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Radioactive Waste Isolation in Salt: Peer Review of the Texas Bureau of Economic Geology's Report on the Petrographic, Stratigraphic, and Structural Evidence for Dissolution of Upper Permian Bedded Salt, Texas Panhandle

D. F. Fenster, R. Y. Anderson, S. Gonzales,
V. R. Baker, D. E. Edgar, and W. Harrison

ARGONNE NATIONAL LABORATORY
Energy and Environmental Systems Division

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RADIOACTIVE WASTE ISOLATION IN SALT:
PEER REVIEW OF THE TEXAS BUREAU OF ECONOMIC GEOLOGY'S
REPORT ON THE PETROGRAPHIC, STRATIGRAPHIC, AND
STRUCTURAL EVIDENCE FOR DISSOLUTION OF
UPPER PERMIAN BEDDED SALT, TEXAS PANHANDLE

by

D.F. Fenster, R.Y. Anderson,* S. Gonzales,†
V.R. Baker,** D.E. Edgar, and W. Harrison

Energy and Environmental Systems Division
Geoscience and Engineering Group

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U.S. DEPARTMENT OF ENERGY
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MASTER

FOREWORD

Documents are being submitted to the Salt Repository Project Office (SRPO) of the U.S. Department of Energy (DOE) by Battelle Memorial Institute's Office of Nuclear Waste Isolation, by the Texas Bureau of Economic Geology (TBEG), and by other contractors to satisfy milestones of the Salt Repository Project of the Civilian Radioactive Waste Management Program. Some of these documents are being reviewed by multidisciplinary groups of peers to ensure DOE of their adequacy and credibility. Adequacy of documents refers to their ability to meet specified legal or regulatory requirements. Credibility of documents refers to the validity of the assumptions, methods, and conclusions, as well as to the completeness of coverage.

Since late 1982, Argonne National Laboratory has been under contract to DOE to conduct multidisciplinary peer reviews of program plans and reports covering research and development activities related to siting and constructing a mined repository in salt for high-level radioactive waste. The present report summarizes Argonne's review of TBEG's report entitled *Petrographic, Stratigraphic, and Structural Evidence for Dissolution of Upper Permian Bedded Salt, Texas Panhandle*.

Argonne was requested by DOE to review the report on January 5, 1984 (see App. A). The review procedure involved obtaining written comments on the report from three members of Argonne's core peer review staff and three extramural experts having broad experience in the fields of salt dissolution, structural geology, regional stratigraphy, basin analysis, geomorphology, and petrography. The peer review panel met at Argonne on February 7, 1984. Reviewer comments were integrated into the present report by the review session chairman, with the assistance of Argonne's core peer review staff. All of the peer review panelists concurred in the way in which their comments were represented in this report (see App. B), a draft of which was sent to SRPO on February 27, 1984.

PREVIOUSLY PUBLISHED REPORTS IN THE SERIES

"RADIOACTIVE WASTE ISOLATION IN SALT"

- ANL/EES-TM-242** Peer Review of the Office of Nuclear Waste Isolation's Geochemical Program Plan (Feb. 1984)
- ANL/EES-TM-243** Peer Review of the Office of Nuclear Waste Isolation's Socioeconomic Program Plan (Feb. 1984) (revised July 1984)
- ANL/EES-TM-246** Peer Review of the Office of Nuclear Waste Isolation's Plans for Repository Performance Assessment (May 1984)
- ANL/EES-TM-254** Peer Review of the Office of Nuclear Waste Isolation's Reports on Preferred Repository Sites within the Palo Duro Basin, Texas (June 1984)
- ANL/EES-TM-256** Special Advisory Report on the Status of the Office of Nuclear Waste Isolation's Plans for Repository Performance Assessment (Oct. 1983)
- ANL/EES-TM-258** Peer Review of the Office of Nuclear Waste Isolation's Plan to Decommission and Reclaim Exploratory Shafts and Related Facilities (July 1984)
- ANL/EES-TM-259** Peer Review of the Office of Nuclear Waste Isolation's Final Report on the Organic Geochemistry of Deep Groundwaters from the Palo Duro Basin, Texas (Aug. 1984)

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A microfiche copy of the following unpublished report is attached to the inside back cover of this report: T.C. Gustavson, S.D. Hovorka, and D.A. McGookey, *Petrographic, Stratigraphic, and Structural Evidence for Dissolution of Upper Permian Bedded Salt, Texas Panhandle*, University of Texas at Austin, Bureau of Economic Geology (1983).

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The credentials of panel members are summarized in App. C.

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**Review panel chairman and member of core peer review staff.

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SUMMARY OF RECOMMENDATIONS

The following recommendations for improving the Texas Bureau of Economic Geology (TBEG) report entitled *Petrographic, Stratigraphic, and Structural Evidence for Dissolution of Upper Permian Bedded Salt, Texas Panhandle* have been abstracted from the body of this review report. The TBEG report should be revised to conform to one of the following alternatives:

1. If the report is intended to be a review or summary of previous work, it should contain more raw data, be edited to give equal treatment to all types of data, and include summary tables and additional figures.
2. If the report is intended to be a description and interpretation of petrographic evidence for salt dissolution, supported by collateral stratigraphic and structural evidence, the relevant indirect and direct data should become the focal point of the report.

The following recommendations apply to one or both of the options listed above.

1. The text should differentiate more carefully between the data and inferences based on those data.
2. The authors should retain the qualifiers present in cited works. Statements in the report that are based on earlier papers are sometimes stronger than those in the papers themselves.
3. The next revision should present more complete data.
4. The authors should achieve a more balanced presentation of alternative hypotheses and interpretations. They could then discuss the relative merits of the alternative interpretations.
5. More attention should be given to clear exposition.

1 INTRODUCTION

The U.S. Nuclear Regulatory Commission (1982) states in Sec. 3.1.3 (Geomorphic Processes) of 10 CFR Part 60 that site characterization reports for potential sites for nuclear waste repositories (cf. site characterization plans required by the Nuclear Waste Policy Act of 1982) should "describe any geomorphic process that could affect the ability of the site to isolate radioactive waste." This section also states that site characterization reports should contain information useful "for understanding present ground-water systems or predicting future changes (e.g., salt dissolution or collapse breccias)" or information needed for "estimating the potential occurrence of future [geomorphic] processes." The U.S. Nuclear Regulatory Commission (1983, Sec. 60.122[c][10]) siting criteria document states that potentially adverse conditions at a site include "evidence of dissolution such as breccia pipes, dissolution cavities, or brine pockets." The U.S. Department of Energy (DOE) (1983, Sec. 960.4-2-6) considers dissolution to be an important, potentially disruptive geologic process that must be taken into account in evaluating the suitability of sites for repository development. Finally, discussions pertaining to past, present, and future dissolution will form an important part of the environmental reports required by the Nuclear Waste Policy Act of 1982 (Sec. 213[b]) that will be used in evaluating the suitability of possible repository sites.

The Argonne-conducted peer review of *Petrographic, Stratigraphic, and Structural Evidence for Dissolution of Upper Permian Bedded Salt, Texas Panhandle* by T.C. Gustavson, S.D. Hovorka, and D.A. McGookey of the Texas Bureau of Economic Geology (TBEG) involved obtaining written critiques of the report from three members of Argonne's core peer review staff and three extramural experts in relevant research areas. Panelists did not contact TBEG personnel, and none of the panelists have been involved in any programs sponsored by DOE or directed by TBEG such that their participation in the review could be construed as a conflict of interest.

No specific guidance was provided to Argonne by DOE on how to conduct the review (see App. A). The following questions were prepared by Argonne to assist in the review process:

1. Does the report clearly differentiate between the raw data and inferences based on those data?
2. Are the stated conclusions supported by the available data?
3. Are the stated conclusions unique interpretations or are other interpretations equally valid?

Reviewer comments were integrated into the present report by the review session chairman, with the assistance of Argonne's core peer review staff.

2 MAJOR CONCERNS OF THE REVIEW PANEL

2.1 PURPOSE

The TBEG report entitled *Petrographic, Stratigraphic, and Structural Evidence for Dissolution of Upper Permian Bedded Salt, Texas Panhandle* contains some original and valuable information related to salt dissolution. However, the purpose, or objective, of the report is not stated explicitly.

- Did the authors intend for the report to be a review or summary of the evidence for dissolution, based on general observations and with frequent references to previous work, for the purpose of demonstrating the timing and extent of dissolution? or
- Did the authors intend for the report to describe and interpret the petrographic evidence for both dissolution and the stages of dissolution, with supporting collateral stratigraphic and structural evidence?

Although the title suggests that the primary purpose of the report is synthesizing the various types of evidence for dissolution, much of the text is written as a summary and relies on previous research. If the report is intended to be primarily a summary of previous work, its title and abstract should be recast and the text edited to achieve a more balanced presentation. For example, the discussion of petrographic evidence is far more detailed than the discussions of other types of evidence. Also, too many of the supporting data are found in other documents for the report to function well as an independent document. Summary statements supported by summary tables and additional figures are needed. For example, the sequence of events related to dissolution could be summarized in one table. In addition, data concerning the timing, extent, and rate of dissolution could be tabulated, with a short accompanying explanation in the text.

Revision is also required if the report is primarily a description and interpretation of petrographic evidence for dissolution, supported by collateral stratigraphic and structural evidence. In particular, the discussions of the data indicative of salt dissolution should be expanded. For example, the section on petrographic data should be similar to that contained in Hovorka (1983a, b). The sequence of diagrams is original and clear in the Hovorka reports (1983a, Fig. 39; 1983b, Fig. 47). If descriptions and figures from these reports were incorporated into a revision, the petrographic material could serve as a focal point. Because the concept of a petrographic sequence of dissolution is interesting and may not have been developed completely in previous publications, information and interpretations related to this concept should not be scattered throughout a general review or summary report. The discussion of structural data should also be expanded. For example, on page 18 the authors should support their statement that Cretaceous rocks were affected by dissolution. Also, some of the apparently unpublished information developed by Budnik and referred to in the report could be summarized.

One way to resolve the problem of the two apparently conflicting objectives in the current draft is to publish two separate reports. The first could be quite detailed and written to fit the second of the two alternatives listed above. The first report could be followed by a summary or review type of report that draws on previously published information. Both should be carefully documented.

2.2 DATA AND INFERENCES

Although the stated criterion for interpreting subsurface dissolution (i.e., "abrupt or complete truncation of salt beds between adjacent wells without change in thickness or character of adjacent non-salt strata") appears to be reasonable, the authors often fail to distinguish between direct observations and conjectural interpretations. For example, geologic cross sections and zones of dissolution are interpreted from geophysical logs from widely spaced oil and gas wells (see Fig. 15; also see Gustavson et al., 1980). The inferred evidence of dissolution in particular wells is used to produce a map of salt dissolution zones (see Fig. 1). Given the apparent absence of direct evidence of dissolution in the logged wells, relying on such a process is not unreasonable. However, the report should make clear that the locations of the zones are not based on direct observations but on inferences drawn from geophysical logs from widely spaced wells.

Another example of mixing observations and inferences occurs in the section describing petrographic evidence for halite dissolution. The presence of relatively coarse halite crystals is clearly documented on page 8 and in Fig. 2. The interpretation of the authors is that the original smaller halite crystals were recrystallized in the presence of saturated brine. The authors then jump to the conclusion that halite-saturated groundwater caused recrystallization 100 ft below the dissolution front. However, recrystallization could also have resulted from diagenetic changes unrelated to dissolution. Also, a mechanism for brine migration is not discussed. The text gives the impression that microscale dissolution is occurring in advance of a dissolution front beneath the entire area of the Southern High Plains. Further comments regarding recrystallization are given in Sec. 2.4.

The authors again confuse observations with inferences when they ascribe the origin of porosity to dissolution of halite (p. 8). Is this statement supported by observations or is it an inference based on pertinent data?

By not clearly differentiating between data and inferences, the authors weaken the references-cited aspect of the report. For example, the text documents the occurrence of gypsum-filled fractures in outcrop or drill core and relates the fractures to a model for dissolution-induced formation of fractures. However, the cited reference (Goldstein, 1982) considers this model in a conceptual manner only and provides no evidence to support its validity. The TBEG report refers to Goldstein's hypothesis as though it were proven.

Earlier TBEG publications provide the basis for most of the reviewed report. The cited publications use wording like "is probably responsible for," "there is insufficient evidence to determine," "it appears that," "probably," "may have occurred," and so on. In the reviewed report, paraphrases of statements in the earlier publications leave out the

necessary qualifiers, which gives the reader the impression that the evidence is stronger than it is. The failure to distinguish between direct observations and interpretations may derive from the summary nature of the report as discussed in Sec. 2.1.

2.3 PRESENTATION OF DATA

The data as presented in the TBEG report are incomplete. For example, the addition of a stratigraphic column would be helpful to the reader, as several stratigraphic units named in the text do not appear in any figure. In addition, Fig. 1 does not give county boundaries and names even though several features mentioned in the text are described as being located in a particular county. Other omissions are more serious and affect the technical credibility of the report.

Because the original boundaries of deposition of Upper Permian salt are never established, the report fails to provide a proper foundation for subsequent discussions of the extent of dissolution. It should be clear to the reader that the uppermost Permian salts extended into areas of the Rolling Plains where dissolution effects are observed. McGillis and Presley (1981) should be cited.

The terms "alteration," "diagenesis," and "dissolution" are discussed in nearly the same context, with no clear distinction being made between alteration and dissolution. This ambiguity could lead to the erroneous impression that alteration means dissolution. Much of this confusion could have been avoided if the concept of a sequence of dissolution had been developed more carefully. The text does relatively little to help the reader distinguish the relative significance of syndepositional dissolution, dissolution associated with diagenesis, and postdiagenetic dissolution. It is certainly conceivable that these three modes of dissolution had impacts of different orders of magnitude.

Water-quality data, including solute (salt) load values, support the contention that dissolution is ongoing. However, discussions of post-Permian dissolution, relative dissolution through time and space, and location and surface effects of dissolution zones are not usually supported by data presented in the report. The reader must consult referenced TBEG publications to determine the types of data used, to see what interpretations and deductions were made, and to check whether the inferences and conclusions are supported by the available data. In some instances, the conclusions drawn appear to be speculative because the evidence is inconclusive. This is not to say that the conclusions are necessarily incorrect or that they are not the result of logical deduction. It simply means that the conclusions are necessarily tentative and not usually based on direct observations. As a result, the discussion of changes in dissolution rates, both in the text and in the summary arguments, appears arbitrary and weak.

The hydration of anhydrite should be clearly distinguished from dissolution. Some dissolution does accompany hydration, but this process is so universal within 100-150 ft of the surface that it should be considered part of the halite dissolution process only when it is directly associated with halite. This point should be clearly stated in the discussion of the dissolution sequence on page 10.

As stated in Sec. 2.2, zones of dissolution are inferred from geophysical logs from widely spaced boreholes. A revised draft should include additional basin analysis data indicating that the available evidence precludes a facies change toward wells where salt is absent. The authors' argument would be stronger if evidence of dissolution could be obtained from wells where salt is missing.

The report does not present sufficient documentation for readers to determine whether stream alignment coincides with areas of salt thinning in the subsurface. Even more crucial is determining whether salt dissolution caused the stream alignment. A structure contour map of the base of the Ogallala Formation would be helpful in this regard.

Also, the reader has insufficient information to determine whether the present stream solute load can be attributed to dissolution along the Caprock Escarpment or in the more central portions of the Palo Duro Basin. The authors should provide additional information or should refer more extensively to the published literature.

Finally, the credibility of the report is compromised by referring to unpublished data (e.g., analyses of structure contour and salt isopach maps by Budnik) without a supporting discussion.

2.4 ALTERNATIVE HYPOTHESES AND INTERPRETATIONS

Too much reliance on convergence of circumstantial evidence gives the TBEG report an unwarranted aura of finality. A revised draft would be more effective if it achieved a more balanced discussion of alternative hypotheses and interpretations. The authors could then state that the most probable of the listed alternatives was supported to a greater extent by the available data.

A balanced discussion is vital to establishing a sequence of dissolution events. For example, the authors state on page 7 that the rate of dissolution must have accelerated, arguing that all of the salt would have been removed if present rates had obtained since the Cretaceous. In our view, climatic, diagenetic, and other changes could have affected the dissolution rates.

The idea is interesting that the first stage in the dissolution process is recrystallization and void filling and that these processes penetrate quite deeply into bedded salt. However, recrystallization (actually reprecipitation) in itself does not prove the existence of an advancing dissolution front. It only indicates that undersaturated fluids have moved through a formation. Such fluids could have been derived from the stratigraphic units themselves. Similar large single crystals of halite occupying void space or replacing anhydrite were encountered in the ERDA 6 and WIPP 11 cores at the Waste Isolation Pilot Plant site in New Mexico (Anderson, 1979). Investigators did not agree on the source of the responsible fluids — some of them believed that the brines were squeezed from adjacent salt beds and others suggested that meteoric waters had penetrated the evaporites.

In the Delaware Basin, large crystals occur in voids associated with dissolution but also in structures associated with salt deformation. In the Palo Duro Basin, the voids are not related to deformation, and it is more certain that voids of this dimension are associated with a moving dissolution front. Still, it might be prudent to state that recrystallization is not always related to a dissolution front.

On a related subject, the authors' interpretation of why multifaceted dolomite rhombs are present (p. 9) is not clear. Either their arguments must be stated more completely, or data presented by Naiman et al. (1983) must be summarized.

As discussed in Sec. 2.2, the authors ascribe porosity to dissolution of halite (p. 8). However, some of the reduction in volume could have resulted from the conversion of gypsum to anhydrite, a process that has been documented for other evaporite sequences (e.g., Loucks and Longman, 1983). Panelists questioned whether the authors can show conclusively that the porosity in the various lithologies is due solely to dissolution of halite. Also, partial dissolution related to hydration of anhydrite to gypsum is cited on page 10 as the probable source of gypsum cement and fibrous gypsum in fractures. This argument could be presented more effectively as the most probable among several alternatives.

The authors define their criteria for identifying evidence for salt dissolution as opposed to facies changes on pages 12-15. McGillis and Presley (1981, p. 11) also present reasonable criteria to distinguish a facies change from a dissolution edge and conclude that "...the present salt distribution was not depositionally controlled but results from subsequent dissolution." McGillis and Presley identify a geophysical log pattern that they interpret as the edge of the original salt. The authors should cite this reference but also note the original qualifications. Again, the authors must discuss alternative hypotheses and state reasons for their argument being the most probable.

Fractures filled with fibrous gypsum are discussed at several places in the report. As discussed in Sec. 2.1, the authors ascribe the origin of these fractures to a dissolution/collapse model. Because slickensided fractures and faults with small displacements are ubiquitous in the midcontinent region, discussion of alternative hypotheses including differential compaction is warranted. Most, but not all, of the gypsum vein fillings show evidence of a medial scar, a feature important to interpreting the origin of the fillings. Veins that lack a medial scar are neither discussed nor explained. If another mechanism might be responsible, some additional discussion would seem warranted.

Finally, the absence of collapse breccias, sinkholes, and collapse chimneys is not necessarily evidence of slow dissolution and subsidence (p. 23). One alternative explanation is that active dissolution is not occurring beneath the Southern High Plains.

3 TECHNICAL PRESENTATION

3.1 WRITING STYLE

If the TBEG report is to function well as a summary report, some statements should be worded less strongly until the authors have synthesized all the supporting data. For example, the interpretation on page 15 concerning 100 m of salt dissolution is too unequivocal for a document containing insufficient supporting data.

In several places, the writing is difficult to follow. In other places, the intent of the authors' statements is not clear (e.g., p. 14, lines 7-16). A different writing style would help make distinctions clearer. Instead of writing that "data are interpreted as," the authors should write that "data could be interpreted in the following ways." If alternative hypotheses were included, the authors could then show why a certain alternative seems more probable.

The need for greater clarity in writing can be illustrated by the following examples. First, are the streams draining the Southern High Plains (p. 10) the same streams as those previously cited as containing a high solute load based on U.S. Geological Survey data (p. 7), or are they a subpopulation of those streams or different streams altogether? Second, the paragraph at the top of page 23 indicates that post-Permian dissolution has resulted in structural collapse of overlying strata. Although such structural collapse has been clearly documented for the Tertiary and the Quaternary, and in one localized area for the Late Cretaceous, it has not been demonstrated on the evidence presented thus far for the Triassic through Early Cretaceous. Gradual subsidence has been ascribed to these geologic time intervals, but for areas outside the main Texas Panhandle. Thus, a somewhat more accurate statement relative to structural collapse over geologic time is needed.

3.2 ILLUSTRATIONS

More detailed comments on figures are given in Sec. 4.1. However, the following examples illustrate the types of concerns expressed by the panelists.

1. **Figure 1.** Panelists found Fig. 1 to range from partially useful to relatively useless. The boundaries of the physiographic provinces are not clear, and the pattern of the geologic units does not correspond to the locations of zones of dissolution. There are nine letters for six boreholes. Finally, the locations for the three photos should be keyed to figure numbers.
2. **Figure 7.** This photo illustrates the occurrence of gypsum-filled fractures in the Yates Formation, but their relationship to the proposed underlying dissolution zone is not established. Medial scars are present in some but not all gypsum veins. What is the explanation for their absence?

3. **Figure 15.** The pattern of salt dissolution shown on the cross section is a viable interpretation. However, with distances of 15-20 mi between wells, the extent and location of the dissolution zones are suspect. This figure will not dispel, for most readers, the possibility of facies changes or salt pinchouts.

3.3 REPORT ORGANIZATION

At times, the report seems to be focusing on the Southern High Plains. At other times, it seems to be summarizing all of the evidence for dissolution in the Texas Panhandle. The text shifts back and forth between different geographical settings, subtly suggesting that features and processes indicative of dissolution in one setting also pertain to other settings, when in fact they may or may not. The authors would also appear to imply that dissolution comparable to that evidenced in the Rolling Plains and Canadian River Valley has affected or is affecting the Southern High Plains or, in particular, the Palo Duro Basin. While the authors do present some evidence for dissolution in this latter area, it is less direct than would be desirable. Evidence of dissolution is documented in two physiographic provinces; reorganization would help keep these data separate.

Another source of difficulty with respect to the report's organization is the ambiguous purpose of the document (see Sec. 2.1). If the petrographic evidence approach is chosen as the main theme, the report has several problems. The exposition of the petrographic evidence and the dissolution sequence is too brief, disorganized, and cryptic to do the topic justice. While the authors have some good primary information to share and observations to make, the present treatment does not spell out the dissolution sequence clearly. Distinctions are not made between syndepositional, postdepositional, and posterosional (post-Permian) dissolution. Also, some of the details surrounding use of the sequence are confusing. For example, the section on petrography (pp. 8-11) states that the stage of dissolution is a function of exposure to groundwater, that earlier stages of dissolution occur at the base of the dissolution zone, and that later stages were sampled closer to the surface. However, the discussion of petrographic evidence (pp. 16-17) indicates that later stages of dissolution penetrated further into the salt.

The stratigraphic and structural evidence as presented is largely a review of previous work. However, the authors chose not to include or even cite any of the published cross sections, nor do they refer to the individual geophysical logs that show the response to dissolution of salt. In particular, the authors' case for a stratigraphic-structural response to dissolution would be strengthened if the "before/after" illustration in Hovorka (1983b, Fig. 46) were incorporated.

In short, all of the above items could provide background information to support the reasonableness of the petrographic evidence. Such items should logically appear before the discussion on petrography rather than as part of a general review. Similarly, the section on outcrop evidence could be presented earlier as part of the section on the geologic setting.

The treatment of regional differences in style, development, and timing of dissolution (pp. 15-21) should be reserved for a separate paper. At present, comparative information about the extent and progress of dissolution in cores from areas of active and less active dissolution is lacking. The information that is presented is scattered through the report and is not well indexed to specific cores and localities.

If the authors should decide to discuss the extent and timing of dissolution, the reviewed report would have to be reorganized and some primary information presented. The discussion should focus on comparing the development of dissolution textures (fabrics) and dissolution residues in the several cores. Regional differences should be evaluated by comparing the active areas of dissolution beneath the Rolling Plains to the less active areas beneath the Southern High Plains. The rate and stage of development of dissolution in the Southern High Plains should be described, as should the abundant and well-recognized evidence for dissolution in the surrounding Canadian River Valley and the Rolling Plains. By not clearly referencing the cores and relating the dissolution evidence to these two physiographic provinces, the reader may infer that the entire area is equally involved in the process. Each location should be discussed separately.

Figures 9, 10, and 17 are evidence of hydration or diagenesis. While such processes may be associated with dissolution, they are not direct evidence of halite dissolution. Figures 2, 5, 42, and 43 from Hovorka (1983a) could be used to illustrate dissolution but would be more convincing if accompanied by expanded descriptions of available information related to dissolution in the cores, including descriptions of residues and breccias. These data could then be compared with data from cores in areas undergoing more active dissolution.

4 PAGE-BY-PAGE COMMENTARY

4.1 FIGURES

<u>Figure</u>	<u>Comment</u>
1	Figure 1 is only partially useful as drawn. Although borehole locations are lettered, there is no explanation as to which letters represent which boreholes. Also, nine letters appear on the figure, while the text discusses six boreholes. Instead of indicating photo numbers on the figure, it would be more useful to key those locations to the corresponding figures (i.e., Nos. 12, 13, and 14). Although names are given to physiographic provinces, their boundaries are not clear. Because counties are referred to throughout the text, their boundaries should be shown here or in a separate figure. It would also be helpful for readers to know the locations of the structural basins and other important geologic features mentioned in the text; however, at some point, Fig. 1 will have too much information.
2	The figure and the core (and subsequent cores) should be indexed to the locations in Fig. 1. Add the letter mentioned in the caption to the figure, and consider identifying by another letter the irregular contact between the coarse-grained halite and the mudstone host. Also, if halite recrystallization in the No. 1 Sawyer core is so dramatic that it is the focus of discussion on page 8, why was the No. 1 Grabbe core used for Fig. 2?
3	The authors should consider labeling the irregular wavy laminations. Also, if all the material shown is supposedly dissolution residue and the unusual dolomite grains a distinctive diagenetic product, why are the grains apparently concentrated in a fairly narrow band in the upper half of the figure, namely near the main mudstone-anhydrite contact. Dolomite also appears to be present in the anhydrite areas. How can one discern whether the mineral is a dissolution residue or a diagenetic replacement for anhydrite? In other words, dolomite could be a diagenetic replacement mineral for both halite and anhydrite.
4	Figure 4 should come before Fig. 3, because Fig. 4 apparently shows the precursor. However, is it warranted to state so categorically that Fig. 4 shows the precursor?
5	The captions for Figs. 5 and 6 are reversed. Is the multifaceted dolomite that is reported to surround the mudstone clasts supposed to be visible? Also, it is difficult to tell in which portion of the figure the fine light bands (dolomite in gypsum cement) are actually located. The mudstone, clasts, gypsum bands, and dolomite, if visible, should be identified by letters.

FigureComment

Wavy laminations in evaporite sequences can also result from successive development of nodular anhydrite. Such laminations might appear similar to some of those in Fig. 5. An explanation is needed of why the wavy laminations discussed in the text and in this and other figures could only be the result of dissolution.

If insoluble residues resulting from dissolution are largely anhydrite and mudstone, and these residues can be distinguished from ordinary anhydrite by their wavy bedding, why is the core that is used to illustrate these points described as being composed of gypsum? The caption of Fig. 5 is yet another example of failing to differentiate between observations and interpretations. It might be better to indicate that the accumulation is postulated to be an insoluble residue and that the residue is believed to have formed by dissolution of a mudstone-halite mixture.

- 6 The wavy contact between the accumulated insoluble components (dark area) and the salt (light area) is evident. If one compares this figure to Fig. 5 and, to a degree, to Fig. 2, the wavy laminations are supposed to indicate dissolution and consequent accumulation of insoluble residue. However, the material being dissolved in the cored intervals shown in the other figures is a mixture of halite and mudstone, whereas the dissolved material in Fig. 6 is relatively clean salt (halite). How can dissolution of fairly different materials (salt versus a mixture of mudstone and halite) result in the same kind of wavy lamination or contact, at least in the context of accretion as discussed in lines 5-8 on page 9?

Is this a tilted breccia block? What are the lineations in the mudstone?

- 7 Fractures and expanded bedding planes filled with fibrous gypsum are discussed on page 12. The medial scars generally exhibited by gypsum of this type are not evident in the figure. If such medial scars are not present, what is the explanation for their absence?
- 8 The lithologies of the clasts should be labeled. This breccia supposedly provides evidence of a cavern over a vertical interval of some 30 ft (see p. 9). On what basis is the reader to accept the 30-ft interval? Figure 8 shows only that a breccia has been cored.
- 9 The vague wording of this caption and the lack of orientation markings tend to obscure what the authors are trying to portray. The caption states that the vertically oriented fibrous fabric of the fracture-filling gypsum possibly reflects the stress field during fracture opening. How does this orientation relate to that of the medial scar, which has been cited as documentation for crystal growth in response to fracture propagation?

FigureComment

- 10 The argument in the caption for Fig. 10 that no apparent volume increase means that gypsum must have partially dissolved is alright. However, the gypsum roses in the figure are not peculiar to a halite dissolution front. The authors need to distinguish between processes that take place at a halite dissolution front and diagenesis and alteration in general.

The authors should not imply that hydration usually takes place with no volume change and accompanying dissolution. A volume change is probably the more usual response.

- 11 Figure 10 appears to illustrate a case where volume considerations dictated that dissolution accompany hydration of anhydrite to gypsum. Figure 11 shows a case where hydration of anhydrite to gypsum and the accompanying volumetric expansion may have contributed to the formation of fractures, which allowed additional fluids to penetrate. If additional fluids penetrated openings like fractures and bedding planes, why didn't dissolution accompany hydration?

The caption to Fig. 11 states that both vertical fractures and bedding planes are involved. Which of these is illustrated here? In other words, what is the orientation of the photomicrograph? Finally, the expansion comment is unneeded.

- 12, 14 Figure 12 corresponds to the caption for Fig. 14; Fig. 14 goes with the caption to Fig. 12. In the caption for Fig. 12, the breccia bed is supposedly located in Caprock Canyons State Park. Gustavson et al. (1980, Figs. 14 and 18) indicate that this feature is located on the west side of the Canadian River valley. Which location is correct? According to the caption labeled as being for Fig. 14, the collapse chimney is located north of Borger. According to Gustavson et al. (1980, Fig. 14), this feature is located southwest of Borger.

- 15 The edge of dissolution in the stratigraphically higher salt in the Artesia Group (Salado Formation) does not quite match the edge of the Salado Formation in Fig. 1? The stratigraphic terminology should be consistent.

- 16 The several discussed features (euhedral gypsum crystal, calcite replacement, and mudstone matrix) should be identified by letters. Because there is no foundation for the statement "and may represent diagenesis during the Mesozoic," it should be omitted.

- 17 The pseudomorphs of gypsum crystals (P) and the crystalline dolomite are not identified by letter on the photomicrograph.

- 18 Could the rosette of microquartz represent replacement after calcite? The features discussed in the caption should be identified by letters on the photomicrograph.

FigureComment

- 19 The contorted fabric (C) is not well illustrated in the photomicrograph. Also, the relationship between the two lengths of core is not clear from the caption. An appropriate photomicrograph should be added of the other core mentioned in the caption.

Did the deformation of soft sediment occur during the Permian?

4.2 TEXTPage(s)Line(s)Comment

1	9-15	Is all petrographic evidence confined to the San Andres Formation or is this the formation in which local recrystallization was observed? If the latter, delete the reference to the formation. The other features mentioned are not keyed to specific formations or units of formations.
2	13	In what sense are the collapse events "catastrophic"? In size, loss of life, rate of occurrence, or widespread nature? Are these events comparable to the rapid draining of glacial Lake Missoula and formation of Channeled Scablands or the eruption of Mt. St. Helens?
2	14	Does "less affected" mean fewer in number, slower rate, smaller in size, or less surface expression?
4	9, 15	For consistency with the report title, replace the various basin names with the term "Texas Panhandle." The references cited in line 10 cover more than the two basins named, and the three basins listed in line 15 embrace the region of interest.
4	11-13	Figure 1 shows nine core locations but does not give core names or an index. This omission makes it very difficult to evaluate regional differences. The abstract and the text should be revised to emphasize the dissolution sequence.
4	20-23	The addition of a regional index map and stratigraphic column would permit shortening this section by approximately one-half to two-thirds.
5-6	10-27; 1-18	The discussion here centers on the Palo Duro Basin; no information is given about depositional conditions in the other basins of the Texas Panhandle. Since the title of this section is "Geologic Setting of the Texas Panhandle," some modification appears warranted.

<u>Page(s)</u>	<u>Line(s)</u>	<u>Comment</u>
6	4, 12	What is the reference "Presley, this volume"?
6-7	27; 1-4	This discussion of the Southern High Plains should probably mention the Caprock Escarpment.
7	7	The solute load given in the referenced manuscript is 1,600,000 tons annually. Here, it is given as 1,300,000 tons. Which is correct? On page 19, the total of the three drainage basins (which cover the same region as that cited on p. 7) is 1,650,000 tons. On both pages, the U.S. Geological Survey is cited, but the years are not the same. These differences need to be resolved. Furthermore, on line 22 of page 20, the 1,300,000-ton figure is introduced again. Here, however, the reference is Gustavson and Finley (in press) and not the U.S. Geological Survey. Does this value represent another value, a repeat of the one on page 7, or something else?
7	7-8	The U.S. Geological Survey citation in the reference list is for 1969-1977, not 1964-1977.
7	8-12	Sinkholes and open fractures are generally the result of active dissolution (i.e., ongoing and modern). Collapse chimneys and breccias, which are discussed elsewhere in the report, are the result of past dissolution (i.e., Tertiary and Quaternary but before present). Making this distinction would seem advisable.
7	13-17	This sentence begins by stating that it is not known if dissolution is active beneath the Southern High Plains. Later, the authors appear to conclude that dissolution is continuing slowly in places beneath the Southern High Plains. Whichever is true, the evidence supporting the notion of dissolution beneath the Southern High Plains should not be labeled as "widespread." The undue emphasis, whether implied or otherwise, tends to be misleading.
7	18	Whatever is known about the original presence and distribution of salt needs to be added to the introductory paragraphs before the heading "Petrographic Evidence of Halite Dissolution."
8	1-4	Petrographic analyses of cores may be a unique source of data, but they do not provide unique confirmation of conclusions derived from noncore evidence. Core petrography may furnish supporting evidence, but interpretation is still involved. The phrase "unique confirmation of these conclusions" seems overly strong.

<u>Page(s)</u>	<u>Line(s)</u>	<u>Comment</u>
8	5	<p>The section "Petrographic Evidence of Halite Dissolution" needs revision, expansion, and clarification. The sequence of dissolution steps should be spelled out at the outset. Each step might then be used as subheadings in the ensuing discussion. How many steps are there and in what order do they occur? What is their geometric relationship to a dissolution front? Much of the treatment of Hovorka (1983a, b) could be used, along with diagrammatic sketches. Syndepositional, cyclic-depositional, and posterosional types of dissolution should be distinguished, with appropriate figures.</p> <p>The authors tend to overgeneralize and extrapolate to all evaporites. Generalizations should be confined to the Texas Panhandle.</p>
8	19	The verb "restores" is correct only if the porosity was there before. Do the authors mean "increases"?
8-9	14-25; 5-8	The discussion implies that dissolution is dominated by the vertical component; however, other TBEG publications stress a strong lateral component in present processes, with only minor changes in the vertical. If this difference is real, what are the conditions causing this difference? Is the explanation of wavy bedding surface in lines 5-8 on page 9 the only plausible origin of such features?
8-9	21-25; 1-8	Discussion of the dolomite crystals should be in a separate paragraph. Does the dolomite occur in all evaporites? Are other examples cited in the literature? How has it been established that this type of dolomite is unique and is therefore evidence of diagenetic replacement of halite.
9	3-8	The sentences pertaining to insoluble residues should be in a separate paragraph; alternatively, the topic should be considered for a subheading. Figures from Hovorka (1983a, b) should be added. A diagrammatic illustration could show the development of residues and breccias in the different cores.
9	11-12	"Small faults and bedding-plane fractures are abundant only in rocks above a dissolution zone." This statement is not always accurate.
9	15	"Breccias are composed of all lithologies which overlie halite." Such statements should be limited to the particular occurrence.

<u>Page(s)</u>	<u>Line(s)</u>	<u>Comment</u>
9	22-24	Again, such generalizations should be made more specific.
9	27	Do the gypsum crystals form in the mudstone or in the mud?
10	9	"Hydration of anhydrite to gypsum" could be a subheading. Also, the theoretical value for the change in volume appears to be about 64%, not the 30% cited. Anderson and Kirkland (1966, Pl. 4, Fig. 2) illustrate a 50% increase.
10	14-15	The statement that "dissolution of gypsum is also documented in clasts in breccias" should be supported with a figure and some additional information.
10	17	Why is "pressure solution" needed? Can't dissolution between gypsum clasts occur just by contact with fluids? Is there any evidence?
10	20-23	The whole question of volume change and volume loss is troubling. Why are they needed to account for gypsum dissolution?
10	24-25	The dehydration of gypsum to form anhydrite is somewhat rare and restricted to hot climates, high brine concentrations, etc. References to other examples in the literature would be helpful.
11	6-8	More detail is needed here. Indicate which cores showed evidence of certain stages, the extent to which the stages were developed, the depth of penetration below the surface, the vertical extents of the stages, etc. This information should be indexed to a figure showing the locations of the cores.
11	14-15	What is the observable evidence in outcrop of the former presence of salt?
11	20-22	The text describes the breccia in Fig. 12 as containing blocks of gypsum cemented with gypsum. The caption to Fig. 12 states that the breccia is composed of anhydrite blocks. Which mineralogy is correct?
11-12	9-25; 1-22	The references to collapse chimneys are confusing. What subsurface information can be cited to demonstrate the position of a chimney? What subsurface information can be cited to identify the presence or former presence of salt caverns associated with those chimneys? Are these modern features, or were they formed during the geologic past?

<u>Page(s)</u>	<u>Line(s)</u>	<u>Comment</u>
11-12	20-25; 1-4	The use of the terms "thick" and "long" is confusing. The text refers to a cavern visible in the outcrop but not clearly visible in the photograph. What is the proposed relationship between the breccia in Salado-equivalent strata and the existing cavern that can't be seen? What is the basis for determining the time of formation of the exposed breccia to establish a cause-and-effect relationship with the cavern? What is the meaning of "...to accommodate the anhydrite breccia"? For the breccia to form and be preserved, its entire vertical dimension would not have to be within the confines of the cavern as implied by this statement.
12	17-20	Delete this sentence.
12	23	Use the opening paragraph to establish the original continuity of the salt.
13	3	Replace "flat" with "low relief."
13	12-25	Incorporate this paragraph into the discussion of petrography and syndissolution.
13-14	27; 1-2	This statement needs to be supported by data from specific logs.
14	3	The phrase "every known case of" is too general. Be more specific.
14	9-12	Use Hovorka's (1983a) correlation diagram of "dissolved-undissolved" here.
14	17-23	Omit this paragraph unless some examples can be cited.
15	2-3	The statement that salts are undergoing dissolution across the entire basin is not strictly correct. According to McGillis and Presley's map (1981, Fig. 7), the southernmost part has not been involved.
15	7-13	Additional details are needed, including descriptions of the evidence and interpretations of the observations and core locations.
15-16	21-25; 1-5	Indirect evidence and direct observations should be presented to substantiate estimates of the amount of salt lost. Furthermore, what steps were taken in deducing the presence of a 10-m cavernous interval based on the cored megabreccia sequence?

<u>Page(s)</u>	<u>Line(s)</u>	<u>Comment</u>
16	14-16	The necessary background for this statement has not been laid.
16	12-16	Inferred differences in the rates of dissolution in different areas must be questioned. Does it necessarily follow that rapid dissolution must be accompanied by collapse, whereas slower dissolution will not? What are the possible reasons for such differences in rates? Is it possible that insoluble residues or materials might be thicker in areas showing evidence of dissolution but not collapse?
16-17	18-25; 1-12	This paragraph should be incorporated into the section on petrography.
17	1	The text includes the Dewey Lake Formation as one in which carbonates diagenetically replace sulfates. The caption to Fig. 16 omits this formation. Which statement is correct?
17	3-4	The phrase "other carbonate units where halite cement was dissolved" needs clarification. What units? Where? References may be needed.
17	5-8	What represents early and late stages of dissolution? How can late-stage dissolution penetrate farther than early-stage dissolution when the dissolution sequence was defined the other way around?
17	8-12	The term "carbonate-dominated diagenesis" is not defined. A citation is needed or the topic should be omitted.
17	13-22	What subsurface evidence is available to show that collapse accounts for the increased thickness of basal Dockum sediments? Is it possible that irregularities in thickness could be related to deposition on a pre-Dockum erosional surface?
		The section "Stratigraphic evidence" is too long to be parallel to the section on petrography and too short for adequate treatment of regional differences. This topic might better be the subject of a second paper. Perhaps the timing, extent, and rates of dissolution could be put into a table that includes a summary paragraph?

<u>Page(s)</u>	<u>Line(s)</u>	<u>Comment</u>
19	10-13	Specific basins should be mentioned, or the appropriate publications should be cited. Does this statement conflict with the one on page 7 that says that it is not known if dissolution is active? The inference here is that the closed basins beneath the Southern High Plains, even though less widespread ("less" is not quantified and probably should be) and only similar (not identical) to those beneath the Northern High Plains, also owe their origin to dissolution. Because this contention appears to be open to question, the paragraph should be modified.
20	2-14	The data and methodology used to determine the proposed relationship between physiographic features and dissolution zones should be discussed. If cause-and-effect relationships are proposed, the rationale and evidence needed to determine what is cause and what is effect must be clear.
20	15-26	<p>What evidence and deductions can be given to support an increased rate of dissolution during the Tertiary and Quaternary as compared with earlier times? Calculations of modern rates of dissolution based on solute loads of streams draining the region will vary if slightly different assumptions are made. For example, the rates of present vertical and lateral dissolution published in related TBEG reports are based on the inferred positions of active dissolution zones as interpreted from geophysical well logs and stratigraphic relationships. Thus, the position of these zones shown on cross sections is based principally on indirect evidence. If these inputs change, the area of the drainage basins estimated to be the source of these solute loads will change as will the calculated rates of migration of the dissolution zones. Extrapolated over a long period of time, these differences would be measured in miles of lateral migration and perhaps hundreds of feet of vertical dissolution.</p> <p>Also to be considered is the salt contribution to the total solute load of local and regional streams resulting from the exploration for and production of oil and secondary recovery activities involving oil field brines. Such activities should be examined as a possible solute input.</p>
21	15	Change "Palo Duro Basin" to "Texas Panhandle."
21	23-24	How is it known or interpreted that the multifaceted dolomite is a late-stage diagenetic mineral? The previous discussion on page 9 says nothing about the timing of mineral formation. Why is this concept introduced in the conclusions?

<u>Page(s)</u>	<u>Line(s)</u>	<u>Comment</u>
22-23	27; 1-4	The authors imply that all post-Permian dissolution has resulted in structural collapse of overlying strata. While structural collapse has been clearly shown for the Tertiary and the Quaternary, and for the Late Cretaceous in one localized area, it has not been demonstrated for the Triassic through the Early Cretaceous. Gradual subsidence has been ascribed to this geologic time interval, but for areas outside the main Texas Panhandle. Thus, the situation with regard to structural collapse over geologic time should be stated more accurately.
23	13	"Palo Duro Basin" should probably be changed to "Texas Panhandle."
23	13-16	The absence of collapse breccias and other collapse features does not necessarily document slow dissolution and subsidence beneath the Southern High Plains. One alternative explanation is that active dissolution is not taking place.
23	18	Only one remnant cavern was discussed, that is, the one detected in the core from the No. 1 Sawyer well. If other remnant caverns can be inferred from the core data, they should be mentioned on either page 10 or page 16, not at the end of the report.

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- U.S. Department of Energy, *General Guidelines for Recommendation of Sites for Nuclear Waste Repositories*, 10 CFR Part 960, draft (Nov. 18, 1983).
- U.S. Nuclear Regulatory Commission, *Disposal of High-Level Radioactive Wastes in Geologic Repositories; Licensing Procedures*, Code of Federal Regulations, 10 CFR Part 60 (June 30, 1983).

23/24

U.S. Nuclear Regulatory Commission, *Standard Format and Content of Site Characterization Reports for High-Level-Waste Geologic Repositories*, Regulatory Guide 4.17 (July 1982).

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APPENDIX A

U.S. DEPARTMENT OF ENERGY LETTER REQUESTING PEER REVIEW

27/28

Department of Energy
National Waste Terminal
Storage Program Office
505 King Avenue
Columbus, Ohio 43201

RECEIVED	
W. HARRISON	
ASSOC. DIR. EEO DIV.	
(GEORGETOWN AND ENG. DEPT.)	
JAN - 6 1984	
ACTION	_____
COPIES	_____
COMPLETED	_____

January 5, 1984

Dave Fenster
EES-362
Argonne National Laboratory
9700 South Cass Avenue
Argonne, Illinois 60439

Dear Mr. Fenster:

SUBJECT: REVIEW OF TBEG REPORT "PETROGRAPHIC, STRATIGRAPHIC, AND STRUCTURAL EVIDENCE FOR DISSOLUTION OF UPPER PERMIAN BEDDED HALITE, PALO DURO BASIN, TEXAS" BY T. C. GUSTAVSON, S. D. HORVORKA, AND D. A. MCGOOKEY

DOE requests your assistance in performing a technical review of the subject report. The paper was submitted to NPO at the end of June, 1983, and as is evidenced by the current date, the review process and subsequent resolution have lasted longer than we would like. The initial comments, sent on November 1, 1983 pointed out inadequacies of the report, and we requested a final draft from TBEG. Responses to comments and a rewritten manuscript were received at the beginning of December, 1983. The conflict which has arisen seems to be based upon the writing style, content, and documentation. NPO is committed to accurate scientific reporting and clear statements of opinion versus fact. We are seeking an objective review from an independent party in hopes of resolving these conflicts; please consider these factors in the reviewing procedure.

Included in the package are three copies each of: the original report, initial review comments, responses to the comments from the authors, and a rewritten version of the report. We will send higher quality prints (see Figures 2 through 14 and 16 through 19) as soon as reproductions can be made.

Any comments regarding the report would be appreciated on or before February 3, 1984. There will be a meeting in the NPO office in Columbus on February 3; at this time any discrepancies or problems with the paper will be discussed by reviewers and authors. The attendance of your designated reviewers will be appreciated. Once you have decided on the reviewer(s), please call me or Jay Jones (FTS 976-5916) to inform us of your selection. If you have any further questions, do not hesitate to call.

Sincerely,

Thomas A. Baillieu

Thomas A. Baillieu
Acting Director
Site Exploration Office
NMTS Program Office

NPO:JGJ:0833A

cc: P. Doyle, NPO
L. McClain, NPO
W. Newcomb, ONMI
D. Ballmann, ONMI

GS#243-84

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APPENDIX B
CONCURRENCE SHEET

APPENDIX B

CONCURRENCE SHEET

I concur that the Argonne National Laboratory report on TBEG's manuscript entitled *Petrographic, Stratigraphic, and Structural Evidence for Dissolution of Upper Permian Bedded Salt, Texas Panhandle* fairly represents my comments, where incorporated, to the peer review panel.

Roger Y. Anderson

David F. Fenster

Victor R. Baker

Serge Gonzales

Dorland E. Edgar

Wyman Harrison

APPENDIX C
CREDENTIALS OF PEER REVIEW PANEL MEMBERS

Roger Y. Anderson

University of Arizona: B.S., Geology (1954)

University of Arizona: M.S., Geology (1955)

Stanford University: Ph.D., Geology (1960)

Dr. Anderson is Professor of Geology at the University of New Mexico, where he has been a member of the faculty since 1956. From 1979 to the present, he has been a WAE staff scientist with the U.S. Geological Survey's Branch of Pacific-Arctic Marine Geology. Also from 1979 to the present, he has served as a consultant on the Waste Isolation Pilot Plan project for the New Mexico State Environmental Evaluation Group. From 1975 to 1979, Dr. Anderson served on the peer review panel for the Waste Isolation Pilot Plant project.

Recipient of numerous research grants, principally from the National Science Foundation and Sandia National Laboratories, Dr. Anderson has published 45 articles in journals and has presented numerous papers at professional meetings and conferences. As consultant to the New Mexico State Environmental Evaluation Group, he wrote a series of reports related to dissolution of evaporites. He is also the author of three consulting reports for Sandia National Laboratories, the latest of which was entitled *Deep Dissolution of Salt, Northern Delaware Basin, New Mexico*.

Victor R. Baker

Rensselaer Polytechnic Institute: B.S., Geology (1967)

University of Colorado: Ph.D., Geology (1971)

Dr. Baker has been Professor of Geosciences at the University of Arizona since 1981. From 1982 to the present, he has held a joint appointment at that university as Professor of Planetary Sciences/Lunar and Planetary Laboratory. From 1971 to 1981, Dr. Baker served as Assistant and then as Associate Professor in the Department of Geological Sciences at the University of Texas at Austin. In 1979-1980, he was a Fulbright-Hays Senior Research Scholar at Australian National University. Prior to 1971, he was City Geologist for Boulder, Colo., and Geophysicist and Geophysicist-Hydrologist for the U.S. Geological Survey.

Dr. Baker has served on numerous technical and advisory committees, study groups, and panels. Author of approximately 80 research papers, plus numerous reports, reviews, abstracts, and encyclopedia articles, he is a member of 11 professional societies and has been active on their committees. He has served as a consultant to governmental agencies and private firms on problems related to flood hydrology, river erosion, engineering geology, and geoarchaeology.

Dorland E. Edgar

Central Missouri State University: B.S., Geology (1968)

Colorado State University: M.S., Geology (1973)

Purdue University: Ph.D., Geology (1976)

Dr. Edgar joined the Geoscience and Engineering Group of the Energy and Environmental Systems Division of Argonne National Laboratory in 1978. Since that time he has worked as a geologist and hydrologist on programs related to waste management and energy and mineral resources development. From 1981 through 1983, he participated in studies of the geologic setting of crystalline rocks of the northeastern and Lake Superior regions of the United States for the purpose of assessing their suitability as sites for a high-level radioactive waste repository. His primary areas of responsibility on this project were surface-water and groundwater hydrology, geomorphology, and surficial geology.

From 1978 to 1981, Dr. Edgar was affiliated with Argonne's Land Reclamation Program and Environmental Control Technology Program, where he studied the relationships between surface mining and reclamation activities, and geomorphic processes, hydrology, water quality, and erosion and sedimentation. Dr. Edgar also served as a U.S. Department of Energy representative to an interagency group that reviewed comments and drafted revised regulatory guidelines for the U.S. Office of Surface Mining.

Before coming to Argonne, Dr. Edgar was employed at Oak Ridge National Laboratory, where he conducted research on surface and subsurface hydrologic and geologic conditions, and their relationship to the shallow land disposal of low-level radioactive waste. One project involved the study of the hydrologic and geomorphic processes involved in transporting radionuclides from burial sites through an instrumented watershed. Dr. Edgar's graduate research was directed primarily toward the relationships between hydrology and the geomorphic processes operating within alluvial stream channels and drainage basins.

Dr. Edgar has published approximately 25 scientific and technical publications, and is a member of two professional societies.

David F. Fenster

City College of the City University of New York: B.A., History (1967)
University of Illinois: M.A., History (1968)
Queens College of the City University of New York: M.S., Geology (1975)
Certified Professional Geologist, No. 4668, American Institute of
Professional Geologists
Certified Professional Geologist, No. 85, State of Indiana

Mr. Fenster joined the staff of the Geoscience and Engineering Group of the Energy and Environmental Systems Division of Argonne National Laboratory in 1982. He is currently Technical Project Manager and member of the multidisciplinary core peer review staff, which is reviewing technical documents for the Salt Repository Project Office of the Civilian Radioactive Waste Management Program. He also served as a technical reviewer for parts of Argonne's Lake Superior regional report related to siting repositories for high-level radioactive waste in crystalline rocks.

Prior to coming to Argonne, Mr. Fenster was Project Geologist with Dames & Moore, Park Ridge, Ill. He had been with Dames & Moore since joining the staff of the Cranford, N.J., office in 1974. During his tenure with Dames & Moore, Mr. Fenster acquired extensive experience in seismotectonics, structural geology, radioactive and hazardous waste disposal, regional geology, engineering geology, hydrogeology, and general geologic field mapping. For example, Mr. Fenster was Principal Investigator for Stratigraphy for the Generic Environmental Impact Statement concerning disposal of radioactive waste in bedded salt, granitic rocks, and argillaceous formations for Oak Ridge National Laboratory. He also worked on a revised draft (unpublished) of a national screening document concerning selecting sites for repositories in crystalline rocks for isolating high-level radioactive waste.

Mr. Fenster also worked on geologic investigations related to nuclear power plant siting and licensing. He is familiar with U.S. Nuclear Regulatory Commission review and licensing procedures, having worked on early site reviews, preliminary safety analysis reports, final safety analysis reports, and detailed fault investigations.

Mr. Fenster has published on the siting of repositories for high-level radioactive waste, midcontinent tectonics, and structural geology in the Northeast, and has worked on more than 30 consulting reports. He is a member of five professional societies, including the Association of Engineering Geologists.

Serge Gonzales

Duke University: A.B., Geology (1958)

Miami University: M.S., Geology (1960)

Cornell University: Ph.D., Stratigraphy (1963)

Certified Professional Geologist, No. 1914, American Institute of Professional Geologists

Registered Geologist, No. 110, State of Georgia

Since 1982, Dr. Gonzales has been a full-time consultant, serving as President of Earth Resource Associates, Inc. From 1978 through 1981, he was Associate Director of the Institute of Natural Resources at the University of Georgia, where he also was Associate Professor of Geology. From 1971 to 1978, he was Staff Geologist at the Institute of Community and Area Development, also at the University of Georgia, with a joint appointment as Assistant Professor of Geology. While Assistant Professor for the Department of Geology at Miami University (Oxford, Ohio) from 1966 to 1971, Dr. Gonzales worked part-time as a consulting geologist for Earth Science Laboratories, Inc. Working as a petroleum geologist for EXXON Company from 1963 to 1966, he specialized in offshore exploration and development geology.

Dr. Gonzales has had extensive experience in the areas of applied geology and mineral resources. In particular, he has expertise in mined-land reclamation, subsurface storage of hydrocarbons, and land burial and subsurface disposal of hazardous and radioactive wastes. He has assessed various evaporite deposits as sources of salt and other minerals, and as sites for waste disposal, with particular emphasis on natural dissolution phenomena.

Dr. Gonzales is a member of six professional societies and has published more than 100 articles and reports, many of which relate to the geologic disposal of radioactive waste and related regional and rock-type assessments.

Wyman Harrison

University of Chicago: S.B., Geology (1953), after three years of undergraduate work at Stanford University

University of Chicago: S.M., Geology (1954)

University of Chicago: Ph.D., Geology (1956)

Registered Geologist, No. 2476, State of California

Certified Professional Geologist, No. 134, American Institute of Professional Geologists, and No. 487, State of Virginia

Dr. Harrison is Associate Director for Geoscience and Engineering for Argonne National Laboratory's Energy and Environmental Systems Division. He directs a 25-person group that performs analytical and experimental studies related to management of energy and mineral resources and to development and deployment of related technologies. Major activities of the group include (1) acquisition of geophysical and geotechnical data bases, (2) analysis of the data of geoscience to support design and deployment of energy technologies, and (3) development of physical and mathematical models of geophysical/geotechnical systems.

Dr. Harrison's group recently completed comprehensive surveys of geoscience data pertaining to crystalline rock complexes in the northeastern and Lake Superior regions of the United States to help assess their potential as possible sites for repositories for high-level radioactive waste. Dr. Harrison has conducted numerous other geological and geotechnical studies at Argonne, ranging from estimating the petroleum resources of selected basins in the Soviet Union to determining near-shore circulation in Lake Michigan.

From 1971 to 1975, Dr. Harrison was Professor of Geography (Associate Department Chairman) at the University of Toronto, where he specialized in geophysical studies related to slope stability in sedimentary terrains and the siting of supertanker ports. Prior to that, he was Associate Director for Physical, Chemical, and Geological Oceanography at the Virginia Institute of Marine Science and a Professor of Marine Science at the University of Virginia. Dr. Harrison was Director of Environmental/Science Services Administration's (now National Oceanic and Atmospheric Administration's) Land and Sea Interaction Laboratory from 1964 to 1968. Before that he was on the faculty of Dartmouth College's Department of Geology and a geologist with the Indiana Geological Survey.

An author of over 100 papers, reports, reviews, and books, Dr. Harrison was made Senior Scientist at Argonne in 1976.

**Department of Energy
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NOTICE TO READERS

At the request of the Salt Repository Project Office, Argonne National Laboratory conducted a review of a Texas Bureau of Economic Geology (TBEG) draft report: "Petrographic, Stratigraphic and Structural Evidence for Dissolution of Upper Permian Bedded Halite, Palo Duro Basin, Texas"* by T.C. Gustavson, S.D. Hovorka, D.A. McGookey. Argonne was asked to objectively review the paper in order to resolve conflicts between the authors and previous reviewers. Concern was expressed about the presentation, content, and documentation in the initial review. Although the report is a worthwhile contribution to studies of salt dissolution, it was felt that technical accuracy and alternative interpretations should be incorporated into the text. The review panel pointed out suggested changes which would improve the report and provide for a more defensible document.

The Argonne review comments have been provided to the authors. These comments will be useful in the preparation of a revised document to be submitted as an internal TBEG publication.

Jo Ann Sherwin
Chief
Site Evaluation
Salt Repository Project Office

*A microfiche copy of the reviewed report is attached to the inside back cover of this report.