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Probability does not exist: some considerations on seismic risk mitigation

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ABSTRACT The aim of this paper is to develop some considerations on seismic risk mitigation, with particular reference to: 1) the role of deterministic and probabilistic methods and the importance of their integration in hazard assessment; 2) the problem of the comparison between alternative probabilistic models, stressing the importance of paying more attention to the believability of the models used than to the validation of the results. A possible procedure to check the believability of the model is suggested; 3) the decision process when dealing with events characterized by low probability of occurrence and by catastrophic consequences. It is important to stress the need for a clear distinction among the three components of the process: the modelling (risk evaluation with the related uncertainties), the decision procedure (comparison of risk levels, uncertainties, aims, costs, and benefits connected with possible alternative decisions), and finally the decision aimed at risk mitigation; 4) clear explanation of the decision procedure, with the modelling containing parameters (or quantities) that can support the decision. They are referred to here as decision parameters. The risk mitigation procedure should imply that all steps of the decision process must be clearly explained to all persons involved, including the population.

Key words: seismic hazard, probability, decision procedure.

1. Can a probabilistic model be a forecasting tool?

"Probability does not exist" is the provocative motto that de Finetti (1974) posited in the English edition of his "Theory of Probability".

What did the founder of the subjective interpretation of probability intend? Bruno de Finetti, one of the greatest mathematicians of the last century, defined probability as "a guide in thinking and acting" (de Finetti, 1965), calling for more acknowledgment and use of probabilistic reasoning in the fields of science, technology, and culture.

What he meant is that probability does not exist as an entity whose objective value has to be calculated, perhaps even with the absurd concern of high precision! He recommended, instead, the subjective interpretation "to dispel secular pseudo-problems". However, he also cautioned against running into a "rough misunderstanding: with regards to the idea that in order to follow the subjectivist point of view, one has to forget the usual considerations taking into account all the known objective circumstances, in particular symmetries that induce equiprobability judgement and observations of frequencies that lead one to predict a certain stability, although thoroughly

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and responsibly examining them, rather than reducing them to rudimentary and presumptuous recipes”[1].

Unfortunately, analyses paying too little attention to de Finetti’s recommendations are sometimes elected to define seismic code.

A probabilistic model does not involve the calculation of a precise number that measures the involved probabilities once and forever. It consists of a continuous process of probability refinement, a process that incorporates all the information progressively available according to the Bayesian analysis and compares different probabilistic models. The analysis does not pursue impossible validations, but allows the judgement of whether one probabilistic model is more credible than another. It can also happen that the preference depends on the forecast object. A probabilistic model provides predictions only in such a relative sense.

In the field of seismic risk reduction, it is crucial, however, to reach a definition of hazard at a site and to provide consistent seismic standards for building safety. And here the opinion of scientists is not uniform even today. How can one handle differences in methods and results?

A few considerations on this intriguing question are presented below.

2. Differences among models and methods

On topics that are particularly complex and not yet treated in established theories, it is quite normal not to get a uniform result. Lakatos and Musgrave (1993) say that “any theory is born in an ocean of anomalies”. This does not mean that we should accept to live with those contradictions, nor that we can prematurely declare one of the adopted procedures to be better than another.

A patient comparison between different adopted procedures will help to increase knowledge, to better interpret results and, possibly, to build new theories and models. Furthermore, it often helps identify convergences, decrease uncertainties, and foster one or another model according to the particular quantity that should be estimated or the decision that should be undertaken.

Surely, it may also happen that different procedures continue to conflict. This should only encourage the continuation of the exploratory effort, with the researcher patient in the comparison between procedures, as well as inexorably severe in the critical control of each procedure.

In this respect, let us remember a historical statement by Rapoport (1962) concerning a not usual but very important paradigm, of *mutual trust*: “At times, we must learn the meaning of the trust. … To convince the other, we must get him to listen to us, and this cannot usually be done if we ourselves do not listen”.

The effort will be rewarded. The conclusions will converge gradually.

Today, however, we are far from convergence, as shown by the hot debates on mutual promotions and rejections, with arguments looking for reassuring (maybe impossible) “validations”, or for debatable slating.

In particular, two mutually opposed factions have formed over time between supporters and opponents of the use of probabilistic models.

[1] “Grossolano equivoco: sull’idea, cioè, che per seguire il punto di vista soggettivista si debba prescindere da considerazioni come quelle usuali tenendo conto di tutte le circostanze oggettive note, in particolare di eventuali simmetrie che inducono a giudizi di equiprobabilità e di osservazioni di frequenze che inducano a prevederne una certa stabilità, vagliandole però approfonditamente e responsabilmente anziché riducendole a rudimentali e presuntuose *ricette*”.

3. Probability and determinism

The provocative title of the conference held by Bruno de Finetti in 1967 is a glimpse into historical quarrels. It is worth quoting the original title: "The adoption of the subjectivist conception as a necessary and sufficient condition to dissipate secular pseudo-problems"[2].

Since then, the disagreements have not subsided, mainly, we believe, for these reasons: 1) on one hand, the users of probabilistic models have often reduced them to uncritically repetitive methods, thereby betraying the stimulus of constant updating proper to the Bayesian approach; 2) on the other hand, the opponents have often made the mistake of rejecting a probabilistic approach on the basis of data fitting, and summarily judging the probabilistic models as lacking in physical knowledge, or even "scientific basis". The probabilistic approach is able to subsume the available physical knowledge. And this is a crucial point, perhaps not widely acknowledged, even if it should be enough to remember Maxwell and Boltzmann in order to appreciate it.

About the Physics-Probability relation, Costantini (2014) presents a very instructive and thorough analysis, which allows him to say: "Probabilistic notions may be linked with ignorance, but sometimes they can also talk about facts and therefore represent reality"[3].

Moreover, inadequate (in our opinion) nomenclature increases the confusion.

What is called Probabilistic Seismic Hazard Assessment (PSHA) is often presented just like the Cornell (1968) model, while many results obtained later (time-dependent models, source and attenuation relationships, magnitude distribution, etc.) could be used nowadays in the frame of a generalized probabilistic Cornell model.

What is called a neo-deterministic model (Peresan *et al.*, 2011) seems free from uncertainty. However, this is not the case, because of incomplete physical knowledge of the phenomenon.

In fact, each definition of seismic hazard, expressed in deterministic or probabilistic form, depends on a complex **procedure** that takes advantage of physical knowledge, historical data, and background beliefs. We have to focus our cautious and critical evaluation on the entirety of the procedure, to operate by pooling results from different and interdisciplinary investigations, connecting them where possible by "and" instead of separating by "or".

Although we must accept that any assessment should be continually reviewed and updated in terms of research, we also have to reach, at any time, accepted hazard values from an operational point of view. We are aware that these are **conventional** values, subjective values, or rather intersubjectively accepted; they are, nevertheless, an indispensable support to decisions.

Therefore, abandoning preeminence claims, we should instead use all available knowledge, including heuristic experiential knowledge, which is often informative, although not expressible in technical terms.

Moreover, in support of this exploratory attitude, a deep epistemological revision process on the knowledge content of scientific theories took place in recent decades, leading to the belief that what does increase the confidence in a given theory is not quite the careful examination of the single theory, but the examination of all the knowledge that, more or less consciously, has been considered during its construction. In this regard the term **background knowledge** has emerged.

[2] "L'adozione della concezione soggettivistica come condizione necessaria e sufficiente per dissipare secolari pseudoproblemi".

[3] "Nozioni probabilistiche possono essere connesse con l'ignoranza, ma talora possono anche parlare di fatti e quindi rappresentare la realtà".

It seems, therefore, appropriate to strengthen the research on forecasting methods (probabilistic and deterministic ones), without generalizations and without assertively favouring one method over another. The two approaches can be complementary, and the decision to assign priority to one of them depends on many factors (McGuire, 2001). Moreover, it is necessary to avoid confusion between criticism of a specific application of a probabilistic model and criticism of PSHA, remembering clearly the distinction between the decisions on safety levels of codes and the PSHA (Wyss *et al.*, 2012).

In this regard, we mention a work undertaken many years ago to face the main uncertainties in the probabilistic field. The fundamental argument can be read in Giuseppe Grandori's 1991 words regarding the estimation, which was under consideration at that time, of the intensity of an earthquake with a return period of 500 years in a given area: "I maintained that the statement that a given earthquake intensity corresponds to a 500-year return period is falsifiable *in principle*. However, to conduct a falsifying experiment in practice would require a period of observation of thousands of years. By contrast, the historical data that are available and statistically significant encompass at best around 300 years. Our proposition is then, *in practice*, not falsifiable. In order to overcome this problem, I propose to shift the attention from the final calculated result, which is not directly falsifiable, to the **procedures** which lead to that result" (Grandori, 1991).

Moreover, we underline that the usual statistical tests used to accept (or, better, not to reject) a model cannot, in general, endorse one model or another, even when the different models lead to significantly different hazard values. In 1988, for instance, Araya and der Kiureghian (1988), describing several magnitude distribution models, observed that "It is often difficult to prove or disprove any of the relations proposed in the literature as they all appear consistent with the available seismicity catalogues".

Thanks to these considerations, the attention shifts backward, **"from the results validation to the model credibility"** (Grandori, 1991; Grandori *et al.*, 2006, 2008), namely, to the procedure by which the model leads to the results.

Trying to choose between competing models, instead of asking, "Which one explains the data better?", we ask, "Which is more reliable for the estimation of a specific quantity in a specific zone?" The issue is not to validate the results, but to construct a test for the model.

Let the model be placed, so to speak, on a test bench, as an interpreter of a conjectural "truth". This conjectural truth, which is supposed to be known, produces, through the model and the Monte Carlo method, a rich set of synthetic samples, each of which is characterized by a large size. So, we no longer have just one available sample, made up of real catalogue with few events, but as many large-sized samples as we want. Then, we can build the sample distribution of that quantity, let us call it *A*, whose uncertainty hassles us; we can calculate the error probability in its estimate, according to the adopted model, assuming, for the moment, that the truth is precisely the one conjectured. This probability is a measure of the *model credibility* in respect to that truth. Its analytical form is

$$\Delta_r^0 = \Pr \{A^0 - kA^0 < \hat{A}_r < A^0 + kA^0\} \qquad (1)$$

where A^0 is the value of *A* in the conjectural truth, $\hat{A}_r$ is the estimator of *A* according to the model *r*, *k* is a significant percentage error threshold. Then, repeat the above procedure with another model, measuring its credibility. Thus, a comparison is established between competing models, which can decide the winner, as long as the truth is the one now taken into account. The problem is that the truth is unknown. So? What do we do? We repeat the process with other conjectural truths, within the reasonable panorama, in which the research is being conducted. This process allows us to build a criterion capable of discerning which model, in what circumstances and for what purposes, is preferable to another. We stress that this survey takes into account both the statistical and the epistemic uncertainties and measures the model's robustness.

A definition of a credibility degree for the model is obtained, not in absolute terms but only in reference to the estimation of a specific quantity, and limited to the extensive but not exhaustive scenario of the explored truths.

If two models are in competition with each other, it is also very interesting to probe the credibility of each one, assuming that the other one is the true model. We report an application of this procedure in section 5, which is also an example of co-operation between a physical hypothesis and the probabilistic approach.

4. Long-, medium-, short-term predictions

Let us go back to the usage, as in the first section, of the term **prediction**, which triggered many unhealthy controversies. In particular, about the often-heard claim "Earthquakes cannot be predicted". In science, the assertion "can" should always be accompanied by specifying the exploratory tools used and the field of investigation in which the statement is asserted. With the knowledge available to date, *we cannot predict the earthquake in deterministic terms*, in the sense of when, where, and with which intensity it will occur. However, all the research is focused just on trying to understand what will happen in the seismic field and to figure it out, possibly with less and less uncertainty, on the basis of the acquired knowledge; in other words, what we try to do is just to predict, even with uncertainty.

So, the proposition "The earthquakes can be predicted probabilistically" has scientific merit.

The harsh debate about prediction mainly results, in our opinion, from there being insufficient attention paid to the decision process. The action aimed at risk reduction is based on the prediction, but not directly implied by it. In any case, particular parameters (or quantities) can be put in evidence from the modelling as possible support for the decision. They can be different for different problems (for example, zonation or seismic building codes). They allow the construction of the scenario of the consequences of the possible alternatives. These parameters will be referred to here as **decision parameters**. Whichever one is chosen by the decision makers will represent an accepted, necessarily **conventional** value.

The prediction acquires different connotations, depending on which reduction-risk problem we are facing. Typically, long-medium-short time problems are the levels of safety for new buildings, with 50 years or more exposure time; the priorities in policies for retrofitting existing buildings, policies that typical may require 10 years; the seismic alert when the probability of an incoming earthquake is increasing, covering a time interval from a few days to some weeks.

4.1. Long term prediction

The prediction is usually done by a Poisson process with a constant value of the hazard rate. The decision parameter can be the expected average annual number of earthquakes of a given magnitude, or, equivalently, their return period, or the expected maximum magnitude.

The meaning of these values can be judged differently. The choice among them and their use go beyond the prediction problem. It is a decision-making process.

4.2. Medium-term prediction

Some issues could be usefully addressed on the basis of time-dependent forecasting, such as the retrofitting of historical buildings. Such retrofitting is so expensive that it requires a temporal articulation of interventions with defined priorities. Time-dependent forecasting provides useful information about the priority criteria to be adopted. Numerous models of this type already exist in the literature. An application reported in section 5 also shows a possible decision parameter, which is applied as a conventional choice to decision-making in governing the retrofitting priorities.

4.3. Short-term prediction

This kind of prediction is needed when earthquakes of medium intensity occur, the so-called precursory phenomena. Obviously, we do not know whether they actually presage a violent earthquake, but we call them precursors with all right (it would be better to call them potential precursors). In fact, the probability of an event within a few days, conditioned on the occurrence of a medium-intensity earthquake, is much greater than in quiet periods. However, this probability has little significance, because it is small (about 0.02) and, up to now, its value has only been estimated on the basis of seismic history. Nevertheless, it should be recalled that the probability of an incoming earthquake increases dramatically if two conditionally independent precursory phenomena occur, when the probability increases to about 0.60!

We wonder why the research on precursory phenomena has been neglected, as the "Decade of Natural Disaster Reduction (1990-1999)" had been proclaimed and several phenomena have been defined as candidate precursors (particularly reliable among them is radon gas emission).

Moreover, we should remember the words of Vere-Jones *et al.* (1998): "It is something of a paradox that scepticism concerning the feasibility of earthquake prediction seems to have reached its climax at just the point where for the first time the quality and quantity of current data make such a programme look distinctly more plausible. This paradox may represent a change of paradigm, a belated recognition that the initial dream of deterministic earthquake prediction, even with some errors attached, has to be replaced with the lesser ambition of defining regions of greater or lesser transient risk".

Perhaps a reborn interest in the subject can be read in the title of the work "Earthquake forecasting gone and back again" (Johnson, 2009). Recent literature seems to give an affirmative answer (see for instance: Jordan *et al.*, 2011; Albarello, 2015; De Santis *et al.*, 2015) and, in particular, these words "…as a result of the new climate surrounding the topic of earthquake forecasting, the Italian Department of Civil Protection (DPC) and the National Institute of Geophysics and Volcanology (INGV) decided to promote new explorative studies on earthquake forecasting (in the long, middle and short term) to provide national institutions with new tools for earthquake hazard assessment. This DPC-INGV-S3 project (short-term earthquake forecasting and preparation) was the result of this initiative" (Albarello, 2015).

Major uncertainty about short-term prediction persists. Precisely for this reason, a very careful **decision procedure** is essential to support the final decision about seismic alert. The procedure needs, not only the probability of the incoming earthquake, but also those of the precursor false and missed alarms. On this basis, a possible decision parameter can be constructed that, moreover, has to involve a wide evaluation: buildings vulnerability, social costs, available knowledge about the stress state of the source, the attitude of citizens once they are informed, and other possible symptomatic phenomena. An example of a decision parameter, named *utility of the alarm system*, had been proposed (Grandori *et al.*, 1988) as a first rough attempt. It is based on the expected values of false and missed alarms relative to a possible alarm system operating over a long period. It can offer the scenario of different utilities relative to alternative choices governing the alarm system.

5. Continuing from the Cornell model

In the frame of the Cornell (1968) model, we made an effort to overcome, in particular, the two main drawbacks: the Gutenberg-Richter relation and the Poisson hypothesis (Petrini, 1993a). Moreover, we used a *mixed method* to improve the local hazard estimation.

5.1. Overcoming Gutenberg-Richter relation

The Gutenberg-Richter relation generally underestimates the contribution of strong earthquakes. Responsible for this is the exponential magnitude distribution F_1, embedded in the relation. Another distribution F_2 could be better: a mixture between an exponential and a linear one, such as the hybrid model proposed by Young and Coppersmith (1985). Such a model could probabilistically interpret the physical hypothesis of characteristic earthquakes. As said above, statistical validation is not possible. Nevertheless, let us pose the question in these terms: which one of the two alternative models F_1 and F_2 is more reliable for the estimation of the peak ground acceleration (PGA) at a given site? The proposed method, based on the introduced concept of credibility, can give a statistically based answer to the question. The answer depends, obviously, on the mathematical structure of the two models, as well as on the nature of the site and on the level of the considered PGA. It does not depend on the local catalogue.

In a case study where the PGA is relative to an earthquake with a 500-year return period, the method clearly indicates the hybrid model as much more reliable than the exponential model. The analogous estimation, relative to a much shorter return period, reverses the relative credibility of the two models (Grandori, 1991; Grandori *et al.*, 2006).

It must be stressed that we are not looking for the statistical validation of one model. Rather, we want to understand whether purely statistical tests can justify preferring one of two competing models aimed at the estimation of a specific quantity.

5.2. Using a time-dependent model with a mixed method for the local hazard estimation

We have already mentioned the issue of historical building retrofitting. In this case, if we want to engage in effective action, it is necessary to take into account that the involved costs are high and the resources inevitably limited. It is therefore necessary to identify priorities on which to focus.

Logic would require beginning by choosing building classes considered priorities and, among these, concentrating on the most dangerous situations. An application of this principle dates back to the 1990s, with the risk level assessment of public buildings in some Italian regions (C.N.R. – Gruppo Nazionale per la Difesa dai Terremoti, 1993). In that case, once the necessary analysis of building vulnerability was developed, time-dependent models were used to overcome the Poisson hypothesis of the temporal distribution of events, which is typical of the Cornell (1968) approach.

The decision parameter used to define the priority ranking was the expected value of the present cost incurred by the first future event for each of the considered buildings. It takes into account the nearness of the next event, together with the level of building vulnerability. On that occasion, a so-called *mixed method* was also tested, estimating independently the distribution of the intensity at the sources and the distribution of the elapsed time at the site. The whole programme, developed by Petrini (Petrini, 1993a, 1993b), could be called the generalized Cornell model.

6. The decision procedure issue

The decisions regarding the reduction of all major hazards that plague our society are extremely difficult. The difficulties arise, not only from the uncertainties of risk modelling, but also from the small probability of occurrence typically involved, in the face of possible catastrophic damage.

Moreover, these decisions involve discretionary choices about the weighing of different objectives, and between different possible courses of action.

If these discretionary choices are not clearly exposed, what happens is that the debate over possible decisions is guided by prejudices rather than reasoned analyses, overlaying and mixing knowledge and decisions, or sometimes even opinions and decisions. In fact, it is necessary to emphasize the different natures of the various components of the cognitive-decision process concerning major hazards, and the methods for the rational and reliable integration of those components.

An essential part of this process is certainly the risk calculation, with all its uncertainties. We will call this part **modelling** as a whole. In the process, we can recognize two other essential components: what we have already called **decision procedure**, which evaluates, along with the risk, modelling uncertainties, other background information, and costs and benefits for different decision-making alternatives and for different objectives; and, finally, the **decision** regarding the protection against risk. The first two parts, modelling and decision procedure, have a cognitive-analytical nature; the third part, the decision regarding the protection against risk, has a political nature, in the sense of choosing between alternatives, with a large discretionary margin.

We resort to a trivial example of decision-making, in which the three components specified can be easily recognized: deciding whether or not to take an umbrella in the case of predicted rain.

The analytical-exploratory part is entrusted to satellite observations, to mathematical modelling of winds and cloud movement, to meteorological studies, which altogether lead to the definition of rain probability (risk calculation is clearly not essential in the case of normal rainfall). The decision procedure, while not explicit, actually consists of self-awareness: whether we are prudent and want to take precautions against a cold, or whether we are not vulnerable or even “Singing in the Rain” types, happy to get wet. Furthermore, we might want to ask a relative for advice, or we may grant greater or lesser credibility to the forecast. Finally, the decision of whether to take an umbrella or not is a choice, not directly implied by the weather forecast.

We wanted to emphasize the second component, the decision procedure, even if it is usually implicit, because the decision depends precisely on this procedure. This procedure is missing in official directives. It is true that allowable thresholds are ultimately assigned. However, in the absence of a decision justification, these thresholds are what Ulrich Beck calls *the trick of the maximum admissible values*. Even in the technical directives, the decision-making procedure is not usually made explicit. In the case of seismic codes, the standards themselves provide only the explicit evaluation of the risk. However, passing directly from the risk evaluation to the decisional choices leads to the erroneous consideration of the decision as implied by risk assessment. As a result, the judgement of the decision made is often debatable by opposing positions.

As far as the seismic code is concerned, the controversy occurs mainly among scientists.

Regarding seismic alert, the controversy can imply strong social conflicts, and tragic consequences if human losses are involved, like on the occasion of the 2009 L'Aquila earthquake.

What we can say is that, if the clear communication of the decision procedure is missing, the conflict between opposite "yes-or-no" positions prevails over a constructive comparison.

Similar conflicts subsist in a number of topics that are different but of the same nature (e.g., nuclear yes or no).

Coming back to the case of the umbrella, we observe that we have no basis to judge the final decision separately. For example, we do not want judge the decision "no umbrella in case of rain" as erroneous. In fact, if one considers oneself hardly vulnerable or even if one loves to sing in the rain, the decision is effective.

By this observation, we want to stress the importance of the explicit decision procedure in order to make, to understand, and to judge a decision. In other words, the decision can be judged right or wrong on the basis of its consistency with the decision procedure.

The decision procedure provides the elements to judge a decision. It is not the decision itself, in retrospect, that has to be judged right or wrong. Here again, the judgement should be moved backwards, **from the decision to the procedure that supports it**.

Decisions, especially those involving high social costs and small probabilities, have to be the final step of the process: modelling - decision procedure - decision itself.

Two issues are typically related to the decision procedure, issues which now have an international formulation: Who shall decide? How safe is safe enough?

As far as the first question is concerned, the fact that the population feels involved in this issue is not only understandable but even desirable, according to the studies and reasoned opinions of many leading scientists (Kahneman, 2003; Slovic *et al.*, 2004; Vrouwenvelder *et al.*, 2015). As early as the 1970s, the most advanced research in psychology and cognitive sociology devoted its attention to the communication problems between experts and citizens. Kahneman and Tversky were awarded the Nobel prize in 2002 (Tversky unfortunately died in 1996). They showed, among other things, how people perceive risk, evaluate uncertainty, and make decisions. They studied all these processes as a function of cognitive mechanisms. Those studies evaluated judgement errors in human decisions, risk propensity or aversion, depending on how people are informed about the different decision-making options.

This work underscored the importance of communication. However, we must admit that the difficult, yet necessary, communication between experts and citizens has not been sufficiently put into practice, or has even been completely neglected.

As specialists, if we want to reach shared choices in terms of seismic code and other seismic risk decisions, we have to examine the decision procedure in depth. The severity levels of the seismic standards or the emergency decisions are derived, not only from technical knowledge, but also from the set of assessments, which we call decision procedure, and which, so far, has not yet been explicitly addressed. In this framework, a first historical example of explicit and shared decision procedure can be found in the confrontation and cooperation of different hazard models, which led to the drafting of the first Italian seismic map in 1980 (Petrini *et al.*, 1980, 1981).

In Italian seismic engineering, the attempt to answer to the second question, "How safe is safe enough?", has not been pursued, even if, in the early 1960s, Giuseppe Grandori introduced the concept of the *marginal cost of a saved life* to face the problem of the *acceptable risk*. This concept was used in the first Italian seismic map, to guide choices and to get homogeneous risk protection in different zones.

In recent times, perhaps because of the tremendous increase in risks faced by society, the question "How safe is safe enough?" receives renewed attention (Nathwani *et al.*, 2009). International conferences are devoted to the subject. Even our not yet pacified seismic analyses could be usefully updated in this international frame.

7. Conclusions

This paper does not contain new research results. It underlines the importance of two issues crucial to facing the decision-making process and to overcoming useless infighting that has recently surfaced among scientists engaged in seismic risk reduction.

1) Probabilistic and deterministic approaches can be used cooperatively to increase knowledge and credibility of adopted models.
2) Researchers should reach a set of decision parameters as a possible basis for the decision-making.
3) All decisions that have to be made in risk reduction must be supported by a careful, complex, and not-only-technical analysis that can be usefully developed into what we call *decision procedure*.

We also illustrated a method for assessing which, among competing probabilistic models, is best for a particular estimation, showing that the choice of the model depends on the hazard quantity to be estimated.

The importance of communication about *decision procedure* is stressed, in order to make decisions that can be understood and accepted.

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