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RISK PERCEPTION AND CREDIBILITY OF RISK COMMUNICATION

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ABSTRACT

Sjöberg, L. Risk perception and credibility of risk perception. *RHIZIKON: Risk Research Reports No. 9, 1992.* Experts and the public frequently disagree when it comes to risk assessment. The reasons for such disagreement are discussed, and it is pointed out that disagreement among experts and lack of full understanding of real risks contributes to skepticism among the public. The notion that people are in general reacting in a highly emotional and non-rational, phobic, manner is rejected. The very conditions for risk assessment present to the public, and common-sense cognitive dynamics, are better explanations of risk perception, as are some social psychological concepts. If trust is to be established in a country where it is quite low some kind of politically regulated public influence on decision making and risk monitoring is probably needed, e.g. by means of a publically elected and responsible *ombudsman.*

Introduction

People's reactions to risks have become an issue of central importance in policy making. The most well-known case is, of course, that of nuclear power, but many others could be mentioned as well: toxic waste, genetic engineering, food additives, etc. In most, or all, of these cases experts judge risks to be minor or even non-existent while the public is quite concerned about the risks and perceive them to be high.

Of course, experts rarely agree completely among themselves and the evaluation of most risks is uncertain for many reasons: lack of experimental data and incomplete theoretical understanding of the mechanisms behind a risk being perhaps the most important ones (Otway & von Winterfeldt, 1992). Yet, the overwhelming scientific majority frequently finds itself pitted against a public opinion which simply does not accept its conclusions. Social turbulence follows and politicians are forced to allocate resources in ways which may bear little or no relationship to the real needs for risk reduction in a society[2], as they see them.

The first reaction that comes to mind upon reflection on this situation is that people are just misinformed and ignorant. Is it not true that experts know much more about these risks than the public does? However, to try to explain the difference between experts and the public with reference to knowledge and lack of knowledge is futile, for several reasons:

[1]. This is a revised version of a paper which was prepared for a Joint Russian-Swedish Symposium on the Psychosocial Effects of Radiation Accidents, and on Possibilities to Improve the Credibility of Officially Given Information, June 10-11, 1992, National Radiation Protection Institute, Stockholm, Sweden. The work was supported by grants from the National Radiation Protection Institute and the Swedish Council for Research in the Humanities and the Social Sciences.

[2]. However, it cannot be assumed that those resources actually would be available if more "rational" considerations were to be brought to bear on the issues of risk reduction in society. Suppose that nuclear power could be made more risky than now, thereby saving some money and making electricity bills somewhat less burdensome. The money saved would probably end up in increased private consumption, which, in turn, is by no ways guaranteed to lead to better health in an already affluent society. It could lead to the opposite, if people e.g. were to change their diets and eat more sweet and fat foods.

(1). People are not *that* misinformed about all risks. Data on judged mortality rates, Fig. 1, that I recently collected in Sweden show that the average public ratings have the same rank order and level as the true values, with one exception (heart attack) where the public grossly underestimated the risk (although they still placed it in the correct rank). Admittedly, there is tremendous variability of ratings behind such data as those in Fig. 1, but, once again, "average man" is just about right in his or her risk perception. A second example: In a study of the perceived AIDS risk carried out last year in Sweden (Sjöberg, 1991 a) I found that people were extremely well informed. In fact, people were so well informed about these basic AIDS facts that it was almost impossible to construct a varying knowledge score!

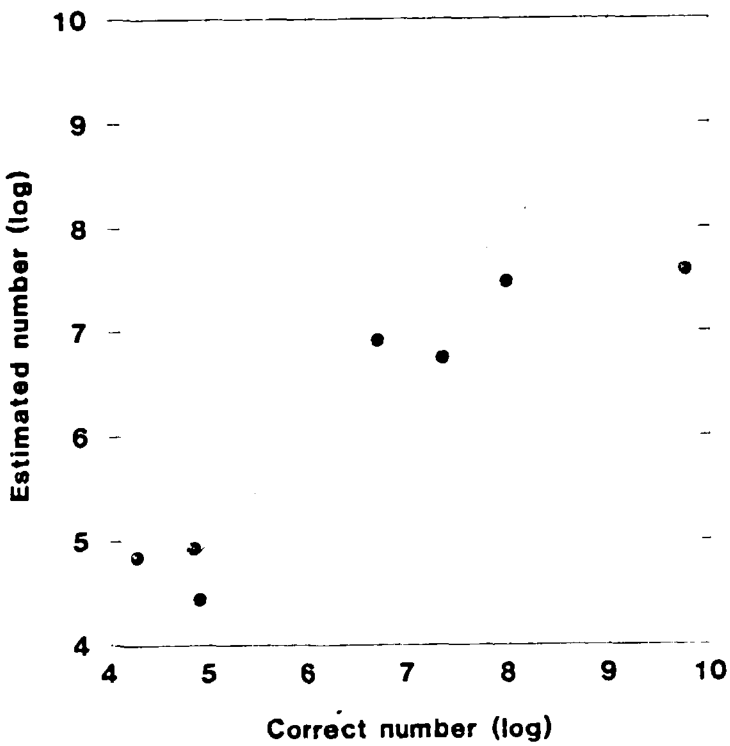

Fig. 1. Estimated number of deaths per year plotted against the actual values, both in logs.

(2). In several studies we have found that there is, indeed, a correlation between perceived risk and knowledge about the issues involved in that particular risk. Those who know more judge the risk to be smaller. However, the correlation is typically quite modest in size, perhaps about 0.2. This means that very little of the variance in risk perception, in a group from the general population, can be explained by variation in knowledge. If knowledge is measured by self ratings, i.e. people are asked to rate how much they know about a topic, the small correlation vanishes altogether. This finding does not deny that the variability between experts and the public reveals a clear correlation between knowledge and perceived risk, of course.

(3) The difference in perceived risk between experts and the public does not necessarily demonstrate a causal influence on risk perception by a high level of knowledge. As an alternative, consider the possibility that experts *first* acquired their risk perception, *then* decided, perhaps partly on that basis, to devote themselves to the acquisition of expertise in a given area such as nuclear

power or genetic engineering. We (Drottz-Sjöberg & Sjöberg, 1991) studied high school[1] students and found very strong covariation between line of study (science, technology, social science or humanities) and perceived risk. Those who participated in programs that led to university level studies of sciences and technology - and some of them were future experts in these fields - were much less concerned about risks of nuclear technology than others, although they had not yet, at this point in their lives, acquired expert knowledge. These differences were even larger than gender differences, which are always observed with regard to radiation risks, and which we found in our study as well.

Experts and the public

Risk perception is rarely equal for experts and the public, even if they may be, at times, in rough agreement. In a frequently cited study, the US EPA compared experts' rankings of important environmental risks with public risk perception (US Environmental Protection Agency, 1987). They found little agreement between the two sets of rankings. A follow-up three years later gave virtually the same results (Roberts, 1990). A set of French data give a very different picture, however. In a study in Bordeaux, experts on hygiene and safety rated risks and desired risk reductions (Barny et al., 1990). They found extremely close rank order agreement with mean ratings of the same risks made by the public, although the level differed: experts gave lower risk ratings. This discrepancy raises the question who is right: the French or the US data? Perhaps the French experts were less substantial experts than the corresponding EPA experts. It seems unlikely that anyone can really be an expert on all the diverse risks studied in the French investigation. On the other hand, there are other studies showing convergence of expert and public opinion as well. E.g., Wyler, Masuda and Holmes (1968) found that patients and doctors gave similar risk ratings of various illnesses.

In some cases, the public takes risks that experts discourage, such as risks of smoking, drinking, AIDS or high radon levels in homes. Some people do listen to warnings about such risks, of course, and some of them act accordingly, but most ignore them. They may be persuaded that there is a risk *for others*, but not for them. They tend to deny personal risks (Weinstein, 1984).

A good example is alcohol. I have found that people rank the *general* risk associated with alcohol as quite high but that they also rate the *personal* alcohol risk as very small (Sjöberg, 1991 b). Alcohol is a risk that people perceive that they can control. With risks they cannot control they perceive general and personal risks more in agreement.

Thus, experts sometimes warn people about risks and sometimes they reassure them that there is nothing to worry about. What they say seems to be correlated with the role they have accepted. I distinguish between *protectors* and *promoters*. Protectors tend to warn people about risks. They are sometimes whistle blowers or otherwise mavericks who protest against established procedures. Some of them are medical doctors who want to protect people from illnesses. Promoters, on the other hand, are typically concerned with a broader issue than merely a risk under discussion. They are frequently worried about the economic consequences of too drastic risk reduction policies. A few quotations may serve to give the flavor of promoters and protectors.

Rasmussen (1991) recently stated, in a lecture to the Society for Risk Analysis:

"No matter whether you say something might happen the next century, next year, or next week, the response from the public will be 'That's just too often for me'. By the time you are

[1]. They were grade 12 students, 18 - 19 years old. The Swedish "gymnasium" overlaps with the freshman and sophomore college years in the USA.

down to 10^{-4}, you are in a region that is not understood. When you start comparing it with [common] ways people could lose their lives, they think you are trivializing it."

"[We have] strong dislikes and paralyzing fears about any activity that entails risk. Projects are so costly and our systems for dealing with people so involved that in the end we reach agreements that are against logic. We spend and squander our resources to defend ourselves against phantom risks."

Experts on natural hazards tend to have a different view about the publics' reactions to risks. Writing about earthquake risks, Bolt (1991) stated:

"In terms of national welfare, it might be expected that the risk involved in earthquakes would give special force to the claims for funds and resources for earth scientists, engineers, planners and others involved in enhancing seismic safety. Seismological history tells otherwise. Risk reduction is characterized by bursts of activity and political support after damaging earthquakes, and decay curves that have a half-life of a year or so before public effort recedes."

Hence, experts and the public disagree, and experts disagree among themselves. In a situation such as this, it is likely that communication tends to break down (Sjöberg, 1980, 1991 c). Experts see the public as misinformed, badly educated and highly emotional while the public suspects that experts know less than they claim and that they are corrupt due to their being hired by the industry or government. People trust independent experts much more than experts hired by the industry and at least in Western Europe such independence is perceived when it comes to experts who are associated with universities, or who have publically warned about risks (whistle blowers). In a study of the nuclear waste risk we found that there was more confidence in dissident experts than in experts associated with state authorities or the nuclear industry (Sjöberg & Drottz, 1988).

But why are all experts not simply trusted? There are many reasons. First and foremost, experts often disagree. Otway and von Winterfeldt (1992) cited a study of expert assessment of failure probability in a nuclear power plant. Different teams of experts were formed. The error probability estimates converged when the teams were informed about each others' estimates and analyses, but the initial estimates varied by a factor of 1-50. Uncertainty ranges varied even more.

The fact that experts disagree is indeed nothing to be ashamed of. Disagreement is the very air that science breathes[1].

Second, to-day there is much more knowledge about risks, even small risks, than previously. This situation has its problems. One problem has to do with the fact that knowledge about the risks is incomplete. In many practical situations risks are hard to measure and estimate. Perhaps there is knowledge that risks are "small" but they cannot be specified more exactly. We do not know *how* small they are. Knowledge thus has the character that a risk is known to exist but its size cannot be specified. Because of this, there is room for different opinions as to the size of the risk.

Third, many risk assessments are based on animal experiments. Animals are exposed to large doses of a chemical, and the observed cancer risk is extrapolated downwards to the risk levels

[1]. This was to me so obvious as to be a mere triviality until I met with vehement opposition from a professor of physics, who made the truly amazing statement that "experts do not disagree on questions of fact. It is very unusual that they do. Most of the time they agree. Of course, they can all be wrong because new facts may come up which cause a change of opinion later. But at the present time experts usually have the same opinion, admittedly within a margin of uncertainty". (Gerholm, 1992, p.109, my translation).

that exist for humans. This practice has been criticized on two grounds. First, it is uncertain to which extent one can generalize between species. Second, a linear extrapolation can overestimate the risk at small dose levels (Abelson, 1990).

Factors in perceived risk

Hence, the lack of complete knowledge about risks carries with it disagreement among experts. This disagreement exists in a social atmosphere of intense controversy. In the public debate the experts' disagreement is enlarged. Conflicts arise in risk questions, between a concerned public on the one hand, industry and experts on the other.

Once the social conflict is established it lives its own life and it has its own internal dynamics. Sometimes people voice extremely high demands for risk minimization, sometimes, the reactions to risks of a corresponding size are much cooler. Why?

The first factor determining risk perception is of course real risk. E.g., Sjöberg and Drottz (1987) found that perceived risk from the Chernobyl accident in Sweden was related to fall-out. The correlation between real risk and perceived risk shown above in Fig. 1 is substantial. Yet, nobody would argue that real risk is the sole determinant of perceived risk. There are many other important factors to discuss.

One type of answer to the question of what further determines perceived risk is to refer to a number of risk dimensions. About 20 dimensions have been mentioned in the literature. The basic work in this area was carried out by Fischhoff et al. (1978).

It has been argued that reactions to risk are largely dependent on these dimensions, in addition to knowledge about the factual risk levels. A basic notion is that the dimensions afford a basic qualitative description of the risks and that risks which are very different in these respects cannot be compared without a loss of credibility. Covello, Sandman and Slovic (1988) published a handbook of risk communication which is based on these dimensions and the assumption that they are decisive for which risk comparisons are acceptable as reasonable and informative. However, Roth et al. (1990) showed that the risk dimensions did not appear to guide the judgement of the acceptability of risk comparisons in the manner asserted by Covello et al.

In addition it is likely that the importance of the dimensions has been greatly overstated also for the initial aim of explaining variation of perceived risk size and acceptability. The studies that Fischhoff and Slovic carried out around 1980 were based on small, non-representative convenience samples. Later work, such as carried out by Slovic et al. on perceived nuclear waste risks (Kuhnreuter et al., 1990) is marred by extremely low response rates in telephone interviews (about 35 percent). Furthermore, the analyses that were carried on these dimensions were based on mean values, not individual ratings. This is a quite confusing practice if there is an interest in explaining individual risk perception, which in turn is probably a reasonable and common goal. Only rather recently was there a study which was based on data obtained from the population at large and which analyzed data at the individual level (Gardner & Gould, 1989). Gardner and Gould found that the explanatory power of the risk dimensions of Fischhoff and Slovic was *sizably* lower than in the earlier work based on mean data.

Let us devote some more thought to the determiners of perceived risk. "Psychodynamic" notions are rather popular. It is e.g. argued that perceived nuclear risks are really expressions of "existential anxiety", and that such risk perception therefore has causes which are completely different from real risks. This argument presupposes, of course, that psychodynamic theory is valid. As shown by many authors, this is not the case (Eysenck, 1985; Grünbaum, 1984).

Another alternative which is somewhat similar in explaining risk perception by something else than real risk is that of cultural theory (Douglas & Wildavsky, 1982). They claim that there are three basic cultural orientations: egalitarian, hierarchic and individualistic, and that people "choose" risks to worry about in order to sustain these social orientations. This theory has been supported by its originators but their data have not been replicable (Sjöberg, 1991 c).

I will devote special attention to a risk dimension which has been largely neglected by American investigators, *viz. morality.* We found clear indications that it was very important in a project aiming at a review of social science risk research (Sjöberg, 1987). Several of the Fischhoff dimensions are more or less clearly related to moral aspects. I will argue here that risk reactions are largely dependent upon how responsibility and morality are regarded.

In a study published some years ago (Sjöberg & Winroth, 1986) we asked very different groups of people to judge the acceptability of risks. Other groups were instructed to judge if the risky activities were morally inferior or morally praiseworthy. The relationship between moral value and acceptability was quite strong, especially for individual activities. We found in this study that the morality aspect had even more importance for the acceptability of risks than the excepted outcomes of these actions.

By way of comment, it should be noted that there is a basic conceptual fuzziness in much work on risk acceptability. What does it mean, indeed, to *accept* a risk? It seems meaningless to speak about accepting *any* risk as such, what we accept is to carry out certain activities that may bring about negative consequences.

In a recent study 157 subjects made ratings of 43 risks, both potentially fatal risks and more banal ones (Sjöberg, 1992 b). They judged perceived risk size, probability of a negative outcome, size of such a negative outcome and the risk mitigation that they required. Half of the subjects judged a general risk, and half judged personal risk. Here I will give some results for general risk, noting that they were quite similar for personal risk.

The mean ratings for all 43 risks were computed and correlated across risks. The results are given in Table 1.

Table 1. Correlations between risk ratings, general risk, across 43 risks

	Consequences	Probability	Size of risk
Size of risk	-0.067	0.966	-
Required risk reduction	0.813	0.093	0.169

It is seen in the table that size of risk and required risk reduction were virtually uncorrelated. Hence, there is no guarantee that people will demand that a large risk be reduced and that they ignore a small risk. Second, it is very clear that risk reduction demands are related to the perceived consequences should a negative event occur, while risk level is related to the probability of such an event. Hence, risk communication which is often concerned with showing that probabilities are very small is not likely to lead to small demands of risk reduction, as intended, should these data be valid.

Risk perceptions may be caused in different ways in different situations. E.g., when there has been a major accident it is possible that people perceive risks in relation to concrete events that have been caused by the accident, or that they believe have been so caused. I will return to this aspect of risk perception when I discuss reactions to a nuclear accident later in this paper.

Research on perceived risk has not proceeded very far, perhaps because most investigators have failed to make a crucial distinction, viz. the distinction between personal and general risks. I now turn to that topic.

Personal and general risks

In most studies of perceived risk there is no distinction between various *risk objects*. In other words, it is not made clear to whom the risk pertains. E.g., studies of risk perception and nuclear attitudes following the Chernobyl accident did not, as a rule, investigate perceived *personal* risks. Yet, it is known that perceived personal risks tend to be quite different from, and lower than, perceived risks to people in general (Weinstein, 1984), and Weinstein has argued that we have here a case of *risk denial*.

Many researchers ask for risk ratings without reference to any explicit object (e.g., Slovic et al., 1989). The question that naturally arises in such a case is if such ratings resemble those of personal or general risk ratings. As a third alternative, consider risks to family and friends. Some researchers ask for risk ratings with reference to one's own person and family. It is interesting to inquire about the properties of such ratings as well.

In a recent study I obtained data from 395 subjects (a wide sampling of people but with a predominance of people with a high level of education). They were divided at random into several subgroups and they judged risks to

- their own person
- family and friends
- Swedes in general
- any one Swede, picked at random
- unspecified risk (i.e. singular)
- unspecified risks (i.e. plural)

The results can be seen in Fig. 2. It is clear that there was a tremendous difference between own risk and risk to others, and that risk to one's family and friends came somewhere in between.

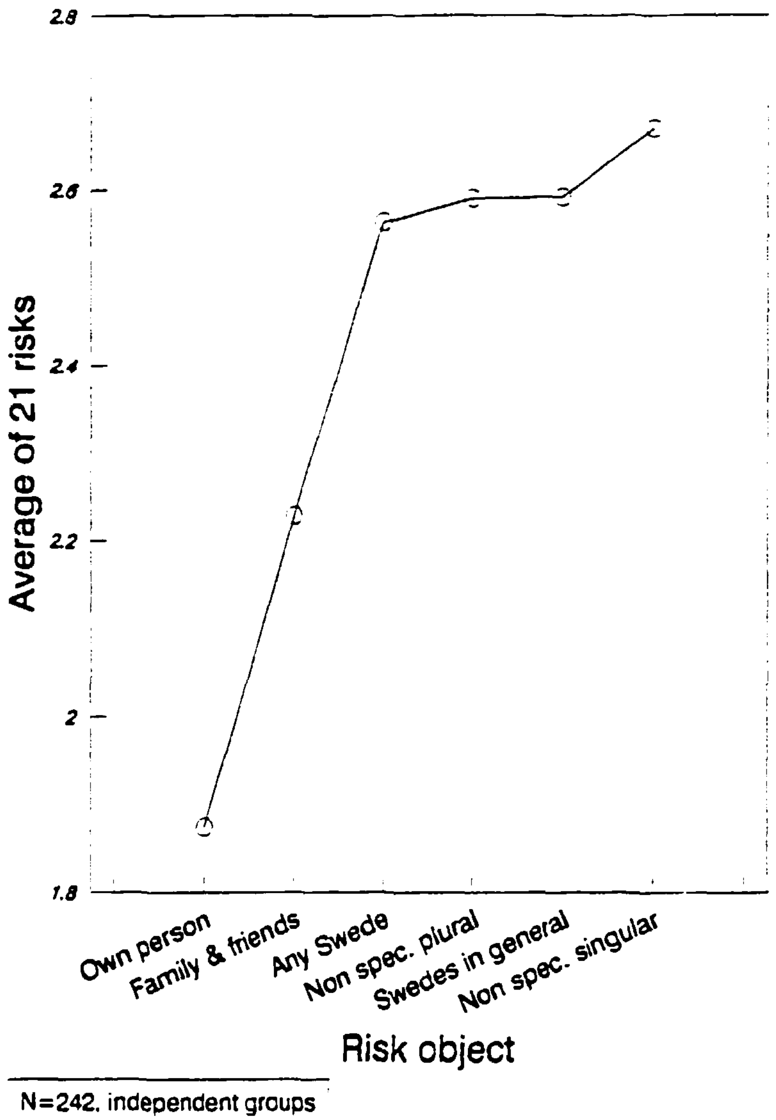

Fig. 2. Average risk ratings for 5 different risk objects.

It is interesting to note that results on *structure* resembled these results on average risk level. When average risk ratings were correlated, the results of Table 2 were obtained. It was found that personal risks deviated in correlational structure quite a bit from general risks, and that, again, risks to family and friends came in between. When the details of the findings were scrutinized I found that some risks were judged in a drastically different way when they were personal as compared to when they were general. The most notable example was alcohol. The alcohol risk was rated as No.1 among the risks to people in general but as the *smallest* of all risks to one's own person!

What accounts for the difference between personal and general risks? In a study of the risks of tanning and sun-rays, (as yet unpublished) I asked subjects to judge the degree to which they could protect themselves from risks, to control the risks as it were. The mean difference between general and personal risk has been plotted against control ratings in Fig. 3. It is seen that control accounted for a very large share of the difference between mean general and personal risk.

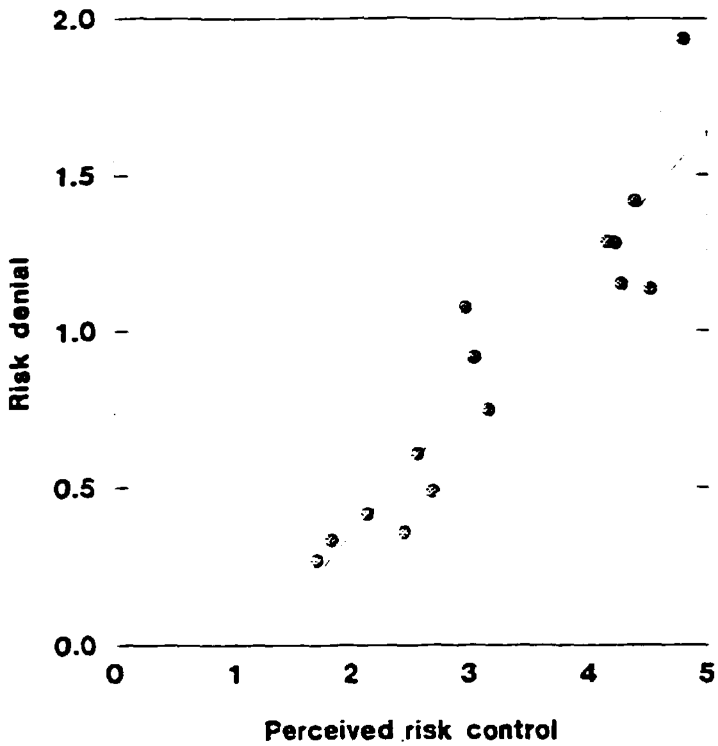

Fig. 3. Risk denial (general risk minus personal risk) plotted against perceived personal control over a risk.

What, then, about risk ratings with no explicit risk object? The findings are given in Fig. 2.

Summing up:

(1). Personal and general risks are very different, personal risks being rated as *much* lower. Risks to family are in between.

(2). When no risk object is specified people seem to interpret their task as referring to general, rather than personal, risk.

(3). The difference between general and personal risk is largely accounted for by perceived control. When perceived control is low, there is little difference between the two.

One conclusion of the study is that perceived risk should always be specified as to the risk target. A second conclusion is that, given the necessity to choose, it is most likely that perceived *personal* risk is the most important aspect to study. These risk ratings tend to be quite low in most circumstances but they do contain variation and important information. It is possible that perceived risk to family and friends could also be fruitful to study, perhaps because there is less risk denial in that case, but it is yet too early too tell if that is the case.

Attitudes and behavior - are we really willing to pay?

Risks are always judged in a context of economic costs and benefits in policy making. In Sweden, few authorities have, however, taken the step to put a prize on human life. The Swedish Radiation

Protection Institute (SSI) has recently decided that life saving measures at the level SEK 5 million to SEK 25 million are justified; see Bengtsson (1992) who also gives data from other Swedish and Nordic authorities. According to these data, road traffic authorities work with a policy of SEK 7 million per life, and Vattenfall (a major energy producer) with SEK 10 million in the case of construction work in nuclear power plants. Other figures are much lower: The social welfare authority SEK 2 million for radon induced lung cancer, SSI 0.2 million for melanoma induced by sun rays. US figures vary between sums corresponding to SEK 5 and 50 million (Miller, 1989).

Thus, a few cases of explicit evaluation have been published, but they are quite exceptional. E.g., in Swedish medical care planning there is no explicit life value. A reason for this reluctance is suggested by data I recently collected, where I had asked people to judge how much they felt that the state or local government should be willing to pay in order to save a life (of any Swede), or even of themselves. The results for any Swede are given in Fig. 4. It is seen that the modal judgement is SEK 1 million (the same was true for one's own person). Very few people judged a difference between the value of another person and themselves, and those who did were equally divided among egoists and altruists.

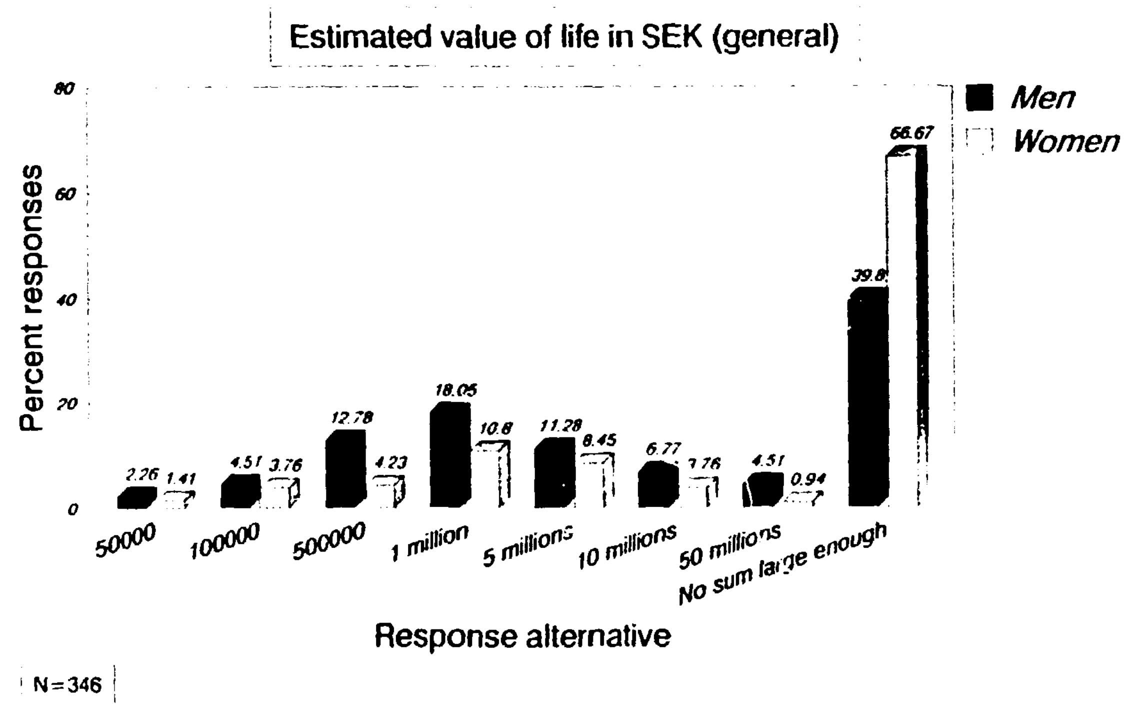

Fig. 4. Response distribution to the question how much the authorities should pay to save a human life.

However, the most striking result in Fig. 4 is that a majority of all respondents, and most clearly female respondents, chose the alternative stating that no value could be high enough when it came to saving a life.

This does not mean, of course, that they would actually endorse a policy giving absolute priority to risk reduction over all other goals of society. But, on the other hand, policy decisions are rarely or never formulated in that manner. Rather, one issue at a time is dealt with. And the policy maker then meets with resistance to assign a finite value to a human life. The resistance to make an exchange between health risk and money was documented also by Kuhnreuter and Easterling

(1990) who found that people were unwilling to accept a nuclear waste deposit in their neighborhood even if economic benefits were offered. On the other hand, people may be willing to *reduce* a risk. Hamitt (1990) found, in a small scale study, that people were willing to pay up to 50 percent more for biodynamically produced food. In Sweden, there seems to be a current upsurge of interest in biodynamically produced food and it can apparently be sold at prices some 20-30 percent above the ordinary price level.

Reactions to a nuclear accident

Following the TMI and Chernobyl accidents there has been a considerable amount of research on their effects on nuclear power attitudes. Few studies, however, reported data on *personal* risks, only risks in general. A special issue of the *Journal of Environmental Psychology* (No. 2, Vol. 10, June 1990) was devoted to studies of reactions to the Chernobyl accident but no data on personal risk perception are presented, with the exception of our own paper (Drottz-Sjöberg & Sjöberg, 1990 b).

For the occasion of a Russian-Scandinavian symposium in Tromsö, Norway, in November of 1991, I re-analyzed our data on reactions in Sweden to the accident. These data were collected in September, 1986, from widely different groups of people and in different areas of Sweden. The analysis is reported in Sjöberg (1992 a). The main finding was that a good fit could be obtained with a model in which personal risk was driving the process, leading to perceived health damage, worry, perceived general risk and a change of attitude to nuclear power. Further work, including latent variable modelling and PLS methodology[1], has now confirmed these findings.

It is noted here that the economic value of nuclear power contributed much less (0.19) to nuclear attitude than its (general) risk (-0.36). It is possible, of course, that people would pay more attention to economic aspects when not under acute threat from a nuclear disaster but it is by no means sure. The release of a small amount of Caesium in Goiania, Brazil, led to tremendous social and economic turbulence, to take one example from a country with large economic problems (Roberts, 1987). A moderately large railway accident in Sweden in 1864 (7 dead), the country's first accident of this kind, caused a tremendous media response (Drottz-Sjöberg & Sjöberg, 1990 a), in the poor country which Sweden was in those days. Gullström Nyland (1992) found that slum dwellers in Sao Paulo perceived risk much the same way as well-to-do and high achieving engineering students in Stockholm, only at a higher level.

At the Tromsö symposium I also presented results from re-analysis of data on perceived risk of nuclear waste, data collected in the spring of 1987 (Sjöberg & Drottz, 1988). These data were especially interesting because they showed the influence of credibility of experts on perceived risk. The regression analyses showed that attitude could explain a large share of the variance in risk perception. However, regression analysis says nothing about the direction of the influence.

I reanalyzed the 1987 data on nuclear waste risks, using two latent variable models. Both models assumed that perceived nuclear waste risk was a function of general risk sensitivity, and the credibility of experts, but they differed as to the direction of causal flow. Basically Model 1 assumed attitude to be driving the process while Model 2 assumed attitude to be a consequence. The results are given in Figs. 5 and 6.

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[1]. This methodology was devised by the late Swedish statistician Herman Wold (1981, 1985). It is basically similar to Jöreskog's LISREL method but much easier to use, especially with large numbers of manifest variables and less than perfect models.

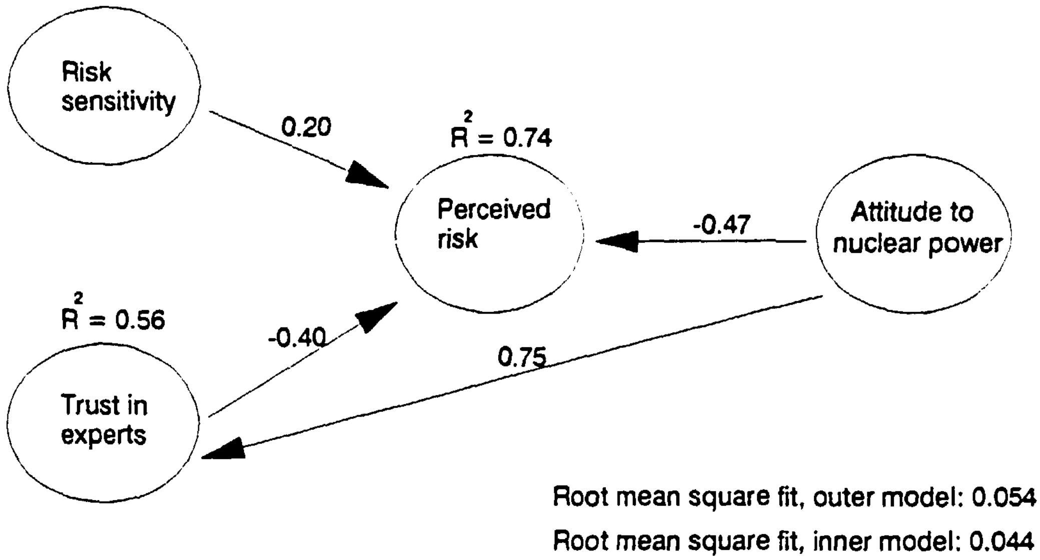

Fig. 5. Attitude driving perceived risk

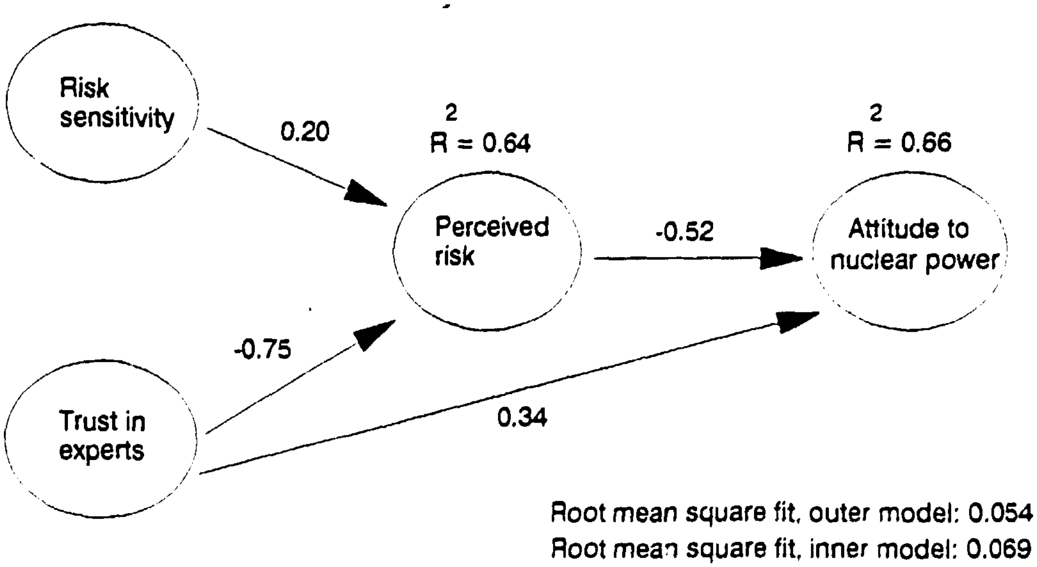

Fig. 6. Perceived risk driving attitude

As seen in the figures, model 1 gave a much better fit of the "inner" model, i.e. the relations among the latent variables were much better fitted by this model than by model 2. Hence, the 1987 data on nuclear waste risk differed in an important way from the data collected in the acute phase of the accident: risk was not driving the process, attitude was. It is reasonable that this should be the case. When people are not confronted with an acute danger, their global attitude may be most important, also for risk judgments, while an acute risk situation has the effect of making perceived risk a primary psychological force.

Radiophobia

Drottz-Sjöberg and Persson (1992) have discussed thoroughly the concept of "radiophobia" and they argue that it is misplaced. People perceive radiation risks which experts deny, yes, but this is a phenomenon which is not necessarily tied to phobic fear. Why, then, do people have these risk perceptions, following a nuclear accident? Is perceived personal risk in some way related to the experiences that people have?

To answer this question we must first consider the fact that most people are quite concerned about radiation even *before* there is an accident. Radiation is associated with cancer, it cannot be sensed and avoided and it is even associated with the horrible images of nuclear war. An accident involving sizable radioactive fall-out therefore easily triggers fear. To alleviate such fear, people need to be informed about the levels of radiation that have actually been produced by the accident, and, if these levels are not high enough to be dangerous, they need to be informed about this fact. This would be a normal process of information, *and it would tend to work if people trusted the government and its experts.*

However, it is obvious that experts enjoy far from 100 percent trust from the public. The public has other notions and other beliefs. These beliefs are not completely irrational, on the contrary I shall argue that they are formed on a basis of experience which everybody uses. They can sometimes lead astray - and sometimes they serve us quite well. I will briefly discuss the character of everyday knowledge and compare it to complete irrationality on the one hand, science on the other. The purpose of the discussion is to suggest an explanation for why people are convinced of a causal attribution of their state of health which is unacceptable tom experts.

People "know" about their environment and its risks in several ways. They may just feel that something is risky, have an intuition about it, without being able to explain why they feel that way, or they may base their perceptions and thoughts on something they have experienced. It is the latter alternative I will discuss here.

There is knowledge and knowledge. Every adult person knows some things such as

- we live on a large globe, a planet, which rotates around the sun
- water freezes to ice when the temperature is low enough
- all men are mortal
- a week has seven days
- most men strive for pleasure and try to avoid pain

These are quite different examples. The first two refer to the physical world, the third is a biological fact, the fourth a social convention and the fifth a psychological principle. Science does not deny these statements, of course, and it has even historically contributed at least one of them, the first. For how could you know that Earth is a globe, it certainly cannot be *seen* (unless you are an astronaut) as such. The answer, of course, is that you trust scientists when they make the assertion.

For the other cases you need not really trust science to believe in them. These are things that you can find out for yourself, by talking to other people and by observing nature. Yet, no one denies that knowledge acquired this way is both trustworthy, necessary and used by everybody.

Hence, there is perfectly good knowledge which is not scientific and which guides us in our everyday lives. Let us look a little further at a strategic aspect of such knowledge, i.e. causal attribution.

Michotte (1954) performed classical studies of perceived causality. A very close temporal contiguity in the order *first* A *then* B of two events compels us to perceive that B was caused by A. Our perceptual apparatus is tuned to the discovery of causes and it organizes our world view according to cause-effect relationships. We do not easily "see" randomness, or its consequences. If purely random events are presented we see systematic patterns. In addition, when asked to produce or simulate random patterns of symbols people usually fail and produce systematic deviations from randomness. E.g., they produce too few long runs of one symbol. This could be explained by the famous gambler's fallacy effect: people believe that the likelihood of change increases the longer they have been exposed to a series of repetitions of one event (Bar-Hillel & Wagenaar, 1991).

Later work has been more concerned with inferred causality rather than directly perceived causality. Kahneman and Tversky have demonstrated that perception is affected by similarity and salience. If A and B are similar in some important manner and A preceded B, A may be regarded as the cause of B[1]. If A is made salient by much media attention it is more likely to be regarded as the cause of events that followed it - especially if A is a very potent event, such as major technological disaster. Finally, people have a bias to perceive only *one* cause of an event, thereby greatly simplifying things, sometimes to the level of nonsense.

In my view, all this constitutes a plausible explanation why people are so convinced that the Chernobyl accident has caused illness, *regardless of whether the claims are true or not.* For if the claims are true, they could still hardly be substantiated by informal, spotwise impressions.

Data collected in the summer of 1992 in Novozybkov by Britt-Marie Drottz-Sjöberg illustrate the point. The subjects, 185 persons living in the area, rated each of 33 dangers on a 7-point scale, and they also rated change in those dangers since Chernobyl. Price increases topped the list, but it was otherwise dominated by nuclear and radiation dangers. Such common risks as smoking, traffic and alcohol came last. See Fig. 7.

[1]. This is a principle of magic thinking. There are many such fables, like the one made famous in the successful movie "The Elephant Man". The growth of this person was allegedly disturbed by his mother being scared by an elephant when he was still a foetus. As a consequence he became a "monster", an "elephant man".

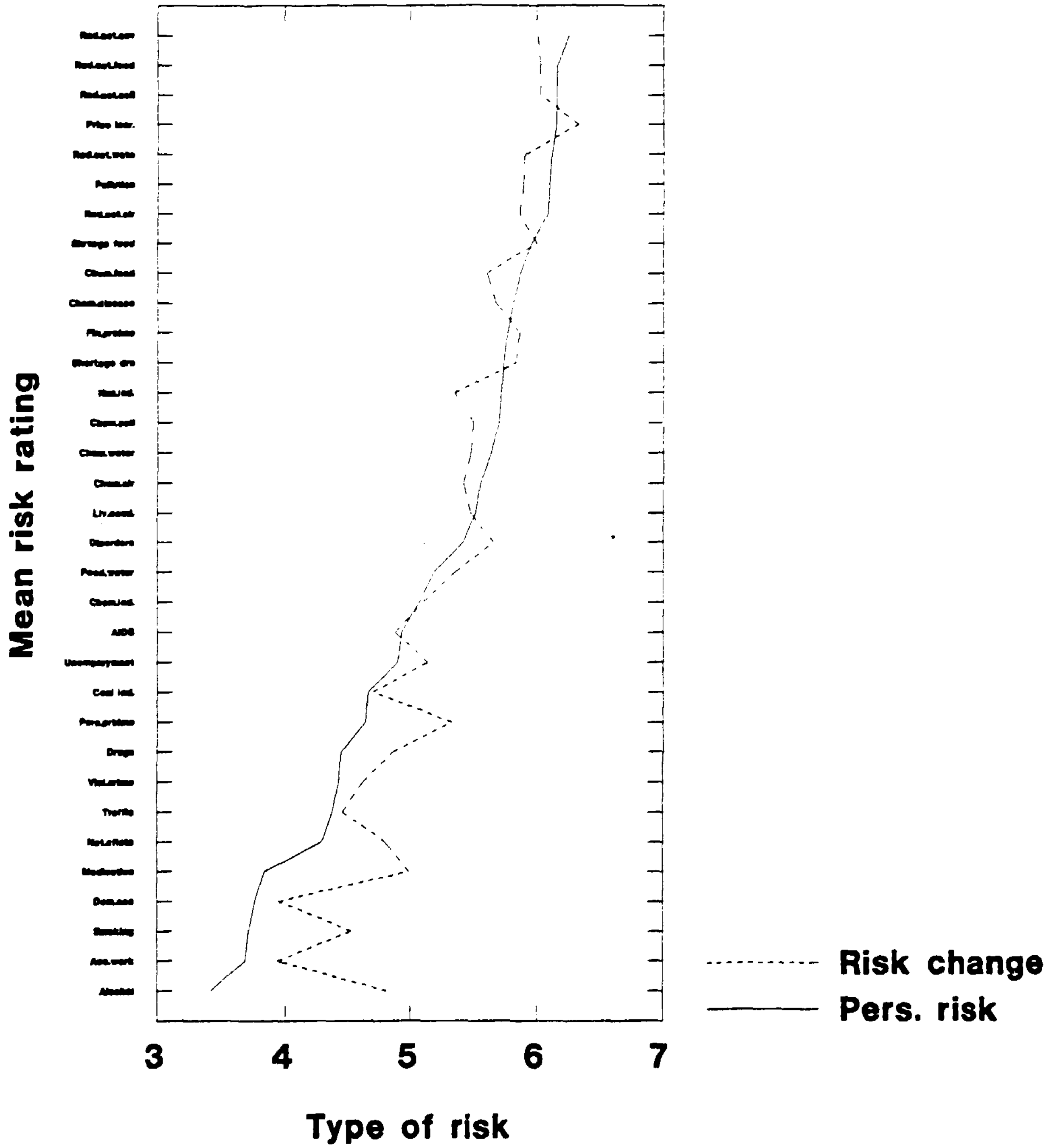

Fig. 7. Perceived danger and change in level of danger since the Chernobyl accident. Novozybkov, summer of 1992.

The task of establishing the existence of a causal link between illness and environmental toxic agents is very difficult and requires close epidemiological scrutiny. People could not possibly detect a probabilistic link between illness and a toxic agent administered once. In addition, radiation illnesses, in particular cancer, could not have developed in the short time since the accident, according to the presently held views of experts.

To illustrate the problems, let us consider the simple case where all information can be given in fourfield tables containing frequencies, or probabilities, of occurrence of various cases. Cheng and Novick (1992) have discussed how people make causal inferences on the basis of probabilistic information. Suppose that we have the hypothesis that red hair is associated with emotional instability. Consider the following fourfield table, Table 2.

Table 2. A simple structure for analyzing the perception of causality.

	Emotionally unstable	Emotionally stable
Red hair	a	b
Not red hair	c	d

Smedslund (1963) argued, in a seminal study, that people pay attention mostly to the frequency *a* in the four-field table in the perception of correlation. If this number is large enough they will conclude that there is a causal relationship. Of course, the frequency in isolation says nothing about a relationship between hair color and emotional stability. Cheng and Novick (1992) suggest that the important factor is the difference *a-c*. Other suggestions have been made, most notably that the critical features are either the sum of confirming cells *(a+d)* minus the sum of disconfirming cells *(b+c)*, or a comparison of conditional probabilities (Allan & Jenkins, 1983; Beyth-Marom, 1982).

Let us now look at a table for the Chernobyl accident, Table 3:

Table 3. Causal attribution structure subsequent to the Chernobyl accident.

	Illness	Health
After Chernobyl	a	b
Before Chernobyl	c	d

The difficulties here are much greater than in the ordinary case depicted in the previous table. We cannot replicate the values *c* and *d* any more, we have to rely on a rapidly fading and more and more distorted memory of times before 1986. Therefore, if people indeed function according to Smedslund's hypothesis the existence of poor public health alone is sufficient for a perceived causal link to a salient threatening event. If Cheng and Novick are right, a tendency to remember things as better than they actually were contributes to the illusion. Since everybody is now 6 years older than at the time of the accident and therefore on the average somewhat more likely to be ill (and many of those very ill before 1986 are dead by now) this factor alone could explain the perception of a declining health standard perceived as being caused by the accident.

Russian media openness about illnesses which was presumably less pronounced before Glasnost should contribute strongly to these tendencies. In studies from Hungary and the Soviet Union carried out before Glasnost it was found that those people rated risk levels as quite low. Lowest, indeed, were risk ratings made by a group of Soviet scientists, see Table 4.

Table 4. Mean risk ratings of 51 hazards in 6 countries (Gullström Nyland, 1992). Scale 0-100.

Country	Mean	SD
Brazil	46.6	20.5
USA	41.4	16.9
Norway	27.5	15.0
Sweden	27.2	16.7
Hungary	23.4	13.1
Russia	17.3	11.6

These findings deserve more discussion than I can devote to them here. It is likely that media exposition of risks is an important factor in explaining why Brazilians and US citizens gave such high risk ratings. However, Swedish media certainly do not suppress risk information. Here, the fact that Sweden is a safer society in most respects than Brazil, and safer than the USA in many respects, may be important. In other words, real risks and media attention may both be necessary for a high level of risk perception. In the period studied it seems that the Hungarian media mostly reported accidents and risks abroad, and perhaps the same was true in the Soviet Union before Glasnost.

Science is superior to everyday life knowledge in two basic respects: it builds upon empirical evidence and it organizes such evidence in cumulative theoretical structures, which are subjected to continuous testing. But there are no clear boundaries between science and everyday life knowledge, and the latter is clearly superior to other forms of beliefs, such as paranoic delusions, phobias and magic, see Table 5.

Table 5. Various knowledge modes characterized by degree of theoretical elaboration and empirical basis.

		Theoretical elaboration		
		Low	Medium	High
Empirical basis	Low	Phobia		Paranoic delusions
	Medium	Magic	Everyday knowledge	
	High			Science

Phobia is a reaction almost totally devoid of any rational basis and it is recognized as such by the phobic him- or herself. Paranoic delusions have a similar rigidity but they are believed by the paranoic and they lend themselves to endless cognitive elaborations. Everyday knowledge can lead astray but it is connected with reality in a manner not common to phobic or paranoic notions. Therefore, one should not quickly dismiss public notions phobic reactions. They may be incorrect because they are based on the insufficient evidence that we always have to base our common-sense beliefs on, as soon as we are not dealing with phenomena in a scientific manner.

Rumors and superstition

The previous section involved seeing risk perception as a cognitive phenomenon. This is a fruitful approach, but not the only possible one. There are also useful notions from social psychology.

Kapferer (1989) has described a case of rumors concerning the alleged carcinogenic effects of certain food additives and well-known brands such as Coca-Cola. The rumors started in Paris when somebody, whose identity has remained unknown, distributed leaflets making statements about food additives and well-known brands asserting that researchers at a Paris hospital had found that they had carcinogenic effects. The rumor spread quickly and gained considerable credibility, *in spite of being completely false.* Kapferer interviewed a group of 39 MD's and found that the majority of them believed the rumors to be true and that only 8 had bothered to check with the hospital about the asserted research findings. In an experiment the leaflet was distributed and data were then collected about the actions people had taken. Twenty percent brought it with them while shopping, 12% posted it in their homes and 40(!)% spoke to others about it.

Why has a rumor such as this such a strong effect? Research on rumors has shown that they develop when an issue is important and ambiguous (Allport & Postman, 1947; Shibutani, 1986). Surely, food additives is an issue which is hard for the non-expert to understand (Sapolsky, 1986) and therefore they have to rely on experts. Yet, they rely even more on *non-establishment* experts. Kapferer writes:

> "It is somewhat perplexing to see how, once planted, despite official denials, false information circulates and gains such a high degree of popular acceptance". (p. 481).

It is interesting to note that the false information was accepted not only by the public but also by many physicians who did not bother to check its sources. Maybe some of its credibility was due to the use of code labels which were perhaps seen as time consuming to check upon. And the code names were unfamiliar, thus maybe leading to larger risk estimates.

Why should that be so? Perhaps the reason is that experts judge *familiar* risks to be less than non-familiar ones. This is, at least, a trend that has been found in the public. Experts grow to be familiar with a risk, e.g. while they work in power plants. It is natural that the everyday surroundings come to be perceived as safe. People who live very close to a nuclear power plant judge it to be safer than those who live a little longer away from it.

The striking effects of familiarity and understanding on perceived risk can be seen in this quotation from a nuclear weapons scientist:

> "When you're a physicist and you know how a nuclear weapon works and what all the parts are, it's no more strange than a vacuum cleaner...You don't fear it at all. I understand that to people who don't work on them, nuclear weapons are an alien thing. I felt the same way before I went to the lab. I feel very bad that people are afraid. I feel bad for them." (Gusterson, 1992, p. 22).

On the other hand, it has been found that experts judge risks to be larger when the toxic agent is presented with its name, than when only its critical properties were specified (Carlo et al., 1992).

Magic and superstition is by no means absent from the contemporary scene, in spite of the impressive progress by modern science. Many people have beliefs in the supernatural, in parapsychology etc. Witch hunts are often mentioned as a supreme example of the follies of public opinion, but they were different from current superstitions in several aspects. They concerned religious beliefs that were held by elites as well as the public, and witch trials were carried out and encouraged by the church and the authorities, who only reluctantly gave up on them - perhaps partly because they and their own families started to be among the accused[1]. Witches were probably perceived as enemies of the establishment, and certainly not members of it, and the witch risk should probably be counted among the risks which provokes the hierarchic rather than the egalitarian cultural orientation, to use the system of values suggested by Douglas and Wildavsky (1982). It is notable that the courts even allowed such - even then - illegal practices of torture and child testimony in the witch trials (Lindroth, 1975).

Improving credibility

Some problems of risk communication are so difficult that they may turn out to be impossible to solve. Consider the example of nuclear waste long-term disposal in the USA. Plans to site a deposit at Yucca Mountain, Nevada, have encountered fierce resistance among the people of that state. Local politicians find it very hard to be elected unless they oppose the plans formulated by the Federal Authorities. Studies of risk perception performed by Slovic and co-workers (Slovic, Flynn & Layman, 1991) have shown that most people perceive that the deposit will have only drawbacks and no advantages, and that they deeply distrust experts, administrators and politicians who work in favor of the project. Slovic et al. concluded that there is little or no hope to persuade these people that the message of the Federal Agencies is valid.

Kuhnreuter and Easterling (1990) also argued that there is a breakdown of confidence in the authorities when it comes to nuclear waste. They wrote (p. 256):

> "...residents will be confident that the risks are low only if they trust the agencies responsible for constructing and operating the facility; standard risk assessments proffered by experts will not be sufficient.
>
> ...although these studies [of the proposed deposit in Yucca Mountain] had taken two years and cost $500 million, scientific disputes regarding the integrity of the data had fueled effective public opposition to the Yucca Mountain site..."

The siting decision is structurally different from the continuous administration of existing power plants. The plants *have* already been located. They may of course be shut down but such a decision is clearly more economically painful to the local population that not siting a facility there in the first place. However, if people do not perceive, and cannot be persuaded by experts and the government, that plants are safe, there will be continued opposition and social and political turbulence.

Bord and O'Connor (1990) reported very similar findings for radiated food. Confidence in industry was a key factor in attitudes to this type of food preservation. Lack of confidence was characterized by them as follows:

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[1]. In Sweden, witch hunts were only prominent during the decade of the 1670's (Lindroth, 1975) and were officially terminated in 1677. About 300 witches had by then been convicted and executed, by no means all from the lowest social strata. In the final phase, even the wife of a prominent priest (the Vicar) in Gävle was accused and sentenced to death. A lawyer and a physician were prominent in the opposition to the witch trials and pointed, in particular, to the unreliability of children's testimony, which was exposed as fraudulent in some salient cases. Some professors of theology also protested against the witchcraft commissions. Yet, witchcraft was outlawed in Sweden for another 100 years following 1677, and witch trials are said to have taken place even in the end of the 18th century.

"...the respondents' view that complex technology bears a burden of too much uncertainty, too much greed on the part of its sponsors, and too little effective government control. The point was frequently made that even if the scientific-technical plan were flawless, the people executing the plan and managing the technology would inevitably create serious problems". (p. 505).

A suggestion

Let me finish by making a concrete suggestion as to how credibility could be achieved in a society with a low level of trust in experts, authorities and media. The suggestion is based on the following premises (cp. Shalpentokh, 1985):

(1). People deeply distrust the government, industry and experts employed by the government and industry.

(2). University or Academy researchers are not trusted much more, because there is a lack of a tradition of politically independent institutions of higher learning.

(3). External experts, from other countries, may have a higher level of trust but when and if they join the local government in their evaluation of risks they will lose much of their credibility.

(4). There is only low trust in the media[1].

These assumptions are largely supported by data collected in the summer of 1992 by Britt-Marie Drottz-Sjöberg in Novozybkov. In Fig. 8, mean ratings of trust in various sources regarding the Chernobyl accident are given (based on all 185 subjects which participated). It can be seen that all information sources are trusted rather little, but that especially national and local political bodies are mistrusted. Most trust is exhibited in foreign sources, especially foreign experts.

[1]. These assumptions are of course not true in general. E.g., we have obtained data showing considerable trust in media (Sjöberg 1991 a).

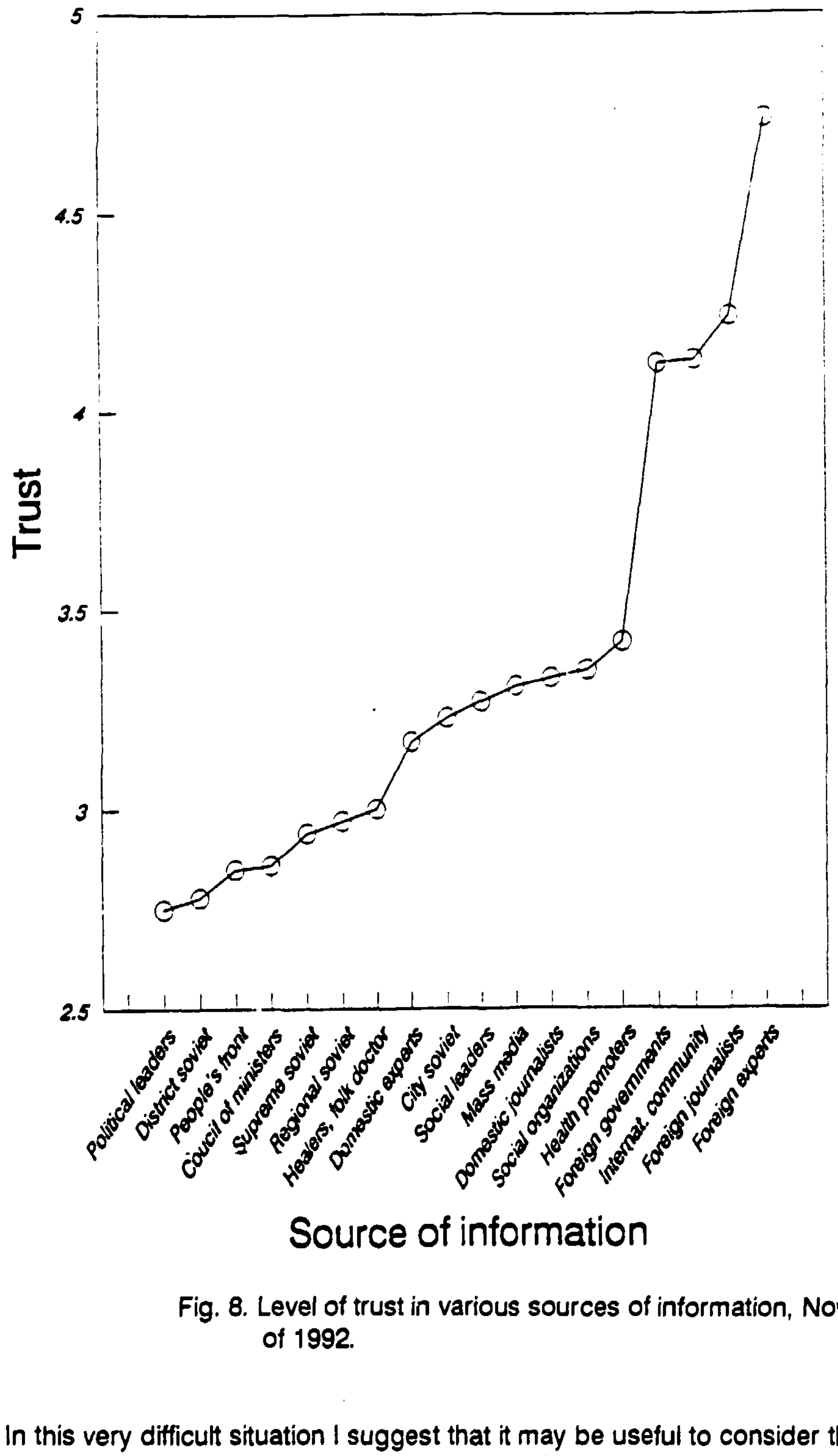

Fig. 8. Level of trust in various sources of information, Novozybkov, summer of 1992.

In this very difficult situation I suggest that it may be useful to consider the traditional office of *ombudsman*. An ombudsman is a representative of the people and should act in the interest of the people, and in no other capacity. An office of **nuclear ombudsman** could be made responsible for nuclear safety and authorized to inspect power plants and other facilities, to order improved safety programs and, in extreme cases of acute necessity, to order the shutdown, permanent or temporary, of nuclear power plants and other facilities. The person in charge of this office, the ombudsman or ombudswoman, should not be appointed by the government, because then we have the credibility problem back in full swing. He or she should be elected directly by the people and be responsible only to them, not to the government or to industry.

References

Abelson, P. H. (1990). Incorporation of new science into risk assessment. *Science, 250,* 1497.

Allan, L. G., & Jenkins, H. M. (1983). The effect of representations of binary variables on judgment of influence. *Learning and Motivation, 14,* 381-405.

Allport, G., & Postman, L. (1947). *The psychology of rumor.* New York: Holt.

Bar-Hillel, M., & Wagenaar, W. A. (1991). Perception of randomness. *Advances in Applied Mathe matics, 12,* 428-454.

Barny, M.-H., Brenot, J., Dos Santos, J., & Pages, J.-P. (1990). Perception des risques majeurs dans la population bordelaise et chez les experts. Note SEGP/LSEES 90/17. Paris: Centre d'Etudes Nucléaires de Fontenay-aux-Roses.

Bengtsson, G. (1992). Vad får skydd mot strålning och andra risker kosta? SSI-rapport 92-10. Stockholm: Statens strålskydssinstitut.

Beyth-Marom, R. (1982). Perception of correlation reexamined. *Memory & Cognition, 10,* 511-519.

Bolt, B. A. (1991). Balance of risks and benefits in preparation for earthquakes. *Science, 251,* 169-174.

Bord, R. J., & O'Connor, R. E. (1990). Risk communication, knowledge, and attitudes: Explaining reactions to a technology percieved as risky. *Risk Analysis, 10,* 499-506.

Carlo, G. L., Lee, N. L., Sund, K. G., & Pettygrove, S. D. (1992). The interplay of science, values, and experiences among scientists asked to evaluate the hazards of dioxin, radon, and environmental tobacco smoke. *Risk Analysis, 12,* 37-43.

Cheng, P. W., & Novick, L. (1992). Covariation in natural causal induction. *Psychological Review, 99,* 365-382.

Covello, V. T., Sandman, P. M., & Slovic, P. (1988). *Risk communication, risk statistics and risk comparisons: A manual for plant managers.* Washington, DC: Chemical Manufacturers Association.

Douglas, M., & Wildawsky, A. (1982). *Risk and culture.* Berkely, CA: University of California Press.

Drottz-Sjöberg, B.-M., & Persson, L. (1992). Fear, anxiety and phobia of radiation. Submitted for publication.

Drottz-Sjöberg, B.-M., & Sjöberg, L. (1990 a). High speed trains and the perception of risk. Working Paper, European Institute for Advanced Studies in management, Bryssel and *RHIZIKON: Risk Research Reports, No. 2.* Stockholm: Center for Risk Research, Stockholm School of Economics.

Drottz-Sjöberg, B.-M., & Sjöberg, L. (1990 b). Risk perception and worries after the Chernobyl accident. *Journal of Environmental Psychology, 10, 135-149.*

Drottz-Sjöberg, B.-M., & Sjöberg, L. (1991). Attitudes and conceptions of adolescents with regard to nuclear power and radioactive wastes. *Journal of Applied Social Psychology, 21,* 2007-2035.

Eysenck, H. J. (1985). *The decline and fall of the Freudian empire.* Hammondsworth: Penguin Books.

Fischhoff, B., Slovic, P., & Lichtenstein, S. (1978). How safe is safe enough? A psychometric study of attitudes towards technological risks and benefits. *Policy Sciences, 9,* 127-152.

Gardner, G. T., & Gould, L. C. (1989). Public perceptions of the risk and benefits of technology. *Risk Analysis, 9,* 225-242.

Gerholm, T. R. (1992). Diskussionsinlägg. In *Makt, media och miljö. Redovisning av seminarium den 9 april 1991 anordnat av Kungl Skogs- och Lantbruksakademin, pp. 109.* Rapport Nr 59. Stockholm: Kungl. Skogs- och Lantbruksakademin.

Grünbaum, A. (1984). *The foundations of psychoanalysis. A philosophical critique.* Berkeley: University of California Press.

Gullström Nyland, L. (1992). Risk perception in Sweden and Brazil. In preparation.

Gusterson, H. (1992). Coming of age in a weapons lab. Culture, tradition and change in the house of the bomb. *The Sciences, May/June,* 16-22.

Hammitt, J. K. (1990). Risk perceptions and food choice: An exploratory analysis of organic- versus conventional produce-buyers. *Risk Analysis, 10,* 367-374.

Kapferer, J. N. (1989). A mass poisoning rumor in Europe. *Public Opinion Quarterly, 53,* 467-481.

Kuhnreuter, H., & Easterling, D. (1990). Are risk-benefit tradeoffs possible in siting hazardous facilities? *American Economic Review, 80,* 252-261.

Kuhnreuter, H., Easterling, D., Desvousges, W., & Slovic, P. (1990). Public attitude toward siting a high-level nuclear waste repository in Nevada. *Risk Analysis, 10,* 469-484.

Lindroth, S. (1975). *Svensk lärdomshistora. Stormaktstiden.* Stockholm: Norstedts.

Michotte, A. Ed. (1954). *La perception de la causalité.* Louvain: Publications Universitaires de Louvain.

Miller, T. R. (1989). Willingness to pay comes of age - will the system survive? *North Western University Law Review, 83,* 876-907.

Otway, H., & von Winterfeldt, D. (1992). Expert judgment in risk analysis and management: Process, context and pitfalls. *Risk Analysis, 12,* 83-93.

Rasmussen, N. (1991). Rasmussen summarizes "The challenges that remain". *RISKnewsletter, 11*(1), 8.

Roberts, L. (1990). Counting on science at EPA. *Science, 249,* 616-618.

Roth, E., Morgan, M. G., Fischhoff, B., Lave, L., & Bostrom, A. (1990). What do we know about making risk comparisons? *Risk Analysis, 10,* 375-387.

Salter, L. (1988). *Mandated science: Science and scientists in the making of standards.* Boston: Kluwer.

Sapolsky, H. M. (1986). *Consuming fears.* New York: Basic Books.

Shalpentokh, V. E. (1985). Two levels of public opinion: The Soviet case. *Public Opinion Quarterly, 49,* 443-459.

Shibutani, T. (1986). *Improvised news.* New York: Bobbs Merrill.

Sjöberg, L. (1980). The risks of risk analysis. *Acta Psychologica, 45,* 301-321.

Sjöberg, L. (1991 a). Aids: riskuppfattning, attityder och kunskaper. *RHIZIKON*: Rapport från Centrum för Riskforskning, Nr. *1.* Stockholm: Centrum för Riskforskning, Handelshögskolan i Stockholm.

Sjöberg, L. (1991 b). Alkoholens risker, upplevda och verkliga. *Nordisk Alkoholtidskrift, 8,* 253-267.

Sjöberg, L. (1991 c). Risk perception by experts and the public. *RHIZIKON: Risk Research Reports, No. 4.* Stockholm: Center for Risk Research, Stockholm School of Economics.

Sjöberg, L. (1992 a). Psychological reactions to a nuclear accident. In J. Baarli (Ed.), *Proceedings of a Conference on The radiological and radiation protection problems in Nordic regions, Tromsö 21-22 november, 1991.* Oslo: Nordic Society for Radiation Protection.

Sjöberg, L. (1992 b). Risk level and risk tolerance of everyday and potentially fatal risks. In preparation.

Sjöberg, L. (Ed.) (1987). *Risk and society. Studies in risk taking and risk generation.* Hemel Hempstead, England: George Allen and Unwin.

Sjöberg, L., & Drottz, B.-M. (1987). Psychological reactions to cancer risks after the Chernobyl accident. *Medical Oncology and Tumor Pharmacotherapy, 4,* 259-271.

Sjöberg, L., & Drottz, B.-M. (1988). Attityder till radioaktivt avfall. SKN Rapport 23. Stockholm: Statens kärnbränslenämnd.

Sjöberg, L., & Winroth, E. (1986). Risk, moral value of actions, and mood. *Scandinavian Journal of Psychology, 27,* 191-208.

Slovic, P., Flynn, J. H., & Layman, M. (1991). Perceived risk, trust, and the politics of nuclear waste. *Science, 254,* 1603-1607.

Slovic, P., Kraus, N. N., Lappe, H., Letzel, H., & Malmfors, T. (1989). Risk perception of prescription drugs: Report on a survey in Sweden. In B. Horisberger & R. Dinkel (Eds.), *The perception and management of drug safety risks* pp. 90-111. Berlin: Springer-Verlag.

Smedslund, J. (1963). The concept of correlation in adults. *Scandinavian Journal of Psychology, 4,* 165-173.

U.S. Environmental Protection Agency (1987). *Unfinished business: A comparative assessment of environmental problems.* Washington, D. C.: U.S. Environmental Protection Agency.

Weinstein, N. E. (1984). Why it won't happen to me: Perception of risk factors and susceptibility. *Health Psychology, 3,* 431-457.

Wold. H. (1981). Model construction and evaluation when theoretical knowledge is scarce: Theory and application of Partial Least Squares. In J. Kmenta & J. Ramsey (Eds.), *Evaluation of econometric models.* New York: Academic Press.

Wold, H. (1985). Systems analysis by Partial Least Squares. In P. Nijkamp el al. (Eds.), *Measuring the unmeasurable* pp. 251-287. Dordrecht: Martinus Nijhoff Publishers.

Wyler, A. R., Masuda, M., & Holmes, T. H. (1968). Seriousness of illness rating scale. *Journal of Psychosomatic Research, 11,* 363-374.