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# United States Department of the Interior

GEOLOGICAL SURVEY  
RESTON, VA. 22092

In Reply Refer To:  
Mail Stop 905

February 22, 1980

Dr. Robert Jackson  
Chief, Geosciences Branch  
Division of Site Safety &  
Environmental Analysis  
U.S. Nuclear Regulatory Commission  
Washington, D. C. 20555

Dear Bob:

Transmitted herewith is our review of Christopher Newhall's "Review of Volcanologic Discussions in the PSAR and Related Documents, Philippine Nuclear Power Plant #1".

This review was prepared by Robert I. Tilling, Chief, Office of Geochemistry and Geophysics, U.S.G.S., with concurrence of Dr. Richard D. Krushensky, Acting Chief, Office of Environmental Geology, U.S.G.S. Mr. Newhall is a part-time employee of the Geological Survey but his comments and our review of them have been developed independantly. There have been no discussions of this subject between Drs. Tilling and Krushensky and Mr. Newhall.

I wish to reiterate here that the Survey has not reviewed the studies of Ebasco nor have we studied the specific volcanic site in question. Our comments are based on Mr. Newhall's report and our expertise in volcanology in general.

We have no objection to your making this review part of the public record.

Sincerely yours,

James F. Devine  
Deputy for Engineering  
Office of Earthquake Studies

copy to: Newhall

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"Review" of Newhall's "Review of Volcanologic Discussions  
in the PSAR and Related Documents,  
Philippine Nuclear Power Plant #1

GENERAL COMMENT

While comments cannot be made directly on the specific issues related to the Ebasco studies reviewed by Newhall, he has raised in his review a number of important questions in a forthright, scientific manner. So far as it can be ascertained, he appeared to have objectively presented Ebasco's evidence and interpretations drawn therefrom, before proceeding to state his own views. Indeed, in the main, his arguments are persuasive. The Survey reviewers support his general and fundamental thesis, namely: the lack of sufficiently detailed geologic mapping of the eruptive products of Mt. Natib and of adequate stratigraphic control on samples dated and chemically analyzed, coupled with an apparent disregard of what Newhall terms "volcanologic common sense," casts serious doubts on the interpretations made by the Ebasco team that bear on the volcanic history of Mt. Natib in particular and of the region in general. Without such a firm understanding of the historic and pre-historic record of eruptive activity, a meaningful volcanic hazards assessment of the Mt. Natib region, let alone the Napot Pt. site, is not possible.

SPECIFIC COMMENTS (keyed to Newhall's summary of 10 principal points)

- 1) Most volcanologists would agree that Mt. Natib is a "dormant" rather than "extinct" volcano. It is important, however, to keep in mind that the distinction between these categories is necessarily subjective and depends on the information available. Perhaps one of the most famous, and destructive historic eruption of an "extinct" volcano was that of Vesuvius in 79 A.D. that destroyed Pompeii.
- 2) Newhall, rightfully, it would appear, criticizes Ebasco for not fully applying commonly used and widely accepted techniques of determining the eruptive history and evolution of a volcano, the foundation upon which any assessment of volcanic hazards must be built. These techniques are neither difficult nor expensive (in terms of manpower and money) relative to the complex, time-consuming, and expensive process to harness nuclear energy safely. Instead, considerable effort was expended on newer, potentially powerful but not as yet thoroughly tested techniques, whose successful analytical utility hinges on a solid knowledge of the geologic and stratigraphic of the samples studied.
- 3) Newhall's point here is directly related to point (2). Assuming his analysis of the Ebasco documents and maps is correct, then clearly the requisite detailed geologic mapping and stratigraphic correlation of eruptive products with volcanic centers with time and space have not been done. Such studies--keeping in mind the lines between

"reconnaissance" and "detailed" is fuzzy--must be conducted to establish the basic framework for the sophisticated laboratory studies and statistical analyses of the type done by Ebasco. Newhall simply makes a plea for more fieldwork to: a) better establish the geologic setting of samples dated or chemically analyzed; and b) improve evaluations of the various kinds of volcanic hazards on a point-by-point basis for the mapped area, including and especially the vicinity of the Napot Pt. site. Newhall's observations and questions (p. 20-23) are valid and pertinent, and should be addressed.

- 4) The Ebasco evidence discussed by Newhall provides a very weak case for the conclusion that the age of the most recent eruptive activity was 69,000 years before present. Newhall's analysis of the available evidence and interpretation that eruptive activity may have been more recent appear to be well reasoned. The question of "age" of the youngest volcanic deposits--and reliable "ages" of all detailed deposits--is one of the most critical issues that must be adequately resolved to provide a meaningful evaluation of the volcanic hazards at the plant site. Such "ages" should not be obtained on random samples, as apparently has been done by Ebasco in Newhall's view.

Newhall appropriately raises some specific questions: why no C-14 ages were obtained for Mt. Natib? Were C-14 age determinations made on the carbonized wood in the Napot Pt. tuff? If not, why not? Also, the apparent discrepancies cited by Newhall regarding ages and sample numbers in the various Ebasco documents need to be clarified.

- 5) No volcanologist would question the need to obtain the best estimates of the number of eruptions per unit time for each of the volcanic centers. Survey reviewers share Newhall's concern that Ebasco's analyses of eruption probabilities are based on incomplete historical and geochronological data. If it is not possible nor practical to improve the data sets, then Ebasco must clearly outline the assumptions and limitations of both the Stepp and Wickman methods in the statistical treatment of geochronological data, as well as of McBirney's analysis of the historic eruptive activity.

Newhall's long discussion (p. 30-38) of the Volcanic Explosivity Index may border on being somewhat self-serving. Nonetheless, his contention that attempts should be made to factor in "magnitude" or "intensity" of eruptions in assessment of eruption and related hazards probabilities raises a legitimate scientific issue that merits further consideration. Similarly, other questions raised by Newhall (p. 40-46) should be addressed: Is Ebasco's estimate of eruption rates and probabilities in reality a "summary" of the "sampling program?" Are estimates of eruption probabilities "age dependent?" If so, what is the nature of such age dependence relative to average recurrence interval for a given volcano?

Admittedly, influenced by the analysis of Walker (1974), but from the Survey reviewers' understanding of world-wide volcanism (Newhall's "volcanologic common sense"), it is difficult to accept Ebasco's interpretation that the volcanoes that might affect the Napot Pt. site would be, on average, any less active than other Philippine-Indonesian volcanoes given the present state of knowledge of the plate-tectonic setting of that region.

- 6) There is agreement with Newhall's basic point, namely: the Ebasco evidence, as discussed by Newhall, does not require a tectonic model involving two periods of subduction at two differing arc-trench regimes (first, the West Luzon Trough and then shifting to the Manila Trench). Inasmuch as it is based largely on variations of  $K_2O$  content of analyzed samples and inferred relationships of such variations to distance and depth to subduction zones, the "two-subduction-zone" model represents a large, and speculative, extrapolation of similar geochemical trends proposed for other arc-trench volcanic systems in the world. If the Ebasco tectonic model and its related distinction of volcanic risk for the various eruptive centers is used in volcanic hazards assessment of the "PNPP #1," then it is inconsistent to exclude world-wide analyses of volcanic activity (e.g., Walker, 1974) and other "volcanologic common sense" from commensurate consideration.

Survey reviewers fully support Newhall's recommendation that a closer examination be made of the available geochemical data to try to decipher the evolution of Mt. Natib magmas in time and space in order to better construct its eruptive history, which in turn would result in a more adequate evaluation of volcanic hazards.

- 7) As discussed by Newhall, Ebasco's case for a difference between probabilities of eruption from various parts of Mt. Natib is weak indeed. With available data it would be unjustified to completely rule out the possibility of eruption from the western flank of Natib. A related and perhaps more important question is: what is the probability of another catastrophic (caldera-forming) event that would surely affect all parts of the Natib volcanic edifice? Newhall emphasizes, correctly in our judgment, the need to obtain more information on when the Natib caldera formed and to identify the pyroclastic and ash-fall deposits associated with caldera collapse and related phenomena.
- 8) Having made field inspections of pyroclastic deposits ejected from the volcanoes of the Alaskan Peninsula that have surmounted impressive topographic barriers (cf. Miller and Smith, 1977), Survey reviewers share Newhall's concern that Ebasco has overemphasized the efficacy of topographic protection against lateral blasts or surge.
- 9) Survey reviewers strongly urge that a volcano monitoring system be established in the Mt. Natib region, even apart from considerations of the Napot Pt. site per se. With such a monitoring system (seismic,

geodetic, etc.) in place and operating, a properly trained "surveillance team" would be in a position to establish a baseline of variations in volcano behavior and state and, hence, would be able to detect departures from baseline or "normal" variations. Having such knowledge, experts can formulate criteria for plant shutdown and other contingencies and also to evaluate when and if the volcano state returns to "normal" to permit resumption of plant operations. Recent experience (1976) at Soufriere Volcano (Guadeloupe) clearly shows the disastrous socio-economic consequences of the evacuation of some 73,000 people, precisely because of lack of the type of information obtained from systematic volcano monitoring. Moreover, the problem was compounded because, even after French government officials decided to carry out the evacuation (which resulted in the loss of many thousands of dollars each day it was in effect), there was no body of information that could be used to determine if and when it was "safe" for the populace and tourists to return to normal life activities. The decision to terminate the evacuation was largely predicated on political and socio-economic factors only, in lieu of the absence of definitive volcano monitoring data.

- 10) Survey reviewers whole-heartedly agree with Newhall's suggestion that volcanic hazard guidelines be developed along lines similar to those for earthquake hazards. Although, it is encouraging to note that volcanic hazards investigations and mapping activities are increasing in active volcanic regions of the United States and elsewhere in the world, there is no focused effort, to my knowledge at least, to develop guidelines specifically applicable to nuclear power plant siting. Obviously, the development of such guidelines must be paralleled by increased systematic investigations and monitoring of currently active volcanoes that are analogs to dormant, but potentially dangerous volcanoes.