alternatives. The main question is therefore : under what conditions, if any, is someone in society entitled to impose a risk on someone else on behalf of a supposed benefit to yet others?[6]
As already suggested, solving risk problems raises questions of content as well as questions of procedure. Let us start with matters of content.

At first sight, some kind of utilitarianism might seem to be the answer : an inflictment of risk is acceptable when it increases the total happiness in the world. But such a decision rule leads to rather strange results.[7]

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[5] Cf. e.g.
Dorothy Nelkin (ed.) : The language of risk. Conflicting perspectives on occupational health, 1985, Sage, Beverly Hills
Thomas Brante : Democracy and expert statements - empirical and methodological issues, paper presented at the conference 'Ethics and the Sociology of Science', april 1987, Dubrovnik

[6] Council for Science and Society : The acceptability of risks, 1977, Barry Rose, London, as rendered in Lees, op. cit.

[7] Cf. Amartya Sen, Bernard Williams (eds.) : Utilitarianism and beyond, 1982, Cambridge University Press, Cambridge

First, even when we have an idea of how bad a certain consequence for a particular person may be, it is not immediately clear how we should evaluate a *chance* on such a consequence. For instance, common forms of utilitarianism suggest that when we double the harmful effect and halve its probability, the utilitarian value will remain the same[8] (this concurs with the probability reasoning concerning the acceptability of the risk of nuclear reactors, like discussed above). But it is not evident at all that this is acceptable when we are dealing with very big risks with low probabilities. Psychological research has shown that people resist such risks more than utilitarianism would predict, and there is no a priori reason why they would be wrong and the theory right.

Even more objectionable is the consequence that someone would be allowed to inflict harm upon someone else as long as the tormentor's enjoyment is more intense than the suffering of the victim.

Considerations like these have lead to several modified versions of utilitarianism, for instance by postulating that benefits and costs cannot arbitrarily compensate for each other. A satisfactory utilitarianistic approach, however, has yet to be found.

We could turn to a completely different decision rule, e.g. a variant of the distribution rule put forward by John Rawls.[9] With respect to primary goods, Rawls' theory demands that activities should be directed to improve the condition of those in society who are worst off in the present situation. Since many important risk issues involve primary goods, we could in principle apply the theory here. But Rawls' theory itself is by no means unproblematic.[10]

It seems that no theory can provide a final measure on how to deal with risk, and that we cannot bypass people's own opinions on the matter. But at the same time we would not want simply to accept what anyone claims to be his or her risk perception without further argument. That would put a premium on interpreting risk just according one's own interest, and prevent any reasonable discussion. The dilemma that emerges here is a very teasing one. On the one hand it is clear that certain common aspects in risk perception cannot be accepted at face value. For instance, when presented with equivalent options like '40 % of the people will die' and '60 %

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[8] Or, to speak for utilitarianism in it most general sense, making the event more harmful can always be compensated by a certain lowering of its probability.

[9] John Rawls : A theory of justice, 1972, Oxford University Press, Oxford

[10] See e.g. Norman Daniels (ed.) : Reading Rawls : critical studies on Rawls' A Theory of Justice, 1975, Basic, New York

of the people will survive', most subjects evaluate them very differently.[11] On the other hand we also have major reasons for not letting scientists decide what risk acceptance is reasonable.[12] The most suitable approach usually lies somewhere in between : listening to what the other parties say, and critically discussing each others considerations.

This brings us to the second type of problems, that of procedure. Given the fact that there is no standard measure of what is reasonable in risk issues, it is of crucial importance that all affected can participate as much as possible in the decision making. If not, it is almost sure that the interests of those groups not represented will deliberately or undeliberately be neglected. As long as only a small number of actors is in any way affected, this may not pose unsurmountable problems. But the more actors are involved, the more problematic it becomes to find a procedure in which no one is systematically excluded. Basically, this problem originates in the fact that no matter which institutionalization is proposed, once erected it can always grow fixed, putting itself in the service of some established interest only, and being no longer responsive to the needs it was intended to serve.
Let us look for a moment at the role of government, and the parliamentary system in western democracies. Clearly, these systems do not always bring the justice that one would hope for. This can be attributed to many causes, and different countries make different cases. In certain parts of Western Europe, for instance, the intended distance between government and parliament, necessary for an effective check on the governments activities, seems to have slowly decreased. Some say that this is only natural, since there is no real function anymore for political parties based on general principles. Instead, interest groups have got more involved in the policy making process. This can occur in several forms. In a pluralistic system there are no a priori constraints on the interest groups to have access to the policy making process; in a

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[11] As a matter of fact, psychological research shows that the perception of risk, as in fact all perceptions involving probability, seem to deviate from almost any formal law of probability. The problem here is how to separate those deviations that are simply wrong from those that might contain a not immediately perceived rationality.

[12] Raiffa has suggested that risk could be measured by not following any preconceived theory, but by simply asking someone for how much money one would be prepared to be exposed to a certain risk. But, as Wynne has pointed out, even such an apparently liberal approach would impose a constraint on the discussion that is not a priori acceptable, for it would limit compensations to financial ones, and would thereby frame the discussion within the realm of quantifiable economics.
H. Raiffa : Back from prospect theory to utility theory, in M. Thompson, A. Wierzbicki, M. Grauer (eds.): Plural rationalities and interactive decision processes, 1985, Springer, New York
Brian Wynne : Risk management and hazardous waste. Implementation and the dialectics of credibility, 1987, Springer, Berlin

corporate system, on the other hand, there is a specified number of interest groups with access to policy making. In both cases, however, there is no guarantee that all interests will really be represented.[13] The role of expertise actually forms an additional complicating factor here. The close alliance of expertise with parties in corporate decision making can be an extra force to prevent certain groups from their legitimate share of participation.[14]

Procedure will have considerable effect on which arguments will be accepted as legitimate and which not, and such implicit or explicit constraints on the kinds of arguments can be very disturbing. Wynne has provided some striking and illuminating examples from the Windscale Inquiry, where valid arguments from contenders of nuclear energy were simply declared out of order, or were distorted in such a way that they became so. Two examples from Wynne can illustrate this. First, the Inquirer systematically declined to discuss organizational risk, i.e. the question whether it was not only the technical system that might get out of control, but also the human organization that was to operate it. Questions and remarks about the reliability of the organization were systematically translated into questions and remarks about technical reliability. Second, when opponents, against the contention that every possible harm had been investigated, tried to show that this was not the case, and pointed out that in fact some possibilities had not been investigated, the Inquirer interpreted this as a claim that these possibilities actually presented a danger; when after a superficial inspection it turned out that they perhaps did not, the whole argument was further dismissed.[15]

3. RISK : IMPLEMENTATION OF SOLUTIONS

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[13] cf. Charles W. Anderson : Political design and the representation of interests, in Philippe C. Schmitter, Gerhard Lehmbruch (eds.) : Trends toward corporatist intermediation, 1979, Sage, Beverly Hills

[14] Galbraith already warned for the organizational impact of what he called the 'technostructure' (i.e. those experts who provide the line with technical advice). Since then, the separate organizational impact of technical experts has found its way into the literature on organization theory, for instance in the famous schemes by Mintzberg. Addressing the interweaving of corporatism and technical advice, Fischer has coined the term 'technocorporatism'.
John Kenneth Galbraith : Economics and the public purpose, 1973, Penguin, Harmondsworth
Henry Mintzberg : The structuring of organizations. A synthesis of the research, 1979, Prentice-Hall, Englewood Cliffs
Frank Fischer : Technocracy and the politics of expertise, 1990, Sage, Newbury Park

[15] Brian Wynne : Rationality and ritual. The Windscale inquiry and nuclear decision in Britain, 1982, British Society for the History of Science (BSHS Monograph), Chalfont St Giles

Since the emphasis of the workshop is more in the direction of problem construction than in the direction of implementation, I will be a bit briefer here. Even so, it is the implementation which determines what the distribution of costs and benefits will actually be. Solutions cannot be considered isolated from their implementation. Unfortunately, implementation questions are often neglected in practice, which leads to ineffectiveness, and to all kinds of side effects.

When a risk problem affects only a few persons, a solution could be reached by an informal arrangement between them, e.g. a stated intention, a promise, or an agreement. One main advantage of such arrangements is their flexibility : because the basis is an informal understanding, one can always refer back to the original intention of the arrangement, and the context in which it was devised. Formalisation will never be able to capture this original intention in all its finesses, and therefore will be prone to unintended use of the letter of the formal agreement; on the other hand, wherever it cannot be misinterpreted, it can be enforced with a formal system of sanctions. When the actors are part of a local community that provides social bonds that tie rather close, it will not be so easy to break an important promise without being confronted with considerable sanctions, to be socially cast out, or even worse retaliations. When the social bonds are not so strong, and mutual surveillance is less effective, it gets more difficult to ensure that promises will be kept, and agreements will be fulfilled.
This is one of the major dilemmas in policy making. In Dutch environmental policy, for instance, there currently is a trend towards arrangements and covenants, as a reaction against the approach in the recent past, which is now seen as 'overregulatory'.[16] As a very global outline, the following types of implementation can be distinguished[17] :
(1) Covenants, 'gentlemen's agreements', self regulation.
In this case the various groups of actors, or their representatives, will negotiate themselves on a solution and its way of implementation.
(2) Government measures
Here national or regional government, or even local authorities, are responsible for solution and implementation. There are basically two types of instruments that can

[16] It may be more appropriate, though, to say that prescriptions had been applied without much analysis of whether they would work or not.

[17] It should be noted that the distinction between (1) and (2) does not refer to who has actually most influence on the outcome, it is only concerned with formal responsibilities. These do not tell us where the actual influence lies. In corporate configurations for instance, there can be a dominant influence by either the government, or one or more of the interest groups.

be used by a government :
(a) Prescriptions
We can think here for instance of norms for amounts of emission that should not be exceeded. To enforce these prescriptions, sanctions will be needed.
(b) Financial incentives
They can either be subsidies, or taxes.

Which kind of implementation will be most effective for a particular situation is often very difficult to tell, and subject of severe dispute. Because there seem to be no general recipes, policy shows considerable trend waves, like in the case of the Dutch environmental policy. It clearly is impossible to enforce every desired result by government measures. At least for a part, one must depend on arrangements between local actors, and on self regulation in e.g. whole branches of industry. But these will only work as long as there is enough social pressure to make it unattractive for one party to violate the agreement. This seems to put a limit to e.g. the number of covenants that can be kept operative at the same time.
Many cases, however, cannot be solved by informal arrangements alone. When some affected parties do not have the power to negotiate a fair agreement, or in situations which take the form of a prisoner's dilemma or a 'tragedy of the commons', the actors may be either unwilling to see further than their own immediate interests, or waiting on each other till someone takes the first step. In such situations, intervention by a separate instance is needed. This is the case in for instance many environmental issues, a full solution of which can only be found at a national or international level. As for the various instruments governments can use, here too the choice is often difficult. When we stay with the example of environmental issues, all instruments have their pro's and cons. Detailed prescriptions will have a relatively clear-cut effect (provided they will be enforced), but they are rather inflexible, and tend to freeze some status quo situation. A limit to the amount of sulphur or nitrogen emission e.g. will provide little incentive to get far below that limit. Financial incentives, on the other hand, can have more unpredictable effects, and often require more bureaucracy. For instance, when companies are presented with high taxes on toxic waste, they will be strongly tempted to dispose of the waste illegally, shifting the problem even more beyond control. Subsidies, on the other hand, interfere with the sound intuition that somehow the costs of production should simply include environmental damage. In many comprehensive cases of risk a combination of different types of measures will be necessary.

Two more aspects of implementation should be mentioned. The first one is the

importance of information and education. People can be made more aware of issues where the state is not able to enforce necessary steps, and where internalisation and self-regulation must do the work. At the same time people must be better informed about decisions in which their interests are involved, so that they can claim their right to participation, and decisions can get their active support. A second crucial factor is the importance of checking, auditing, etc. An agreement or measure is only as effective as the checks on its fulfilment. Often it is discovered only afterwards that a certain measure cannot really be enforced or checked. Many policy measures are hardly ever evaluated for their effects. As long as there as such possibilities for learning are evaded, things can hardly be expected to get any better.

4. RISK : META-PERSPECTIVES

Given the complexity of risk issues, and their often confusing character, it can be illuminating to widen the scope beyond individual risk problems; this may provide insights that remain obscured when dealing with isolated issues. I have selected three levels here for a brief discussion : (a) structural implications of modern forms of risk for society, (b) stances towards the diversity of risk perspectives, and (c) reflexive inclusion of the role of expertise.

Risk society

Various authors have in one way or another suggested that the large scale risks produced in modern society will eventually have to lead to a major restructuring of thinking as well as of institutions, since current practices are unable to deal with these new problems. Such diagnoses do of course implicitly or explicitly involve a judgement on what are to be perceived as major societal problems in the first place. Beck[18] points to the global, worldwide character of modern risks, their invisibility and often irreversibility. He sees the inadequacy of the technical reductionism still operative in most thinking about and dealing with risks, as well as social inequality, as the major obstructions to an appropriate handling of these issues. For Luhmann[19] it is rather the systemic imperatives of the social system as it is, that is inadequate to deal with environmental problems. Economy, politics, law, education, all have their

[18] Ulrich Beck : Risikogesellschaft. Auf dem Weg in eine andere Moderne, 1986, Suhrkamp, Frankfurt am Main

[19] Niklas Luhmann : Ökologische Kommunikation, 1986, Westdeutscher Verlag, Opladen

own systemic characteristics which cannot handle the kind of self-endangerment presented by modern environmental issues. Jonas, Naess, and several others have drawn attention to changes in responsibility, concern for future generations, and the principle of 'precaution'.[20]

Constructivism

We have seen that a risk problem can often be constructed in several different ways, depending on the perspective taken, on perceived costs and benefits, on socio-cultural choices and preferences. This notion of pluriformity, combined with a reactive tendency against the reductionistic framework of technical risk analysis, has lead to a strong emphasis on risk perception, and a trend towards more relativistic and constructivistic frameworks. This relativistic trend is also present in the sociology of science, where researchers tried to get away from the reconstruction of science as a story of cumulative rational progress, and proposed approaches in which the truth content of scientific claims was more or less considered as contingent (and where risk issues soon became a major topic of study).
It is a matter of dispute to what degree one could see risk as a social construction without removing the basis for any practical discussion of risk issues. If risk is only a construction, why bother about risk anyway? In the discussion between proponents and opponents of constructivism there is still a lot to be clarified. Anti-constructivists and anti-relativists often seem only to want to bring the old positivism back in again. Constructivists and relativists, on the other hand, are usually very unspecific about which particular claims they want to study relativistically, and which not, and why.

Reflexivity and the role of expertise

The role of experts often appears to be that of uninvolved, impartial bystanders. However, since Galbraith, Mintzberg, and others, it has become clear that scientists and experts play themselves a role in society, a role that is far from 'impartial' or 'neutral'. Many risks in our society are of such a nature that we cannot do without the information from experts. But this information may be highly biased, like the reductionistic perspective from quantitative risk analysis. A meta-perspective on what actually happens when decisions are taken about risk issues may uncover such biases. But if expertise can be biased, we cannot exempt meta-research itself from scrutiny.

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[20] E.g. Hans Jonas : Das Prinzip Verantwortung. Versuch einer Ethik für die technologische Zivilisation, 1979, Insel Verlag, Frankfurt am Main

A constructor of a meta-perspective is her(him)self an actor in the risk arena, affecting its own subject of study. Could expertise be made reflexive, i.e. could expertise be arranged in such a way that it takes into account its own effects on the issues it is studying? And what kind of meta-perspective would allow such reflexivity? Would it be constructivistic, or not? What would such a perspective have to say about the role of expertise? Convincing answers have yet to be developed[21] (an attempt will be made in the Part II).

[21] Cf. Brian Wynne : Risk and reflexivity : From discovering uncertainty to negotiating indeterminacy, in Shelly Krimsky, Dominic Golding (eds.) : Theories of risk, 1992, Praeger, New York

II. RISK AND EXPERTISE : A REFLEXIVE VIEW

Building on the more or less general overview of Part I, this second part presents some more specific analysis. Its main theme is what the shift in perspective on risk issues - away from the technical, more or less one-dimensional picture, towards a much more complex, socially contextualized one - implies for the role of expertise in general, and of risk experts, sociologists of science and other experts dealing with risk issues in particular. It will be argued that perspectives on risk must be made reflexive, i.e. that they must be able to include their own role in the discussion arena of risk issues (section 1), and that this implies that we must find ways to evaluate perspectives not only as some kind of 'representation of reality', but also as a regulative (section 2).

1. A FEEDBACK PERSPECTIVE ON RISK AND EXPERTISE

Part I has brought up quite a number of unanswered questions. No doubt one of the most teasing ones is the status of different perspectives on risk. It looks as if there is not one universal perspective from which one can ultimately judge risk situations. As Ravetz has aptly summarised it, 'The hope that one can produce a taxonomy, evaluation, and finally a technical fix to the problems of risk is in substance as ambitious as the program of putting all of human experience and value onto a scale of measurement for mathematical or political manipulation.'[22] So a new universalistic paradigm is not likely to deliver resolution. For most risk problems, there will be at least several different risk perspectives around, leading to different framings, and to different acceptabilities of various solutions. As we have seen, it is of crucial importance that the discussion remains open, and does not rule out certain aspects in advance.

But if there is no universal a priori standard to judge which risk perspective is the right one, is then rational, argumentative discussion about risks at all possible? Some provisional reassurance can be obtained from the works of authors like Bernstein, Toulmin, and several others.[23] Even when there is no final grounding of arguments,

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[22] Jerome R. Ravetz : Public perception of acceptable risks as evidence for their cognitive, technical, and social structure, in : Meinolf Dierkes, Sam Edwards, Rob Coppock (eds.) : Technological risk : its perception and handling in the European community, 1980, Oelschlager, Gunn & Hain, Cambridge (Mass.), p. 46-7

[23] See e.g. :
Richard J. Bernstein : Beyond objectivism and relativism : Science, hermeneutics, and practice, 1983,

when there is no definitive standardized measure by which to judge who is right and who is wrong, discussion is not necessarily subjective or arbitrary. Practical reasoning is never merely the following of technical rules. Even when a rule is applied, its application always involves interpretation, choice and deliberation.[24] Practical reasoning need not be arbitrary, since we can give practical reasons for various standpoints, and discuss and compare the reasons put forward.[25]

But that is only a first step in coming to terms with the repercussions of the lack of ultimate foundations. Because inspection of argumentation by *content* alone will not be enough to evaluate its adequacy (since there are no final judgement standards), we are forced to examine the *process* of argumentation and decision as well.[26] We already saw this in Part I, when we concluded that in risk issues content and procedure are not independent. This means that the process of the construction and use of risk perspectives must itself be problematized. Actually, one would like the result of this process analysis to be assimilated in the risk perspectives themselves. That is to say : risk perspectives should become reflexive, i.e. a risk perspective should include, and concur with, a perspective on itself, its construction, and its use. This is not just a pursuit of theoretical perfection. In fact, as Wynne has pointed out, a risk perspective can itself become a source of risk, in the sense that e.g. certain aspects of risk, like socio-organizational ones, tend to be excluded from discussion (cf. Part I, section 2). The content of risk perspectives should be contextualized within the process of construction and use of risk perspectives; only after having analyzed this broader process context, we could try to draw conclusions about how to correct the content of these risk perspectives.

But when no perspective on risk can claim to be the 'ultimate' one, the same goes for reflexivity. So to make risk perspectives more reflexive is not so much to be taken as

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University of Pennsylvania Press, Philadelphia
Stephen E. Toulmin : An examination of the place of reason in ethics, 1958, Cambridge University Press, Cambridge

[24] Cf. Bernstein, op cit., who refers to the Aristotelean notion of 'phronesis'.

[25] Toulmin, op. cit.

[26] Habermas has been one of the most persistent pursuers of this point, in his early work in the form of 'herrschaftsfreie Diskussion', later in the form of 'ideale Sprechsituation'. For the latter, see :
Jürgen Habermas : Theorie des kommunikativen Handelns (2 volumes), 1981, Suhrkamp, Frankfurt am Main

to provide a new blueprint, but rather as to make a practical diagnosis of *what goes wrong (or could go wrong)* in the process of the construction and use of risk perspectives. It should involve the identification of practical priorities, i.e., an indication of the *main* pitfalls in the process of construction and use of risk perspectives. The question then becomes by which standard we should evaluate what are the most problematic pitfalls in constructing and using risk perspectives. It seems natural to take the following criterion here : pitfalls can be identified with choices and decisions in which the interest of one or more of the groups affected are manifestly and seriously neglected.[27]

Transferring this to the more specific issue of risk, the question becomes where in the process of constructing and solving risk issues we could expect to find major sources of bias in choices and decisions. My answer, as developed in a more general context elsewhere[28], can be briefly paraphrased along the following lines. If democratic procedures are indeed crucial to the outcomes of discussions, then a major source of bias can be found where authorities and (scientific) experts are in a position to exclude certain groups or viewpoints from the construction of risk perspectives, and the search for solutions. As for scientists and experts, it seems to me that the main origin of this capability lies in their to a certain extent intrinsic monopoly on knowledge. They produce information that is of relevance to practical decisions, but which only they can fully evaluate. In complicated issues like risk, where there is not one ultimate standard, this monopoly implies that to a considerable degree the (scientific) experts have the freedom to align with whichever party they want, since they can choose their perspective (paradigm). Sometimes dedication to a particular paradigm will have mainly scientific reasons; other times it may be motivated by sympathy for a certain socio-political view behind it. But there is not much use in trying to determine for each individual case whether a certain alignment should be explained from purely 'intellectual' motives or from social interests. The point is that, no matter the motives, each one-sided 'overstatement' of a particular paradigm introduces a bias that is hard to fight by non-experts, and therefore presents a

[27] The idea here is not that in all cases we can unambiguously determine whether there is such 'neglection', nor that 'the interests' of parties can always be objectively established, but merely that we can and should identify (or, if necessary, negotiate) what are such evident and serious violations of these interests that they should be corrected, or at least more openly discussed.

[28] Frans A.J. Birrer : Counteranalysis, paper presented at the conference 'Sociology of Science : Science, social controversies and ethics - Sociological and normative perspectives', Dubrovnik, may 1990; to be published in the proceedings.

monopolization problem.[29]

If this is accepted as a diagnosis, then a necessary ingredient for making the role of science and expertise in the construction of risk perspectives reflexive, would be to introduce 'countercheck' mechanisms on the production and use of scientific expertise. The production and use of (scientific) expertise should be independently inspected for major 'sources of bias' (in the sense explained above), by people who have enough knowledge to make a detailed evaluation, but who are at the same time distant enough to make an independent judgement.[30]

Equipped with such a 'countercheck' component, the production and use of knowledge is no longer a linear process, a form of cumulative progression, but rather a process with negative feedback. In order to be effective, such feedback mechanisms must satisfy at least the following two conditions :

(1) They must be practice based. They should not be guided by the internal priorities of science (nor in fact by the private priorities of any particular group), but by the practical priorities based on the interests of society as a whole.

(2) They must be somehow institutionally separated from the processes they are to monitor.[31]

The whole process of knowledge production and use, including feedback should be conceived as a bootstrap process.[32] It does not rest on any final foundation or standard. The only basis we have to further develop our wisdom is the wisdom we already have.[33]

My objective here is not to specify a detailed implementation for 'countercheck'

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[29] It is very clear that experts are not just victims of the uncertainties in scientific research. They often explicitly deny even common knowledge, as Wynne's examples about social-organizational aspects show. For some reason Wynne himself does not explicitly distinguish between experts ignoring knowledge in their own domain of expertise (e.g. about uncertainties), and experts ignoring common knowledge. I would say, however, that it is a rather essential distinction, for it is precisely the latter that shows how deep the problems with expertise really are, and that it is not just a matter of friendly pointing out what they were ignoring.

[30] For this kind of 'checking' activity I have used the term 'counteranalysis' elsewhere (op. cit.).

[31] Birrer, op. cit.

[32] Cf. Larry Briskman : Historicist relativism and bootstrap rationality, The Monist 60 (1977) 509-39.

[33] The present analysis shows very elegantly where that most famous bootstrap model, Popper's falsificationism, essentially goes wrong. Although Popper realizes that checking mechanisms are necessary *within* science, he seems to forget that scientific experts can also form coalitions with parties outside science, *and that to counter this one cannot depend upon mechanisms restricted to the scientific community itself.*

mechanisms. That is a practical question, and of a different order than the ones discussed here. Like all attempts to bring about changes in the common interest, in the end its effectiveness will crucially depend upon the willingness of citizens to stand for their own interests; all we can do is to facilitate an even access, and break down monopolies. What I have tried to show is merely that in one way or another 'countercheck' mechanisms form an indispensable element in the improvement of the role of science in society. What I have presented here as an essential requirement for contemporary perspectives on issues like risk, is precisely what most dominant perspectives in social science fail to provide: they are unable or unwilling to include themselves in the same perspective as their object of study. This is particularly embarrassing for meta-disciplines like sociology of science, where such self-inclusion seems an even more obvious aim. The question is usually evaded by one of the following three escape routes. Either the approach does not specify any image of itself at all. Or the approach is presented in a decontextualized way, as an abstract description or instrument, without specification of the priorities and selection criteria motivating its construction and use. Or, in post-modern fashion, the self-presentation is turned into the subject of investigation itself, but again without stating any specific societal priorities underlying this operation. But neither of them, I think, is good enough as an excuse.

2. SCIENCE AND EXPERTISE AS REGULATIVE

The foregoing has one important implication that has not been made fully explicit so far. If science, technology, policy measures, etc. are all to be seen as instruments to be used for the improvement of society in some democratic perspective, then it will not be enough to evaluate merely their *intended* working. Not only may the actual working be very different from the intended one, some intended uses may turn out to be entirely unfeasible because of the dynamics of the social system where it is to be used. In Part I we already saw this for policy implementation. High taxes on hazardous waste, for instance, may be intended to lower the amount of waste, but when observance of waste handling regulations is not or cannot be sufficiently checked, the measure may actually lead to an increase of illegal dumping, and more environmental damage. So measures must be evaluated not as a mechanistic constraint, but as a regulative in an existing social system. All this may seem rather straightforward, but the structural inadequacy or even lack of evaluation of policy measures shows that in actual practice even the obvious is not always observed.
Section 2 of Part I also showed, that in an issue like nuclear energy, matters of

organizational reliability are often categorically dismissed by experts and authorities, whose thinking tends to be dominated by technical reductionism. That is to say, they see technology as something that will operate according to a previously conceived technical plan, and fail to recognize that the actual operation of technology will depend as much upon the people who operate it, as upon its technical characteristics.

The awareness of regulative issues is even less in the case of science and expertise. Both are largely looked upon as exogenic input in society, internally governed by mainly autonomous rules (with some allocational steering by state and industry), and providing neutral advice, according to objective standards. Paradoxically, even in disciplines like sociology of science, where this technocratic image of science and expertise has been under constant and effective attack, researchers have difficulties in explaining what this means with respect to their own results and their own role in society.
The position to be defended here is, that, in the absence of absolute standards in science, scientific statements should be evaluated as a regulative too, and that in as far as scientific statements have effects in society, these effects should be included in the evaluation. Scientific methods, frameworks, procedures, and statements, should all be evaluated not only with respect to their consistency and their qualities as 'representation of reality', but also with respect to their function as a regulative, inside the scientific community as well as outside. Remember that this was precisely the intention of the 'countercheck' component proposed in the preceding paragraph. Let us examine the precise consequences of this evaluation of science and expertise as a regulative a bit more specific now, by focusing on the evaluation of general perspectives, and scientific frameworks.

My thesis can be summarized as follows : scientific frameworks, theories, etc, should be evaluated not only according to whether they adequately represent what we decide to accept as 'facts', but also according to their regulative effect, that is, the effect that a framework has for the scientific community and for society at large when it is used by scientists and others to 'frame' the world and its (our) problems.
The easiest way to see what this means is probably by examining a concrete example. The case of relativism can be an instructive one. Let us define relativism here as the assumption that the validity of certain claims cannot be established in a universal, absolute sense, but only relative to some frame of reference: claims that are correct in one frame of reference, may be wrong in some other.
Most opposition against relativism has been based on the idea that relativism is either

inconsistent or self-defeating.[34] The first objection suggests that a relativist is bound to believe a claim if someone else holds that claim, even if the relativist him(her)self believes at the same time that the claim is false. This argument is simply incorrect. By holding that truth is relative to reference frame, one does not commit oneself to accept claims that are true in other frames than one's own. The second objection holds that it would be senseless for a relativist to try to convince people who are stuck in a different reference frame; in particular, it would be senseless to try to convince adversaries of relativism of their wrong. This argument is a bit more sophisticated, but certainly not enough to dismiss relativism. There could in fact be several sound reasons for a relativist to keep on arguing. First, the relativist may just want to convince those who share the same reference frame. Second, it is far from unlikely that one could find out only in discussions on concrete issues whether discussants have different reference frames or not. And third, even if people's reference frames were known, one could still try to 'convert' someone from one frame to another.

Can we now conclude that there is no reason whatsoever to feel uncomfortable about relativism? I don't think so, but the objections are to be found at a different level. It is not so much that relativism is definitely wrong as a representation. Who could claim universal knowledge, free of any bias or particular reference frame? I suggest that unease with relativism should rather be related to the fact that application is often accompanied by the suggestion that these different frameworks will never be overcome for a more common perspective. It is this threat of the disintegration of communication, the shimmering of an 'everyone for himself' mentality, that I see as the major hidden flaw of many relativistic approaches. It is not a flaw in the traditional terms of 'representation', but one in the regulative sense : it is the social dynamics of its use that easily leads to unwanted consequences. These consequences can only be tackled when relativists would address their own framework as a regulative, and realize that relativism is as unsafe as positivism until some proper 'countercheck' mechanisms are installed.

The implications of the evaluation of risk perspectives as regulatives are not confined to the problems of relativism or constructivism. The evaluation of regulative aspects should be applied to other aspects of risk as well. Take the dominance of technical reductionism in risk issues. We have seen that this forms an important problem. However, this problem cannot be solved by inventing another universal view of what

[34] See e.g. Harvey Siegel : Relativism refuted. A critique of contemporary epistemological relativism, 1987, Reidel, Dordrecht

risk *really is*. The question rather is how to keep the contribution of reductionism within the limits where it has indeed something substantial to say. A serious answer to this question must include matters of content as well as of procedure. It should contain an analysis of the social drives behind the over-dependence upon technical analysis, like the desire for 'certainty' and legitimation. And even if quantification and mathematical modelling sometimes seem to function more as a ritual rather than as a substantive contribution, we should ask whether we know a better, realistic social practice to deal with the problems, before ridiculing the lack of substantial content. It is easy to dismiss existing practices with the eye fixed on a longing, distant goal, but those who look too far ahead fail to see the obstacles on the way, and scientists and experts often are only too eager to assist in the construction of mirages.

To close this paper, I would like to explore some similar implications for another domain that is relevant to risk issues, and to the role of science and expertise in general, namely that of normativity and ethics. For here too, the evaluation according to regulative effect could bring some better understanding.

First, I think that the distinction between facts and values, which seems to be more or less abandoned in much research on risk, as well as in e.g. sociology of science, can be restored in a more fruitful way as a regulative. To make a distinction between facts and values should not so much imply that for any particular claim we have a clear-cut, infallible idea whether it is factual or normative, but rather that it is sensible to try to distinguish as good as we can between factual and normative claims. Then the merit of the fact/value distinction is no longer that we could produce a complete, infallible categorization, it is in the improvement of the quality of discussion we can achieve when everyone tries as good as possible to distinguish between 'factual' and 'normative' claims. This is important, since one of the ideas behind the proposal of 'counterchecks' was that we can identify gross scientific 'overstatements' originating in tacit normative assumptions. Elsewhere, I have proposed to define the fact/value distinction in a regulative way as follows[35] : 'By calling a claim factual, the speaker expresses the assumption that any person, even if (s)he would calculate from his/her personal interest only, would accept the claim for him/herself on the same grounds. Calling a claim normative means that the reasons for acceptation invoke an evaluation and balancing of the interests of all parties affected, and would not remain valid if everyone was calculating purely from his/her own personal interest.'

A second implication concerns a variety of publications on the role of science in society that argue for a more cooperative rationality. In the trail of Habermas, they

[35] Birrer, op. cit. (version december 1990)

hold that as a complement to the technical reductionism of modern science we need a more 'communicative' rationality. They might in this respect be contrasted with e.g. interest-centred approaches, which seem much more conflict oriented. But such a mere opposition is relatively unfruitful. Certainly many problems with risk, or with the role of science in society, could be solved if there was more cooperation. However, in many situations cooperation requires trust and shelter[36]. This can only be provided if the discussion can be on terms of equality, and if there are 'countercheck' mechanisms to ensure some protection. On the other hand, interest analysis often is too one-sided as well. In the feedback perspective, the analysis of interests would be a means to detect some systematic 'deviations', it would not have the pretension to be a full picture.

Finally, I would like to mention that normative principles too can be better understood when we apply the idea of evaluation as regulative, and the feedback perspective proposed earlier. As was the case with relativism, some approaches in ethics fail only because of their regulative qualities, not because they misrepresent intuitions about justice as such.[37] One could also question the status of certain normative 'principles'. It could for instance be asked whether the so called 'precautionary principle'[38], which not only has been embraced by some philosophers, but also in politics, is not an attempt to counter presently operative principles (and thus part of the 'feedback'), rather than having a clear positive meaning itself. Perhaps the perspective put forward in this paper could bring a clearer view of the status of such 'principles'.

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[36] E.g. prisoner dilemma's, or a 'tragedy of the commons'.

[37] Cf. Frans A.J. Birrer : A two-sided approach to morality, in Aant Elzinga, Jan Nolin, Rob Pranger, Sune Sunesson (eds.) : In science we trust? Moral and political issues of science in society, 1990, Chartwell-Bratt (UK)/Lund University Press (Sweden)

[38] Usually to be taken as something like 'we must refrain from doing anything of which we cannot prove that it will not cause serious damage'.