La Soufrière, volcanology and forecasting

Last year Haroun Tazieff, of the Laboratoire de Volcanologie, Centre de Faibles Radioactivités, Gif-sur-Yvette, France, had to leave his post as director of the Service Volcanologique at the Institute of Physics of the Globe over the eruption of La Soufrière. Below, his account of the problems

IN THE past six years at least four erroneous volcanological diagnoses, based either on wrong interpretations of actual facts or on deliberately false data, have induced governmental authorities in four countries to take unnecessary and expensive measures. Together they show that volcanologists may sometimes be confronted with quite serious socio-economic consequences of their own forecasts.

● The first case was in 1970 in Italy's Neapolitan area. A submarine eruption was claimed to have burst out offshore of the city of Pozzuoli, approximately 2 km west of the well-known Solfatara fumarolic field. After the mass-media told of possible explosions, ash-falls and sea-waves, the Army and the *carabinieri* (police) helped the frightened population to evacuate the city. The claimed symptoms were an upheaval of the ground in the whole area, which had been pushed up about one metre in a "very short while"; extremely shallow earthquakes, located 1 to 2 km offshore; a conspicuous temperature-rise of the Solfatara's fumaroles; and the outflow of hot fluids on the bay's floor, inferred from dead boiled fish fishermen took in their nets.

The last three items were subsequently proved totally false by a team of six scientists led by myself, and I disclosed the fact at a press conference. On the other hand, the upheaval, the so-called brady-seism of Pozzuoli, had not been faster than usually noticed over the centuries during which up-and-down level variations of that peculiar area had been classically observed. Eventually it was stated that deliberately wrong data had been produced by the scientist in charge of the geophysical institute of Naples. This was connected to a construction operation in which high-rank people were involved. The evacuation of the inhabitants was proved to be expensive and useless.

● The second case happened during the 1972 eruption of St Vincent's Soufrière in the West Indies. Notwithstanding the clearly expressed and reassuring opinion of Dr J. Tomblin, the volcanologist in charge, an alarming cable from a scientist some thousand kilometres away was taken into consideration. This 'televolcanologist' had deduced his frightening conclusions from earth-satellite imagery and the whole population was consequently moved away. Here too it proved both expensive and useless.

● The third case happened in 1973 during the last days of the Heimaey eruption. Persuaded by a somewhat inexperienced foreign volcanologist, Icelandic authorities agreed to use fire-boats to sprinkle water on a tongue of the thick lava flows which over several weeks had progressed at a distressing speed towards the harbour entrance. No arguments could prevent the exercise: not even the evidence that the Atlantic ocean itself, with all its water, had not been able to stop the main part of the flows which had crawled over the sea-floor for two months.

● The fourth error occurred in 1976 during the eruption of La Soufrière in Guadelope in the West Indies. Here too, in spite of the firm statement of the volcanologist in charge that no danger lay ahead, the authorities followed the alarmist opinion expressed first by a petrographer and then by a geochemist, both of whom lacked experience of eruptive phenomena. Consequently 73,600 people were evacuated on 16 August and were kept away from their homes and jobs for 3½ months. It proved very expensive, dramatic for the population, and totally useless.

A volcanologist is actually as responsible for his diagnoses as is a physician—even more so, because of the number of people involved and because the costs are usually far bigger. This implies that some sort of deontological code, similar to the medical one, should exist for practising volcanologists, and that illegal practice should be prohibited in this field just as it is for medicine. Whatever their field, scientists cannot remain detached from the eventual effects of their work on the everyday life of other people.

The easiest course for the authorities is naturally to choose, amongst various scientific counsels, the most pessimistic if not necessarily the best one. Their responsibility is then shielded against the worst, and if the catastrophe eventually does not occur the only reproaches they suffer are minor ones that are swiftly forgotten. To express a volcanological forecast is always an awkward business. Like civil administrators, many a volcanologist will naturally tend to be pessimistic rather than optimistic, if only to avoid exposing people to some danger he would have disregarded; he would want to avoid being accused if his own optimistic forecast proved unfounded. Though understandable enough, such an attitude is not in accord with the deontology code; a deliberately pessimistic physician is not necessarily considered a good medical doctor.

But some points seem clear. First of all, no volcanologist should state any opinion if his knowledge of eruptive phenomena is below a minimum level. Secondly, the consultant volcanologist should express his scientific opinion without altering it in either an optimistic or pessimistic way. An honest volcanologist, whatever his experience, all too frequently will not be able to describe much more than his incapacity to tell what will follow next. Nevertheless, a valuable forecast is sometimes possible and our own experience shows examples of it.

It actually happened in 1976 at the Guadelope Soufrière, when a rather unusually flat and valid conclusion seemed evident to any experienced volcanologist: absolutely no risk existed that the volcanic event which the whole Caribbean population most feared—*nuées ardentes* (glowing avalanches) of the Mt Pelée type—could happen at all in the near future. I delivered this view, while several other scientists claimed the opposite. But it was obvious that the eruptive events, which were started during the autumn of 1975 by a volcano-seismic crisis, had then passed to phreatic kinds of outbursts in which no fresh magma at all was involved, and would not develop into *nuées ardentes*. The French government nevertheless chose not to listen to this plain argument, asked geologists unskilled in volcanology and proceeded to evacuate the entire population of 73,600 people. The whole issue developed in phases.

● The seismic activity of the volcano started in October 1975 when the seismographic array of the Soufrière Observatory began recording swarms of microearthquakes. For several months, these swarms increased in both number and intensity; the first felt earthquake occurred in late March 1976. At this point several scientists expressed anxiety, more because of the high number of recorded microearthquakes than because of their actual energy. None amongst them had any experience in eruptive phenomena.

The opposite opinion was based upon the two following facts. First, far more frightening volcano-seismic crises had previously been observed in the Carribean, and many more

still in Japan, where the most earnest attempts in the world have been made in volcanological forecasting. At the Omuro volcano in 1930, for example, 4,880 shocks located between 2–7 km below the surface were recorded in less than three months; at the Hakone volcano in 1959–60, the foci of the recorded earthquakes were located 0.8–5 km below the surface, but on the basis of the complete lack of any B-type earthquake shallower than 0.8, Minamaki predicted that no volcanic eruption would happen and he was right. In the Carribean, two volcano-seismic crises occurred at Montserrat island in 1897–98 and 1933–36, and are well know to volcanologists; neither of these well-studied crises finished with volcanic eruption. It therefore made sense not to be unduly alarmed by the 1976 seismic events of the Soufrière.

Secondly, the focal depths of the earthquakes during the Soufrière crisis were located at 2–6 km, depths quite similar to those which had characterised the Hakone and the Omuro crises. This meant that many months, or more probably years, should be necessary for the Soufrière viscous magma to reach the surface, for any magma is supposed to be far slower than its own lavas, and all the Soufrière lavas had obviously been comparatively viscous and slow. Experience has shown that most frequently the average speed of lava flows of the same kind as those of the Soufrière varies between less than 1 and about 100 cm h^{-1}. From a spot located below 6 km down, the corresponding feeding magma should therefore require several years to reach the surface. If any factors had been overlooked, or if the magma velocity would drastically increase, several months seemed a minimum for it actually to become eruptive. Consequently one could quite confidently speak of a period of several weeks, at least, before the start of any magmatic event.

To the objection that a body of molten (and therefore not seismical) magma could be stored *above* the uppermost foci, that is, between the surface and the 2 km depth —meaning that a hypothetical reservoir was located within the 1,400 m thick volcanic heap itself—the answer was that both the temperatures measured close by in the geothermal field of Bouillante (70 °C near the surface and 240 °C 350 m down) and the gas composition of the Soufrière fumaroles (in vol % : CO_2, 90–93%; H_2S, 0.6–1%; CH_4, 0.5%; H_2, 0.5–1.2%) proved such a hypothesis to be quite improbable.

● On 8 July 1976 the eruption started its second, actually eruptive, phase when so-called 'ash' outbursts were superimposed on the almost continuously increasing volcano-seismic activity. This second phase lasted eight months, during which 20 phreatic eruptions occurred, the last one on 1 March 1977. Each outburst lasted only a few minutes (less than 20) and expelled steam and water droplets mixed with some volcanic gases laden with ejecta. These ejecta, 'ashes', sands and blocks, were exclusively composed of old rock material, with no trace of any fresh magmatic material. The first of these phreatic eruptions started noiselessly and propelled a dark mushroom hundreds of metres above the top of the mountain. This plume was then driven south-westwards by the wind. From the volcano windwards, the sky became totally dark, and a thick rain of small lapilli and dust started, which lasted for about 20 minutes. Several thousand people fled as soon as the darkness cleared up.

The scientists called by the authorities were divided. Some of them felt afraid, firstly because they feared that these 'ash explosions' could lead either to *nuées ardentes* or to mud-flows, or both. The opposite opinion was that these risks did not exist because this eruption obviously belonged to the phreatic kind: gas analyses, measured steam temperatures, as well as examination of ejecta showed that no fresh hot magma or lava were involved and that, consequently, no *nuées ardentes* could be expected. On the other hand, as in phreatic eruptions, the very first outburst usually is the strongest one, and as the 8 July one had generated only a small mud-flow, it was to be supposed that further lava hazards would be minor. In spite of an apparent tendency to prefer pessimistic diagnosis to reassuring ones, the government authorities were consequently prevented from evacuating people.

The second phreatic outburst occurred on 24 July. Asked by the *prefet* and having obtained the data from and opinion of my collaborators, now permanently monitoring the fluid and solid exhaltation of the volcano, I concluded that there was no more danger ahead than two weeks before.

● The third outburst happened on 9 August. I was in the Ecuadorian Andes involved in a volcanological expedition with two of my collaborators, but our three colleagues were still on La Soufrière, mainly to monitor the chemical and physical evolution of the gas phase, which may be considered as a good indicator of the eruptive volution itself. These skilled volcanologists declared that no threatening change was to be feared in the eruptive events. This was because of the following facts: first, the gas composition had not changed. The proportion of CO to CO_2 was about 10^{-3}–10^{-4}, and of H_2S to $S0_2$ was about 10; in addition, the focal depth of the earthquake had not moved, no trace of any magmatic activity was detectable anywhere, either in the erupting area or on the lower slopes of the volcano, and old rocky material *exclusively* was to be found around the eruptive vents.

Disregarding these volcanologists' opinions, the civil authorities nevertheless called from France two geologists without any experience in the field of eruptive activity. The first decided to have the whole population immediately evacuated; the second decided to enforce the precautions and maintain them for several months on.

Their diagnoses were based, first, on the assumption that the seismo-volcanic crisis—up to 1,257 shocks, half a dozen of which had been slightly felt, in one day—had reached such a climax that nothing but a catastrophe could proceed, and secondly on the claimed presence of fresh volcanic glass in the erupted 'ashes', the meaning of which was the imminence of *nuées ardentes*. Professor C. Allègre, on 5 September 1976, produced an official report in which he stated that 50–60% of fresh volcanic glass was present in the erupted ash. He was later compelled to admit that actually no trace of fresh material had ever been detected.

According to *Le Guern* (in press) the twenty outbursts which occurred between the first (8 July 1976) and the last one (1 March 1977), as well as the more or less continuous emission observed at the active vents throughout the same period, have poured out a total output of the order of $6–10\times10^6$ tons of steam and $1.5–2\times10^6$ tons of 'ashes' and blocks. According to the same author, a surface less than 1 km long and 300 m wide received a total 'ash' fall 300 m thick for the whole nine-months-long event. The closest town, Matouba, some 3.5 km west (windwards) from the craters, got a total 'ash' fall of 5–15 mm according to the spots. The closest city, St-Claude, 4.5 km west-south-west from the erupting vents, got a total 2–5 mm thickness of 'ash'. The longest mud-flow, which resulted from the first ran eastwards over less than 3 km.

These figures show how modest was the scale of this eruption. The above mentioned criteria and their interpretation explain how our so-called 'optimistic' diagnosis had been attained. The main lesson of this unfortunate experience is perhaps to confirm the absolute necessity for a good volcanological interpretation of all the available, geological, geophysical, chemical and phenomenological data before expressing any forecast. Volcanologists, just as medical doctors, should be responsible, skilled, experienced, different specialists closely co-operating with each other. And they should keep as cool as a cucumber. □

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Editorial

A DEONTOLOGICAL CODE FOR VOLCANOLOGISTS?

The recent eruption of the Soufrière on the island of Guadaloupe and the extraordinary sequence of events it entailed seem to have revealed a good deal more about the political and sociological problems faced by volcanologists than about the nature of the eruption itself. The dispute raised by conflicting views and precautionary measures recommended by various geologists reached the international press and attracted wide attention.

Apart from the lack of agreement among "experts", which has long been a source of embarrassment in our profession, the incident raised the basic question of the responsibilities of volcanologists who find themselves called upon to assess volcanic hazards.

During the mild phreatic eruptions and associated seismic activity that occurred over a period of months in 1976, sundry geologists who visited the island were asked to evaluate the risk to the populace. Although the volcanologist in charge firmly maintained that there was no serious danger, other geologists who had little or no experience with active volcanism took a more pessimistic view. Faced with this divergence of opinion, the authorities decided to take the most conservative possible action and proceeded to evacuate 73,600 persons for three and a half months.

Haroun Tazieff, who probably has as much experience with active volcanism as any person in the world, advised the government officials that there was no cause for alarm and that there would be no catastrophy like that which occurred at St. Pierre in 1902, as several persons feared. He was correct, of course. Nothing happened. Nevertheless, the intervention of self-appointed experts and the confusion generated by conflicting interpretations resulted in wholesale disruption of the life and economy of the island.

Examples of this type of exaggerated response are also seen in the costly over-design of certain industrial installations, such as nuclear power plants that have been proposed in volcanic regions. The conservatism of planners who are sensitive to any allegations of unsafe design and wish to guard against every possible hazard can easily be exploited by anyone who is motivated by a desire to obstruct the project and is able to do so by advising prohibitively expensive precautions against risks that are exceedingly unlikely but difficult to disprove. As Tazieff points out (Nature 269 (1977) 96—97), there is need for a deontological code to guide volcanologists, and this code should include a stricture against exaggerating hazards and recommending excessively conservative precautions regardless of the economic and social consequences they entail.

DEREK BOSTOK

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Reply to Editorial

As a member on the editorial board of this journal I am forced to put forward a protest and regret that the provocative "Editorial" by Derek Bostok was accepted for publication in the last issue (Vol. 4, No. 1/2, August 1978) of the journal. An "Editorial" is supposed to reflect the opinion, or at least the general feeling, of the leaders of a newspaper or a journal and I have reason to believe that Mr. Bostok's judgement concerning the role of volcanologists in general and his interpretation of the Soufrière affair in particular is open to question.

It so happens that three members of the editorial board of this journal were asked to participate in a committee, headed by Dr. Frank Press, to review work done by French geoscientists on the active volcano Soufrière on Guadaloupe in the West Indies. The committee of six people of four nationalities was established by the French Research Council (CNRS) in order to cut through the unfortunate stalemate which the disagreement amongst French geoscientists had engendered. Considering that the French government was faced with the problem of 73,000 refugees it was the prime task of the committee to pass an objective judgement of the risk involved in moving the refugees back to their homes. One possible consequence of the committee's work was that its conclusions would be interpreted to favour one of the opposing groups of scientists, condemning the other for incompetence and failure.

A few facts on the Soufrière affair may help to show who was "right" in this much publicized dispute. Following are some of the arguments which led to the evacuation: (1) increased seismicity; (2) explosive activity in the summit crater shedding tephra over inhabited areas; (3) increasing amount of fresh volcanic glass shards in the tephra which otherwise consisted mostly of water-soaked mud; (4) appearance of epidote in the tephra which was believed to indicate that the explosions threw out material from successively greater depth in the volcano; (5) comparison with historic volcanic events in the French West Indies where political intrigues led to catastrophic misjudgement; and (6) extremely difficult road situation, which made quick evacuation impossible.

The committee found that what had been identified as fresh volcanic glass was in fact aggregates of very fine grained clay. What had been identified as epidote was in fact pyroxene.

The critics of the evacuation had the following arguments: (1) a general feeling based on extensive experience with active volcanoes; (2) focal depths of earthquakes did not migrate upwards; and (3) chemistry of thermal gases did not indicate the presence of shallow magma.

The committee found that objective, scientific judgement must be based on

factual information. That the seismic array did not allow the accurate determination of focal depth and no information was available which would show migration of earthquake foci. That the only possible indication of the presence of a shallow degassing magma was to be found in the gas analyses of Mr. Tazieff and his coworkers. During a short period the gas analyses indicated appreciable amounts of the high-temperature component sulphur dioxide.

An objective evaluation of the scientific information which led to the evacuation is that it was not sufficiently rigorous on several points which at the time of decision were given high priority. The decision itself was in the hands of the public authorities who in addition to scientific information have to take account of economic and social factors. Furthermore the authorities have to define the acceptable risk which can be highly variable from one situation to another. In Guadaloupe it was decided that the acceptable risk was zero, which gave very little room for further evaluation and practically meant that even the evacuation of 73,000 people had to proceed without the risk of a road accident.

Any criticism of the decision reached has to take account of this most important attitude of the authorities.

It must further be remembered that both scientists and the authorities were working under extremely high tension. When men are strained, inexperienced and faced with an unexpected situation they are likely to make decisions which later prove to be overdimensioned. That is a simple fact of life.

The principal critic of the evacuation, Haroun Tazieff, was not present when the decision was taken. He had left on a mission to Ecuador. When he returned, his criticism, based on the points given above, was widely publicized and became highly embarrassing for his scientific colleagues as well as the French authorities. All of us know that Mr. Tazieff is a popular man and he has good contact with the mass media. Reporters request a statement; they are less concerned with the factual basis on which that statement rests.

The international committee did ask for the factual basis of his statements and found emotional attitudes and misjudgements equally serious as those made by the scientists who in the absence of Mr. Tazieff acted as advisers to the authorities.

During the preparation of a final report in Paris in November 1976, I recall that the committee members were relieved to discover that their findings could not be interpreted in favour of either party. The principal reason why matters got out of hand was the lack of up to date, sophisticated monitoring systems and experienced personel. Too many easily-obtainable facts were either not available, incorrectly measured, or neglected.

The evaluation showed that the refugees could be moved back to their homes provided (1) that the authorities were willing to accept some risk, (2) that high-quality monitoring systems were installed on the volcano, and (3) that the supervision of collection of data and its interpretation were performed by people trained in volcanology.

A wise and admirably conceived conclusion of the whole Soufrière affair was put forward by Dr. Chabbal, the president of CNRS: the blame for the Soufrière misjudgement did not lie with the scientists. For a number of years they had asked for funds to improve monitoring systems on Soufrière, and to make detailed volcanological studies in the area. Their requests had been turned down. For this reason the necessary background knowledge and expertise was not available at the time of need. Immediate and generous increase in the funding of volcanological research should be the reaction to the unfortunate Soufrière affair.

When the findings of the international committee were made public in Paris, the mass media made a simplistic interpretation of their own: "Tazieff was right". And now I find this same phrase repeated in an "Editorial" of a scientific journal on volcanology: "He was correct, of course. Nothing happened". I sincerely hope that the scientific profession has not degraded to the practise of flipping coins in matters of such concern as the prevention of volcanic hazard.

Today volcanology is emerging as a scientific discipline. Some of us have already experienced hard clashes with unyielding public officials or the embarrassments of false predictions and professional disagreements. This is the price we have to pay and there is no way around it. Nothing will be solved with a deontological code for volcanologists. We will have to learn the hard way, as everybody else before us, who had to deal with matters so highly involved in, and affected by, human behaviour. The Soufrière affair is a stepping stone in our evolution. It was a costly experiment and therefore we should try to gain as much from this experience as possible . That, however, is not possible unless the truth and nothing but the truth is respected.

Let me finish these remarks by paying tribute to the French Research Council for its wise handling of an extremely difficult affair. The open and frank discussions, which finally led to the Research Council itself taking the blame for the incident, can only have happened in a country of great democratic tradition and vast cultural heritage.

They are to be admired.

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Letter to the Editor

A DEONTOLOGICAL CODE FOR VOLCANOLOGISTS? — A RESPONSE TO DEREK BOSTOK'S EDITIORIAL

The Editorial by Derek Bostok in the August 1978 issue of this Journal regarding the recent volcano crisis at La Soufrière, Guadeloupe, presents some provocative thoughts. As a scientist who made four visits to Guadeloupe during the course of the crisis, and who served as one of the six non-French members of the ad hoc Comité Sciéntifique International sur La Soufrière, convened by the French government to deal with the situation, I feel obligated and qualified to respond. I should say at the outset that Professor McBirney kindly supplied me with a preliminary copy of Dr. Gudmundur Sigvaldason's comments that were published in the December 1978 issue. I shall not, therefore, repeat many of the details of the crisis that he has already described.

One of the most important aspects of the La Soufrière crisis of 1976 was that there was an imperfect scientific understanding of many aspects of the ongoing activity. In summary form, some of the more important aspects of the situation in August 1976 were as follows:

(1) Hundreds, even thousands, of local earthquakes were recorded each day, but the absence of an accurate velocity model at depth and the inadequacies of the seismic network did not permit the accurate location of hypocenters. Most events were known to be originating in a general region several kilometers beneath the summit of the volcano, but is was not possible to determine whether there was any systematic upward migration of these events.

(2) The ash that was continuously erupted from the summit of the volcano, sometimes discharged with almost explosive force, was thought to contain increasing quantities of fresh volcanic glass. This observation later turned out to be incorrect, but, at the time of the crisis, it was believed by many people that fresh magma was reaching the surface.

(3) Analyses of volcanic gases by Haroun Tazieff and his team did not indicate to them that a magmatic eruption was underway or impending. However, the collection of gases and interpretation of their chemistry is far from a perfected science. A great deal of additional work on many different volcanoes will be required to determine whether gas chemistry is a really accurate indicator of volcanic hazards.

(4) No measurements of ground deformation had been made, and it was therefore possible that the volcano was inflating without anybody knowing it.

(5) The reconnaissance geological studies that were available at the time were not able to provide the vitally needed details regarding the eruptive history of the volcano during the past 10,000—20,000 years. Therefore there was very little geological insight as to what the volcano was apt to do in the future. Poorly sorted pyroclastic deposits crop out at many localities on the volcano, but it was not known whether these deposits were the product of

pyroclastic flows, hot or cold mudflows, or cold avalanches. Moreover, the age and frequency of emplacement of most of these deposits was not known.

It was in the context of these five major areas of uncertainty that the Governor of Guadeloupe ordered the evacuation of all 73,000 people living on the slopes of the volcano. Controversies broke out immediately as to whether such a massive evacuation was justified, whether the evacuation lasted too long, and whether there was panic or complacency with regard to the assessment of the hazard. Scientists disagreed with scientists, and the rest is history.

But how does this bring us to the question of a deontological code — a code of "duty" or "moral obligation?" Bostok asks us to endorse Haroun Tazieff's version of a deontological code — a code based on the assumption that the volcano never did pose a great threat, and that, if a rapid buildup did occur, the people could have been evacuated in time. According to such a code, those who *overestimated* the hazard should be censured for exaggerating the implications of the available data and for recommending, in panic, large-scale evacuation.

On the other hand, it is possible to consider another version of a deontological code that could be applied to the La Soufrière situation — a code based on the acknowledgement that (1) there were enormous gaps in the geophysical, geochemical, and geological understanding of the ongoing activity, (2) there was a reasonable probability (perhaps 1:20 or 1:50) that the activity could have progressed rapidly to the production of pyroclastic flows or destructive phreatic activity, and (3) there was legitimate uncertainty as to what was really going to happen. According to this code, those who *underestimated* the hazard should be censured for not acknowledging the wide gaps in the available data and, for arguing, with unrealistic coolness, against evacuation.

I have obviously oversimplified the development of these two deontological codes for La Soufrière, but, to me, it is telling that it is possible to argue for either of two very different codes, on the basis of only one set of facts. And that's the important point — a really workable code would have to be based on a framework, a single set of criteria agreed to by all parties involved... if not by volcanologists in general. In theory, such criteria might include threshold levels of local seismicity, ground deformation, and gas chemistry, beyond which a truly hazardous situation would automatically be declared.

In reality, however, it is doubtful that such a code could ever be devised. As monitoring activities improve and are extended to more and more volcanoes, complications and equivocations will doubtless arise. We already know that outwardly similar volcanoes often function in remarkably dissimilar ways, and it is likely that this pattern will continue to be encountered in the future. This, of course, should in no way dissuade us from persuing our work. Techniques and insights will doubtless improve in future years, but, as Gudmundur Sigvaldason has indicated, we must be ready to accept criticism and "hard knocks" as we continue our efforts to provide the best scientific evaluations possible.

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Letter to the Editor

DEONTOLOGICAL CODE, PROBABILISTIC HAZARD ASSESSMENT OR RUSSIAN ROULETTE?

As one of the "sundry geologists" who visited Guadeloupe three times at the specific request of the civil authorities, and spent a total of 14 days on the scene during the early stages of the 1976 eruption, I would like to make a few points which may not have been evident to the writer of the editorial of the August 1978 issue of the Journal of Volcanology and Geothermal Research.

Mr. Bostok in his second paragraph identifies "the basic question of the responsibilities of volcanologists who find themselves called upon to assess volcanic hazards", and this indeed is the crux of the matter. These responsibilities include, in my opinion:

(1) *Admission that nuées cannot be specifically predicted.* It must be stated clearly that there are no specific precursors to nuée ardente emission, and hence that absolute predictive statements about the occurrence or non-occurrence of nuées cannot honestly be made. At any volcano judged to be capable of violent activity, the occurrence of abnormal earthquakes, fumarolic, phreatic or magmatic activity *greatly increases the probability* of destructive eruption in the early future. It is possible and extremely desirable for volcanologists to give quantitative estimates, even though crude, of this probability, in the form of a series of values relating to different future intervals of time, especially the minimum time necessary for complete evacuation of those zones which have been identified as exposed to significant risks.

From the above it follows that it is unscientific to have claimed that "there would be no catastrophy (sic) like that which occurred at St. Pierre in 1902". Based on the available, very limited global statistics, namely 43 well-described nuée ardente and similar types of eruption, and weighted according to the ratio of nuée to all eruptions, including phreatic, which have taken place historically in the Lesser Antilles (4 to 15), my own quantitative estimate was that, with 5 weeks elapsed from the eruption onset, there was a probability of about 1 in 6 that the eruption would eventually emit nuées ardentes. Superimposed upon this there was a probability of 1 in 8 that in the event of the nuée emission, recognizable build-up activity would be of less than 2 hours' duration, and a probability of 1 in 4 that this build-up activity would be of less than 12 hours' duration. Hence in the second week of August 1976, there was a probability of about 1 in 24 that the Guadeloupe eruption would produce nuées at less than 12 hours' notice, and a probability of about 1 in 48 that nuée ardente activity would develop at less than 2 hours' notice. The above figures do not take into account (a) the fact that seismic and phreatic activity were increasing significantly during the first two weeks in August which immediately preceded the evacuation, and (b) the possibility of dangerous volcanic activity other than nuées ardentes.

It is noteworthy in the above context that the volcanologist who after the end of the eruption described the evacuation as "totally useless", had stated in a widely circulated letter dated 29 July 1976 (17 days before the evacuation) that "an absolute minimum of 2 hours would separate the beginning of threatening activity from any catastrophic phase". Among the suitably detailed accounts which I have found in the world literature, there have been at least 5 out of 43 nuée-type eruptions (Arenal, 1968; Hibok-Hibok, 1948; Lopevi, 1960; Mayon, 1814; Bezymianny, 1955) in which the "build-up" time from abnormal seismic, fumarolic or mild magmatic activity to potentially destructive nuées was less than 2 hours. It is also very dubious whether a period of warning as short as 2 hours would be sufficient for a complete evacuation, especially if this were during the night. The possibility of so brief a warning might not be an acceptable risk, the more so because in the Guadeloupe activity of 1976, government authorities in Paris had declared that no risk was to be taken for the population.

(2) *No gambling.* A second responsibility arises from the relatively high odds quoted above that activity will *not* become severely destructive at short notice. There may be a temptation for the volcanologist to seek to enhance his own reputation by disregarding the low probability of a catastrophe and giving firm reassurances that there is no danger. Such a volcanologist is playing a kind of Russian roulette, with somewhat better personal odds of survival than in the pistol game, but involving the lives of whole populations, not simply his own. In this context, Mr. Bostok's comment that "He was correct, of course" needs some qualification.

(3) *The need to remain on the spot.* A third responsibility is that any volcanologist-in-charge, or who claims to be in charge, should remain continuously on the spot. If he goes elsewhere, he should be available day or night for consultation at very short notice. This was not the case in Guadeloupe in 1976.

(4) *The need to deploy all relevant monitoring methods as soon as clearly abnormal activity develops.* One of the most serious scientific omissions in Guadeloupe was the failure to deploy a wide range of monitoring techniques when, by March 1976, the local earthquake swarm had reached major proportions. The assessment of hazard depends not only on historical precedent but equally on the measurements made of the activity in progress. It is the responsibility of the scientist-in-charge to establish rapidly, once clearly abnormal activity has started, the widest variety and highest quality of relevant monitoring techniques. This involves the planning of scientific co-operation in advance of any crisis, as well as the rapid response to the onset of clearly abnormal events, and the careful scrutiny of all scientific observations for accuracy.

(5) *The need to limit opinions to one's field of professional competence.* A final responsibilty is that the volcanologist should remember, and if necessary remind the civil authorities, that the decision to evacuate involves not simply the numerical assessment of the hazard probability but also the off-

setting of this against the economic and social consequences of evacuation. Most volcanologists have little or no expertise in sociology or economics and are therefore not in a position to provide the best judgement as to what constitutes the limit of acceptable risk. This is emphatically not a case of avoiding a responsibility: it is a case of limiting one's opinion to one's field of professional competence. An analogy is that in a court of law, an expert witness is allowed to give opinions only on subjects within his field of expertise.

This indicates to me the need for a multidisciplinary team to decide upon the limit of acceptable risk, preferably in advance of any volcanic crisis. On the part of volcanologists, a readiness to present and discuss hazard estimates in quantitative, probabilistic terms will serve the public interest far more directly and will be a subject on which reasonable consensus can be achieved far more easily than the creation of a deontological code.

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Letter to the Editor

The Editorial in this journal entitled "A deontological code for volcanologists" by Derek Bostok opened an interesting debate among volcanologists who are actively involved in the assessment of risk from volcanic eruptions and have direct experience in the delicate matter of interaction with public authorities during an emergency.

For some years both of us have been involved in these problems in Italy. It happens that we have also been members of the International Committee appointed by the French Research Council in November 1976 for the Soufrière affair. We feel that the problem of the behaviour of volcanologists during an emergency was correctly formulated by Derek Bostok. Therefore we cannot share the "protest and regret" of Gudmundur Sigvaldason (Vol. 4, No. 3/4, December 1978).

At two years'distance, the volcanological community should think about the Soufrière episode in order to learn from it as much as possible. It is a matter of fact that many mistakes and misjudgements were made during and after the emergency period that led to the evacuation of 73,000 people.

The crucial point is not to discuss which of the parties involved was right and which was wrong, but rather to understand which circumstances favoured these errors and misjudgements.

A serene evaluation of the entire episode shows that it was mainly biased by two facts. The first was the attitude of the public authorities in requiring that the acceptable risk be zero. This absurd attitude points out the urgency of educating public authorities on the meaning of a probabilistic prediction of natural hazards, in order to be able to formulate correctly questions to the scientists involved in these emergencies. It must be emphasized that, the problem of correct relations between public officials and volcanologists is far from being satisfactorily solved in most countries.

It is obvious that a correct relationship cannot be created "ex abrupto" during an emergency, but it must be prepared by a long and patient joint effort. It is even more difficult to extemporize a correct evaluation of an ongoing eruptive event and its associated phenomena without: a previous systematic collection of pertinent physical and chemical data, a sound knowledge of the eruptive history of the volcano and an experience of eruptions and eruptive mechanisms. The lack of these conditions led, in the Soufrière case, to the second biasing fact: a number of observational mistakes and misjudgements. The clayish matrix of the ejecta was misidentified as new volcanic glass, pyroxene was misidentified as epidote, preliminary ground-tilt results were interpreted as indicating a dangerous inflation of the volcano, without any critical evaluation of their reliability. Tilt measurements were actually made on unstable slopes made of water soaked clayish soil. These errors, and the lack of volcanological experience of the scientists present on the spot prevented the phreatic character of the eruption from being recog-

nized, and led to an overestimate of the actual risk. A serious aspect of the problem was the attitude of some scientists after these errors were recognized, when they refused to accept the evidence and tried to hide the reality under the veil of scientific controversy about the interpretation of the phenomenon. This attitude prevented a re-evaluation of the risk once these errors were recognized.

This shows quite clearly that the call for a "deontological code for volcanologists" is justified. A scientist should accept the responsibility of contributing to a decision which involves the destiny and well-being of other people only if he has the actual professional experience which allows him to make a positive contribution to the problem. Errors and misjudgements can be made by any human under the stress of an emergency situation. But he must be ready to frankly admit them and not let his defence of a personal reputation take precedence over the general interest of the community. A scientific controversy can exist on the interpretation of factual data, but a clear and definite boundary must separate facts from opinions. By confusing facts with opinions in front of the public authorities volcanology was discredited, because it generated the impression that volcanology is more uncertain and approximate than it really is.

We agree with Gudmundur Sigvaldason that the French National Research Council, and particularly its President Mr. Robert Chabral, have to be admired for the courage and open mind shown in facing an embarassing situation. They would be of further help to volcanology if they would decide to publish the principal scientific reports on the Soufrière 1976 event, which certainly remains as a stepping stone in the difficult and delicate matter of evaluating the risk that an active eruptive event constitutes for the people living on a volcano.

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