

Comment

Should Social Amplification of Risk Be Counteracted?¹

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The importance of the conceptual statement, by Roger Kasperson *et al.*, on social amplification of risk lies, firstly, in the identification of a phenomenon as one worth studying, instead of being irritated and frustrated about it and concerned only to get it out of the way. Accusations of “public hysteria” and “irresponsible media” are commonplace, without any real attempt at understanding causes and mechanisms, let alone a closer look at the normative qualifications of “hysteria” and “irresponsibility”.³ Kasperson *et al.* provide a forceful summing up of the limitations of traditional, technical risk analysis, and propose to overcome the limitations by adding the phenomenon of public reactions to risk and further repercussions (“secondary impacts”). One may wonder whether this is sufficient; but it clearly is necessary.

Secondly, the attempt at systematic description usefully articulates a number of dimensions and aspects of the problem. The added benefit is that, in doing so, some of the ambiguities become apparent—of the proposed analysis, but also of the way we tend to treat the phenomena of social amplification of risk. For example, although the phenomenon is defined in a neutral way, in the introduction and later when communications theory is invoked (“amplification denotes the process of intensifying or attenuating signals during the transmission of infor-

mation”), the focus as well as the concern is about intensification and the additional social costs accompanying “exaggerated” responses. The aftermath of Three Mile Island is given as a paradigmatic example of social amplification, while there is no complementary example of the social costs of attenuation of risk (as, say, in the thalidomide affair or in some aspects of the swine flu vaccine affair). To emphasize this particular ambiguity, I ask in my title if it is self-evident that social amplification (in the sense of exaggeration, as all readers, and the authors sometimes as well, will take it) must always be counteracted.

There are other limitations and ambiguities to be noted, and I shall highlight them in my comment, because I think the paper is right in addressing the issues, and deserves constructive criticism more than praise. In other words, I applaud the paper (both its content and the fact that it appears) as far as it goes, but it does not go far enough. I want to indicate some possibilities for further progress.

The starting point of the paper is that conventional risk analysis neglects the important domain of *responses* to risk events and their *repercussions*, the social impacts for short. Therefore, the arsenal of risk assessment tools must be enlarged. One has to identify and characterize the phenomena in this domain and add them, somehow, to traditional risk analysis. The authors choose to do this by adding a new event-consequences-effects chain to the one that is central to the traditional problem definition of (technical) risk analysis: “The social structure and processes of risk experience, the resulting repercussions on individual and group perceptions, and the effects of these responses on community, society, and economy.” (In addition, the authors seem to distinguish two stages in the consequences step, when they discuss “response mechanisms.”)

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³See for examples the Report of the Twentieth Century Fund Task Force on the Communication of Scientific Risk.⁽¹⁾ The irony of this publication is that the Task Force accuses the media of irresponsibility, in spite of the findings in the background paper, by Dorothy Nelkin, which is included with their report.

As a tactic, this approach may well be heuristically useful.⁴ A full assessment, however, is not easy, because in the present paper, the third step, effects and their evaluation, is not discussed. The three-step model is in fact reduced to a two-step model: risk events or risk experiences, and their consequences in terms of responses. The paper actually stops rather suddenly when it briefly indicates that there will be “next steps.”

Analysis of effects is indeed complicated, and one might be well advised to relegate them to further work. But one should at least discuss some of the issues, like feedbacks (which are mentioned), and the question of when attenuation of the signal is good (reduction of unnecessary social costs) and when bad [a warning signal does not get sufficient (political) weight]. Without due attention to “effects,” the approach is too limited (and would require more sociology and political science inputs) and may be misunderstood as just a tool to handle those exaggerated, irrational fears that people turn out to have. This is certainly not the intention of the authors,⁵ but it is difficult to avoid such a message. I shall come back to this issue when I comment on each of the three steps of the full model in turn.

⁴Other tactics can be thought of, for instance case study analysis and social experiments without any prestructured and stepwise model. Or one could, as I will argue later, start with a sociological model instead of communications-theory and psychological model. One could also, instead of adding to the arsenal, take the very different route of fundamental criticism of the possibilities and practical usefulness of risk analysis. That would require an improbable degree of reflexivity and relativization of risk analysts.

⁵This is clear from passing remarks in the paper, and is also emphasized by Paul Slovic in the closing paragraph of his paper “Perception of Risk”⁽²⁾: COPY MISSING

Perhaps the most important message from this research is that there is wisdom as well as error in public attitudes and perceptions. Lay people sometimes lack certain information about hazards. However, their basic conceptualization of risk is much richer than that of the experts and reflects legitimate concerns that are typically omitted from expert risk assessments. As a result, risk communication and risk management efforts are destined to fail unless they are structured as a two-way process. Each side, expert and public, has something valid to contribute. Each side must respect the insights and intelligence of the other.

My proposal below to look at reception of risk signals in terms of reinvention is an example of a two-way conceptualization.

To start with the first step, what is the “risk experience”? The metaphor of the stone in the pond, with the ripples spreading out, is a powerful one, and is used to good effect. But one may gloss over some important issues this way. What *is* the stone? In the paper, a variety of terms are used: “risk object,” “hazard event,” “risk event,” “risk source,” “physical risk itself.” For communications theory to apply, however, one must have a “signal about risk” (as the abstract says), and it is not clear whether an “event” or an “object” as such can be a sender. I am not quibbling about words, but making explicit an ambiguity. A sentence like “risk sources create a complex network of direct and indirect effects that are susceptible to change through social responses” now hides rather than highlights, because the ambiguity is not resolved. Is there an event with impacts, independent of perceptions and social responses, while the (social) impacts, i.e., the social costs, can be increased or reduced through these responses? Or does one start with perceptions, recognitions, responses, and their repercussions?

This is not idle speculation. There are examples of concerns about risk, responses, and social amplification, without a risk event: the Seattle wind screen pitting, the Christmas scares about gas explosions.⁶ But I assume that in those cases there *is* a risk signal that can be seen as the start, even if it is vacuous, or based on misunderstanding of another signal.

The implications of this point are, first, that it is not, at least not primarily, the nature of the “specific risk” or the “event characteristics” that determine the process of social amplification, as the paper says, e.g., in Fig. 2, but the nature of the signal. In other places, the notion of “clues” is introduced to indicate “signal potential” (cf. Table 1 in their paper), which seems a much more fruitful notion.

Second, given the concern with unnecessarily high social costs because of social amplification, it is more important to understand why people do not *check the quality* (or substance) of the signals they receive, than to understand how they transform and transmit whatever they receive. In other words, the problem is not so much individual bias, but social interaction. People—including experts and risk assessors—are biased, but what we hope is that in

⁶See my paper in Ref. 3. The Christmas scares example was mentioned to me in discussion by David Edge (Edinburgh).

spite of bias, something acceptable is produced out of the interactions.⁷

The neglect of the sociological returns in the second step of the model, where the process of social amplification is conceptualized. Although the concept features the term “social,” the focus of the paper is on the individual. There is discussion of social alignments, of networks, of mass media, but the central mechanism is seen as the reception of information by an individual, its processing and further transmittance. This conceptualization is assumed as self-evident, and indeed allows straightforward application of communications theory.⁸ The further assumption, however, is that the processes of social aggregation and the transformations that occur there need not be studied as such, but can be captured through factors (like cultural bias or competing interests) that impinge on the information processing. This is the way cultural residues of experience with hazardous events and risks are treated in the paper, but there is no indication *how* such cultural residues might emerge and sediment.

A possibility to address social aggregation within the conceptualization used in the paper is to analyze the maintenance of socially defined objects, like “dread risks,” by modeling individuals as label-attachment devices. Barnes has used this heuristic to discuss phenomena of routine concept application, public confidence in banks, and self-fulfilling prophecies.⁽⁴⁾ This approach, though still rudimentary, seems to be relevant for the phenomena in social amplification of risk as well. It may well be necessary, however, to introduce more “active” models of the individual, and, by that, leave the simplistic focus on signals as information and social amplification as information processing. (To avoid misunderstanding, I add that the authors do treat social aspects and processes, but they seem to separate them from the primary information processing. For example, when they discuss the “chief attributes that may influence the social amplification of risk,” these are all attributes of the information, like volume, degree of disputation, misinformation, and symbolic connota-

tions. The arena in which the processing occurs, and the strategies of the different actors are discussed in a later section, under the heading “social group relationships.”)

An interesting alternative possibility is to look at the phenomena in terms of *adoption* of signals, and use the literature on adoption and diffusion processes. Studies of adoption and diffusion of innovations, done by economists and policy analysts, now recognize adopters to be active, and emphasize that the process of adoption is not a matter of accepting or rejecting a given innovation, but modifying and transforming, in a sense, reinventing it.⁹ Such adoption and reinvention processes are treated as carried by an organization, group, or milieu, rather than being primarily an individualistic, atomistic process. One might look at the “hazard signal” as something that can be adopted and transformed by a *group*, and study the processes that occur at that level. If one does this, one might also be able to address the second aspect implied by “activist” models: often, a hazard signal is not just information-to-be-processed, but includes a (subculturally determined) action precept, or just a general call for action without prescribing any yet.¹⁰ Thus, the signal requires some “invention” of the people involved. Even when they fall back on general cultural categories, as Douglas and Wildavsky⁽⁸⁾ and other cultural-bias theorists posit,¹¹ they still have to creatively relate the concrete situation to those categories.

In many cases, one could usefully introduce other perspectives, especially those drawn from social psychology and symbolic interactionism. I think, for example, of Weick’s attempt to analyze responses and interactions in terms of “equivocation.”⁽¹⁰⁾ Ambiguous cues or items have to be clarified (“organized”), for instance, by placing them into a context; Weick includes an example of the Swiss watch makers and their reaction to the ambiguous cue “digital”—which evoked, at first, a response of attenuation of the signal (although they now have come back with a vengeance). An important point is that symbolically guided actions or “enactment” may become stabilized when they seem to help address

⁷Compare the idea that the quality of scientific knowledge results from the critical review processes in the scientific community rather than the craft skill, methodological purity, and ratiocinations of individuals.

⁸The references to communications theory are, to my surprise, rather old (1948, 1966, 1969). Has nothing happened since?

⁹See the review by Irwin Feller,⁽⁵⁾ and especially Refs. 6 and 7.

¹⁰This is to be contrasted with the sequence in the paper, where information processing is taken to generate a propensity to act.

¹¹See also Ref. 9.

relevant circumstances. Weick discusses organizations, but the following quote seems just as applicable to public handling of hazard signals:

Whatever people do during enactment, whether it be operating without goals, misplacing personnel, operating a technology that no one understands, improvising instead of forecasting, dwelling on opportunities, inventing solutions rather than borrowing them, cultivating impermanence, arguing, or doubting, if those "strange" actions promote rapid adaptation to shifting conditions, they're likely to persist, be enacted repeatedly, and to be frequent inputs in the selection process (Ref. 10, p. 185).

When discussing effects (the third step of the model), analytic progress is hindered by the ambiguity in the use of "amplification" that I noted already in the introduction. Communication theory may use the term to denote both intensification and attenuation of signals, but the reader will not always remember that, and the authors follow him (or her) by sometimes contrasting amplification and attenuation. Apart from readers' associations with the term "amplification," it is also the authors' concern that risks loom unnecessarily large because of amplification processes. Such an evaluation is implicit in many phrases, for example in equating "erroneous" with "exaggerated." To show up implicit evaluations, one should try a little experiment, and replace "amplification" by "attenuation" everywhere in the text, to see how the argument runs then.¹²

The problem with the terminology has also to do with the audience that the authors want to reach. It seems to be the audience of policy makers and risk analysts that experience exaggeration of risk, or what they see as exaggeration, and who do not understand the underlying processes. So they reaffirm technical risk assessment in spite of its limitations, and specifically underestimate the variety of adverse effects attendant on risk events. The authors of the paper hope to broaden their understanding—which implies that one has to go along with *their* perceptions, and put the problem in their terms. I have no quarrel

¹²The same tactic is used to show gender bias in texts. It is indeed cumbersome to continually talk of "he or she," or write "s/he." But a text written with plain "he" and other masculine words can have a dramatically different impact when all masculine words are substituted by feminine words. What about the famous (early 16th century) book written by Machiavelli, titled "The Princess"? (I owe this example to Sharon Traweck.) In the same way, a paper on "Social Attenuation of Risk" will suggest a different argument, and enroll a different audience.

with that, but it may reaffirm another bias of policy makers, viz., that their responsibility is primarily to avoid those social costs stemming from exaggerated responses to risk. There is another duty of public officers and all those working for the public interest: to avoid social costs due to suppression (or other forms of attenuation) of risk signals. There is not only specious inflation of risk signals, but also specious reduction.¹³

Thus, there is another side to the *problematique*, and one way to explore it is by looking at early warnings and their filtering. The paradigmatic example here is Rachel Carson's 1962 book *Silent Spring*. One problem in understanding early warning processes is that only the tips of the iceberg become visible, for example in whistle-blowing and so-called professional dissent.¹⁴ But there is increasing attention to this "other side" of the issue, for example, in new approaches to technology assessment in Europe, which include institutionalization of early-warning networks.¹⁵ Designing what one could call "interventions in the social attenuation of risks" occurs often without full understanding of the processes involved, but experience with them will certainly be informative. Another potentially very fruitful approach starts from the ways people handle technology: directly, in the routines that evolve—think of Morton-Thiokol's handling of the checks of the space shuttle,¹⁶ and indirectly, through the strategic games in which technological development and usage are framed.¹⁷ In organization studies, the phenomenon of attenuation of signals has of course been studied (although not as extensively as one would wish). From the cultural bias theorists, there is now also an attempt to look at rejection of information as the primary process.⁽¹⁹⁾ These brief references show that there is sufficient

¹³See Refs. 11 and 12. The terminology of specious inflation and reduction is adapted from Ref. 13.

¹⁴See Ref. 14 and the very interesting case study in Ref. 15.

¹⁵In West Germany there are experiments with *Frühwarnungsnetze*, and in the Netherlands, the Organization for Technology Assessment is creating a "societal address" that receives concerns and warning about technology. See Ref. 16 for some details.

¹⁶For a full discussion of the example and the general approach, see Ref. 17. Note that the approach differs from so-called "human factors analysis," in that processes at the group level are taken as constitutive.

¹⁷For a discussion of this approach in general, although without application to issues of risk attenuation, see Ref. 18.

interest and material to start analyzing the "other side" in earnest.

In conclusion, I think that the conceptual statement has much to recommend it, both in contents and in the way it attempts to identify issues for study. Further work is necessary, not only in the directions the authors set out, but also, as I tried to show, by the inclusion of more sociological analysis, and by a more explicit and symmetrical treatment of intensification and attenuation, or of inflation and reduction. I do not know whether it is possible to overcome the bias of sequential analysis, where there is a source and there are consequences, with the attendant assumption that influences "on the way" will be distortions of the original signal. One thing should stand out: the phenomena of social amplification of risk are so important that we must mobilize all our intellectual resources to achieve some understanding.

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