

PDR 6/13/92

## MEMORANDUM

TO: Richard Major, Chief  
Nuclear Waste Branch

FROM: Kenneth Poland, ACNW Consultant

SUBJECT: Comments addressed to the ACNW

DATE: April 4, 1992

APR 13 1992

This memorandum is intended for the ACNW only. I ask that you please distribute it as soon as possible, since illness has precluded an earlier communication of these comments.

At the conclusion of the March 13, 1992 session of the 41st ACNW meeting, Dr. Moeller asked for written comments to be sent to him. Below are some of my major comments concerning those presentations and discussions regarding the HLW Research Program. In these I concentrate on only issues which are of potential concern. These comments are to be forwarded to Dr. Moeller and the rest of the ACNW as soon as possible. Of course, I am available for amplification or clarification if the members so wish.

- The HLW program clearly presents a formidable task to the staff in part because it has demands from a wide variety of technical areas. In view of the complexities, the staff has done a good job, in my opinion, of integrating and various aspects into an overall program described in the draft of NUREG 1406.
- The issue arises in many instances as to specific research objectives in connection with generic and site-specific research. There seems to be an attempt to remain generic, although clearly many aspects point directly toward Yucca Mountain issues. They simply should be candid because some issues really are about Yucca Mountain.

However, it would have been refreshing indeed to see a more generic approach to the issue of repository site were explicit. Indeed, it seems to me that a more global approach might yield important information which otherwise may not be apparent. For example, studies on the tectonic, volcanologic, hydrologic, etc. regimes of Yucca Mountain should characterize the entire region and thus reveal areas with given degrees of suitability with regard to specific phenomena. Indeed, one could ask the question whether alternate sites would be preferable or even apparently acceptable should Yucca Mountain prove to be fatally flawed. For example, it seems likely that it would be possible to identify sites with a probability of

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volcanic disruption many orders of magnitude less than for Yucca Mountain. If early performance assessment (is this called IPA?) indicates that this is critical, perhaps a nearby site would merit some consideration.

I do not know the full history of the selection of Yucca Mountain as a specific site but it was obviously made at an "early" stage. It seems ironic that the actual choice of the exact site probably was made with a much, much less firm understanding of the region and that the planned activities may narrowly focus on this isolated area. It would indeed seem unfortunate if Yucca Mountain appears to be unsuitable but that there is a suitable (but unidentified due to narrowly focussed site characterization) site in the region.

- There are several instances for which the research appears to be more "reactive" than "proactive" and thus dependent upon DOE ultimate decisions. Some examples are the canisters and their seals, systems engineering, and waste package issues. It will be difficult to anticipate the DOE plans so it seems advisable to adopt a more generalized strategy which focuses on key links. It would seem to matter little who finds particular good or bad container materials, for example, so that NRC research may at the same time provide important information rather than merely to verify particular expectations of DOE at the present time. Perhaps this is merely an issue of stress, but it is a point which was raised during the oral presentations of the CNWRA review last June and stressed, as I recall, by Dr. Steindler.
- The HLW staff has now adopted several important natural analogs into the research agenda which in my opinion, is very good. DOE does not seem to be leading in this area and it is one which is surely to provide important information particularly for the all-important time and space dimensions. At the same time, I wonder if the document would be improved with more attention to the specific tasks and goals of the natural analog studies. At the workshop sponsored by NRC-CNWRA last summer which I attended, it seemed to be the general consensus that natural analog studies are very important but that they must be defined with specific and clear objectives and expectations.
- The issue of  $^{14}\text{C}$  vapor transport appears to be one that is weak in the present document.
- The matter of research prioritization was questioned at length during the HLW staff presentation. It seems clear that the staff is sensitive to, and cognizant of, the importance of making wise decisions when money and staff are limited. At the same time, it is clear that priori-

ties are inevitably influenced by the backgrounds of those charged with making decisions. It is only human nature that staff expertise will be disproportionately emphasized in setting priorities. This also extends to the CNWRA influence upon the research agenda. Basically, as I see it, NRC staff outlines a SOW and CNWRA helps refine it in a process which might be characterized as "negotiation." The actual research then will be at least partly predestined toward the technical expertise of the staff involved.

- In my view, the HLW program has a limited staff in some areas, particularly in some geologic areas. As a result a few staff members must cover wide ranging issues. From my perspective, the geologic concerns are vital at this stage in terms of site suitability when compared, for example, to concerns of waste packages. More scientific staff in key geologic areas would, of course, be helpful. From my knowledge of CNWRA, a similar bias may exist. Clearly, this impacts prioritization as pointed out above.
  
- A related issue is the question of how the staff can proceed to initiate research on a given issue. At present, it seems that the focus of all research is through the CNWRA. I see the need to have a mechanism by which other researchers might be utilized for two reasons. The first is the expertise of personnel. To illustrate, who is able to address issues related to long-term climatic conditions in the Yucca Mountain region? The second concerns built in bases for geologic issues. Specifically, it is acknowledged that there is unlikely to be a single, universally accepted model for tectonic and volcanologic phenomena so that various geologic models will need to be evaluated. As a result, various "schools" will probably not be well represented or favored with a limited staff. A mechanism for incorporating the scientific opinions of those strongly advocating alternative models is desirable. After all, the duplication and evaluation of data by different scientific groups has long been an important aspect of systematic scientific inquiry.