Attorney General of New Mexico

PO Drawer 1508
Santa Fe, New Mexico 87504-1508

505/827-6000
Fax 505/827-5826

TOM UDALL
Attorney General

MANUEL TIJERINA
Deputy Attorney General

January 20, 1995

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Mr. Robert Bills
U.S. Department of Energy
Carlsbad Area Office
P.O. Box 3090
Carlsbad, New Mexico 88221

Dear Bob:

This letter contains comments on the December 15, 1994 draft paper on **Non-Salado Flow and Transport** (the "draft"), presented at the Carlsbad SPM meeting on January 9, 1995.

The subject matter of this paper clearly calls for expert assistance in understanding and commenting upon it. DOE has not provided such assistance, although it has been requested.

Comments on the general subject are contained in our December 29, 1994 comments, on the Compliance Status Report. Other comments are as follows:

1. The paper lists its objectives (draft at 1-2) and states that "[o]bjective 4 will be addressed after the positions regarding conceptual and mechanistic models are finalized." The paper is therefore incomplete, making comment very difficult. Please advise when objective 4 -- identification of conceptual and computational models and appropriate parameter values for use in the second iteration of the SPM -- will be addressed. Please provide a copy of the position paper which addresses those issues, and hold a stakeholder meeting to receive comments.

2. The comment that the features, events and processes to be considered are those described in the scenario development position paper does not illuminate much (draft at 2-1). That paper, which is itself under revision, mentions various FEPs, screens some out, says that some may be screened out later based on arguments not yet developed, and leaves others in. The draft needs to state clearly, naming them, exactly which scenarios are considered to be included in PA. The discussion at pages 2-1 and 2-2 does not do this.

3. It is said that vugs are an important part of Culebra porosity (at 3-10). How, if at all, are vugs modeled as an element

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of the flow model? If they are not explicitly modeled, how are they accounted for?

4. The clay fraction in the Culebra is disputed and affects retardation factors applicable to radionuclide transport. EEG has shown that the data supporting any particular clay percentage are inadequate. (See EEG comments on Compliance Status Report, Nov. 1994, at 6-9). The claims as to clay content must be withdrawn (draft at 3-10, lines 20-28).

5. The draft refers to a mean thickness of the Culebra of 7.7 meters (at 3-11). Should not a thickness value be derived from data specifically concerning the travel path of released radionuclides? On the thickness issue, Table 2-2 cannot be located.

6. The draft says that two "conceptual models" of non-Salado groundwater flow exist (at 3-17). In fact, as "conceptual model" is defined in the SPM glossary and in general, the confined aquifer and groundwater basin models are not different conceptual models but are mathematical models designed to represent flow and transport in different areas, but adhering to the same conceptual model, is this not so?

7. What is the document cited as "Swift et al., 1994" on page 3-12?

8. It is said that in the groundwater basin model differences in elevation of the water table generate the gradients to drive groundwater flow (at 3-18). Are other driving gradients, such as density differences, represented in the model? Which gradients are represented?

9. There is reference (at 3-18) to pumping tests, geochemical data, and hydrologic modeling studies relating to vertical flow through Rustler confining units. Please provide citations to the pertinent materials.

10. It is said that flow models have not considered vertical flow through areas of nonuniform infiltration, such as sinkholes (at 3-19, lines 33-39). Given the presence of sinkholes in the vicinity as reported, what will be done to model such flow and to determine its importance?

11. The draft states that three-dimensional simulations show that the current different flow directions in the Magenta and the Culebra can be explained as a product of regional heterogeneity and vertical flow through the Tamarisk (at 3-21, -22). Please demonstrate how this is so or cite the source so demonstrating. An understanding of the site hydrology, including the different flow

directions, is important to a compliance demonstration. This theoretical explanation should be set forth so that it can be evaluated.

12. The draft says that the three-dimensional numerical representation of the groundwater basin model (SECO_3D) is currently being developed (at 3-22). Is the model available for computer exercises by outside groups, such as our office, or EEG?

13. Please explain what hydraulic conductivity data are employed in the groundwater basin model, as described in text on page 3-24 (lines 3-17). What plans exist to develop additional field data to support such model? What plans exist to represent localized features disrupting confining layers?

14. There is reference to evaluation of changes in groundwater flow due to climate changes, subsidence over potash mines, and shallow boreholes (draft at 3-24) and to a specific simulation of flow fields at two different recharge rates (at 3-25). What are the published reports concerning such studies? If no reports are published, please provide the materials supporting the claims in the draft.

15. What plans exist to study whether a shift in flows to the west may result in shorter travel times (as discussed at 3-25)?

16. References or supporting data should be provided for the statements concerning a transient simulation of the effect of climate change on flow patterns (at 3-25, lines 17-22) and of the effect of climate change on the Dewey Lake saturation zone (at 3-25, lines 30-39).

17. What is the impact on flow through the Dewey Lake of the increase in saturated thickness described at 3-25, lines 33-39?

18. The draft states that results from the three-dimensional model suggests that the apparent inconsistency between hydrogeological and geochemical data as to flow patters can be reconciled (at 3-29, lines 17-22; 3-30, lines 27-35). Please provide information supporting such assertion.

19. Claims are made about the frequency of encounter of Castile brine reservoirs (at 3-31). What support is there for the implication that in oil and gas drilling operations a brine reservoir will be detected if encountered and will be recorded if detected?

20. The draft states that Castile brine volume shall in future calculations be limited to 1 x 10^7 m^3 maximum (at 3-33). The 1992 PA is said to have used a volume of 2 x 10^8 m^3. I have looked

at the 1992 PA, volume 3, and do not find a fixed volume. Rather, in Table 4.3-1 these reservoir parameters appear (units in meters)

	median	range		distribution type
radius, equiv.	2.32×10^2	3×10^1	8.6×10^3	constructed
thickness	1.2×10^1	7	6.1×10^1	uniform

The reference is given as Reeves et al., 1991. These values would appear to generate a maximum in excess of 1×10^7 m^3. (However, the ranges were not sampled values.) DOE must better justify use of a maximum value (with unstated values for minimum and mean) of 1×10^7 m^3 based on figures in a 1983 report, in preference to figures based on a study in 1991. DOE must discuss in this connection all the available data on Castile brine occurrences, such as the data in EEG-17.

21. Concerning the asserted error in the storativity range used in the 1992 PA (storativity being a sampled value) I have looked at the publications reflecting the asserted error and do not see the support for the asserted value of 13,469 m^3. Please explain specifically how an error was made and corrected.

22. The limitation of the number of reservoirs to four is unacceptable (draft at 3-33). The May 5, 1994 presentation on brine reservoirs at the DOE-EPA technical exchange pointedly omitted to state that the TDEM data showed four reservoirs. Al Lappin then stated that "[a] surface-based geophysical study, correlated with known stratigraphies and presence/absence of brine at holes WIPP-12, DOE-1, and ERDA-9, indicates that a high-conductivity zone may be present above the top of the Bell Canyon Formation under a <u>portion</u> of the WIPP waste-emplacement panels." That is the limit of the data. The most sensible course of action, and the only one defensible in light of the data, is to assume that each hole penetrating the Castile encounters a brine reservoir, and that each such reservoir is separately pressurized, given the evidence of the isolation of such phenomena.

23. The discussion of multiphase flow raises the question of sources of gas other than the repository and the Castile. Is it possible that gas would be introduced from formations below the Castile, and, if so, what probability should be assigned? What pressures may exist?

24. What transient effects upon a borehole seal might be caused by the escape of pressurized gas? More broadly, even if the presence of gas in the brine flowing through the Culebra might in itself inhibit radionuclide transport, as was said on January 9, is there any respect in which it is not conservative to ignore a possible encounter with gas, other than gas in the repository?

25. Transmissivity values have been conditioned only with the use of data up to the year 1989. There are considerable post-1989 data on transient events, which should also be used to calibrate the fields. See the paper by David Snow, Nov. 9, 1994, attached hereto.

26. "Transmissivity is assumed to be constant over time." (draft at 3-35). This is unacceptable. Climate change or ongoing processes may change the hydrologic characteristics of the Culebra. See the paper by Roger Y. Anderson, attached hereto. It is neither scientifically realistic nor conservative, and thus not defensible, to insist that the hydrologic characteristics of a rock body will not change during the time span of interest.

27. Storativity data are admittedly sparse (draft at 3-35), and storativity is inaccurately represented as spatially invariant (draft at 3-35). Transient situations are inaccurately represented, and it is not known whether such inaccuracy is conservative. Further, DOE has stated that porosity is insufficiently known to characterize its spatial variability (Compliance Status Report, §2.1.2.6.2).

28. Further, the use of a constant storativity value in calibrating transmissivity fields affects the definition of transmissivity and thus the calculation of velocities in constant-flow conditions. There is reference to possible further work to "solve for both the transmissivity and storativity fields" (draft at 3-36, lines 8-9). What does this refer to, and how will it work? Until it is done, why should the transmissivity fields be considered defensible?

29. It is said that variable density flow considerations will not affect groundwater flow directions (Beauheim presentation, Jan. 9, 1995). We do not have the references cited and will study them, but it seems unlikely that they examine the importance of density gradients using a model that incorporates the real-world factors of fluctuating recharge, return to primitive (pre-intrusion) conditions, regional dip, varying bed thickness, and possible climate change. Has the impact of density flow been effectively isolated for study?

30. The text says that Davies (1989) concludes that ignoring density-driven flow is not acceptable in the southern portion of the WIPP domain (draft at 3-37). How, then, can it be defensible to do so?

31. Previously, PA's have ignored the effect of introducing fluid from an intrusion borehole into the Culebra. The draft says that an increase in the head in the borehole area can be implemented (at 3-37). Will this be done, and, if not, why not?

32. The discussion of double-porosity flow omits to mention the existence of a large area in the southern and southwestern part of the site, where there are inadequate data to characterize flow and transport. Further, it appears that a seven-well tracer test will be conducted at some location on the site, although we do not have the current plan. Such testing may provide the needed data. Since the tests will be done, apparently, regardless of the outcome of the SPM-2 study, because they are deemed needed, the draft should discuss them. At the May 3-5, 1994 technical exchange Rick Beauheim proposed that field tracer tests be conducted in accordance with the following plan:

1. Design and execute tracer tests that focus on specific key assumptions in existing and alternative transport models:

   → Matrix diffusion (double-porosity and alternative models)
   → Anisotropy (double-porosity model)
   → Flow/transport channeling (alternative model)
   → Vertical heterogeneity in Culebra (all models)
   → Scaling effects (all models)

2. Examine field-scale chemical retardation (using reactive tracers) in the same sequence of tests to provide the technical basis for extrapolation of lab-scale chemical retardation tests to the field scale.

3. Run test sequence in phased approach with pretest predictions for 2nd and 3rd phases based on previous results to maximize information gained from model analysis and for rigorous model testing/validation.

Is it the project's position that such tests are needed to come up with a defensible model of Culebra flow and transport?

33. At the same meeting Rick Beauheim stated the following objectives of proposed new tracer tests:

1. prove importance of matrix diffusion

2. address vertical heterogeneity (through hydraulic testing)

3. provide sufficient data to verify existence of hydraulic conductivity tensor and anisotropic approach to test interpretation

4. provide data to distinguish among alternative conceptual models--heterogeneous and anisotropic double-porosity models, channeling models, and discrete-fracture models

5. provide defendable ranges of values for important parameters

6. provide information on transport over an order-of-magnitude range in scales

7. address concerns about tracer-injection technique

8. provide data on chemical-retardation processes and properties within the Culebra

9. obtain core samples from field tracer-test site to allow comparison of laboratory and field transport studies

Does the project agree that these objectives all relate to concerns that must be resolved to develop a defensible model of Culebra flow and transport? Please explain any disagreement and state how these concerns may be resolved by other means.

34. As the draft states, the model parameter of matrix block size is not well characterized (at 3-38). Also, the model is used to develop figures for fracture porosity and other parameters within the SWIFT-II model of continuous, isotropic, radial flow in cubically fractured medium; thus, other parameters in the model are not correctly characterized.

35. Is it accurate or conservative to model the Culebra as an orthagonally fractured medium, when fractures do not appear to take that form, in fact, and in reality fractures of different orientation are characterized by different length and different mineral deposits (draft at 3-39, lines 21-25)?

36. At the January 9 meeting it was said that for baseline modeling fracture porosity would be fixed at 0.001, matrix porosity would be fixed at a "mean" value of 0.15 based on "core measurements," tortuosity would be given a "medium low measured value" of 0.08, and free-water diffusion coefficients were derived from "laboratory measurements." These values are not reflected in the draft. Please explain how they were derived, and provide the data underlying them. How can values for these parameters be determined from laboratory tests, if the parameters in facts have a distinct meaning as defined in the SWIFT-II model?

37. It has been previously noted that the data available do not exclude channeling behavior; this model should form the baseline. The draft does not assert otherwise (at 3-41).

38. At the May 3-5, 1994 technical exchange Rick Beauheim reported the feedback by INTRAVAL on tracer test results as follows:

1. Agree that matrix diffusion in a double-porosity framework may be a significant process at WIPP.

2. Feel that conceptual model of uniform matrix-block size (fracture spacing) along individual travel paths but with different average block sizes along different travel paths is <u>not</u> a physically realistic model.

   → An alternative conceptual framework for this model may be different effective surface areas for matrix diffusion along different travel paths. However, this conceptualization may be strongly scale dependent.

3. Agree that conceptual model based on anisotropic transmissivity is a viable model. However, consideration should be given to testing this model with an additional multi-well test configuration and alternate pumping wells, thereby providing better geometrical constraints.

4. Feel that alternative conceptual models that incorporate transport channeling should be used to examine existing tracer test data and that additional field tracer-tests should be considered to differentiate alternative physical transport models.

Does the project position differ from these statements? If so, please state how the project believes that these concerns have been resolved.

39. At the May 3-5, 1994 meeting it was stated that a channeling model has the following potential impacts:

1. Tracer transport along multiple fracture channels with varying transport efficiency may be capable of producing breakthrough curves similar to those observed in WIPP tests:

   → significantly different breakthrough behavior along different travel paths;

   → breakthrough curves with sharp peaks and tailing on the falling limb.

2. Present tracer data is insufficient to determine whether or not channeling flow/transport occurs in Culebra Dolomite.

3. If transport in Culebra occurs as fracture channeling with no matrix diffusion, this would eliminate a significant amount of the potential physical and chemical retardation.

4. If transport in Culebra occurs as fracture channeling with matrix diffusion, there is significant uncertainty in how much surface area will be available for matrix diffusion.

Does the project position agree with these statements? If not, please explain why not.

40. At the January 9 meeting it was said that the effects of channeling are subsumed in the low value of specific surface used in the baseline model. Please explain in detail how this is so. The draft states that fracture channeling may result in up to a factor of 10 increase in radionuclide release relative to uniform flow over entire fractures planes (at 3-41, lines 13-14). Does the proposed baseline have that effect?

41. At the meeting on January 9 we were told that dispersion has been found to be relatively unimportant. Please provide the data supporting such assertion. It will be important to know what model was used in such calculations.

42. What data underlie the choice of 100m as the baseline parameter for dispersion, as stated in the January 9 meeting?

43. The draft states that no experimental data for actinides exist on physical retardation due to matrix diffusion (draft at 3-47, lines 8-9). Does the draft mean to imply that experimental data exist on chemical, as distinguished from physical, retardation, and if so what data?

44. There is a description of the fracture characteristics of the Culebra (subhorizontal fractures with "adjacent" clay concentrations; high-angle fractures without clay) (draft at 3-48, lines 29-40; 3-49, lines 1-10). What is the source of these statements (other than Beauheim & Holt (1990))? Please provide the data relied upon, if unpublished.

45. What is the data referred to as "observations" of flow data (draft at 3-39, lines 12-14)? Please provide the data, if unpublished.

46. The baseline assumption is that advective flow occurs through the high-angle fracture set only (draft at 3-50). It is more accurate to say that clay associated with fractures is discounted?

47. The draft says that a weighted distribution coefficient is used, calculated from expert panel estimates of K_ds. (draft at 3-50). This is not defensible. The expert panel estimates are not supported by data.

48. The draft also refers to the "baseline conceptual model, with K_d =0" (at 3-51). Is the baseline model that K_d =0 or not? What is the role in the baseline of the stated position that the project will "take no credit for adsorption" (draft at 3-52)?

49. A linear isotherm model of adsorption is not conservative or defensible for any actinide, is it? See draft at 3-52. Also, the assumption of chemical equilibrium between solutes and solid phases is not founded (draft at 3-53).

50. It is claimed that K_ds are independent of scale and that spatial variations will be due to heterogeneity, length of equilibration time, and different processes. (draft at 3-53, 3-54). What tests are planned to account for these influences? We would not agree with a randomly based distribution function.

51. Is any lab work contemplated to confirm the stated conclusion that "hard sphere" carrier colloids will agglomerate and settle (draft at 3-55)?

52. What is the schedule for completion of the "ongoing colloidal experimental program" (draft at 3-56, line 12)?

53. What is the schedule for completion of the experiments involving actinide intrinsic colloids, referred to at draft 3-57, line 31?

54. The discussion of colloid transport is tied to the question of mean pore throat diameter of the Culebra matrix (at 3-58). Given the heterogeneity of that rock body, how can the pore throat diameter be deemed established for the entire area of interest?

55. At the January 9 meeting we were given new information about a "source-term baseline for mobile actinides" in Hans Papenguth's presentation. These data depart from the inventory limits model which Craig Novak said a month previous were the only defensible data. Use of such expert panel solubility values is indefensible.

56. Without any quantification of the scope of dissolution from brine mixing, it cannot be deemed accounted for. The discussion at 3-59, 3-60 is inadequate.

57. The discussion of climate change (draft at 3-62 et seq.) is limited to the single effect of a rise in the water table. The paper by Roger Y. Anderson enclosed with this letter discusses several other effects of climate change which have not been accounted for in the 2-D or 3-D modeling reported by DOE. These processes must be modeled to analyze climate change adequately.

58. The draft says that local gradients can exceed the slope of the surface (at 3-62 lines 24-26). How may this come about with relation to climate change, and how is it modeled?

59. There are statements as to the possible changes in flow direction and gradient associated with climate change and a higher water table (at 3-62, lines 27-37). Please cite the source of these statements, and, if unpublished, provide the data supporting the statements.

60. It is said that a wetter climate could increase the thickness, lateral extent, and flow directions in the saturated zone of the Dewey Lake and Dockum Group (at 3-63). What would be the effect in an intrusion scenario? Has this been modeled, and is there any plan to do so?

61. The draft clearly does not answer all questions concerning shallow drill holes (at 3-63, 3-64). The groundwater basin model has yet to be used to analyze that situation in any published materials. What, in any case, is the supposed effect if Culebra flow "pivots" to the west-southwest? The issue must be addressed.

62. The discussion of mining and subsidence is likewise incomplete. It seems to limit itself to increases in conductivity of Rustler strata. Even as to that issue the paper reaches no conclusion and cites no research. Moreover, mining can do more then increase conductivity in the Rustler. It can create a highly conductive channel -- the mine workings -- in the Salado, a question that has not been faced.

63. In Tom Corbet's presentation on January 9 we were told that for the SPM-2 baseline flow would be partitioned to the Dewey Lake, Magenta, and Culebra according to their respective hydraulic heads. What are the hydraulic head data to be used for that purpose for the Dewey Lake and the Magenta? We were also told that 25% of the flow to the Dewey Lake would be deemed released. What is the source of that figure, and why should it be deemed conservative in all instances?

64. I refrain from detailed comment on Chapter 4, the data narrative, because it does not deal with the use of specific data in modeling repository performance.

65. The draft acknowledges the difficulty of identifying the undisturbed potentiometric surface of the Culebra in light of the numerous intrusions (shafts, wells) in the area (at 4-12). The enclosed paper by David Snow emphasizes the nature of the problem. What effort will be made to deal with this serious issue, which is especially troublesome in light of the significant water level

increases recently noted in numerous wells about the site? DOE has not yet faced this issue.

66. The Salado Flow and Transport position paper states (at p. 12) that the non-Salado paper will discuss the scenario of a borehole penetrating a fractured anhydrite. Where is that discussion?

67. When may we expect that results of 3-D modeling of the groundwater basin will be published?

Thank you for considering these comments.

Best regards,

Lindsay

LINDSAY A. LOVEJOY, JR.
Assistant Attorney General

LAL:mh

Enclosures

cc: George E. Dials, DOE/CAO
Larry Weinstock, EPA
Robert H. Neill, EEG
Mark E. Weidler, NMED
Christopher Wentz, EMNR

November 9, 1994

To: Lief Eriksson, WTAC
From: David T. Snow

Subject: Review of SNL reports on "Anomalous Water Level Rises in Culebra Wells Around the WIPP"

Subject report by Rick Beauheim, September 30, 1994, and prior reports to which it refers have been reviewed for content and implications to the project. Culebra piezometry has been a topic for ongoing debate. Background well data, reports and simulations may have been well analyzed in the past (La Venue, et. al., 1988, for example), but I conclude that the piezometry, in general, is incompletely understood. Consequently, the basis for PA calculations of the fastest flow path and average travel time to the accessible environment is in doubt. At the very least, an intervenor could readily disparage a liscence application on the grounds that the regional hydrology of the Culebra Dolomite is not sufficiently defined to support prediction. In lieu of proofs, the SNL staff might have to submit their best opinion to peer review processing, seeking a consensus acceptable to the regulators.

The uncertainty of head distribution is largely because it contains artifacts of disturbances since before the site was considered or drilled for a repository. There seems to be no record of the primitive conditions. Hydrocarbon and potash exploration and development wells, requiring casings through the Rustler, may have failed to isolate the Culebra from effects of casing or annulus leakage, permitting either positive or negative head changes, depending upon connections and actions. Well drilling, well development and testing introduced known and unrecorded perturbations in piezometry and fluid densities. In transmissive regions, these man-made changes may have produced brief excursions of the head distribution unless cross-formational leakage was ongoing, in which case the effects could have been far-reaching and semi-permanent. In low-transmissibility regions, large head changes occurred (such as P-18 hydrograph, attached), very slow to recover. Attached hydrographs for wells DOE-1 and P-18 (Figures 2 & 3) illustrate the extreme range of water-level recoveries, 15 feet to 680 feet, respectively, for areas differing by four orders of magnitude in transmissibility.

As a first step towards an understanding of the inherited changes reflecting drainage to the repository shafts and of pumping test impacts, among other causes, I have plotted in Figure 4 the reported rise (Figure 1 identifies wells with hydrographs, in Beauheim, Sept 30, 1994) of levels from March, 1989 through August, 1994. The area of greatest concern to SNL, lying south of the site in the H-8 and H-9 vicinity, seems not to be anomalous, but only a

peripheral effect of the shaft drainages, especially the AIS that lies between H-16 and WIPP-21. As can be seen, the rises during that period can be smoothly contoured around the shafts, where over 100 ft of recovery has occurred. Smoothly diminishing amounts outward from that center seem to indicate that the shaft sinking and drainage had a widespread effect on the Culebra, most severe to the WNW and SSE, in areas of highest transmissibility. Wells south of the WIPP site apparently have recovered consistent with the interpretation of general recovery after shaft-induced drawdown.
I am inclined to discount the theory that rising heads in the H-8 and H-9 region S of the site are a direct result of casing leaks at injection wells such as Todd #3 (Figure 13), though such interferences between WIPP wells and hydrocarbon wells may operate undetected. By the same token, meteorological and river-stage changes probably had no measurable effects (papers of Webb and Davies).

The Rustler Formation contains three aquifers, for which we have some local and regional piezometry (notably charted by Mercer, 1983). The Magenta Dolomite, overlying the Culebra, is shown to possess (Figure 5) a westerly gradient for flow towards outcrop areas near Nash Draw. The dissolution zone at the base of the Rustler is another aquifer, with a head distribution (Figure 6) strikingly akin to that in the Culebra (Figure 7), but both differ from the Magenta in indicating flow southeasterly into a depression S of the site, flow then turning SW towards the Pecos River. Presumably, the same aquifer drainage effects have altered piezometry in the Culebra and Rustler-contact aquifers.

The Culebra distribution, (Figure 7, from La Venue, 1991), expressed in freshwater heads for times prior to shaft excavation (1981), suggests regional drainage to the Pecos River, contours forming smooth arcs and promoting southwesterly flows everywhere except at the site and south of the site. The pattern is consistent with a natural one governed by topography, broadly-distributed recharge N and E of the site and drainage to the Pecos River, altered by a strong, persistent local drainage, such as a copious well might produce. Note the indentation of the piezometric surface defined by the 910 to 935 contours. This is important because if restored to hypothetical southwesterly directions, flows from the repository would enter Culebra areas of increasing transmissibility and flow rate and shorter paths than those presently modeled by PA in accordance with the current southeasterly gradient.

In a primitive basin, flow directions should be roughly parallel in a succession of stacked leaky aquifers governed by similar areas of recharge and discharge. Thus, we should look for evidence that the ultimate (hundred-year) condition in the Culebra, say after wells have closed by casing corrosion and salt or clay-induced creep, is a restored southwesterly gradient like the Magenta displays. One of the few available lines of evidence is the reconstruction of piezometry by extrapolating hydrographs of individual wells.

Another would be to determine former well productions, injections, connections to other aquifers and changes of salinity.

Along the first line of investigation, 36 hydrographs from Beauheim, Sept. 30, 1994 have been examined. All but two have demonstrated persistent positive trends, starting in early 1988 to 1989, to the 1994 end-of-record. The exceptions are CB-1, with falling levels due to a failed breach plug since 4/90, and DOE-2, which rose until 9/92, and has fallen since. The asymptotic heads were estimated by curve extension, the column of brine to the Culebra (Figure 8) of variable density (Figure 9) was converted to Culebra pressure, and equivalent freshwater heads computed and plotted as a prediction of future piezometry for the Culebra (Figure 10). The generality of this rising behavior throughout the site area evidently reflects severe drainage at the shafts, followed by prolonged recovery (Figure 4). Contours of predicted head (Figure 10) convey other information. If the kriged densities shown in Figure 9 differ from the actual column densities in wells penetrating the Culebra, contours positions may err by as much as one contour interval, an uncertainty that would not alter the general pattern displayed by Figure 10.

In Figure 10, the essential feature that apparently perturbed a likely primitive southwesterly gradient is a trough of depression south of the site. La Venue's (1991) interpretation, Figure 11, is nearly the same, except for lower heads representing the present, and the depression is centered around well CB-1. But the hydrograph of well CB-1, supplied as Figure 12, shows that it could not have been responsible for the depression, since its fall didn't start until 2.3 years after it and all others had been rising. It must be one or more unmonitored wells, perhaps among the many hydrocarbon wells given in Figure 13, communicating with either Bell Canyon or Pennsylvanian production intervals. The head distribution for the Bell Canyon, Figure 14, indicates a gradient for northeasterly flow from the Capitan Reef isolated from overlying units by the evaporites, and with heads beneath the Culebra depression that are 140 to 150 meters higher than the Culebra heads. Thus a connecting well cannot drain the Culebra over the long time suggested by the piezometry in Figures 7 and 11. Heads in deeper units, such the Pennsylvanian sands, may be higher or lower than shallow units, but whatever the natural condition, oil and gas production can have reduced the heads, so that an unknown well may drain downwards. I believe that the only impediment to knowledge of the controlling interconnections and heads is the difficulty of data collection. Though the oil and gas industry maintains abundant records that should be investigated (Energy and Minerals Div., Santa Fe, New Mexico; New Mexico State Land Office, etc.), the critical information about injection history, well repairs and accidents that could be clues to the interconnections with shallow aquifers may be very obscure. At the very least, we need to ascertain whether the Pennsylvanian beds have and have had higher or lower heads than the Culebra that could explain the hydrographic history.

When the perturbation of heads disappears due to natural sealing of wells, the restored piezometry may resemble the primitive piezometry, with a southwesterly gradient across the WIPP site. Lacking insight into historic changes, PA may persist in the belief that a southeasterly gradient has been and will be controlling flows along the fastest path.

In addition to the perceived need to re-examine piezometry for its direct influence on flow, this study has revealed opportunities to modify and perhaps improve upon the boundary conditions of regional hydrologic modeling, fit subject for sequel memoranda. Furthermore, if a well or wells act as drains to some other horizon, they need to be evaluated as potential routes of contaminant migration.

As indicated in the first paragraph, this topic merits continuing effort, by WTAC or SNL. Direction would be helpful in furthering this work.

References:

Beauheim, R., Jan. 5, 1990, Anomalous Culebra Water Levels Around the WIPP Site, memo to distribution, 5 p. and 15 figs.

Beauheim, R., Jan. 4, 1991, Status of Culebra Water Levels Around the WIPP Site, memo to distribution, 2 p. and 15 figs.

Beauheim, R., March 6, 1992, Rustler Responses at Well H-16 to Culebra Coring in the Air Intake Shaft, memo to distribution, 3 p. and 14 figs.

Beauheim, R., Sept. 30, 1994, Anomalous Water Level Rises in Culebra Wels Around the WIPP Site, memo to M. McFadden, 2 p. and 38 figs.

Davies, P., Feb. 27, 1990, Pecos River Discharge Data (Background Information for Rustler Water Level Rise Analysis), memo to S. Webb, 2 p., 4 figs. and 25 tables.

Konikow, L., June 25, 1990, Comments Based on Visit to WIPP Site, May 24-25, 1990, memo to distribution, 6 p.

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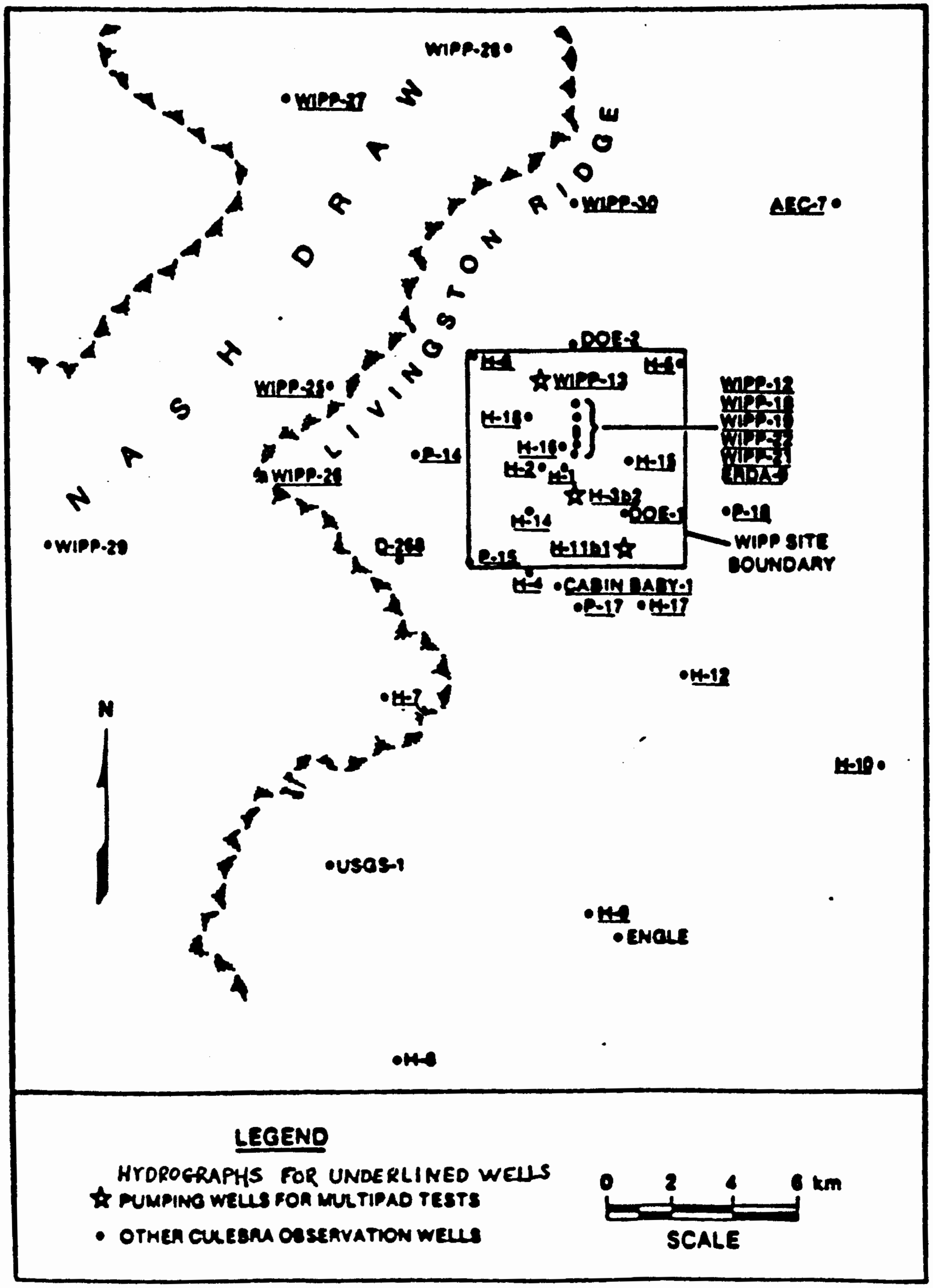
Figure 1. Locations of Culebra Dolomite Wells
Around the WIPP Site



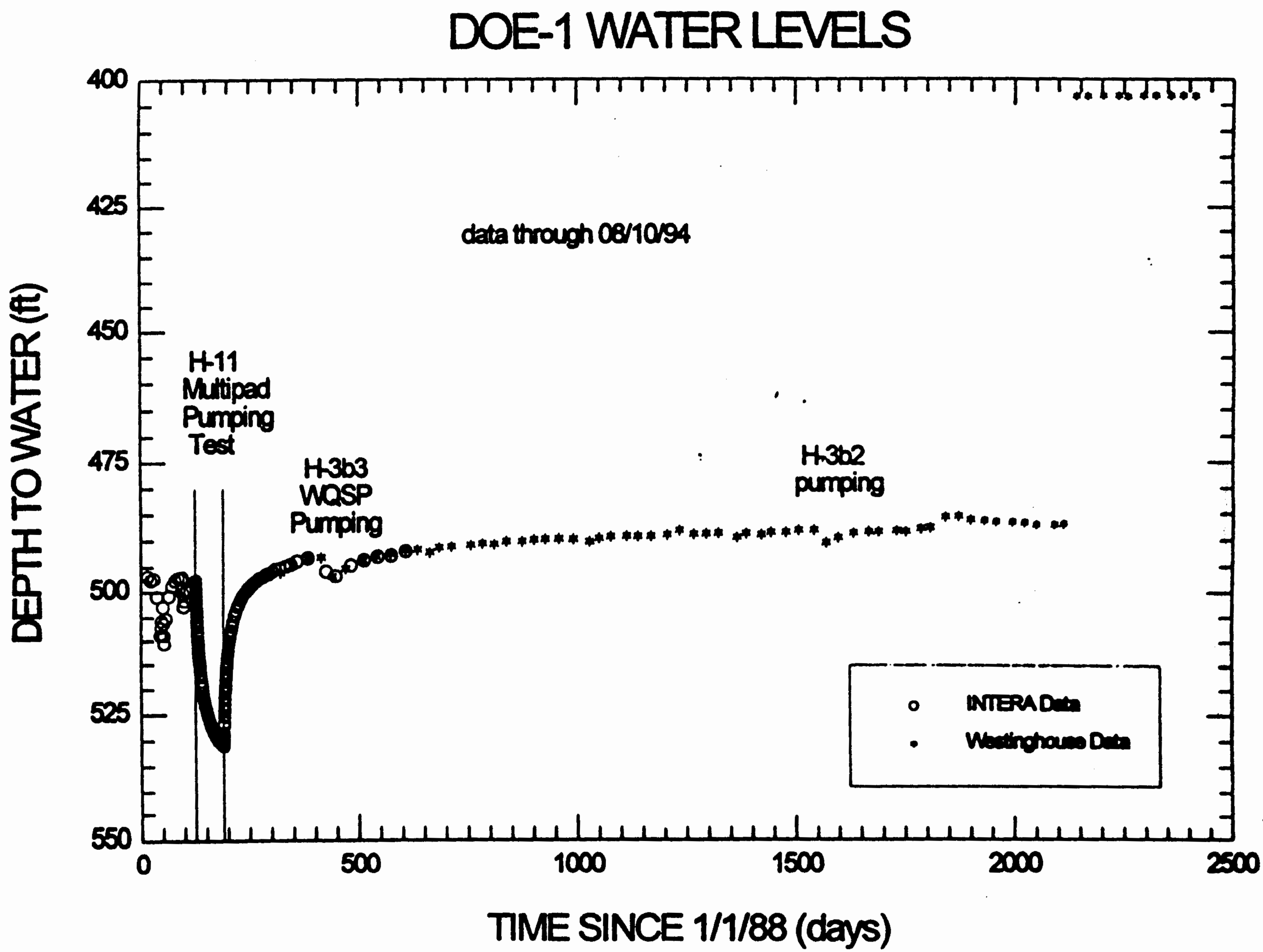
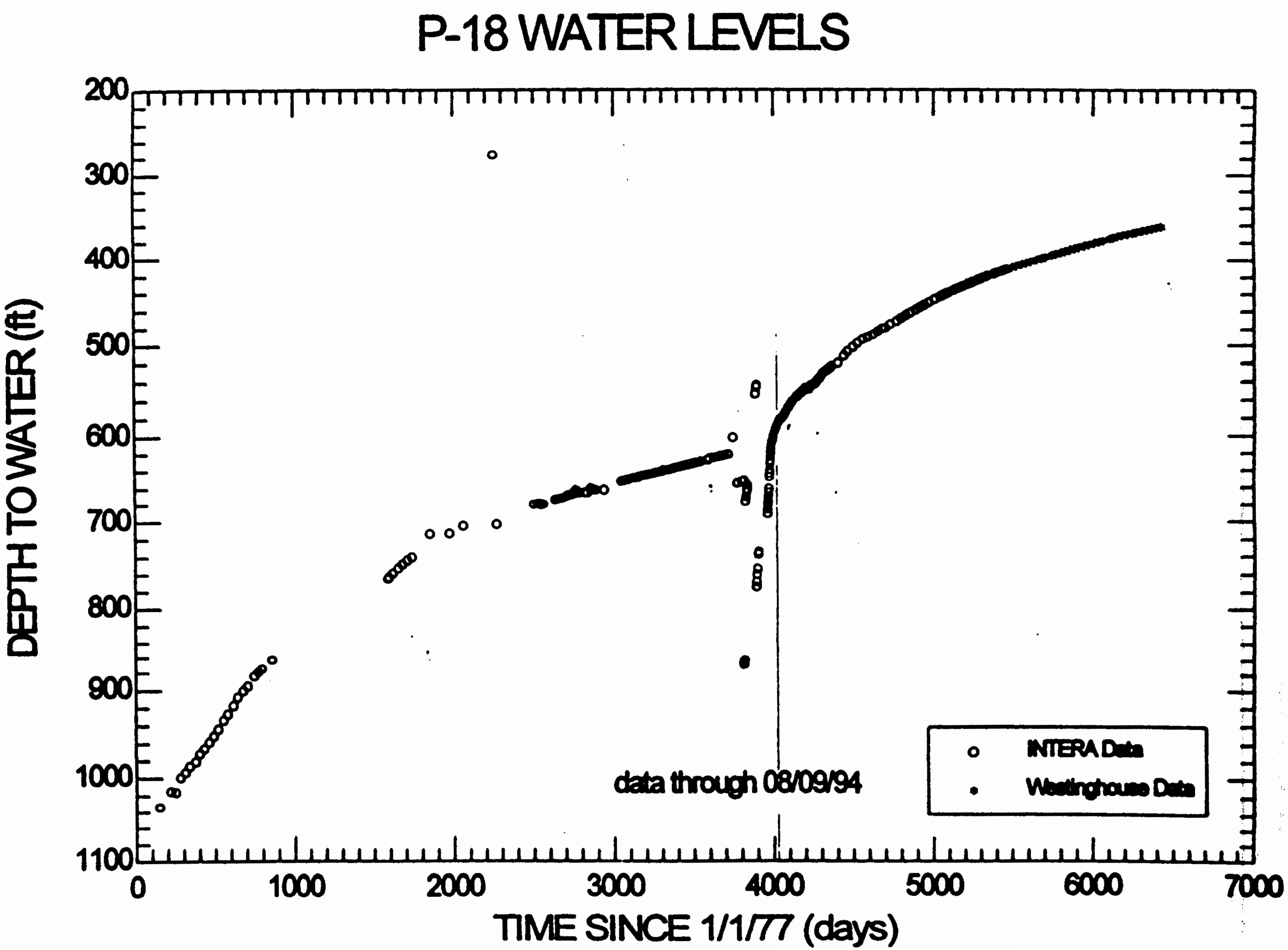
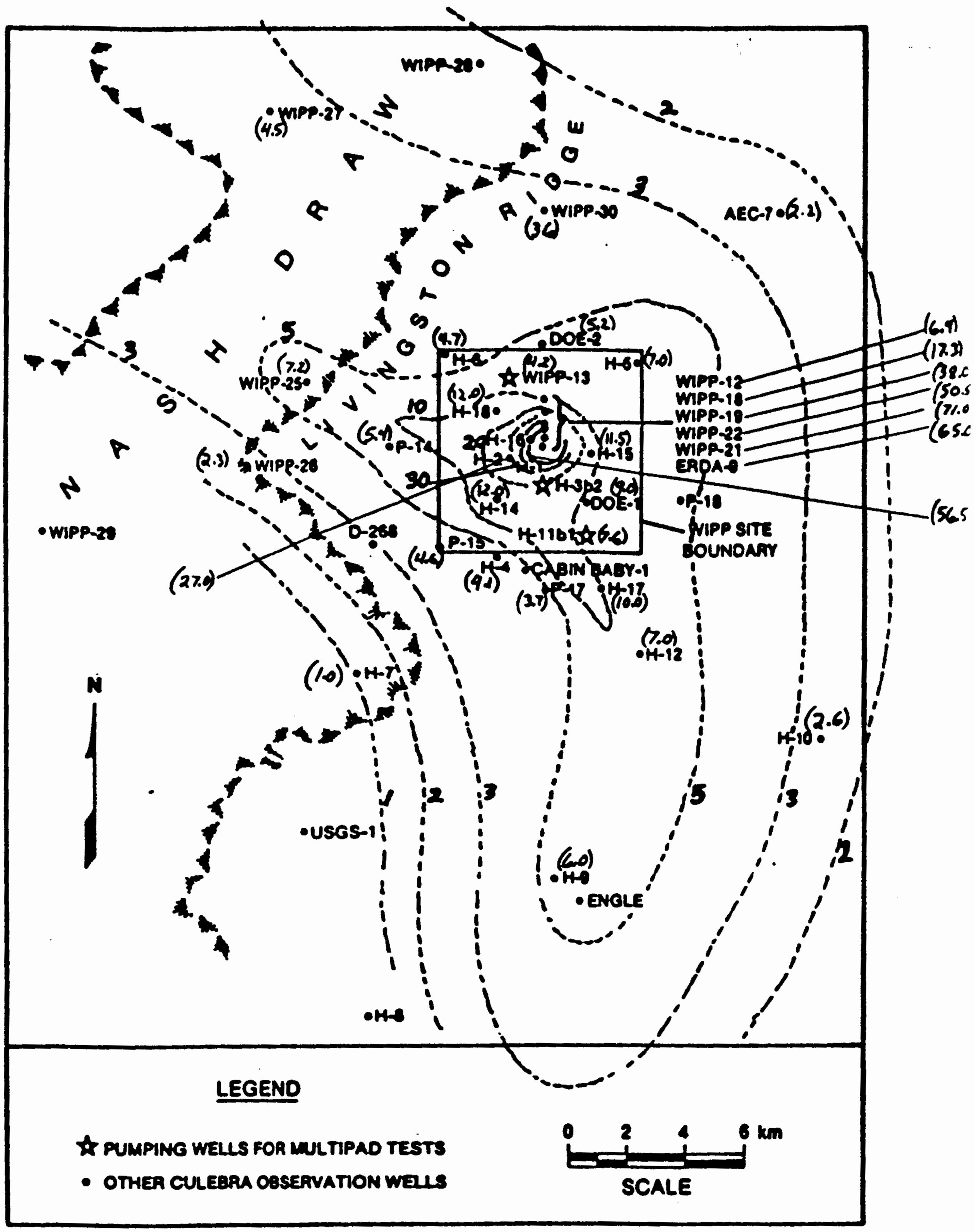

DOE-1 WATER LEVELS
data through 08/10/94
H-11
Multipad
Pumping
Test
H-3b3
WQSP
Pumping
H-3b2
pumping
INTERA Data
Westinghouse Data
DEPTH TO WATER (ft)
400
425
450
475
500
525
550
0
500
1000
1500
2000
2500
TIME SINCE 1/1/88 (days)

Figure 2

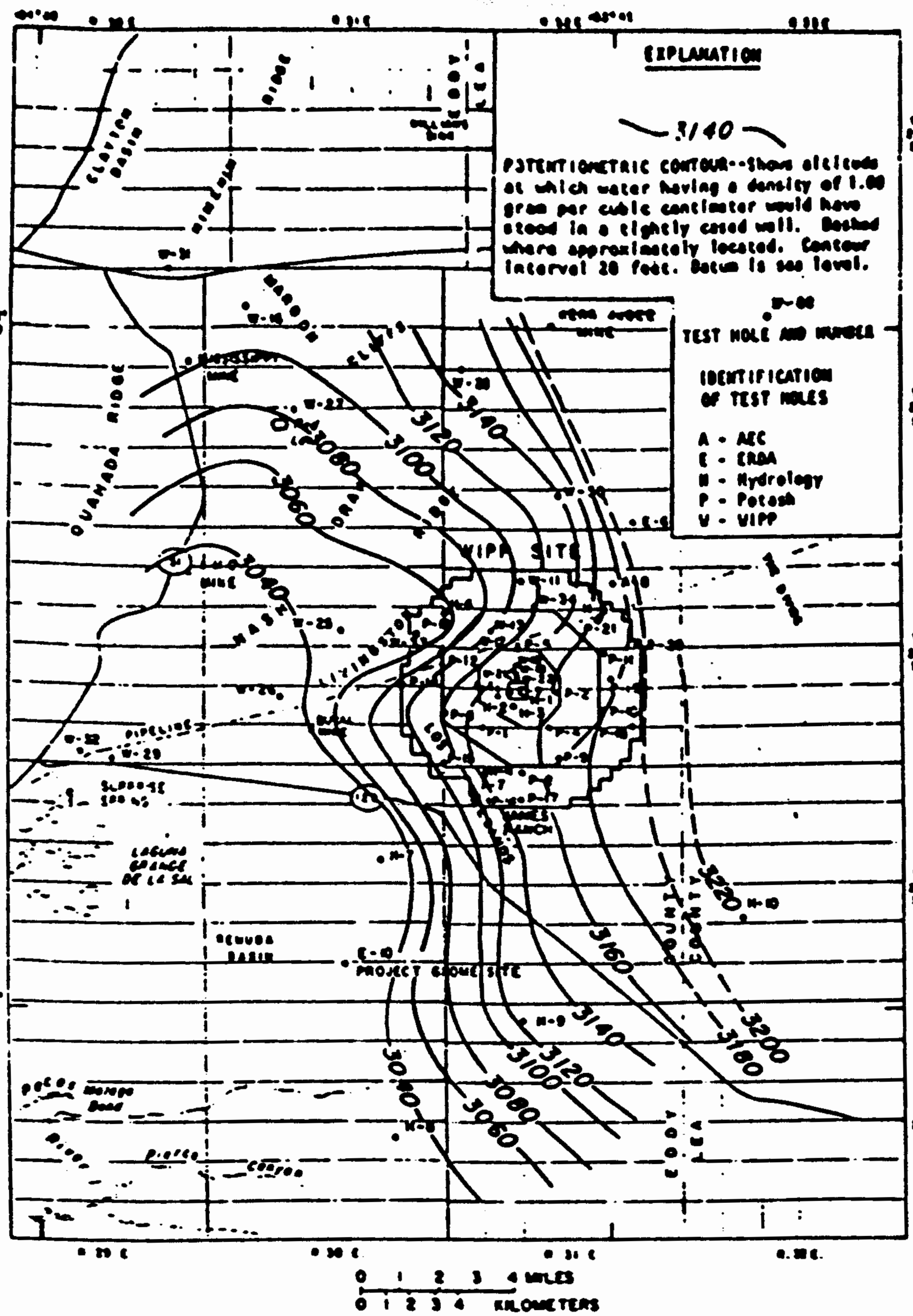

P-18 WATER LEVELS
DEPTH TO WATER (ft)
TIME SINCE 1/1/77 (days)
data through 08/09/94
INTERA Data
Westinghouse Data
200
300
400
500
600
700
800
900
1000
1100
0
1000
2000
3000
4000
5000
6000
7000

Figur

L TIONS OF CULEBRA D MITE WELLS AROUND THE WIPP TE

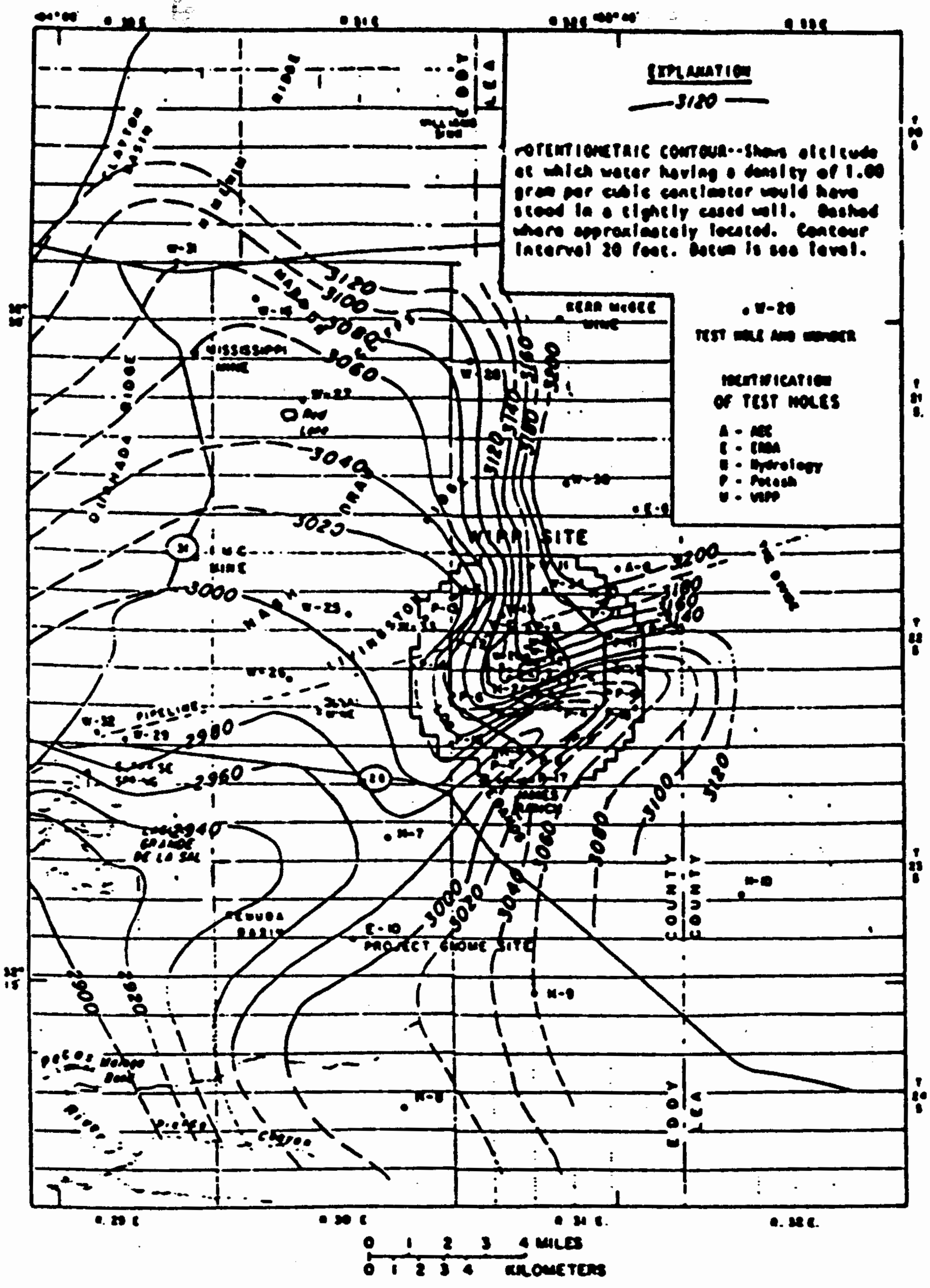


Recovery of water levels in Culebra Dolomite from March, 1989, through August, 1994, (Contoured in feet) (Contours not uniformly incremented.)



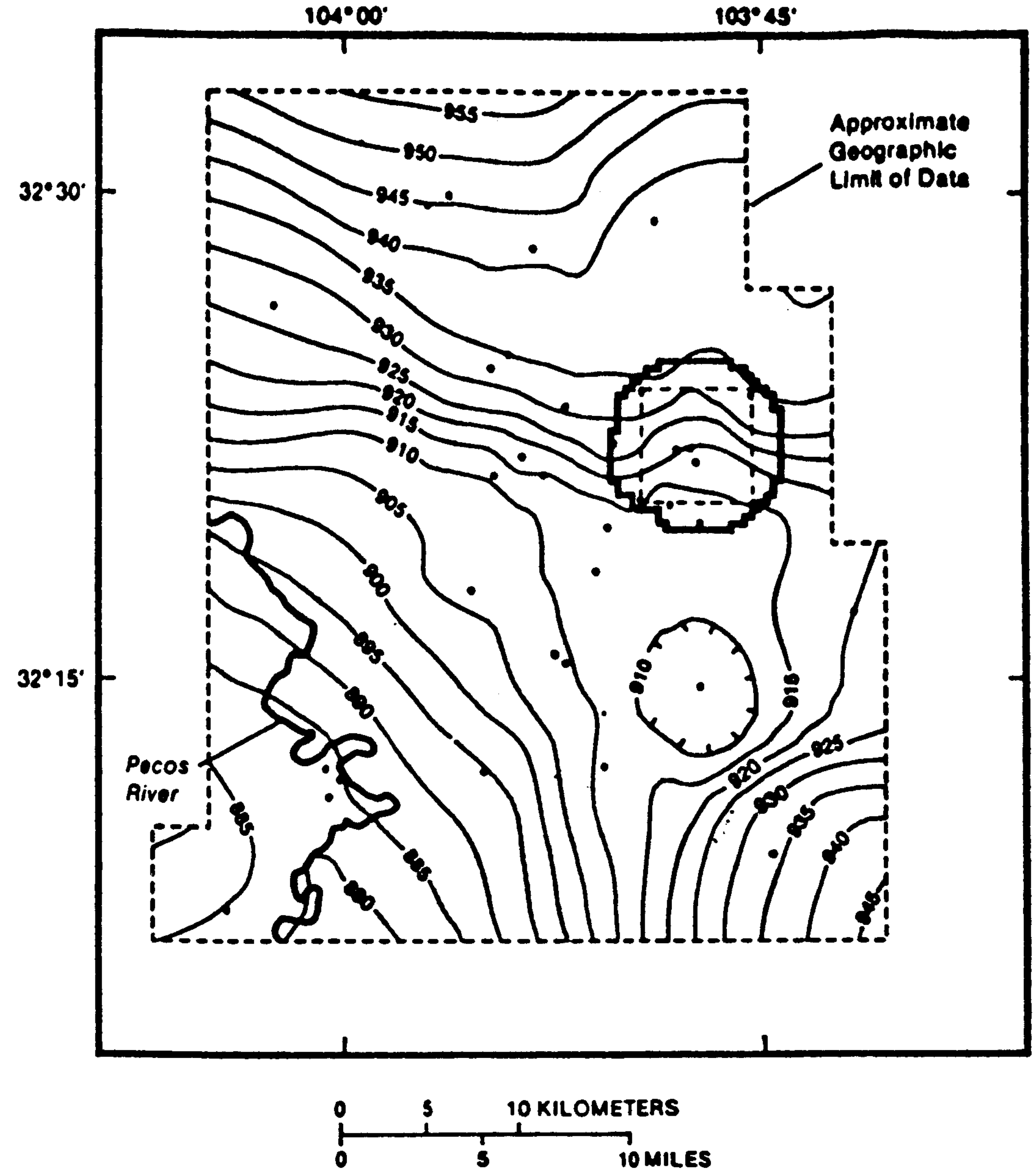
Magenta Dolomite

Figure 4-1. Potentiometric contour -- shows altitude at which water having a density of 1.00 gram per cubic centimeter would have stood in a tightly cased well. Dashed where approximately located. Contour interval 20 feet. Datum is sea level (from Mercer (1983)).



RUSTLE-SALADO RESIDUUM

Figure 4-3. Potentiometric contour -- shows altitude at which water having a density of 1.0 gram per cubic centimeter would have stood in a tightly cased well. Dashed where approximately located. Contour interval 20 feet. Datum is sea level (from Mercer (1983)).



EXPLANATION

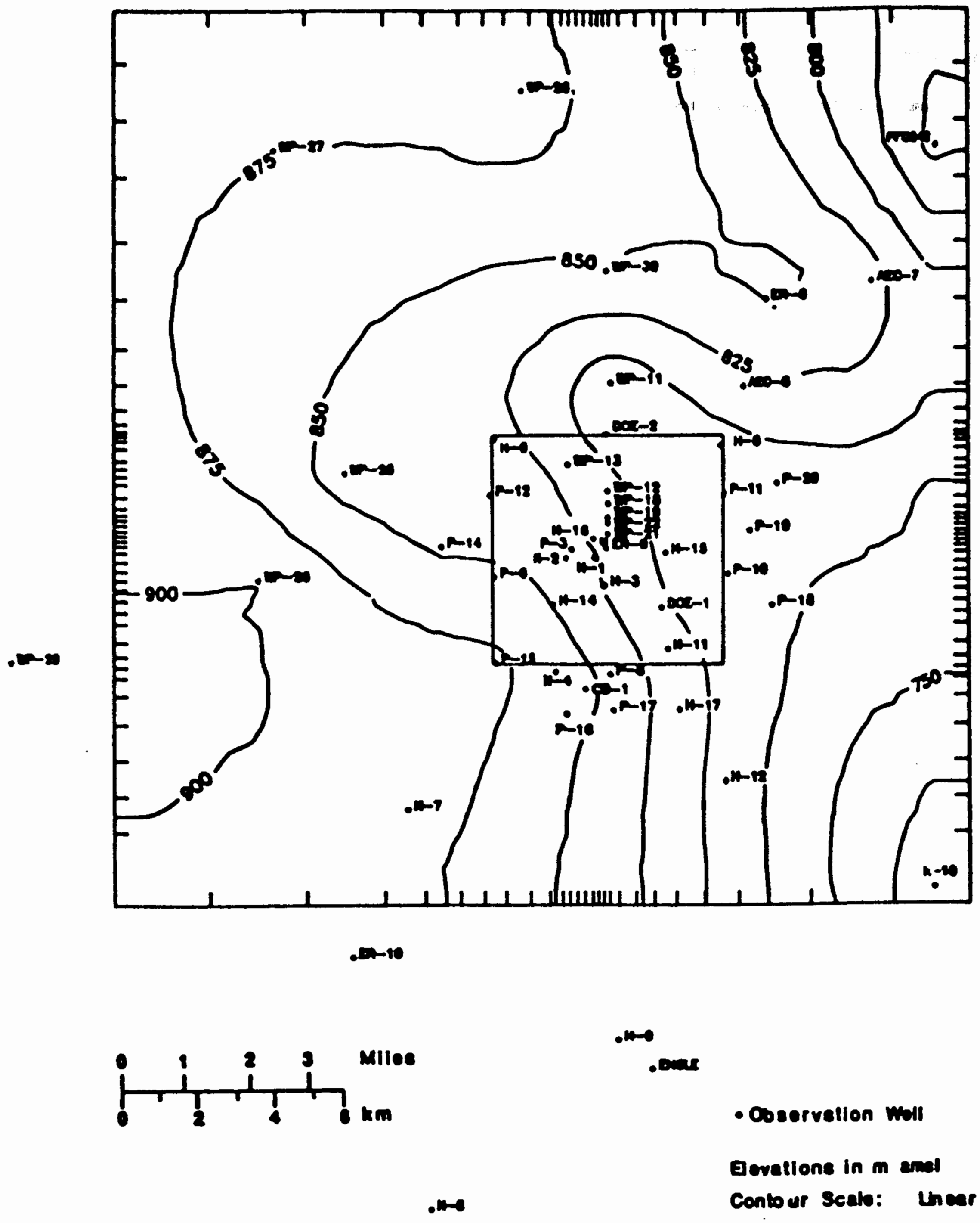
——— WIPP ZONE IV BOUNDARY

- - - - - - WIPP SITE BOUNDARY

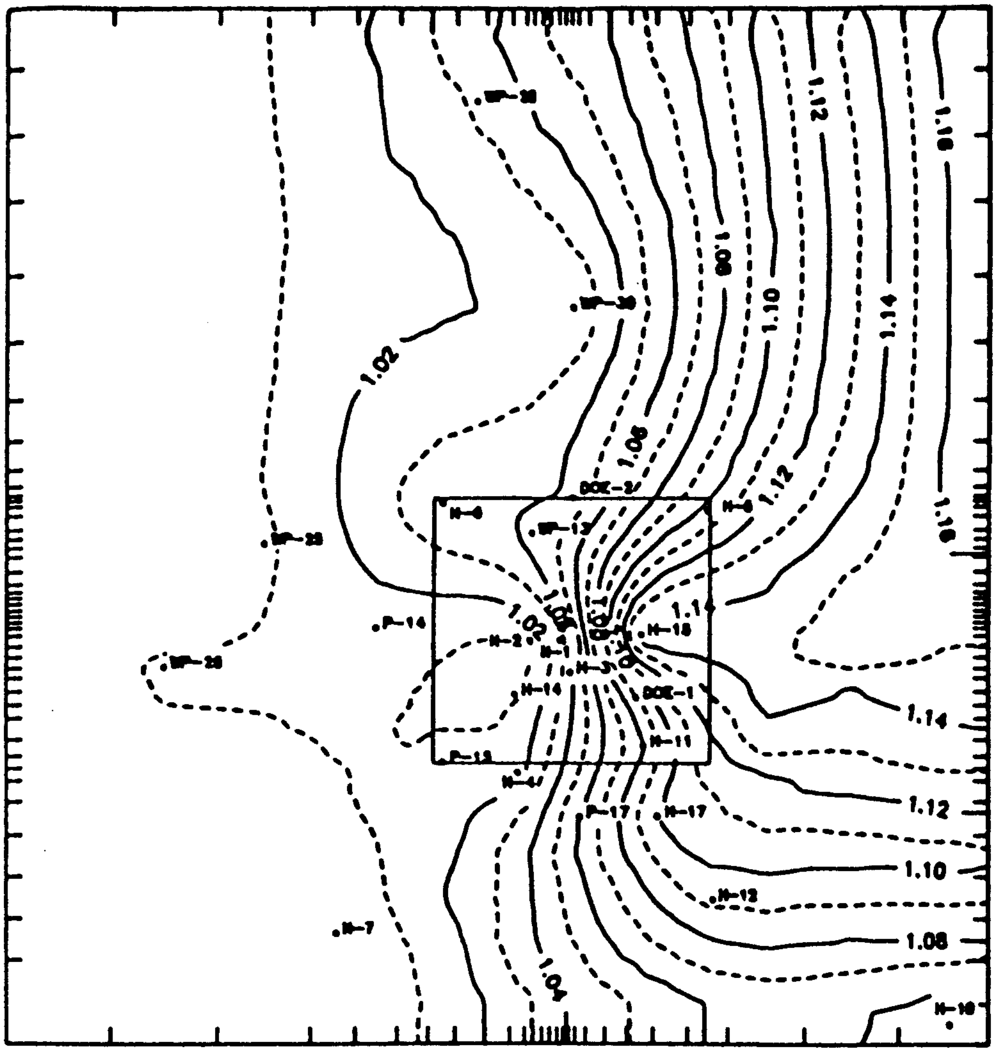
— 900 — FRESHWATER HEAD CONTOUR——Shows altitude at which water having a density of 1.00 gram per cubic centimeter would have stood in a tightly cased well. Contour interval 5 meters. Datum is sea level.

• WELL OR TEST HOLE

Figure 19.-- Estimated equivalent-freshwater head in the Culebra Dolomite Member of the Rustler Formation prior to shaft excavation at the WIPP site in 1981.



Drawn by	Date	Center-of-Culebra Elevations Over Modeled Region
Checked by	Date	
Revisions	Date	
HD9700R554		
INTERA Technologies		Figure 3.4



0 1 2 3 Miles

0 2 4 6 km

• Observation Well

Formation-Water Densities in g/cm^3

Contour Scale: Linear

Contour Interval: 0.01 g/cm^3

by	Date	
by	Date	Kriged Formation-Fluid Densities Used in Model (Using AKRIP)
M.L.	Date 1/25/88	
...0R654	1/27/88	
INTERA Technologies		Figure 3.15

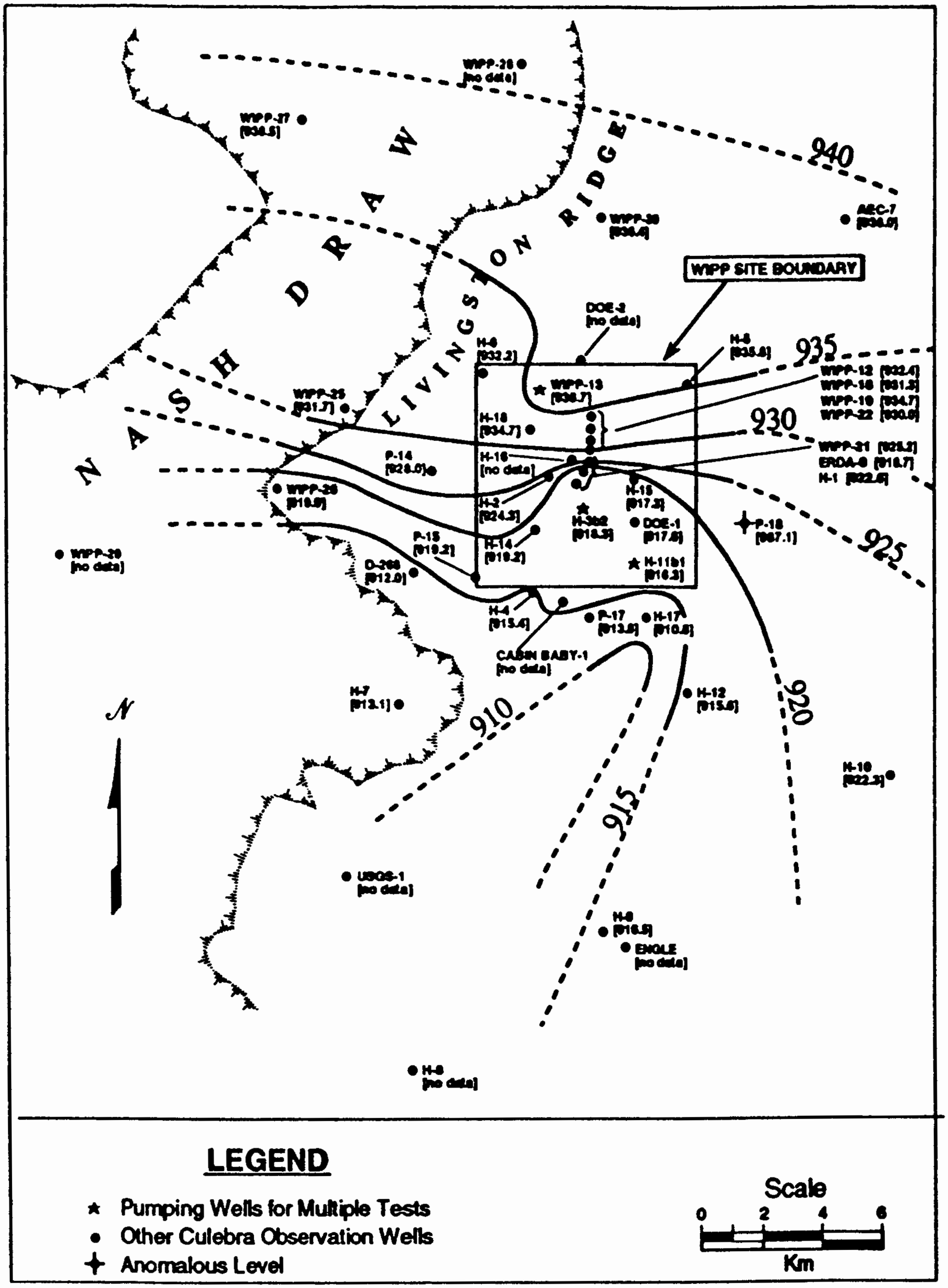
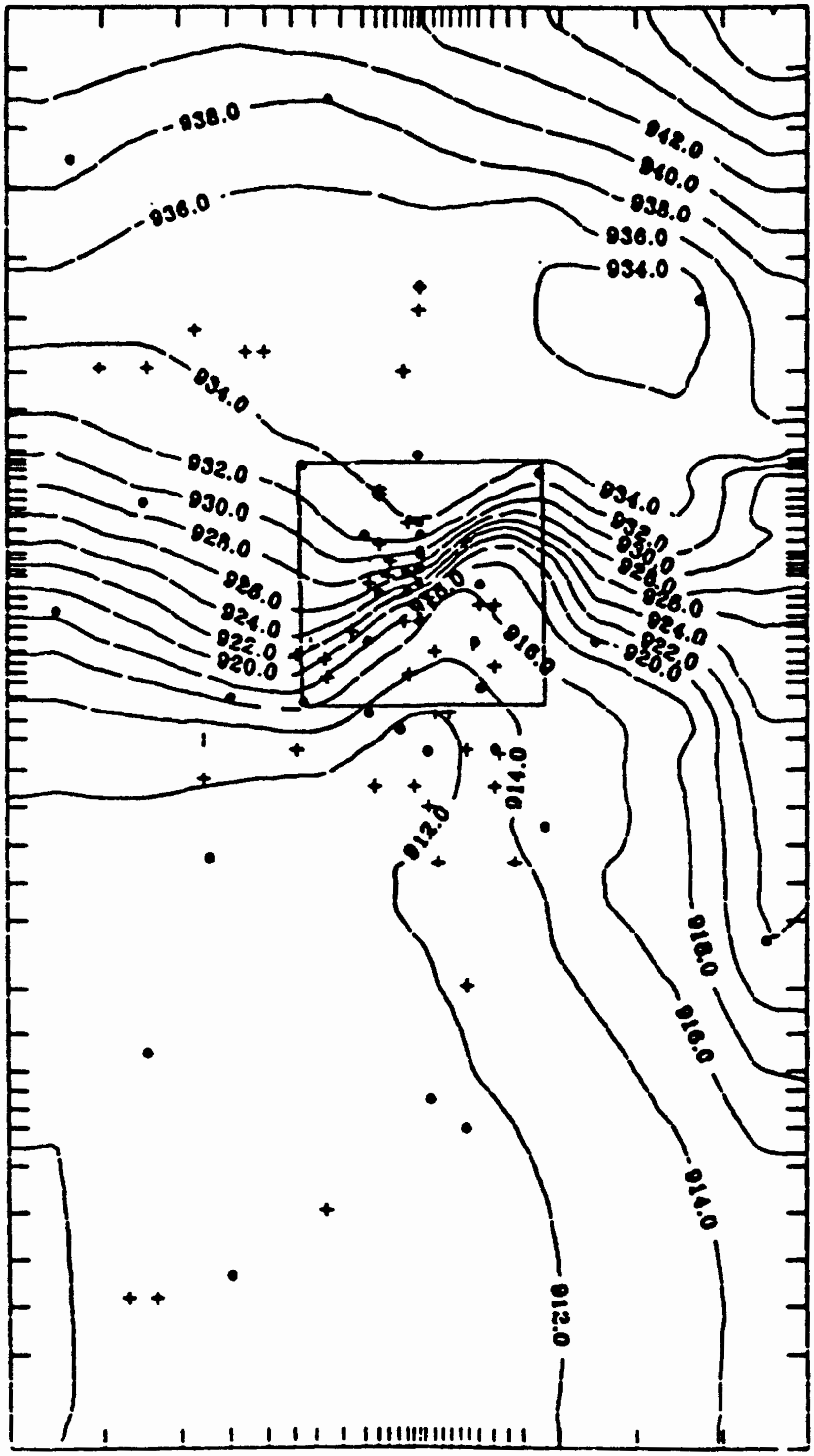


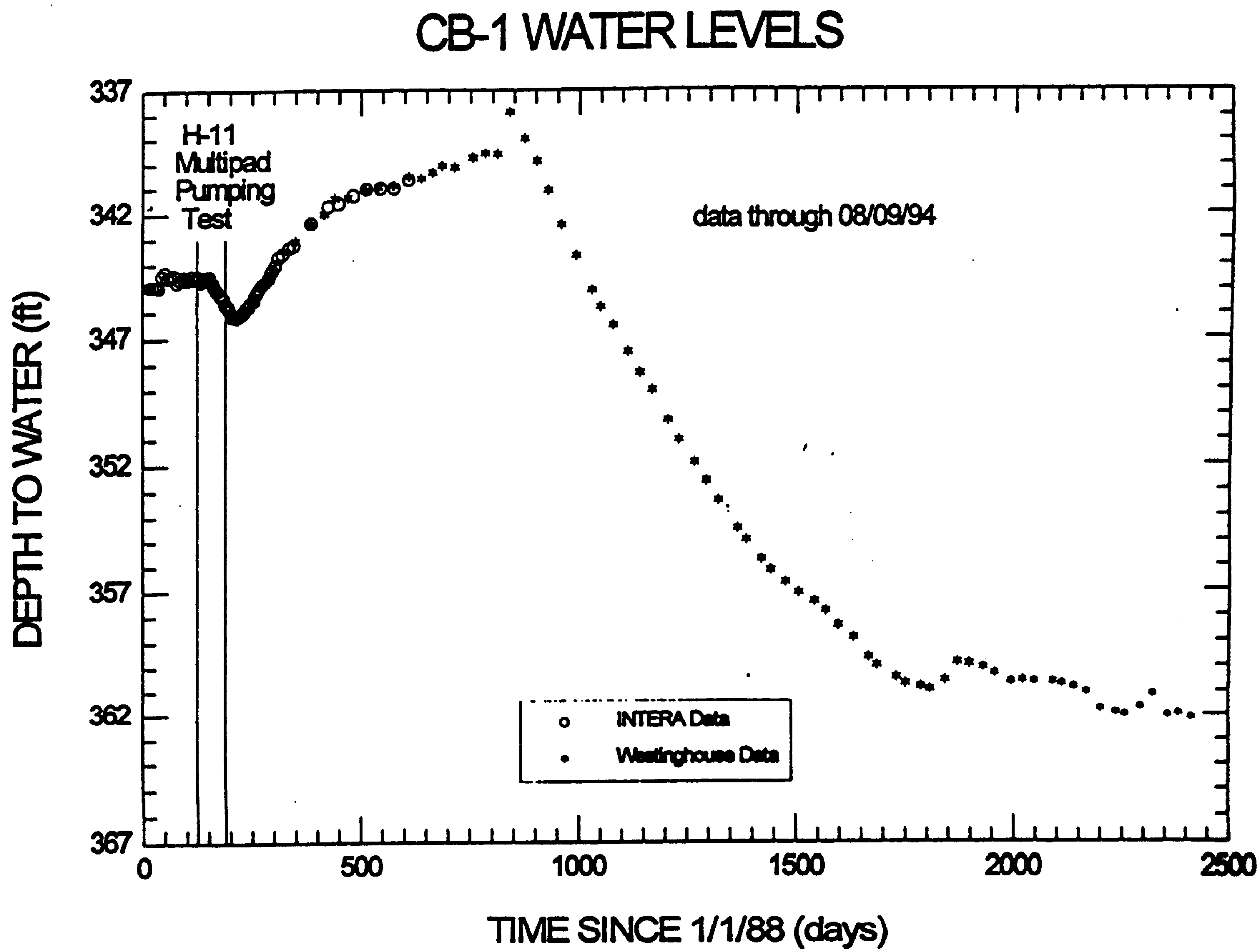
Figure 10 Culebra Freshwater Heads at the WIPP-Area Boreholes (After Cauffman, et. al., 1990). Revised to reflect assymptotes of hydrograph trends (Snow, 1994).

Figure 1[illegible]

Figure 11. The Extended Steady-State Calibrated Model Freshwater Heads (msl)

LiVernee, Jan. 28, 1991

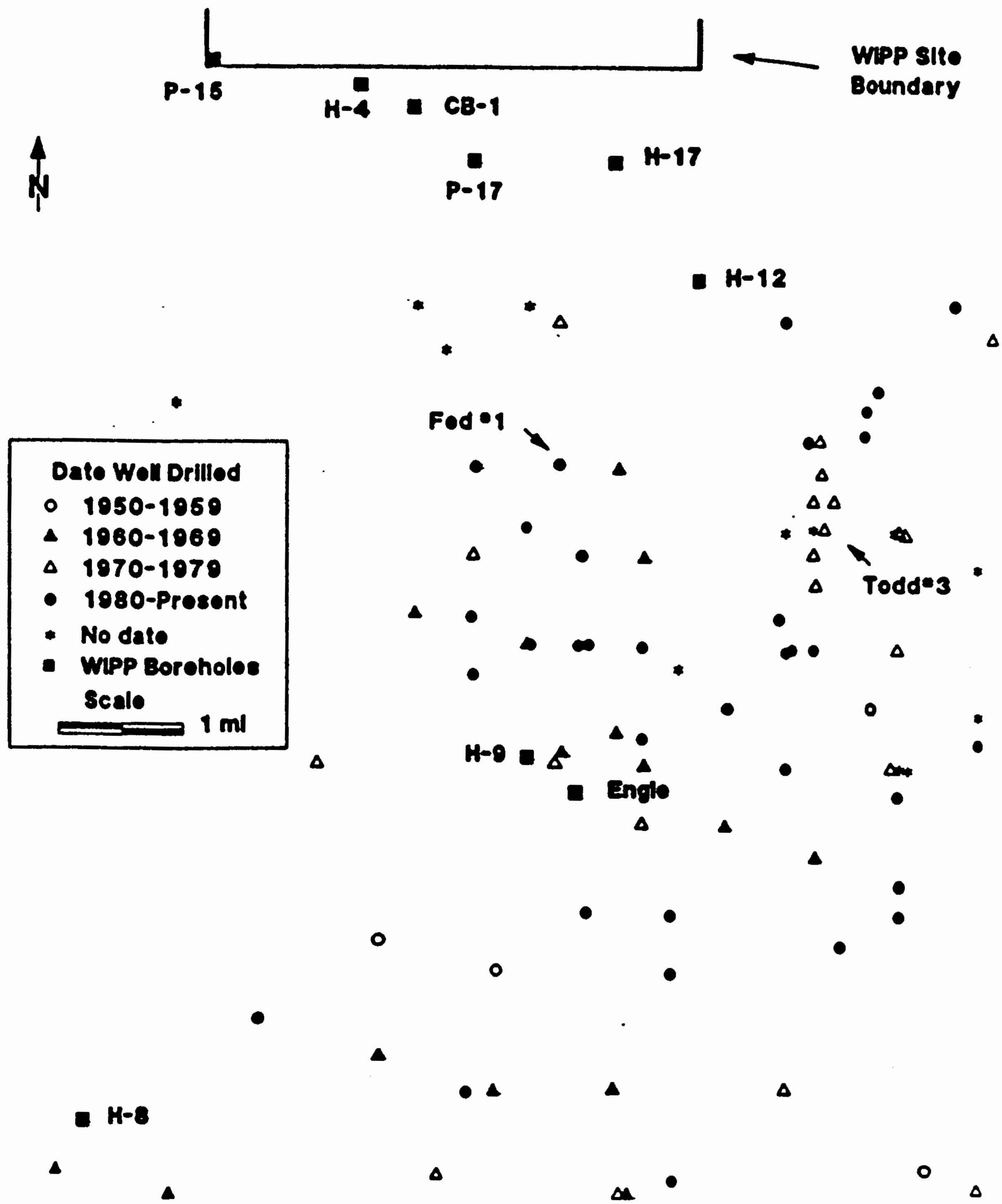




CB-1 WATER LEVELS
DEPTH TO WATER (ft)
337
342
347
352
357
362
367
H-11
Multipad
Pumping
Test
data through 08/09/94
INTERA Data
Westinghouse Data
0
500
1000
1500
2000
2500
TIME SINCE 1/1/88 (days)

Figure

Figure 6. Oil and Gas Wells Within 5 Mile Region of H-9



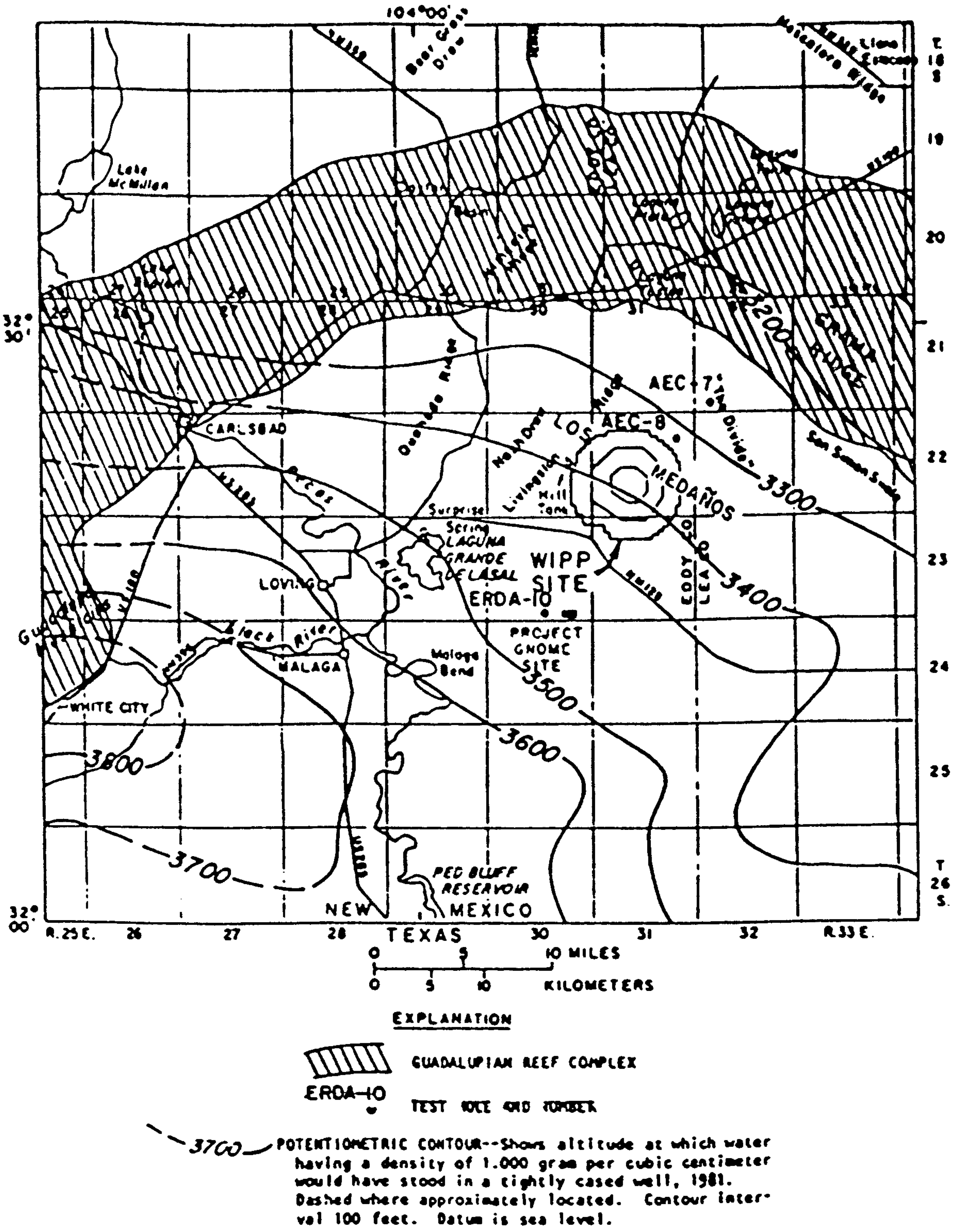


Figure 4-5. Potentiometric contour – shows altitude at which water having a density of 1.000 gram per cubic centimeter would have stood in a tightly cased well, 1981. Dashed where approximately located. Contour interval 100 feet. Datum is sea level (from Mercer (1983)).