DOCKETED
USNRC

April 8, 1999

'99 APR 13 P3 :22

UNITED STATES OF AMERICA
NUCLEAR REGULATORY COMMISSION
BEFORE THE ATOMIC SAFETY AND LICENSING BOARD

OFFICE OF SECRETARY
RULEMAKINGS AND
ADJUDICATIONS STAFF

The Honorable Peter B. Bloch, Presiding Officer

In the Matter of	)	
	)	
HYDRO RESOURCES, INC.	)	Docket No. 40-8968-ML
(2929 Coors Road, Suite 101	)	ASLBP No. 95-706-01-ML
Albuquerque, NM 87120)	)	
	)	

ENDAUM'S AND SRIC'S REPLY IN RESPONSE TO HRI'S AND THE NRC STAFF'S RESPONSE PRESENTATIONS ON GROUNDWATER PROTECTION ISSUES

INTRODUCTION

Pursuant to the Presiding Officer's Memorandum and Order (Motions to Reply and Rebut) (March 24, 1999), Intervenors Eastern Navajo Diné Against Uranium Mining ("ENDAUM") and Southwest Research and Information Center ("SRIC") hereby submit their reply to the initial presentations filed by Hydro Resources Inc. ("HRI") and the Nuclear Regulatory Commission ("NRC") Staff regarding groundwater protection at the proposed Crownpoint Project. This reply incorporates the reply testimony of Dr. Richard J. Abitz, attached as Exhibit A hereto ("Abitz Reply"), Dr. William P. Staub, attached as Exhibit B hereto ("Staub Reply"), and Michael Wallace, attached as Exhibit C hereto ("Wallace Reply").

HRI's filing amounts to little more than a display of smoke and mirrors, designed to confuse the issues. It is rife with errors, and practices which Dr. Abitz

20241

U.S. NUCLEAR REGULATORY COMMISSION
RULEMAKINGS & ADJUDICATIONS STAFF
OFFICE OF THE SECRETARY
OF THE COMMISSION

Document Statistics

Postmark Date FE (emailed)
Copies Received 3
Add'l Copies Reproduced 0
Special Distribution
OGC, RIDS

aptly describes as "simple thinking and simple science". Abitz Reply at 8.[1]

FACTUAL BACKGROUND

On January 12, 1999, Intervenors' served their initial presentation on groundwater issues via e-mail and overnight delivery. Intervenors Written Presentation in Opposition to Hydro Resources, Inc.'s Application for a Materials License with Respect to: Groundwater Protection ("Groundwater Presentation"). Corrected Certificate of Service (January 27, 1999). The presentation included expert testimony from Dr. William P. Staub, Dr. Richard Abitz, and Mr. Michael G. Wallace. Intervenors' Groundwater Presentation, Exhibits 1, 2, and 3. On January 18, 1999, with leave from the Presiding Officer, Intervenors filed an amended legal brief for the groundwater presentation. On February 20, 1999, HRI filed Hydro Resources, Inc.'s Response to Intervenors' Brief in Opposition to Hydro Resources, Inc.'s Application for a Materials License with Respect to Groundwater Issues ("HRI Response"). On March 12, 1999, the NRC Staff served the NRC Staff's Response to Intervenors' Amended Presentation on Groundwater Issues ("Staff Response").

---

[1] The Staff Response states that it agrees with the HRI technical information and Staff employee William Ford directs his affidavit at supplementing HRI's Response. Ford Affidavit ¶6. Consequently, in addressing the problems with HRI's response, Intervenors also address the problems on which the Staff relies in its Response.

REPLY ARGUMENT

I. THE AFFIDAVITS OF HRI'S AND THE STAFF'S EXPERTS CARRY LITTLE EVIDENTIARY WEIGHT.

Mark S. Pelizza, Craig S. Bartels and Frank Lee Lichnovsky based their qualifications to provide statements on behalf of HRI on their experience in ISL mining, rather than academic credentials. Pelizza Affidavit at 2; Bartels Affidavit at 1; Lichnovsky Affidavit at 1. Similarly, Staff member William Ford bases his qualifications on his industry experience. Ford Affidavit at 5-6. The fact that these individuals have worked in the industry for several years, does not mean they have the skill, incentive, or knowledge to properly design, model and operate an ISL mine. Intervenors have contested the technical qualifications of HRI/URI/Uranium Resources Inc. employees in their written presentation on HRI's lack of technical and financial qualifications.[2] This lack of expertise is further demonstrated by the numerous errors in the affidavits of Ford, Pelizza, Bartels and Lichnovsky. Abitz Reply at 4-11; Staub Reply at 2-5; 7-9.

The Affidavit of Shlomo Orr carries little weight because he is clearly unfamiliar with the project and takes no time to substantiate his general assertions. He

[2] Eastern Navajo Diné Against Uranium Mining's and Southwest Research and Information Center's Brief in Opposition to Hydro Resources, Inc.'s Application for a Materials License with Respect to: Hydro Resources, Inc's Lack of Technical and Financial Qualifications (January 11, 1999). ENDAUM's and SRIC's Responses to the Presiding Officer's March 3, 1999 Questions (March 29, 1999).

either makes a vague critique of the Wallace Testimony without citing any supporting data or references, e.g. "[t]he heterogeniety in the Westwater formation is on a much smaller scale." Orr Affidavit at 3. Or, he makes a citation without any elaboration to illuminate an argument, e.g. "[i]n my judgement, the hydraulic control analysis done by Geraghty & Miller Inc. (G&M), Oct.3, 1993, for Churchrock [sic] (Section 8) is essentially correct." Id. at 5. HRI lays no foundation to establish that Dr. Orr is such an extraordinary expert he need not provide reason nor analysis for his findings.

II. NEW INFORMATION PRESENTED IN HRI'S AND THE STAFF'S RESPONSES DEMONSTRATES HRI'S LICENSE IS INIMICAL TO HEALTH AND SAFETY.

HRI and the Staff posit some new data, analysis, and arguments in their responses. Each of these pieces of new information is identified and dissected in the attached Replies of Dr. Abitz, Dr. Staub and Mr. Wallace. Their Replies are incorporated herein. Exhibits A, B, and C. None of this additional information demonstrates that HRI can carry its burden of proof in this area of concern. The new information either supports Intervenors' concerns, as in the case of Pelizza's Exhibit 13, which supports Intervenors position that monitor wells should be spaced closer together downgradient (See Abitz Reply at 3-4), or contains material errors that call into question HRI's application materials, and HRI's and the Staff's very credibility. Exhibits A, B, and C.

III. THE STAFF COMMITS A NEW LEGAL ERROR BY CONFUSING THE REQUIREMENTS OF THE SAFE DRINKING WATER ACT WITH THE ATOMIC ENERGY ACT.

The Staff, in error, makes the presumtion that Intervenors must prove mining at Section 8 will result in harm to drinking water. *See* Staff Response, legal brief at 3-8; Ford Affidavit at ¶¶5,7. Apparently, the Staff has confused the requirements of the AEA with the SDWA requirements.[3] Intervenors do not have to demonstrate mining at Section 8 will harm drinking water to demonstrate HRI's license is inimical to health, safety and the environment. In the separate context of the requirements of the Safe Drinking Water Act, however, Intevenors demonstrate in their brief that Section 8 already qualifies as an "underground source of drinking water".

In the context of Intervenors' arguments under the Atomic Energy Act ("AEA") and its implementing regulations, Intervenors describe how mining at Section 8 will contaminate a resource of high quality groundwater in a community that is already using Westwater aquifer wells in the vicinity for drinking water purposes. *See* Intervenors' Groundwater Presentation at 8-9, 15-59. Separately, in the context of Intervenors' arguments under the Safe Drinking Water Act ("SDWA"), Intervenors have demonstrated that Church Rock is an "underground source of drinking water," which is a term of art under the SDWA and its implementing regulations. Id. at 59-65. Church Rock cannot

---

[3] In their confusion, the Staff represents that Intervenors argue contamination may travel from Church Rock to the Crownpoint municipal wells. Staff Response, legal brief at 4-5, 8. Intervenors have not at any time made this argument. This idea is the Staff's flight of fancy.

qualify for an aquifer exemption because it currently serves a domestic water supply well, potentially can serve a public water supply system, the water is of good quality, and the TDS content is under 3,000 mg/l. Id. at 63-65.

Besides being incorrect, the Staff's argument demonstrates a shocking lack of understanding of Navajo rural communities. The Staff asserts that "there are no communities" in the vicinity of Section 8 and refers to the "town of Church Rock" as the focus for analyzing a groundwater contamination threat. Staff's Response, legal brief at 6-8. In fact, there are 87 residences (representing 350-450 people) that are within a 2.5 mile radius of the Section 8 site. *See* ENDAUM's and SRIC's Environmental Justice Presentation, Exhibit 1, Testimony of Dr. Robert Bullard at 25.

CONCLUSION

As demonstrated above, and in the attached replies of Dr. Abitz, Dr. Staub, and Mr. Wallace, HRI's Response, like its application itself, and the Staff's Response are fraught with significant distortions of material facts, analytical error, and legal. Intervenors reiterate their request that the Presiding Officer:

1. Reject HRI's application as inadequate to protect groundwater and thus inadequate to satisfy the requirements of the Atomic Energy Act and the pollution control standards of the SDWA;

2. Find that the licensing of the Crownpoint Uranium Project is not supported by an adequate FEIS that complies with NEPA; and

3. Revoke HRI's license because it was unlawfully issued and supported by an inadequate EIS.

Respectfully submitted,

Johanna Matanich
Douglas Meiklejohn
Lila Bird
New Mexico Environmental Law Center
1405 Luisa Street Suite 5
Santa Fe, N.M. 87505
(505) 989-9022

Diane Curran
Harmon, Curran, Spielberg,
& Eisenberg, LLP
1726 M Street NW Suite 600
Washington, DC 20036
(202)328-3500

DOCKETED
USNRC

April 8, 1999

'99 APR 13 P3:22

UNITED STATES OF AMERICA
NUCLEAR REGULATORY COMMISSION
ATOMIC SAFETY AND LICENSING BOARD PANEL

ADJUDICATIONS STAFF

Before Administrative Judge Peter B. Bloch, Presiding Officer

In the Matter of	)	
	)	
HYDRO RESOURCES, INC.	)	Docket No. 40-8968-ML
2929 Coors Road	)	
Suite 101	)	ASLBP No. 95-706-01-ML
Albuquerque, NM 87120	)	
	)	

CERTIFICATE OF SERVICE

I hereby certify that:
On April 8, 1999, I caused to be served copies of the following:

ENDAUM'S AND SRIC'S REPLY IN RESPONSE TO HRI'S AND THE NRC STAFF'S RESPONSE PRESENTATIONS ON GROUNDWATER PROTECTION ISSUES

to the following parties marked by an asterisk via e-mail and express mail. Service was also made upon the following persons by U.S. mail, first class, and in accordance with the requirements of 10 C.F.R. § 2.712. The envelopes were addressed as follows:

Office of the Secretary*
U.S. Nuclear Regulatory Commission
One White Flint North
11555 Rockville Pike
Rockville, MD 20852
Attn: Rulemakings and Adjudications
Staff

Administrative Judge
Peter B. Bloch*
Atomic Safety and Licensing Board
U.S. Nuclear Regulatory Commission
Two White Flint North
11545 Rockville Pike
Rockville, MD 20852-2738

Office of Commission Appellate
Adjudication
One White Flint North
11555 Rockville Pike
Rockville, MD 20852

Administrative Judge
Thomas D. Murphy*

Special Assistant
Atomic Safety and Licensing Board
U.S. Nuclear Regulatory Commission
Two White Flint North
11545 Rockville Pike
Rockville, MD 20852-2738

Administrative Judge
Robin Brett
Special Assistant
c/o Administrative Judge Peter Bloch
Atomic Safety and Licensing Board
Two White Flint North
11545 Rockville Pike
Rockville, MD 20852-2738

Jep Hill, Esq.
Attorney for Hydro Resources, Inc.
Jep Hill & Associates
816 Congress Ave. Suite 1100
Austin, TX 78701-2443

Mitzi Young
John T. Hull
Office of the General Counsel*
One White Flint North
11555 Rockville Pike
Rockville, MD 20852

Roderick Ventura
Samuel D. Gollis
DNA - People's Legal Services, Inc.*
PO Box 306
Window Rock, AZ 86515

Diane Curran
HARMON, CURRAN, SPIELBERG &
EISENBERG, LLP*
1726 M Street NW Suite 600
Washington DC 20036

Brit Clapham, Acting Attorney General
Steven J. Bloxham, Esq.
Navajo Nation Department of Justice
P.O. Drawer 2010
Window Rock, AZ 86515

Anthony J. Thompson
Frederick Phillips
David Lashway
SHAW, PITTMAN, POTTS &
TROWBRIDGE*
2300 "N" Street, N.W.
Washington, D.C. 20037-1128

William Paul Robinson
Chris Shuey
Southwest Research and Information
Center
P.O. Box 4524
Albuquerque, NM 87106

Mitchell Capitan
ENDAUM
P.O. Box 471
Crownpoint, NM 87313

Dated at Santa Fe, New Mexico,
April 8, 1999,

Johanna Matanich

UNITED STATES OFAMERICA
NUCLEAR REGULATORY COMMISSION

ATOMIC SAFETY AND LICENSING BOARD PANEL

Before Administrative Judge Peter B. Bloch

In the Matter of	)	
	)	
HYDRO RESOURCES, INC.	)	Docket No. 40-8968-ML
2929 Coors Road Suite 101	)	
Albuquerque, NM 87120	)	ASLBP No. 95-706-01-ML
	)	

WRITTEN REPLY TESTIMONY OF DR. RICHARD J. ABITZ

On behalf of Eastern Navajo Diné Against Uranium Mining ("ENDAUM") and Southwest Research and Information Center ("SRIC"), Richard J. Abitz submits the following reply testimony regarding ground water quality issues raised by Hydro Resources Inc.'s ("HRI's") amended application for a source materials license.

Q.1. Please state your name and qualifications.

A.1. My name is Richard J. Abitz. My qualifications are set forth in my testimony and attachment A thereto, which were submitted on behalf of Intervenors' Written Presentation in Opposition to Hydro Resources, Inc.'s Application for a Materials License with Respect to: Groundwater Protection (January 11, 1999) ("Intervenors' Groundwater Presentation").

Q.2. What is the purpose of your testimony?

A.2. The purpose of my testimony is to reply to the new information presented in Hydro Resources, Inc.'s Response to Intervenors' Brief in Opposition to Hydro Resources, Inc.'s Application for a Materials License with Respect to

EXHIBIT
tabbies
A

Groundwater Issues ("HRI Response") and the NRC Staff's Response to Intervenors' Amended Presentation on Groundwater Issues ("Staff Response").

Q.3. What materials have you reviewed in preparation for your testimony?

A.3. In addition to the materials listed in my testimony in support of Intervenors' Groundwater Presentation, I have reviewed the HRI Response and the Staff Response, including their various affidavits and attachments.

Q.4. What is your evaluation of the Staff's assertion that Bison Basin was a successful commercial restoration?

A.4. This assertion was made for the first time in the Staff's Response. As Dr. Staub testifies, the assertion that Bison Basin was a commercial operation is misleading. The assertion that restoration was successful is also misleading. I evaluated the limited data on the Bison Basin water quality summary before, during, and after leaching restoration, which is listed in Exhibit H of my previous testimony, and it is clear that bicarbonate, chloride, and uranium were not restored to baseline levels. Moreover, there are no reported data for trace-element concentrations in the water quality summary, and it is not possible to evaluate whether arsenic, molybdenum, and selenium were returned to baseline levels. In conclusion, water quality data are incomplete with respect to reporting trace-element concentrations that are commonly elevated during in situ leaching. The limited reported data show groundwater in Bison Basin was not restored to baseline levels.

Q.5. What is your response to the new information submitted in Exhibit 13 of the Pelizza Affidavit?

A.5. HRI's Response provided, for the first time, copies of the U.S. Environmental Protection Agency (EPA) Region VI aquifer exemptions for URI sites in Exhibit 13 to the Pelizza affidavit. As stated in the exemptions, EPA recommended closer spacing of monitor wells when private wells are near a mining area. As noted in answer A.8.b of my written testimony (Jan 99), NRC guidance and common industry practice associated with monitoring contaminated groundwater plumes dictate that a higher density of monitoring wells should be placed between the ISL operation and downgradient private wells. The Texas aquifer exemptions for URI provided by HRI demonstrate that Region VI EPA recognizes and concurs that this is good industry practice. Neither HRI, the NRC Staff, nor their experts appear to be aware of NRC guidance or standard industry practice.

Mr. Pelizza chastises Dr. Staub and myself for not discussing the EPA's underground injection control (UIC) program. Pelizza Affidavit at 15-16. The UIC program is administered by EPA and is generally irrelevant for the purposes of this discussion.[1] The NRC continues to regulate ISL mining, and

[1] To my knowledge, Region IX EPA has approved neither an aquifer exemption nor a UIC permit for any of its mine sites. EPA Region IX administers the Navajo UIC program, and has asserted jurisdiction over Sections 8 and 17. The aquifer exemption approved by Region IV EPA for the New Mexico Environment Department (NMED) was issued in error because NMED lacked regulatory jurisdiction to seek the exemption or issue a UIC permit.

in so doing, has a responsibility to review license applications for their adequacy to protect the environment and public health and safety. In this instance, denser spacing of horizontal monitor wells is necessary under the NRC mandate. The endorsement of this approach in EPA's aquifer exemptions for URI provides additional evidence that an increase in density is good industry practice.

Q.6. What is your opinion of the new arguments presented by the NRC Staff and HRI on issues concerning the shape and formation of uranium roll front deposits in the Westwater Canyon Aquifer?

A.6. Both the NRC Staff and HRI make critical errors in their new arguments regarding roll-front deposits. The study of redox interfaces and their tie to sedimentary facies is founded in geology and chemistry, and long before uranium ISL operations began, geochemists studied redox interfaces. The errors made by HRI and the Staff in the new arguments leads me to believe their credibility in the areas of geohydrology and geochemistry is marginal, at best, and critically questioned in the following specific areas:

Ford, in Paragraphs 11 and 12 of his affidavit, makes some new arguments regarding roll-front deposits, a subject he did not address in his February 20, 1998 affidavit. In doing so, he commits several errors.

First, it is common knowledge to any geohydrologist that hydraulic conductivity is greater in sand and gravel relative to silt and clay. Therefore, oxidized water entering the aquifer in recharge zones moves through the sand and gravel at a rate faster than water moving through silt and clay. The result of this is quite simple, most of the oxygen in the recharge water is delivered to sand and gravel beds relative to silt and clay deposits along the channel-margin facies. This accounts for the preservation of remnant ore bodies in silt and clay portions of the so-called "oxidized" zone of the Westwater Canyon aquifer. The remnant ore bodies exist in silt and clay beds within the oxidized zone because they have not received oxidized water sufficient to remobilize and move the uranium. Therefore, Ford is incorrect when he states in Footnote 2 that redox interface is not dependent on lithology. (see Peterson, Wentworth et al, and other references within New Mexico Bureau of Mines & Mineral Resources, Memoir 38).

Second, Ford contradicts his Footnote 2 in the last sentence of Paragraph 11, when he concludes, correctly, that the shale (i.e., silt and clay along the channel-margin facies) provides the reducing conditions necessary to form the uranium deposit as the groundwater velocity is reduced. This sentence also brings out Ford's indirect testimony to the presence of sand channels bounded by shale, which is in contrast to his assessment of the regional aquifer being an isotropic, homogenous thick sand unit. Further, it is established knowledge that the paleoflow direction in the Westwater Canyon Formation was east to southeast through the Church Rock and Crownpoint areas (see Galloway, Squyres, Wentworth et al, and other references within New Mexico Bureau of Mines & Mineral Resources, Memoir 38), and it is not coincidental that the sand channels and ore bodies parallel this east to southeast trend. Regional groundwater flow is presently to the north/northeast, or perpendicular to the fabric of the sand channels developed in the lobes of the Westwater fan system. Therefore, the sinuous trace of the stacked, east to southeast-trending ore bodies in the Crownpoint area (See Bartel's Exhibit A) reflect reduction and precipitation of dissolved uranium as it encountered the silt and clay channel-margin facies of east to southeast-trending sand channels.

Third, Ford states in Paragraph 11 that the shape of the uranium deposit has nothing to do with the speed of the groundwater. This is incorrect and reflects the Staff's lack of credibility in the area of geohydrology and geochemistry. Fundamentally, the rate at which oxygen in the groundwater is consumed determines where the uranium will be deposited. The depletion rate of oxygen in groundwater is a function of grain size, which affects groundwater velocity, and the composition of the aquifer sediment. Focusing on groundwater velocity, groundwater that moves through silt and clay at a lower velocity will consume its allotted oxygen over a shorter distance, relative to groundwater flowing through sand and gravel. Therefore, groundwater velocity and oxygen both decrease as the sand channel grades into the silt and clay of the channel-margin facies. In the Church Rock and Crownpoint areas, the result is a sinuous, east to southeast-trending ore deposit coincident with the margin of the sand channel. Ford then becomes confused in his ensuing discussion and contradicts his above statement as follows " ... it is not uncommon for some uranium roll front deposits to form where there is a decrease in velocity, such as where groundwater encounters shale-sandstone contacts." This is precisely the scenario I, and other geologists working on the Crownpoint uranium deposits (e.g., Wentworth et al, in New Mexico Bureau of Mines and Mineral Resources, Memoir 38), depict in the discussion of ore deposition along the channel-margin facies.

Finally, in Paragraph 12, Ford forgets his conclusion in Paragraph 11 when he develops the inane argument that a uranium ore body would be no longer than the width of the channel. As noted above, by Ford, myself and other geologists, the deposits may be longer than the width of the channel because the ore deposits can form along the channel margin. Therefore, the width of the sand channel has nothing to do with the length of the ore deposits in the Church Rock and Crownpoint areas. Most of the uranium ore bodies in the Crownpoint area are described as ore pods no longer than 200 feet, which, when stacked, collectively extend for over 2,000 feet (Wentworth et al in New Mexico Bureau of Mines & Mineral Resources, Memoir 38). This sheds light on Ford's statement that "At HRI's Church Rock site, the best evidence shows that the uranium ore body is in fact 5,300 feet long." It is clear from documented investigations of the size of individual ore pods in the Church Rock and Crownpoint areas that HRI's 5,300 foot-long trace is likely to be neither continuous nor at the same stratigraphic horizon, but represents the collective trace of stacked ore pods. HRI has provided no detailed cross-sections of the ore body to confirm their statement that the ore body is a continuous 5,300 foot trace at a single stratigraphic horizon, as Ford implies.

In Paragraph 13, Ford states that a figure showing the Westwater Canyon aquifer to be 200 to 400 feet thick is conclusive evidence that a thick regional aquifer of areal extent is present, rather than a series of narrow isolated aquifers. I concur with Ford on his point of a thick regional aquifer, but I disagree with his misrepresentation of narrow sand channels as narrow isolated aquifers. The thick Westwater Canyon aquifer is comprised of braided stream deposits, which consist of sand and gravel channels with silt and clay comprising channel-margin facies and mudstone beds that separate the main sand facies - all of which are saturated with water and thus comprise a thick aquifer. Ford is obviously not able to grasp the difference between a thick aquifer and a thick sand, and hence has little credibility as a geohydrologist.

The general conclusions reached by HRI's Lichnovsky on the origin of roll-front uranium deposits have not been expressed prior to his recent affidavit. Lichnovsky's opinions reflect the same errors of simple thinking and simple science portrayed by Ford, which have little merit when applied to the complex, dynamic hydraulic system associated with the Westwater Canyon aquifer. Lichnovsky states in Paragraph 8 of his affidavit that "U.S. roll fronts are formed in broadly continuous braided stream deposited sandstones from dilute uranium bearing groundwater migrating down gradient." And, he continues, "This type of deposition requires that the sandstone aquifer is continuous and expansive because oxidized water must pass over large volumes of rock that contain small amounts of uranium and then travel uninterrupted to the redox contact where accumulation or deposition can ultimately occur. Channels would not supply the necessary source rock." In these three sentences, Lichnovsky believes he has accounted for the formation of every roll front deposit in the U.S., which is incredulous thinking for someone who professes to be a scientist. Lichnovsky lives in a myopic world that allows him to exclude all the previous work done on the Church Rock and Crownpoint uranium deposits (e.g., New Mexico Bureau of Mines & Mineral Resources, Memoir 38), published work that shows the ore deposits are not all roll-front deposits, and those that are described as roll-front deposits do not all form in a continuous and expansive sandstone aquifer.

Wentworth et al (New Mexico Bureau of Mines & Mineral Resources, Memoir 38) describe the Crownpoint Section 29 uranium deposits as follows (see pages 140 and 142 of Exhibit I of previous testimony):

> The Westwater Canyon Member is the host unit for the uranium deposits of sec.29 and is 250-350 ft thick. It consists of a series of gray- to light-red, fine- to medium-grained, feldspathic sandstones with a number of well-defined mudstone beds of variable thickness. The sandstones are generally cross-bedded, poorly sorted, and only moderately well cemented. Subsurface studies made with electrical logs show that the member is separated by mudstone beds into five distinct sandstone units that are informally termed, in descending order, the A, B, C, D, and E sands.

> Uranium concentrations occur as a series of pods in well developed channel sandstones composing the upper three-fourths of the Westwater Canyon Member. The individual pods are irregular in configuration and are elongated and parallel to the regional sedimentary trends (N 70-80 W). Ore pods occur in the A, B, C, and D sandstone units, and within each sandstone unit the pods may overlap, coalesce, or occur en echelon.....In sec 29, the uranium occurs in successively lower sandstone units from north to south. Thus, deposits in the D sand are farther north and structurally deeper that those in the upper three sandstone units......The Crownpoint deposits seem to be the result of dissolution and differential movement of uranium downdip by oxygenated ground waters. The main ore deposits in the C and D sands appear to have been moved northward from their original location. Small remnants of C and D ore are preserved along the south side of the Crownpoint Sec. 29 deposits. Studies of Westwater Canyon drill-cuttings samples suggest that the northward movement of the ore in C and D sands was impeded by a decrease in grain size and permeability in the northern part of the Crownpoint depocenter. The ore deposits in the upper, more lenticular A and B sands seen to have been less affected by oxidizing ground waters, and their ore deposits remain very close to where they were emplaced originally.

In these two short Paragraphs cited from Wentworth et al, we have learned more about the uranium deposits in the Crownpoint and Church Rock areas than HRI has told us in thousands of pages of permit applications and testimony. What we have learned is: (1) the Westwater sandstones are poorly sorted and separated by mudstones, making Lichnovsky's broad interconnected sandstone theory illusory, (2) ore bodies are elongated east/southeast and parallel the trend of sand channels, (3) movement of ore bodies by oxidation and reduction is influenced by grain size and permeability as we move north, perpendicular to the trend of stream channels, (4) ore deposits in the A and B sands have been affected very little by oxidizing ground water.

Items (2) and (3) are especially important, as they strongly support my conclusion that HRI, the NRC Staff, and their consultants have not presented a realistic model of the formation of uranium deposits in the Church Rock and Crownpoint areas. The stacked roll-front deposits in Church Rock and Crownpoint parallel stream channels and form within the silt and clay channel-margin facies where decreasing grain size and permeability result in less oxygen and the formation of a reduction zone.

HRI's Pelizza falls into the same trap as Ford when he cites, in Paragraph 12.2 of his affidavit, the testimony of Lichnovsky on uranium mineralization along a geochemical reduction/oxidation (redox) front, rather than a stream channel. HRI, the NRC Staff, and their consultants fail to recognize that the uranium mineralization associated with the redox front in the Church Rock and Crownpoint areas is a direct consequence of the redox front being in the silt and clay channel-margin facies of the south to southeast-trending lobe of the Westwater fan system. Strictly speaking, the ore deposit is not in the sand channel, but it parallels the channel and defines the trace of sand channels in the fan lobe.

Q.7. Does this conclude your testimony?

A.7. Yes.

AFFIRMATION

I declare on this 7 day of April, 1999, at Ross, Ohio, under penalty of perjury that the foregoing is true and correct to the best of my knowledge, and the opinions expressed herein are based on my best professional judgment.

Richard J. Abitz

Sworn and subscribed before me, the undersigned, a Notary Public in and for the State of Ohio on this 7 day of April, 1999, at Ross, Ohio.

My Commission expires on ___________.

CARL F. FAUVER
NOTARY PUBLIC
IN AND FOR THE STATE OF OHIO
MY COMMISSION EXPIRES 4 MAY 2000

Carl F. Fauver
Notary Public

UNITED STATES OF AMERICA
NUCLEAR REGULATORY COMMISSION

ATOMIC SAFETY AND LICENSING BOARD PANEL

Before Administrative Judge Peter B. Bloch

In the Matter of	)	
	)	
HYDRO RESOURCES, INC.	)	**Docket No. 40-8968-ML**
2929 Coors Road Suite 101	)	
Albuquerque, NM 87120	)	**ASLBP No. 95-706-01-ML**
	)	

WRITTEN REPLY TESTIMONY OF DR. WILLIAM P. STAUB

On behalf of Eastern Navajo Diné Against Uranium Mining ("ENDAUM") and Southwest Research and Information Center ("SRIC"), William P. Staub submits the following reply testimony regarding ground water quality issues raised by Hydro Resources Inc.'s ("HRI's") amended application for a source materials license.

Q.1. Please state your name and qualifications.

A.1. My name is William P. Staub. My qualifications are set forth in my testimony and attachment A thereto, which were submitted on behalf of Intervenors' Written Presentation in Opposition to Hydro Resources, Inc.'s Application for a Materials License with Respect to: Groundwater Protection (January 11, 1999) ("Intervenors' Groundwater Presentation").

Q.2. What is the purpose of your testimony?

A.2. The purpose of my testimony is to reply to the new information presented in Hydro Resources, Inc.'s Response to Intervenors' Brief in Opposition to Hydro Resources, Inc.'s Application for a Materials License with Respect to Groundwater Issues ("HRI

EXHIBIT
tabbies
B

Response") and the NRC Staff's Response to Intervenors' Amended Presentation on Groundwater Issues ("Staff Response").

Q.3. What materials have you reviewed in preparation for your testimony?

A.3. In addition to the materials listed in my testimony in support of Intervenors' Groundwater Presentation, I have reviewed the HRI Response and the Staff Response, including their various affidavits and attachments.

Q.4. What is your evaluation of the new data on reverse osmosis performance and restoration at the North Platte pilot project, provided in Tables 9 and 10 of Mark Pelizza's most recent affidavit, and the implications of these new data for monitor well functions?

A.4. Part 1: Mr. Pelizza takes issue with the view I took in my January testimony that sites with naturally poor water quality should be *easier* to restore to baseline than those with good quality water because the amount of contaminant reduction required to meet restoration standards is less at the poor water quality sites. He uses reverse osmosis (RO) water quality data from two ISL sites to support his assertion that aquifers with good baseline water quality are more easily restored than aquifers with poor baseline water quality. These data appear in Table 9 on page 72 of his most recent affidavit, wherein he compares the quality of contaminated water input to the RO unit with the quality of RO output water (i.e., "permeate") from the same unit for ISL mines at North Platte, Wyoming, which has good baseline groundwater quality, and Rosita, Texas, which has poor baseline groundwater quality.

Pelizza's data not only do not support his position, but they actually lend support to mine. They show quite clearly that reverse osmosis units operate more efficiently when the contaminated input water is not particularly high in total dissolved solids. Indeed, the North Platte "RO Feed" water had a TDS concentration of 1,392 mg/l, compared with a TDS concentration of 140 mg/l for the "RO Product" water (i.e., permeate), which represents a nearly 10-fold reduction in contaminant levels. In contrast, the TDS concentration in the Rosita input water was reduced only 6.8 times, from 4,380 mg/l to 643 mg/l.

Mr. Pelizza should have compared *baseline* groundwater quality with permeate quality for the two mines. At Rosita, baseline concentrations of most parameters listed in Table 9 exceed permeate concentrations by at least a factor of four. For example, baseline TDS at Rosita Production Area 3 was 2,524 mg/l in 1996, compared with a permeate concentration of 643 mg/l.[1] If restoration permeate at Rosita were to replace all the contaminated mine water in the well field, water inside the well field would be cleaner than baseline conditions *by a factor of four*.

Baseline concentrations at North Platte are presented in Table 10 of Mr. Pelizza's February 19, 1999, affidavit (at 76). At North Platte, baseline concentrations of most parameters exceed permeate concentrations by factors of only two to three, which suggests to me that reverse osmosis is more effective in restoring Rosita groundwater than North Platte groundwater, despite the higher reverse osmosis efficiency at North Platte. For the critical indicator parameter, chloride, for example, Mr. Pelizza's data (Table 9) show that the permeate concentration of 66 mg/l chloride was more than 7 times chloride baseline concentration of 9 mg/l

1 I obtained the Rosita baseline data from Table 6 of my January 9, 1999, testimony.

(Table 10). If restoration permeate at North Platte were to replace all the contaminated mine water in the well field, water inside the well field would be 7.5 times more concentrated in chloride than it was under baseline conditions. This suggests that groundwater sweep was partially effective in restoring the aquifer at North Platte. In contrast, a comparison of permeate and baseline concentrations at Rosita suggests that restoration could have been accomplished there by reverse osmosis alone. For example, the chloride concentration of the Rosita permeate was 3.7 times *better* than the baseline concentration (257 mg/l v. 952 mg/l, respectively). Therefore, my assertion that good quality aquifers are more difficult to restore to baseline conditions is still valid. Pelizza's reasoning is faulty and he failed to analyze critical baseline groundwater quality data.

An illustration of the difference between restoration of poor quality and good quality aquifers is the Bison Basin project in Wyoming. The Wyoming Department of Environmental Quality (WDEQ) restored the Bison Basin well field after the operator went bankrupt and forfeited its surety bond. When I spoke recently with WDEQ representatives,[2] they acknowledged they "got lucky" because the Bison Basin well field had poor baseline groundwater quality. According to WDEQ, restoration would have been more difficult had the well field been more typical of Wyoming, i.e., had it had generally good baseline groundwater quality. WDEQ was also fortunate to have a nearby source of

2 Telephone call approximately March 18, 1999, with WDEQ representatives.

hydrogen sulfide (from an oil and gas field), which was injected into the well field to lower the selenium solubility in order to reduce selenium levels.[3]

A. 4. Part 2: Mr. William Ford, in his affidavit in support of the NRC Staff Response (at 17), stated that permeate should be "clean" water, implying that it is harmless, distilled, or otherwise benign. This is an incorrect assumption, at least in the sense that he uses the term "clean" to mean potable. While permeate is cleaner than pregnant lixiviant or process waste water, it is still not "clean" in the sense that it does not always meet federal, state or tribal drinking water standards. Indeed, an inspection of Mr. Pelizza's Table 9 for the Rosita site shows that the permeate there is not "clean" enough to be suitable for human consumption because three of the parameters (TDS, chloride, and manganese) exceed their respective federal drinking water standards, and a fourth, uranium, exceeds not only the 1991 proposed EPA standard of 0.02 mg/l, but also exceeds that NRC's uranium standard of 0.44 mg/l.[4] Similarly, despite its good overall quality, the North Platte RO permeate would not be suitable for drinking water, and could not, without treatment, be reinjected into an underground source of drinking water (USDW) because the radium-226 concentration of 43 pCi/l exceeds the federal standard of 5.0 pCi/l by nearly 9 times. The main point here is that RO permeate

---

[3] I am aware of, and do not dispute, Dr. Abitz's views that the Bison Basin project is not an example of successful restoration. Additionally, I am relying on information provided by WDEQ officials verbally, and have not reviewed any data to verify that H_2S injection actually improved selenium solubility.

[4] Various water quality standards, including drinking water standards, and numerical standards established for the Crownpoint Project by the NRC staff are listed in Tables 4.7 and 4.6 of the FEIS (at 4-28 to 4-30), respectively.

is not *clean* water by definition, and therefore, cannot be assumed to be suitable for reinjection into a USDW, like that at the HRI Church Rock site.[5]

A.4. Part 3: Mr. Pelizza's data simply confirm my previous analysis that reinjection of permeate presumed to be "clean" relative to either drinking water standards or site baseline quality *might* interfere with monitor well functions. The North Platte data are useful in this regard for a comparison with conditions at the HRI Church Rock site. Both have very low baseline chloride levels, 9 mg/l at North Platte and 6.2 mg/l at Church Rock. See, FEIS at 3-38. If HRI reinjects permeate having a chloride concentration 66 mg/l at or beyond the perimeter monitor well ring, it runs the risk of causing a monitor well or two to go on excursion status because the permeate chloride level would likely be much higher than the chloride upper control limit for the site. This would be a "false excursion" and detrimental to HRI's interest in not having excursions at all.[6]

Q. 5: Do you agree with William Ford's new assertion that Bison Basin is an example of a successfully restored ISL commercial-scale well field in Wyoming and, if so, does this negate your assertion that well field restoration is taking much longer in Wyoming than anticipated?

A. 5. Part 1: Restoration data for the Bison Basin project contained in the HRI application are sparce.[7] Four of the seven parameters for which restoration data were provided had "restored" concentrations *better* than baseline, and three did

---

[5] The North Platte RO permeate could not be reinjected into the Westwater Canyon Aquifer at the Church Rock site without treatment to remove radium, because the permeate contains about four times more radium than the "baseline" level there. See, FEIS at 3-36.

[6] What actual effect permeate reinjection would have at Church Rock or any of the other CUP sites cannot be estimated here because neither HRI nor the NRC Staff have provided estimated RO permeate water quality data for the project.

not. One of the parameters *not* restored was uranium, which went from a baseline concentration of <0.001 mg/l to a restored concentration of 0.4 mg/l. I agree with Dr. Abitz, therefore, that successful restoration at Bison Basin was not demonstrated fully.

Ford's assertion does not tell the complete story, however. The fact is that the restored commercial site was only slightly larger than that of a pilot test site, according to the WDEQ officials that I spoke with recently.[8] They said that the commercial site at Bison Basin consisted of only a few acres at most, which is much smaller than a typical commercial scale well field that ranges from 50 to 100 acres or more at other sites in Wyoming as well as at the proposed Crownpoint and Church Rock sites. The operator at Bison Basin went bankrupt before becoming fully operational. Ford also downplays (by way of a rather obscure footnote) the fact that restoration was carried out by the State of Wyoming after the Bison Basin operator went bankrupt and forfeited its surety bond. In any event, I remain of the view that <u>no private commercial operator</u> has yet restored a commercial-scale well field in Wyoming.

A. 5. Part 2: Even if Mr. Ford's Bison Basin assertion were to be accepted at face value, it does not negate the fact that good baseline water quality aquifers in Wyoming have taken much longer (7 years and counting) to restore than originally anticipated (2 years). The environmental impact of the much longer

---

7 See, HRI Response to *RAI #52*, Attachment 52-7.4, attached to letter from Mark Pelizza to Joseph Holonich (April 1, 1996).

restoration effort resulted in long term land application, larger holding basins for liquid wastes, and larger consumptive use of groundwater. These circumstances eventually led to the phasing out of land application of liquid wastes in favor of deep well injection.

This is not to say that restoration cannot ever be achieved to the satisfaction of regulators in Wyoming. However, even if a commercial well field is certified as restored, say, this year, restoration to original baseline conditions will have taken nearly 8 years of effort.

Q. 6: What is your evaluation of the new information HRI presents in support of the use of 5 standard deviations in establishing excursions?

A. 6.: Pelizza's February 19, 1999, affidavit includes a memo from Craig Bartels on this subject. The concept of using a composite mean is critically flawed.

Even assuming that 3 standard deviations (sd) would guarantee false excursions, as Bartels explains, the discussion needs to go one step further. A particular excursion parameter has an equal chance of having a baseline concentration that is 3 sd below the composite mean, as above the composite mean. For a monitor well where a baseline concentration for a parameter is 3 sd below the composite mean, to be placed on excursion status using 5 sd above the composite means that as the basis for declaring an excursion, the concentration would have to increase through 8 sd, while the concentration for the well described by Bartels would have to pass through only 2 sd. Now suppose that a monitor well requiring, in effect, an 8 sd increase in concentration is placed between the mine well field and

---

[8] See footnote 2 above.

a municipal well field. What are the chances of that well ever going on excursion status? I would say two chances: slim and none.

As shown above, using 5 sd above the composite mean is a flawed methodology for indicating excursions. As Bartels points out some monitor wells (about one well in six) are much more sensitive to upper control limits than others when using the composite mean. As I point out above, another set of wells (also, one in six) is much less sensitive. In the middle are about 2/3s of the wells, which are neither over- or under-sensitive. The bottom line, when a composite mean is employed, is that some wells are far less likely to go on excursion status while others are far more likely to do so. This is not good science.

Q. 7: What is your evaluation of Pelizza's assertion on pages 34-37 that HRI has operated near private wells at Kingsville, Texas, without any impact on the private wells?

A.7. Pelizza's assertion is not supported by the necessary data. One must know whether these private wells were up or down gradient, what their production rates, and how they are used (domestic, livestock, irrigation?). An area that is referred to as a single aquifer can be divided into separate zones by an aquitard. Thus, the Goliad could contain an internal aquitard that separates the production zones from the private wells. Is there an aquitard between private well producing zones and mine water producing zones? Pelizza does not provide a sufficent description of the aquifer to make this determination. His argument is unsubstantiated.

Q. 8. What is your evaluation of the argument presented by Mr. Lichnovsky that erosion has not compromised the Brushy Basin member at Church Rock?

A.8. There is only speculation at this point in time. Lichnovsky (February 19, 1999 affidavit, paragraph 13, pages 23-24) states that the average thickness of the Brushy Basin member in the Church Rock vicinity is 63 feet. Lichnovsky does not provide us with the Brushy Basin's thickness range at Church Rock. We can only speculate on the range of thickness at Church Rock because we do not have access to critical data (geophysical logs in and around the proposed well fields). Kirk and Condon, 1995, on the other hand, state that the maximum relief on the Morrison erosion surface is 120 feet in the region. Therefore, it is possible in my view that the Brushy Basin member may be absent over parts of the proposed Church Rock site (due to the absence of evidence to the contrary).

Q.9: What is your reaction to HRI's claim that bleed rate is not as important as well-field balance?

A.9. Actually, I am delighted to see that HRI now concedes that "well pattern design and operation . .. *is the primary basis* for control of the lixiviant; the bleed is merely one tool, but not the primary tool used in lixiviant control." HRI Response at 14 (emphasis added). This was exactly the position stated in my original testimony of January 1999. However, while preparing my testimony, I was left with the distinct impression that both HRI and the NRC Staff viewed the bleed rate as the primary method of lixiviant control. In fact, that is why I discussed at length the importance and necessary of proper well field control, operation and balance. As such, I view HRI's current position as a significant change of heart, and I question whether there is any substance to it. With the exception of a comment Mr. Bartels made about checking field operations on a periodic basis, I did not find any plan or detailed approach for implementing and carrying out a comprehensive well field balance program.

I was concerned at the time of my January testimony and remain concerned now that well field balance can get out of control by human error, hesitancy in carrying out needed corrective actions, or inability to recognize excursions in a timely manner. HRI's insistence on having an across-the-board 5 standard deviation approach to excursion indicators and its resistance to having closer spaced monitor wells (see discussion below) make me question its commitment to prompt and effective lixiviant control.

Q.10: Did you find any new information provided by HRI in its recent groundwater response that supports your view that perimeter monitor wells should be spaced closer than 400 feet?

A.10: Yes I did. It was contained in a July 1, 1994, letter from Myron Knudsen of U.S. EPA Region VI to the Texas Natural Resource Conservation Commission, granting EPA approval of an aquifer exemption for URI's Kingsville Dome mine. The letter, which is attached to this reply, stated,

> We recommend that in future Production Area Authorization (PAA) actions that closer monitor well spacing and more frequent monitor well sampling be incorporated in the PAA's that are in closer proximity to private water wells located in the buffer zone.

Knudsen letter at 1. My suggestion and the reasoning underlying it was entirely consistent with EPA's recommendation. Monitor wells need to be spaced closer together in the down gradient direction for two reasons. One, excursions are more likely to occur in the down gradient direction (all other factors being equal) and, two, once an excursion occurs in the down gradient direction it is more difficult to pull back into the well field. Let's consider the following scenario. Poorly controlled injection breaks through the groundwater divide that was created when the well field bleed was functioning as intended. Then, as injection is brought back under control, the groundwater divide closes in behind the escaping lixiviant. In order to recover escaping lixiviant production must increase while injection is reduced. This creates a water storage problem at the surface. In contrast, an excursion in the up gradient direction would soon return to the well field naturally (like throwing a glass of water into the wind) as injection wells are brought under control. Less heroics are required to bring an up gradient excursion under control. In any event HRI and NRC seem to arguing over a trivial number of additional wells compared to the 800 or so wells to be used for production and injection at the Church Rock site alone.

Q. 11: Does this conclude your testimony?

A. 11. Yes.

AFFIRMATION

I declare on this 6TH day of April, 1999, at KNOXVILLE Tennessee, under penalty of perjury that the foregoing is true and correct to the best of my knowledge, and the opinions expressed herein are based on my best professional judgment.

William P. Staub
William P. Staub

Sworn and subscribed before me, the undersigned, a Notary Public in and for the State of Tennessee, on this 6th day of April, 1999, at Knoxville, Tennessee.
My Commission expires on 4-2-2000.

Vivian B. Mowery
Notary Public

UNITED STATES OF AMERICA
NUCLEAR REGULATORY COMMISSION
BEFORE THE ATOMIC SAFETY AND LICENSING BOARD

The Honorable Peter B. Bloch, Presiding Officer

In the Matter of	)	
	)	
HYDRO RESOURCES, INC.	)	Docket No. 40-8968-ML
(2929 Coors Road, Suite 101	)	ASLBP No. 95-706-01-ML
Albuquerque, NM 87120)	)	
	)	

WRITTEN REPLY TESTIMONY OF MICHAEL G. WALLACE

On behalf of Eastern Navajo Diné Against Uranium Mining ("ENDAUM") and Southwest Research and Information Center ("SRIC"), Michael G. Wallace submits the following reply testimony regarding ground water quality issues by Hydro Resources Inc.'s ("HRI's") amended application for a source material license.

Q.1. Please state your name and qualifications.

A.1. My name is Michael G. Wallace. My qualifications are set forth in my testimony and Exhibit A attached thereto, which were submitted on behalf of Intevenors' Written Presentation in Opposition to Hydro Resources, Inc.'s Application for a Materials License with Respect to: Groundwater protection (January 11, 1999) (Intervenors' Groundwater Presentation").

Q.2. What is the purpose of your testimony?

A.2. The purpose of my testimony is to reply to the new information presented in Hydro Resources, Inc.'s Application for a Material License with Respect to Groundwater Issues ("HRI Response") and the NRC Staff's Response to Intervenors' Amended Presentation on Groundwater Issues ("Staff Response").

EXHIBIT
tabbies
C

Q.3. What materials have you reviewed in preparation for your testimony?

A.3. In addition to the materials listed in my testimony in support of Intevenors' Groundwater Presentation, I have reviewed the HRI Response and the Staff Response, including their various affidavits and attachments.

Q.4. What is your evaluation of HRI's new arguments regarding an alleged contradiction between the "pipeline" analogy and pump test studies you conducted?

A.4. HRI has made much of an apparent contradiction between my sand channel travel time analysis and my pump test analyses. I made no attempt to clarify this before because my approaches of conservatism are standard practice, and are well-known to any hydrogeologist or regulator in the professional environmental ground water community.

In this case, I was evaluating two separate issues. The first issue was whether or not HRI was conservative (as they and the NRC claim) in their calculations of ground water travel times from the mine zones to outside locations, including municipal water supply wells. The second issue was whether or not HRI demonstrated that adequate vertical confinement existed at selected mine sites (between the mine horizon and the next overlying aquifer). I argued that HRI was not conservative with regard to the first point. For the second point, I argued that their own data and their curious behaviour suggested that there was insufficient vertical confinement.

In an environmental permitting situation, when critical data and process knowledge are lacking, it is incumbent upon the applicant to apply conservative assumptions and analyses. A 'conservative' assumption is one that, in lieu of better information, incorporates 'worst case' features. It is by its very nature, somewhat unrealistic. If more realistic information can be incorporated, then the applicant is justified in using such, which often helps to bring results back away from the precipice of a worst case conclusion.

Conversely, an applicant cannot use unrealistic information to bolster a non-conservative model. Nor can an applicant ignore realistic information that affects the project in question, if ignorance of this data is used to support a non-conservative model. This is exactly what HRI has done, time and again. Their travel times in the application are not conservative, but are claimed to be so. This is important, because it creates an impression of safety and low environmental impact. Furthermore, the travel times are not even realistic. This is somewhat akin to a medical official setting a TB carrier loose upon the world, prior to the development of effective antibiotics, and claiming that this is a conservative precautionary measure. My earlier arguments addressed the most important of the reasons that conservatism and realism were lacking. Let me clarify some of the fundamental physics of contaminant transport, based on the immense body of knowledge of transport in heterogeneous media (essentially, ALL ground water systems) that has been developed over the past 25 years. To avoid a time-consuming detour into this complicated field, I thought it was an efficient use of time and resources to bypass much of that in my previous testimony (thereby actually benefitting HRI's case in many ways).

However, now HRI has made this an issue by way of their 'contradiction' argument. In that argument, they have applied an aquifer pump test analysis to a perfectly confined sand channel to create what they claim are absurd aquifer parameter values. These so-called absurd values are then used to discredit my arguments concerning travel times.

In doing so, they misuse the principles of pump test analysis as they are applied to contaminant transport prediction. The basic feature of their misuse is the fact that pump tests are somewhat crude instruments, that do not capture heterogeniety on a scale or to a resolution that bears on transport very much. It is well known (see for example Neuman, 1982, **Exhibit 1**) that a pump test interrogates a large volume of aquifer, yet produces nothing but an average value(s) of hydraulic conductivity (or transmissivity) for that volume. This is true even if that conductivity actually varies within that volume by a thousandfold or more.

A perfectly homogenous, isotropic aquifer has yet to be discovered in nature. Contaminants in solution (solutes) moving underground encounter a maze of tortuous pathways around grains, and groupings of sediment and other rocks. Some pockets of solute may encounter zones of extremely low conductivity, and move far slower than the average solute transport rate. Other pockets of solute may encounter zones of high conductivity (such as a sand channel deposit), and move far faster than the average solute transport rate. This 'dispersion' of contaminant plumes is undisputed in the body of hydrologic science Even in the most uniform of porous media environments, 'fingering' and 'channeling' of contaminant plumes have been observed. If a travel time calculation somehow captured the average solute travel rate, it would still not be conservative, since it would underestimate the fastest solute travel rates. By the same token, it would not even be realistic. It would merely be a useless 'average' estimate.

Clearly, HRI's analyses of travel times, based on assumptions of homogeneity and isotropy, were not realistic, no matter what the base hydrogeologic conditions were. My approach was to address the question; if the travel time calculations were not realistic, were they at least conservative? Although their 'transport' model is extremely crude by today's standards, there exist means to accomodate the dispersion phenomena. HRI could have incorporated dispersion into their travel time analyses to approximate a travel time for the 'front' of the contaminants to reach the boundaries of concern. They did not do so. I could only conclude that the model was neither realistic nor conservative. I then proceeded to develop an actual conservative, simple model. I indirectly incorporated longitudinal dispersion by virtue of a fast pathway, a single high permeability sand channel. I had good reasons to develop such a calculation as a reasonably conservative effort, and those reasons are documented in my previous affidavits.

In their most recent rebuttal, HRI has innapropriately applied my conservative travel time framework to pump test analyses, out of context. The following discussion explains why there is a disconnect between what we know about aquifer heterogeniety, and what we perceive of it via pump tests. Pump tests are useful but limited tools in the arsenal of the hydrogeologist. They were first developed in the early part of this century. The vaunted Theis equation, for example, was developed in the 1930s. This was well before issues of contaminant transport in ground water were even on the horizon. The equations were never designed to address contaminant transport issues. Rather, they were helpful aides in estimating sustainable well production rates for municipalities and agricultural operations. As such, they do not actually 'read' the flow of water or transport of solutes through a groundwater system. They only infer the actual flow rate potential of water to a well, primarily by the pressure head response.

The propagation of a pressure-head response (known as drawdown) through an aquifer is much quicker than the actual movement of water. That is because in part, the pressure response travels through the rock as well as the water (just as sound would, another pressure phenomenon), whereas the water can only travel through voids between rock particles. Consider a stone thrown into a still pond, creating an expanding realm of concentric ripples. The actual water in the pond does not move (except up and down), but the wave does. Pressure head changes propagate through an aquifer at about the same relative rate as ripples in a pond. The movement of this cone of depression is influenced to some degree by the aquifer conditions, but these waves are not nearly as influenced as much as the migration of solutes would be.

The analyst has only a record of changing water levels in a few observation wells to record this pressure head propagation. It is amazing that hydrologists can infer as much as they can from this dearth of information. Still, only the coarsest of aquifer features are generally recognizable. Such features as aquitards, dikes, and zones of recharge (lakes, rivers) are often discernable, but little more is. The natural heterogeniety patterns of typical aquifers are almost never observable in such tests. What's more, interpretation of such tests is not unique. In other words, different aquifer features can lead to the same pump test behaviour (Freeze and Cherry, 1979, p.61, of **Exhibit 2**).

This is a much different, messy, reality of aquifer science than the black-and-white production-only oriented ISL mindset. For example, Bartels seems to treat pump test methods as some magic, ultra precise black box, spitting out one and only one correct answer, no matter what the context. Application of the pump test data to an actual pipe, impermeable on all sides is of course absurd, and I would be the first to agree. However, Bartels would have us believe that because such an extreme case cannot exist, no 'sand channels' can exist at their sites. He further challenges me to present any evidence for such channelization.

In fact, I had already referred to such evidence. It was so clear, I did not earlier think it required clarification. Apparently, the black box mindset prevented the applicant from stepping back and looking at the most simple, straightforward evidence of all. If the aquifer were perfectly homogenous, then the cone of depression caused by a pumping well would be perfectly radially symmetrical. In other words, at any given radial distance from a pumped well, the drawdown would not vary. Concentric 'rings' of drawdown would be the net effect. Yet, this is not remotely the case in either of the HRI tests at Section 8 or at Crownpoint. At Section 8, observation well CR5 is 190 feet further away from the pumped well than observation well CR8, but its observed drawdown occurs more rapidly, such that, at the end of the pumping period, the drawdown is 7 feet greater than the closer well. This has not been explained by HRI. Given the expected orientation of sand channels in that area, anomaly is likely caused by preferential flow through such channels. The same can be observed in the Crownpoint test, where observation well CP8 is much further away from the pumped well than observation well CP7, but its observed drawdown is 2 feet greater than the closer well. Notably in both cases, enhanced drawdowns occur in parallel with the orientation of the buried fluvial channel environment. And, as expected, reduced drawdowns occur orthogonal to that orientation.[1] Figures A

[1]What's more, no drawdowns are even recorded for observation well CP9 which is adjacent to obs. well CP6 (which recorded 8 ft. of drawdown). HRI claims that this well only records a portion of the Westwater that is not connected to the other parts, yet they claim it is the same aquifer.

and B of **Exhibit 3** illustrate this.

In spite of pump test quantitative shortcomings, huge steps towards reality have been realized over the past few decades as heterogeniety was recognized as a ubiquitous feature, and more accurate equations were developed to approximate the actual physics of contaminant transport. The NRC has sponsored much of this research itself. With the advent of computers, these features were combined into sophisticated numerical analysis tools that are now routinely used throughout the environmental ground water professional community, *in conjunction with pump test analyses,* to address uncertainties with regard to the transport of chemicals in the subsurface.

Pump test analysis science has not kept up with these advances (see Neuman, 1982 again), but there are movements in this direction. For example, Sánchez-Vila et al. (1999, **Exhibit 4**) present a new analytic method of pump test analysis that accounts for a limited type of areal heterogeniety. In the meantime, many hydrologists develop models of the variability of hydraulic conductivity through inverse techniques, and other stochastic/statistical tools, such as kriging, variograms, and development of covariance distributions. Through such tools, analysts populate a model region with contiguous cells of hydraulic conductivity (K) that may vary over several orders of magnitude. Given the uncertainties, the analyst often produces dozens or hundreds of equally-likely K models for the same region (I myself have spent a significant portion of my professional carreer working in this field, on behalf of Sandia National Laboratories). Each K model is generally conditioned to only a few data points. Those few data points represent the only quantitative information available for the area of concern. That information is usually, some average K values, determined by pump test analyses.

When possible, analysts go further and try to incorporate more qualitative geologic data into their models. For example, analysts might incorporate 'channeling' features into their model if they knew that the aquifer was comprised of fluvial sediments from a high-energy braided stream depositional environment (such as the Westwater Canyon Member, in which the mines are proposed).

Analysts go to all that trouble because they know that aquifers are not homogeneous, in spite of the fact that this is an assumption of nearly all pump test analysis tools. Analysts who don't go to this trouble must defend their simplified approach by adopting conservative assumptions, such as I have. HRI has done neither. Nor does their model even agree with their own data (notably, the models are not calibrated, because it would be impossible to do so). That is why their model is indefensible.

It may also bear repeating, that taking conservative assumptions out of context can lead to improper conclusions. HRI took my sand channel analogy out of context and reached just such a conclusion. Wasiolak followed HRI's lead to reach the same ill-considered conclusion. HRI essentially went to great pains to demonstrate what I already had indicated; that my travel time analyses were conservative. Yet, my original point was that their travel time analyses were neither conservative nor realistic. When all is said and done, they have provided no information to rebut this point. Even as they emphasize that they never 'said' the aquifer was homogeneous, they have provided nothing but more calculations (Bartels bleed calculations) that still assume homogeneity. As shown below, I have now provided a number of relevant model simulations that address heterogeniety, and show plausible ways in which the mining operations could lead to excursions.

Q.5. What is your response to HRI's new arguments regarding your analysis of HRI's pump test data using the Hantush method?

HRI has brought up new responses to certain details of my Hantush analysis. They claimed that, among other things, my match was poor and that my calculated storativity was too low. They also claimed that I mischaracterize the so-called early time portions of the Hantush curve. Orr, for example, claims that for these tests, data from 20 minutes into these tests are far too early to be concerned about.

My primary point was that the pump tests were incorrectly conducted, and misrepresented as a basis for determining vertical confinement. In particular, I found it astounding that HRI would have us believe that upper aquifer observation wells 1,860 feet away, demonstrated confinement from a 3 day pump test. This concern still remains conspicuously unaddressed.[2] To help demonstrate one of the points, I showed that their own pump test data could be interpreted to show leakage. However, that analysis was not the foundation of my contentions in this area.

Although the calculated storativity was low, it was not due to an error.

---

[2] This concern stemmed from a strange observation well development history involving a sequence of Dakota wells drilled and abandoned in an ever-widening circle away from the pumping well. Even the first two wells were too far away. In his most recent response, Bartels claims that their reason for abandoning one of those wells due to poor barometric readings was justified, and based on concerns of diligence. I maintain my position, that this is nonsense. A barometric response of no more than a few centimeters, compared to an attempt to observe aquifer impacts of several feet, is truly the nit on the gnat's behind. If Bartels was so concerned, then why did they not place the Dakota observation well next to the pumping well in the first place?

Storativities resulting from Hantush or Theis are in large part a factor of the underlying assumptions about the aquifer. One who holds an a-priori conviction that the aquifer is perfectly homogeneous, isotropic and confined, would naturally be alarmed by the low number I obtained. But, as Freeze and Cherry discuss, the storativity and transmissivity (T) terms lose their meaning in many circumstances, such as three-dimensional effects **(Exhibit 2)**. The number merely indicates that this aquifer does not fit the mold as well as HRI would have us believe. To humor HRI, I did make an attempt to improve my curve match, and I also attempted to recreate their match (Figures C and D, Exhibit 3) to Theis. In so doing, I discovered an inconsistency or error in their calculation of transmissivity for the CP 2 test. In order for T to equal 2641 gallons per day per ft., the drawdown (s) axis should pair with the W(u) axis at W(u) =1 and s=4.3. At that pairing, a good match to the Theis type curve is impossible.

With regard to the claims that I misused early time data, I would merely step out of the way, so that these experts may take up their case with S. W. Lohman, of the US Geological Survey, and author of one of the most widely used aquifer analysis texts in the nation. I provide the pertinent sections as **Exhibit 5**. Therein, Lohman provides a real-world example of the use of Hantush. In that case, an observation well is considered, that lies much farther away from the pumping well than the cases I considered. Lohman explicitly takes into account very early data, earlier in fact than the data I consider. Consequently, he also considers very subtle drawdown changes at that time. The discussion by Orr, of what is really meant by early time data, as something actually much later than this, is wrong. Clearly there are only three major cases for these type curves; an early, steep portion, a middle, transition portion typified by a strong change in slope, and a late, 'flat' portion. On another note, in evaluating this test and comparing it to an earlier interpretation of the same data, via the Theis method, Lohman points out that Theis lead to overestimation of T in this case by 5 to 20 times more, and apparent values of storativity of from 17 to 25 times more. Given that the HRI data project hydraulic conductivities (T divided by aquifer thickness) at the extreme high end for sandstones (in spite of their being 'gummed up' with humics), overprediction likely occurred here as well. Given that most other tests in the region also likely used Theis, it may explain the general high values.

Citing these generally high values, HRI projects an unreal, homogeneous paradigm once again. On the contrary, as I have shown by references and by their own test data, heterogeniety abounds in that environment. Heterogeniety, in this context, is the change in hydraulic conductivity (K) from one area to another, or even one 'point' to another. Freeze (Exhibit 2) shows that sandstone/shale compositions alone (the case of the Westwater) have conductivities that can vary over 7 orders of magnitude. A 1 order of magnitude difference in test results is entirely consistent with this range, and also with Lohman.

In my later modeling study, I incorporate discontinuous channel features that vary by only 3 orders of magnitude. That study points to the crucial effects that heterogeniety play in lixiviant transport.

Q.6. What is your opinion concerning HRI's characterization of the relationship between roll fronts and sand channels?

A.6. The arguments that the ore bodies have nothing to do with the channelization heterogeniety appear to be unsupported by hard data[3], and go in direct

---

[3]Bartels attachment on roll fronts for example is referred to as a schematic, not as proof.

contradiction to the obvious correlation between the two. One opponent's argument concerning sharp roll fronts allows for the fact that first, humates must exist in the aquifer to reduce the uranium. The humates themselves are obviously concentrated along the margins of sand channels, as even Ford seems to admit. Ford even discusses low permeability barriers to flow that help concentrate the uranium. This is nothing more than the low permeability zone that borders long sand zones, or channels. The fact that the material was deposited in a direction orthogonal to the sand channel axes is perfectly consistent with this development. In fact, I always considered this to be the logical case, particularly given the millions of years available for such deposition.

On the other hand, in lieu of humates and low K barriers, dissolved oxygen would disperse longitudinally for hundreds of meters or more in that huge groundwater basin, just like any other solute. A roll front follows where the oxygen is depleted. A roll front is sharp. If oxygen concentration were to have attenuated over hundreds of meters, then the roll front should have a similar distribution.

Q.6. What is your response to HRI's new claim that bleed rate is not as important as well-field balance and to the new calculations by Bartels that attempt to prove that the bleed re-injection will not compromise lixiviant control?

A.6. HRI now argues that the bleed rate is not the primary tool for lixiviant control See Bartels Affidavit at Paragraph 12. HRI also puts forth a new series of arguments and calculations that purport to show that a bleed rate is not actually necessary in the first place. In light of their new calculations I have modeled the bleed rate and its role in lixiviant control. This modeling is perfectly relevant to the conditions that they have presented, and it solidly supports my position.

HRI makes the argument that what they misrepresented as a process bleed is not important anyway. They go further and dismiss my projections of impact due to re-injection of process bleed. They discuss how well-balance is actually the key to control. However, I agree with Dr. Staub, Dr. Orr, and even V. Reed on this issue. Notably Dr. Orr and Reed state that lixiviant control is accomplished specifically with the aid of a bleed. Dr Orr states that a bleed reinjected far away will have no impact on the control issue. I couldn't agree more. However, the application never stated where the bleed will be reinjected. Bartels now puts forward some vague assurances (accompanied by some very vague calculations) that the bleed will be reinjected somewhere along or outside the monitor well ring. I address others here, using the well known ground water model and particle tracking packages MODFLOW and MODPATH.

First, a partial summary of how HRI's characterization of bleed and lixiviant control has changed:

BLEED RATE/LIXIVIANT CONTROL HISTORY

Prior to discovery by intervenors that bleed is actually reinjected into same unit in same area	After discovery
Mining solutions at ISL extraction operations are controlled by .. continuously removing a minimum volume from the process circuit. This .. bleed, creates a net positive pumping balance in the well field." FEIS, pA-31 ..the overall injection flowrates .. will be less than the total extraction by .."process bleed", resulting in a hydraulic pressure sink which causes native groundwater outside of the ore zone to migrate into the wellfield" HRI, COP-69	
Primary containment is through over-production relative to injection, referred to in the industry as a "bleed" "The simulations performed .. indicate that, with a 1% bleed during mining .. the mining fluids can be adequately contained within the monitor well ring" HRI consultant model report "Analysis of Hydrodynamic Control.." 1993	Intervenor's (sic) mistakenly assume that bleed by itself is the most effective means of excursion control... This is not so. " HRI (Bartells,99)
".. the licensee shall at all times maintain sufficent emergency generator capacity to provide a 50gpm bleed.." NRC licence condition 10.6	..licenses/permits do not require that a certain bleed rate be used or maintained in real, ongoing operations." HRI (Bartells,99)

Second, Bartels claims that his new velocities are simply calculated as a direct result of the principal of superposition. Bartels does not offer a derivation of how this was done. He cannot. This is because velocities are not a direct result of that principal. Hydraulic heads can be calculated directly (under ideal circumstances). However velocities are not a function of heads, they are a function of hydraulic head gradients. Hydraulic head gradients are also not a direct result of the principal of superposition. Their distribution can only be ascertained after all the numerous head influences are 'added' up and, generally, contoured. Therefore, if Bartels did some sort of hand calculation, it is pure nonsense. It is critical for him to provide these calculations and their basis, if they were performed. If Bartels used some other indirect tool, such as a model, that should also be revealed. For now, given lack of crucial information, they should be dismissed as worthless.

Third, my model, performed in response to Bartels' new calculations. This model considers two cases, one similar to Crownpoint area and the other similar to Churchrock. Figures are provided in Exhibit 3. Both cases considering ISL mining 'cells' equivalent in size to the proposed cells. The aquifers are of similar thickness, average hydraulic conductivity, and hydraulic gradient. The following table lists pertinent values:

Selected Model Values

parameter/feature	Crownpoint, case 1	Church Rock, case 2
'avg'. K (ft./day), rounded	1.7 dominant K feature in model	0.9 dominant K feature in model
variation in K (when applicable)	a few discontinuous channel zones, some 10 times higher in K than 'avg', others 10 times lower in K than 'avg'. Total variation in K only 3 orders of magnitude.	same as case 1
orientation of discontinuous channels	roughly consistent with paleo depositional patterns in that are	roughly consistent with paleo depositional patterns in that area
ISL cell size	800 ft. by 1200 ft.	same as case 1
bleed rate	equivalent 40gpm areally distributed through cell	equivalent 40gpm areally distributed
bleed reinject, when used	analytic element well, 40 gpm	analytic element well, 40 gpm
regional hydraulic gradient	influence by municipal wells	0.009
aquifer thickness	200 ft.	200 ft.
steady state flow	yes	yes
model domain size	10,000' by 13,000'	13,000' by 10,000'
boundary conditions	constant head along N and S boundaries no flow along E and W boundaries	constant head along N and S boundaries no flow along E and W boundaries
position of bleed reinject (when applicable)	400' from mining cell, neutrally positioned (neither directly upgradient nor downgradient)	400' from mining cell, neutrally positioned (neither directly upgradient nor downgradient)
boundary effects	all boundaries approx. one mile from zone of concern, minimizing any possible boundary effects	all boundaries approx. one mile from zone of concern, minimizing any possible boundary effects

For both cases I merely applied the claimed bleed rates, and reinjection rates when pertinent, to systems essentially equivalent to those proposed by HRI. I added, in most cases, a relatively small amount of heterogeniety (see previous discussion), to evaluate plausible cases and outcomes.

Case 1: The first figure, 1a, shows a rectangular ore zone, peppered with small well cells that apply a net bleed of 40 gpm. The contour lines describe hydraulic head. A municipal water supply well is shown at right. The small dots between the well cells are origins for subsequent particle tracks. The huge mound of hydraulic head to the south of the ore zone is due to bleed reinjection there. Figure 1b shows two additional items. First, the long shaded rectangles are zones of different K. White, default is the 'avg' K. The darker shaded rectangles are zones of K 1 order of magnitude lower. The lighter shaded rectangles are zones of K 1 order of magnitude higher than the avg. This K distribution was more or less randomly chosen (except for their orientation, based on geologic knowledge). An infinite variety of Kzones and scales could be selected, meeting the constraints I have set. This particular pattern is meant to examine a plausible condition, and nothing more. The lines represent particle tracks which reflect the flow directions around and from the mine zone. Clearly, with this plausible setup and bleed reinjection, water is shown leaving the ore zone and going directly to the municipal water supply well. Note that flow can tend to channel in the high K zones, which are only about 100' wide, narrower than the monitor well spacings. Note that these channels do not directly connect the water supply well to the ore zone, that they are not surrounded by impermeable walls, and that they are not inordinately long. Finer scalings could no doubt be evaluated, but would likely show something of this nature.

Case 2, **Church Rock**. The orientation of the first set up was altered to reflect Church Rock conditions, as the figures show. Primarily, the model zone is now longest from south to north, and no water supply wells are included. Also the avg K is lower by roughly half, as are the corresponding high and low K features. Most interesting is the fact that since this area has a much higher ambient hydraulic gradient, the regional through-flow is even greater than at case 1. The models reveal that under these conditions, the purported bleed rate is not sufficient to control lixiviant under any circumstances (other than HRI's nonsensical homogeneous circumstance, which violates their own data). Figure 2a shows the first Church Rock scenario, with wells and bleed reinjection as before. Figure 2b shows water escaping the ore zone, via a plausible high K channel feature. The rate of escape is even greater than for the case 1. As pointed out before, the high K zones, are only about 100' wide, narrower than the monitor well spacings. The channels are not surrounded by impermeable walls, nor are they inordinately long. Figure 2c shows the same scenario with the distinctive difference that in this case, there is **no** bleed reinjection. Still, much water escapes, primarily due to the high regional flow-through, and the relatively low bleed rate. Finally figure 2 shows how my model replicates the more limited assumptions of the official HRI model. I set K to be homogeneous throughout the model domain. No bleed reinjection is simulated. In a manner quite similar to their model, control is shown. I did this last exercise as a type of benchmark to show that my model can recreate the limited conditions of their model. The reverse, however is not possible.

In summary, my model amply demonstrates that, given more realistic, and plausible conditions than HRI used previously, lixiviant control is not demonstrated. Nor is the ability to effectively monitor any subsequent excursion.

Q.7. Are you aware of any other facts that are necessary for a full and complete record in this case?

A.7. HRI represents in its response that Dr. Shlomo Neuman "did not disagree with the Staff's conclusions regarding the potential for excursions at the site, that he stated that he did not find anything in the FEIS that would disqualify the site from ISL mining, and that "it was his 'gut feeling' that HRI's proposed ISL operation was safe." HRI Response, Brief at 4-5; See Intervenors' Groundwater Presentation Exhibit 3-H at 2. In light of HRI's reliance on the NRC Staff's memo, I contacted Dr. Neuman myself to inform him of the controvery over his statements. Dr. Neuman responded to my electronic message by stating that the Staff memo misrepresents his opinions:

> Any statement or statements made by the NRC concerning opinions that I have allegedly voiced during [the March 19, 1998] teleconference are those of the agency, not mine. I have never been given a chance to review and/or comment on such statements. . .In particular, an NRC memo by Joseph J. Holonich, Chief of the Uranium Recovery Branch, addressed to Peter B. Bloch, Presiding Officer of the Atomic Safety and Licensing Board, dated April 20, 1998, misrepresents my association with the NRC and my opinions about the site.

Electronic Letter from Shlomo P. Neuman to Michael Wallace ¶¶8, 9 (March 3, 1999) a copy of which is attached hereto as Exhibit 6.

Q.8. Does this conclude your testimony?

A.8. Yes.

AFFIRMATION

I declare on this 8 day of April, 1999, at Santa Fe, New Mexico, under penalty of perjury that the foregoing is true and correct to the best of my knowledge, and the opinions expressed herein are based on my best professional judgment.

Michael G. Wallace

Michael G. Wallace

Sworn and subscribed before me, the undersigned, a Notary Public in and for the State of New Mexico, on this 8 day of April, 1999, at Santa Fe, New Mexico.

My Commission expires on 11/03/02.

Allison A. Dellinger

Notary Public

OFFICIAL SEAL
Allison A. Dellinger
NOTARY PUBLIC
STATE OF NEW MEXICO
My Commission Expires: 11/03/02

Geological Society of America
Special Paper 189
1982

EXHIBIT
tabbies
1

Statistical Characterization of Aquifer Heterogeneities: An Overview

SHLOMO P. NEUMAN
Department of Hydrology and Water Resources
University of Arizona
Tucson, Arizona 85721

ABSTRACT

In recent years, ground-water hydrologists have become increasingly aware of the need to describe the spatial variability of aquifer characteristics in statistical terms. This has led to the development of new theoretical models whose parameters, representing the aquifer characteristics, are treated as stochastic variables rather than deterministic functions of space. In this paper, the state of the art in stochastic modeling is reviewed, and the experience and new knowledge gained with these models are summarized. Many of the stochastic models developed to date allow the parameters to fluctuate with equal amplitude at every point in space, including points at which the material properties have actually been measured. A more recent trend has been to try to reduce the variance of the computed hydraulic head values by conditioning the model on measured values of aquifer transmissivities. It is argued that a further reduction in this variance could be effected by conditioning the model not only upon measurements of the aquifer characteristics, but also upon historical data relating to the prevailing flow regime. This additional conditioning can be achieved by estimating the model parameters with the aid of inverse methods that are compatible with the stochastic interpretation of spatial variability. Two such inverse methods are described in this paper. It is suggested that in the future, the output from inverse models should be used as input into stochastic models of groundwater flow. In addition to the need for interphasing inverse models with stochastic models, additional research is required to improve the reliability, versatility, and computational efficiency of these models.

Dedicated to the author's teacher, colleague, and friend, Paul A. Witherspoon, on the occasion of his 60th birthday

The statisticians believe those who prove points deductively to be dangerously intuitive. But, by their colleagues, those who are controlled by numbers are often thought unduly cautious or even dull.

John Kenneth Galbraith
The New Industrial State
1967

INTRODUCTION

Most quantitative methods in subsurface hydrology are founded on the classical theory of flow through porous media. This theory is concerned with macroscopic physical quantities, which vary in a deterministic manner and obey the principles of continuum mechanics. These principles are usually expressed in the form of partial differential equations, the parameters of which represent various material properties of the porous continuum such as hydraulic conductivity, storativity, and porosity, etc. In order to use the available mathematical tools to solve these partial differential equations, it is usually necessary to assume that their parameters remain uniform throughout the entire flow region of interest. Only in rare situations, such as flow to wells in multi-aquifer systems have hydrologists been able to overcome this restriction to some extent by assuming that the flow region can be subdivided into several layers, each of which can be characterized by a distinct set of uniform parameters (Neuman and Witherspoon, 1969).

In reality, subsurface materials are seldom uniform; on the contrary, their macroscopic properties usually vary from point to point in what appears to be a random manner. Evidence of such random spatial variability is provided by laboratory

a discrete summation: Water Resources
h, v. 12, no. 2, p. 181-184.

E., and Dagan, G., 1979, Solute dispersion unsaturated heterogeneous soil at field cale, part 2, applications: Soil Science Society of America Proceedings (in press).

rust, K. J., van Bavel, C. H. M., and Stirk, G. B., 1968, Hydraulic properties of a clay-loam soil and the field measurement of water uptake by roots, part 3: Madison, Soil Science Society of America Proceedings v. 32, p. 322-326.

Bulnes, A. C., 1946, An application of statistical methods to core analysis data of dolomitic limestone: Trans. AIME, v. 165, p. 223-240.

Buyevich, Y. A., Leonov, A. I., and Safrai, V. M., 1969, Variations in filtration velocity due to random large scale fluctuations in porosity, part 2: Journal Fluid Mechanics, v. 37, p. 371-381.

Csallany, S., and Walton, W. C., 1963, Yields of shallow dolomite wells in northern Illinois: Illinois State Water Survey Report Investigations, no. 46.

Dagan, G., 1976, Comment on 'A stochastic-conceptual analysis of one-dimensional groundwater flow in nonuniform homogeneous media' by R. A. Freeze: Water Resources Research, v. 12, no. 3, p. 567.

___1979, Models of groundwater flow in porous formations of a statistically homogeneous random structure: Water Resources Research, v. 15, no. 1, p. 47-63.

Dagan, G., and Bresler, E., 1979, Solute dispersion in unsaturated heterogeneous soil at field scale, I, theory: Soil Science Society of America Proceedings (in press).

Davis, S. N., 1969, Porosity and permeability of natural materials, in De Wiest, R. J. M., ed., Flow through porous media: New York, Academic Press, p. 54-89.

Delhomme, J. P., 1974, La cartographie d'une grandeur physique a partir de données de differentes qualités, in Proceedings, Montpellier Meeting, International Association of Hydrogeologists: v. 10, no. 1, p. 185-194.

___1978, Kriging in the hydrosciences: Advances in Water Resources, v. 1, no. 5, p. 251-266.

___1979, Spatial variability and uncertainty in groundwater flow parameters: Water Resources Research (in press).

de Marsily, G., 1978, De l'identification des systemes hydrogeologiques, tome I, syntheses [Ph. D. dissertation]: Paris, Université P. et M. Curie.

Erdelyi, A., ed., 1954, Tables of integral transforms, in Bateman manuscript project: New York, McGraw-Hill, v. 1, 391 p.

Farengolts, Z. D., and Kolyada, M. N., 1969, Opyt primeneniya metodov matematicheskoy statistiki slya izutcheniya zakonov raspredeleniya gidrogeologicheskikh parametrov: Trudy Vsegingeo, v. 17, p. 76-112.

Freeze, R. A., 1975, A stochastic-conceptual analysis of one-dimensional groundwater flow in a non-uniform homogeneous media: Water Resources Research, v. 11, no. 5, p. 725-741.

Freeze, R. A., 1977, Probabilisitic one-dimensional consolidation: Journal of Geotechnical Engineering Division, American Society Civil Engineers, v. 103, (GT7), p. 725-742.

Gates, J. S., and Kisiel, C. C., 1974, Worth of additional data to a digital computer model of a groundwater basin: Water Resources Research, v. 10, no. 5, p. 1031-1038.

Gelhar, L. W., 1976, Effects of hydraulic conductivity variations on ground-water flows, in Proceedings, Second International IAHR Symposium on Stochastic Hydraulics: Lund, Sweden, International Association of Hydraulic Research.

Gelhar, L. W., and others, 1977, Comments on 'A stochastic-conceptual analysis of one-dimensional groundwater flow in a nonuniform homogeneous media' by R. A. Freeze: Water Resources Research, v. 13, no. 2, p. 477-479.

Gelhar, L. W., Gutjahr, A. L., and Naff, R. L., 1979, Stochastic analysis of macrodispersion in a stratified aquifer: Water Resources Research (in press).

Griffiths, J. C., 1967, The scientific method in analysis of sediments: New York, McGraw Hill.

Gutjahr, A. L., and others, 1978, Stochastic analysis of spatial variability in subsurface flows, part II, evaluation and application: Water Resources Research, v. 14, no. 5, p. 953-960.

Heller, J. P., 1972, Observations on diffusion and mixing in porous media, in Proceedings, Second International Symposium on Fundamentals of Transport Phenomena in Porous Media, IAHR and ISSS, v. 1: Guelph, University of Guelph, p. 1-26.

Ilyin, N. I., and others, 1971, Otsenka tochnosti opredeleniya vodopronitsayemosti gornykh porod: Moscow, Nedra.

Jetel, J., 1974, Complément régional de l'information sur les parametres pétrophysiques en vue de l'élaboration des modeles de systémes aquiféres, in Proceedings, Montpelier Meeting, International Association of Hydrogeologists.

Krasny, J., 1974, Les différences de la transmissivité statistiquement significatives dans les zones de l'infiltration et du drainage, in Proceedings, Montpelier Meeting, International Association of Hydrogeologists.

Krumbein, W. C., 1936, Application of the logarithmic moments to size frequency distributions of sediments: Journal Sedimentary Petrology, v. 6, no. 1, p. 35-47.

Law, J., 1944, A statistical approach to the interstitial heterogeneity of sand reservoirs: Trans AIME, v. 155, p. 202-222.

Lippmann, M. J., 1973, Two-dimensional stochastic model of a heterogeneous geologic system [Ph.D. dissertation]: Berkeley, University of California.

Lumley, J. L., and Panofsky, H. A., 1964, The structure of atmospheric turbulence: New York, John Wiley and Sons, Inc.

Martin, R., 1974, On spatial dependence, bias, and use of first spatial differences in regression analysis: Area, v. 6, p. 185-194.

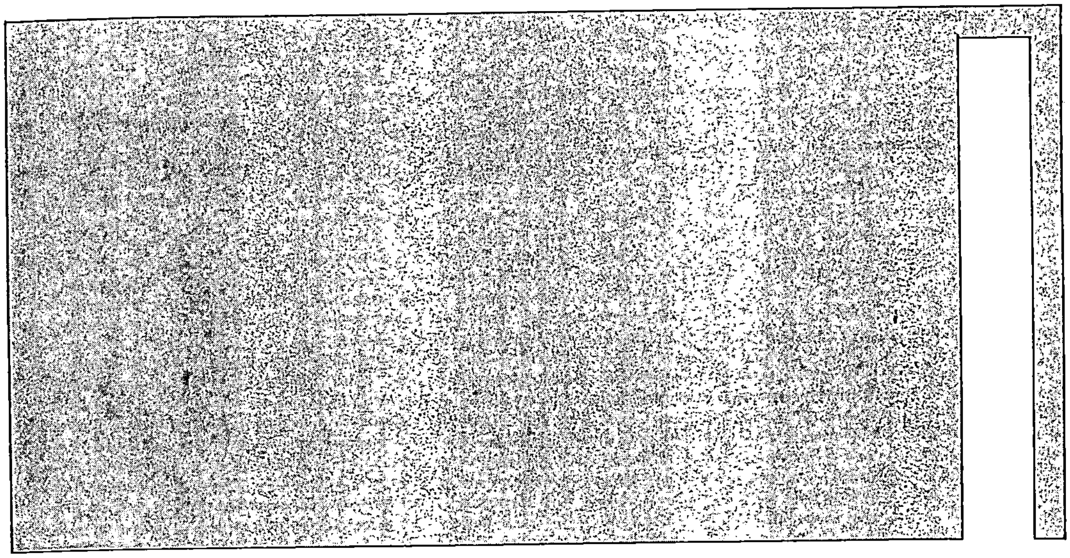
Matalas, N. C., 1967, Mathematical assessment of synthetic hydrology: Water Resources Research, v. 3, no. 4, p. 937-945.

Matheron, G., 1967, Composition des permeabilitées en milieu poreux heterogene, méthode de

ydler et régles de ponderation: Revue de Institut Francais du Pétrole, p. 443-466.
1971, The theory of regionalized variables and its applications: Fontainebleau, École des Mines.
___1973, The intrinsic random functions and their applications: Advances in Applied Prob., v. 5, p. 438-468.
McMillan, W. D., 1966, Theoretical analysis of groundwater basins operations: Berkeley, University of California, Hydraulics Laboratory Technical Report 6-25, 167 p.
Mejia, J. M., and Rodriguez-Iturbe, I., 1974a, Correlation links between normal and lognormal processes: Water Resources Research, v. 10, no. 4, p. 689-690.
___1974b, On the synthesis of random field sampling from the spectrum: an application to the generation of hydrologic spatial processes: Water Resources Research, v. 10, no. 4, p. 705-712.
Neuman, S.P., and Witherspoon, P. A., 1969, Theory of flow in a confined two-aquifer system: Water Resources Research, v. 5, no. 4, p. 803-816.
uman, S. P., and Yakowitz, S., 1979, A statistical approach to the inverse problem of aquifer hydrology, part I, theory: Water Resources Research (in press).
Neuman, S. P., Fogg, G. E., and Jacobson, E. A., 1979, A statistical approach to the inverse problem of aquifer hydrology, part II, case study: Water Resources Research (in press).
Nielsen, D. R., Biggar, J. W., and Erh, K. T., 1973, Spatial variability of field-measured soil-water properties: Hilgardia, v. 42, no. 7, p. 215-259.
Papoulis, A., 1965, Probability, random variables, and stochastic processes: New York, McGraw-Hill.
Rodriguez-Iturbe, I., and Mejia, J. M., 1974, The design of rainfall networks in time and space: Water Resources Research, v. 10, no. 4, p. 713-728.
ousselot, D., 1976, Proposition pur une loi de distribution des permeabilités ou transmissivités: Rapport BRGM, Service Géologique Jura-Alpes.
Sagar, B., 1978, Galerkin finite element procedure for analyzing flow through random media: Water Resources Research, v. 14, no. 6, p. 1035-1044.
Schweppe, F., 1973, Uncertain dynamic systems: Englewood Cliffs, Prentice-Hall.
Seaber, P. R., and Hollyday, E. F., 1968, Statistical analysis of regional aquifers: Boulder, Geological Society of America, Special Paper no. 101, abstracts for 1966.
Smith, L., 1978, Stochastic analysis of steady state groundwater flow in a bounded domain [Ph.D. dissertation]: Vancouver, University of British Columbia.
Smith, L., and Freeze, R. A., 1979a, Stochastic analysis of steady state groundwater flow in a bounded domain, part 1, one-dimensional simulations: Water Resources Research (in press).
___1979b, Stochastic analysis of steady state groundwater flow in a bounded domain, part 2, two-dimensional simulations: Water Resources Research, (in press).
Tang, D. H., and Pinder, G. F., 1977, Simulation of groundwater flow and mass transport under uncertainty: Advances in Water Resources, v. 1, no. 1, p. 25-30.
Taylor, G. I., 1953, The dispersion of matter in a solvent flowing slowly through a tube, in Proceedings, Royal Society, Series A: London, Royal Society, v. 219, p. 189-203.
Walton, W.C., and Neill, I. C., 1963, Statistical analysis of specific capacity data for a dolomite aquifer: Journal Geophysical Research, v. 68, p. 8.
Warren, J. E., and Price, H. S., 1961, Flow in heterogeneous porous media: Society of Petroleum Engineers Journal, v. 1, p. 153-169.
Warren, J. E., and Skiba, F. F., 1964, Macroscopic dispersion: Transactions AIME, Society Petroleum Engineers, v. 231, p. 215-230.
Warren, J. E., Skiba, F. F., and Price, H. S., 1961, An evaluation of the significance of permeability measurements: Journal Petroleum Technology, v. 13, p. 739-744.
Warrick, A. W., Mullen, G. J., and Nielsen, D. R., 1977, Scaling field measured soil hydraulic properties using a similar media concept: Water Resources Research, v. 13, no. 2, p. 355-362.
Whittle, P., 1954, On stationary processes in the plane: Biometrika, v. 41, p. 434-449.
Willardson, L. S., and Hurst, R. L., 1965, Sample size estimates in permeability studies: Journal Irrigation Drainage Division, American Society Civil Engineers, v. 91 (IR1), p. 1-9.
Wu, T. H., Vyas, S. K., and Chang, N. Y., 1973, Probabilistic analysis of seepage: Journal Soil Mechanics Foundation Division, American Society Civil Engineers, v. 99 (SM4), p. 323-349.
Yakowitz, S., 1976, Model-free statistical methods for water table prediction: Water Resources Research, v. 12, no. 5, p. 836-844.

Manuscript Accepted by the Society October 14, 1980

Printed in U.S.A.



R. Allan Freeze

Department of Geological Sciences
University of British Columbia
Vancouver, British Columbia

John A. Cherry

Department of Earth Sciences
University of Waterloo
Waterloo, Ontario

GROUNDWATER

Prentice-Hall, Inc.
Englewood Cliffs, New Jersey 07632

ties and Principles / Ch. 2

fluid-free conductance
·y small, so petroleum
:q. (2.28) is substituted

(2.29)

ıbility that will lead to
l cp under a hydraulic
One darcy is approxi-

lely used for hydraulic
ouched in terms of Eq.

·vey regard to this
ld ent. However,
.s discarded this formal
:emperature of measure-
vironment can influence
ı Eq. (2.28). The effect is
ed. It still makes good
nents have been carried
of measurement are very
ay be dependent on the
·actical rather than con-

conductivity and perme-
· of geological materials.
avis' (1969) review. The
at h lic conductivity
l pa rs that take on
his property implies that
ivity can be very useful.
·tivity value probably has

ıe various common units
n cm² can be converted to
se conversion from ft² to

Table 2.2 Range of Values of Hydraulic Conductivity and Permeability

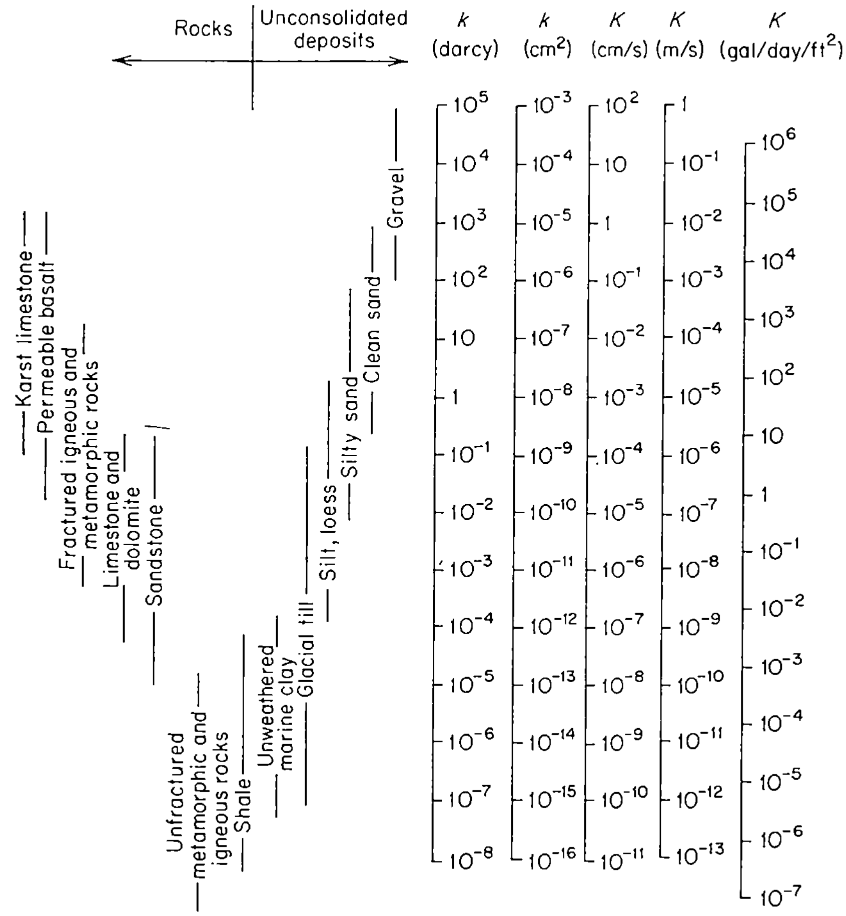


Table 2.3 Conversion Factors for Permeability and Hydraulic Conductivity Units

	Permeability, k*			Hydraulic conductivity, K		
	cm^2	ft^2	darcy	m/s	ft/s	U.S. gal/day/ft^2
cm^2	1	1.08×10^{-3}	1.01×10^{8}	9.80×10^{2}	3.22×10^{3}	1.85×10^{9}
ft^2	9.29×10^{2}	1	9.42×10^{10}	9.11×10^{5}	2.99×10^{6}	1.71×10^{12}
darcy	9.87×10^{-9}	1.06×10^{-11}	1	9.66×10^{-6}	3.17×10^{-5}	1.82×10^{1}
m/s	1.02×10^{-3}	1.10×10^{-6}	1.04×10^{5}	1	3.28	2.12×10^{6}
ft/s	3.11×10^{-4}	3.35×10^{-7}	3.15×10^{4}	3.05×10^{-1}	1	6.46×10^{5}
U.S. gal/day/ft^2	5.42×10^{-10}	5.83×10^{-13}	5.49×10^{-2}	4.72×10^{-7}	1.55×10^{-6}	1

*To obtain k in ft^2, multiply k in cm^2 by 1.08×10^{-3}.

makes it clear that large head changes over extensive areas are required to produce substantial water yields from confined aquifers.

Transmissivities and storativities can be specified for aquitards as well as aquifers. However, in most applications, the vertical hydraulic conductivity of an aquitard has more significance than its transmissivity. It might also be noted that in clay aquitards, $\alpha \gg \beta$, and the $n\beta$ term in the definition of storativity [Eq. (2.63)] and specific storage [Eq. (2.60)] becomes negligible.

It is possible to define a single formation parameter that couples the transmission properties T or K, and the storage properties S or S_s. The *hydraulic diffusivity* D is defined as

$$D = \frac{T}{S} = \frac{K}{S_s} \tag{2.64}$$

The term is not widely used in practice.

The concepts of *transmissivity* T and *storativity* S were developed primarily for the analysis of well hydraulics in confined aquifers. For two-dimensional, horizontal flow toward a well in a confined aquifer of thickness b, the terms are well defined; but they lose their meaning in many other groundwater applications. If a groundwater problem has three-dimensional overtones, it is best to revert to the use of *hydraulic conductivity* K and *specific storage* S_s; or perhaps even better, to the fundamental parameters *permeability* k, *porosity* n, and *compressibility* α.

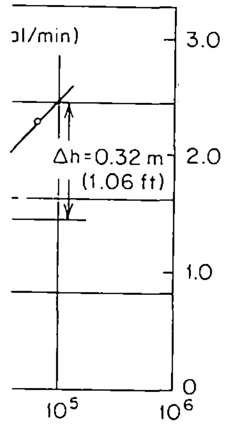
Transmissivity and Specific Yield in Unconfined Aquifers

In an unconfined aquifer, the transmissivity is not as well defined as in a confined aquifer, but it can be used. It is defined by the same equation [Eq. (2.61)], but b is now the saturated thickness of the aquifer or the height of the water table above the top of the underlying aquitard that bounds the aquifer.

The storage term for unconfined aquifers is known as the *specific yield* S_y. It is defined as the volume of water that an unconfined aquifer releases from storage per unit surface area of aquifer per unit decline in the water table. It is sometimes called the unconfined storativity. Figure 2.22(b) illustrates the concept schematically.

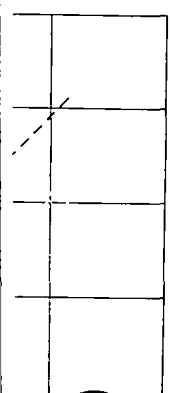
The idea of specific yield is best visualized with reference to the saturated-unsaturated interaction it represents. Figure 2.23 shows the water-table position and the vertical profile of moisture content vs. depth in the unsaturated zone at two times, t_1 and t_2. The crosshatched area represents the volume of water released from storage in a column of unit cross section. If the water-table drop represents a unit decline, the crosshatched area represents the specific yield.

The specific yields of unconfined aquifers are much higher than the storativities of confined aquifers. The usual range of S_y is 0.01–0.30. The higher values reflect the fact that releases from storage in unconfined aquifers represent an actual dewatering of the soil pores, whereas releases from storage in confined aquifers represent only the secondary effects of water expansion and aquifer compaction



).0023 m^2/s

0) = 7.5×10^{-4}



versus t data using the
vicinity of an imper-

nits used. For Δh and r in
).18 and $D = 2.25$. For Δh
.S. gal/day/ft, $C = 264$ and
264 and $D = 0.36$.

Todd (1959) states that the semilog method is valid for $u < 0.01$. Examination of the definition of u [Eq. (8.6)] shows that this condition is most likely to be satisfied for piezometers at small r and large t.

The semilog method is very well suited to the analysis of bounded confined aquifers. As we have seen, the influence of a boundary is equivalent to that of a recharging or discharging image well. For the case of an impermeable boundary, for example, the effect of the additional imaginary pumping well is to double the slope of the $h_0 - h$ versus log t plot [Figure 8.24(b)]. The aquifer coefficients S and T should be calculated from Eqs. (8.43) and (8.44) on the earliest limb of the plot (before the influence of the boundary is felt). The time, t_1, at which the break in slope takes place can be used together with Eqs. (8.19) to calculate r_i, the distance from piezometer to image well [Figure 8.15(c)]. It takes records from three piezometers to unequivocally locate the position of the boundary if it is not known from geological evidence.

Advantages and Disadvantages of Pumping Tests

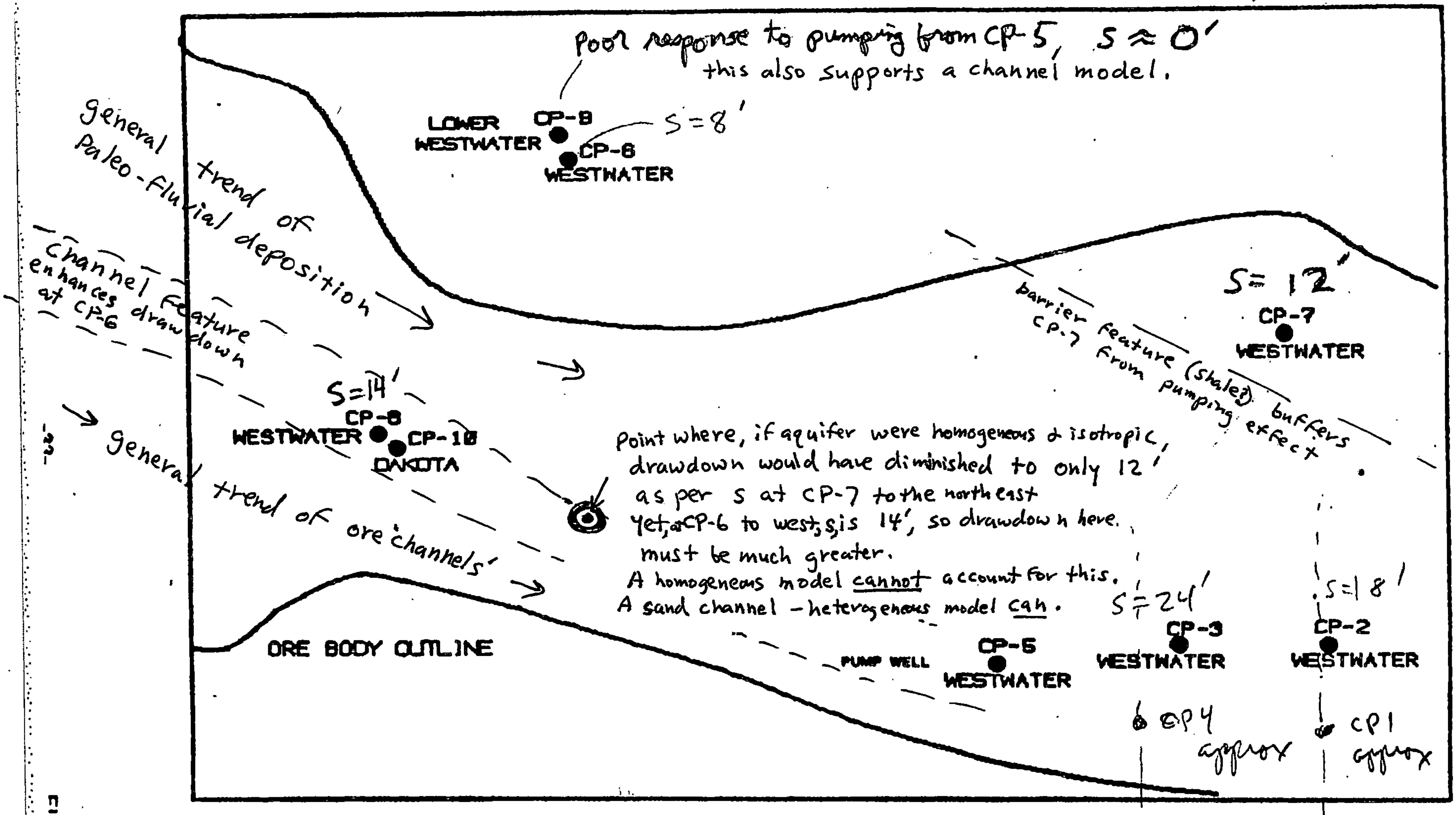
The determination of aquifer constants through pumping tests has become a standard step in the evaluation of groundwater resource potential. In practice, there is much art to successful pump testing and the interested reader is directed to Kruseman and de Ridder (1970) and Stallman (1971) for detailed advice on the design of pumping-test geometries, and to Walton's (1970) many case histories.

The advantages of the method are probably self-evident. A pumping test provides *in situ* parameter values, and these values are, in effect, averaged over a large and representative aquifer volume. One obtains information on both conductivity (through the relation $K = T/b$) and storage properties from a single test. In aquifer-aquitard systems it is possible to obtain information on the very important leakage properties of the system if observations are made in the aquitards as well as the aquifers.

There are two disadvantages, one scientific and one practical. The scientific limitation relates to the nonuniqueness of pumping-test interpretation. A perusal of Figure 8.23(b),(c), and (d) indicates the similarity in time-drawdown response that can arise from leaky, unconfined, and bounded systems. Unless there is very clear geologic evidence to direct groundwater hydrologists in their interpretation, there will be difficulties in providing a unique prediction of the effects of any proposed pumping scheme. The fact that a theoretical curve can be matched by pumping test data in no way proves that the aquifer fits the assumptions on which the curve is based.

The practical disadvantage of the method lies in its expense. The installation of test wells and observational piezometers to obtain aquifer coefficients is probably only justified in cases where exploitation of the aquifer by wells at the test site is contemplated. In most such cases, the test well can be utilized as a production well in the subsequent pumping program. In geotechnical applications, in contamination studies, in regional flow-net analysis, or in any flow-net approach that requires hydraulic conductivity data but is not involved with well development, EXHIBIT

3



Distance from CP-5
CP-4 = 500'
CP-1 = 1000'
CP-10 = 1,860'

S = drawdown, feet.

HRI, INC.

CROWNPOINT PUMP TEST LOCATIONS

S 1/2 SECTION 24 T17N R13W

SCALE: APPROX. 1"=300' MAY, 19..

FIGURE A (adapted from HRI)
Evidence for sand channel heterogeniety from HRI's CP pump test.

Figure B (adapted from HRI)
Evidence for sand channel heterogeniety from HRI's Section 8 pump test.

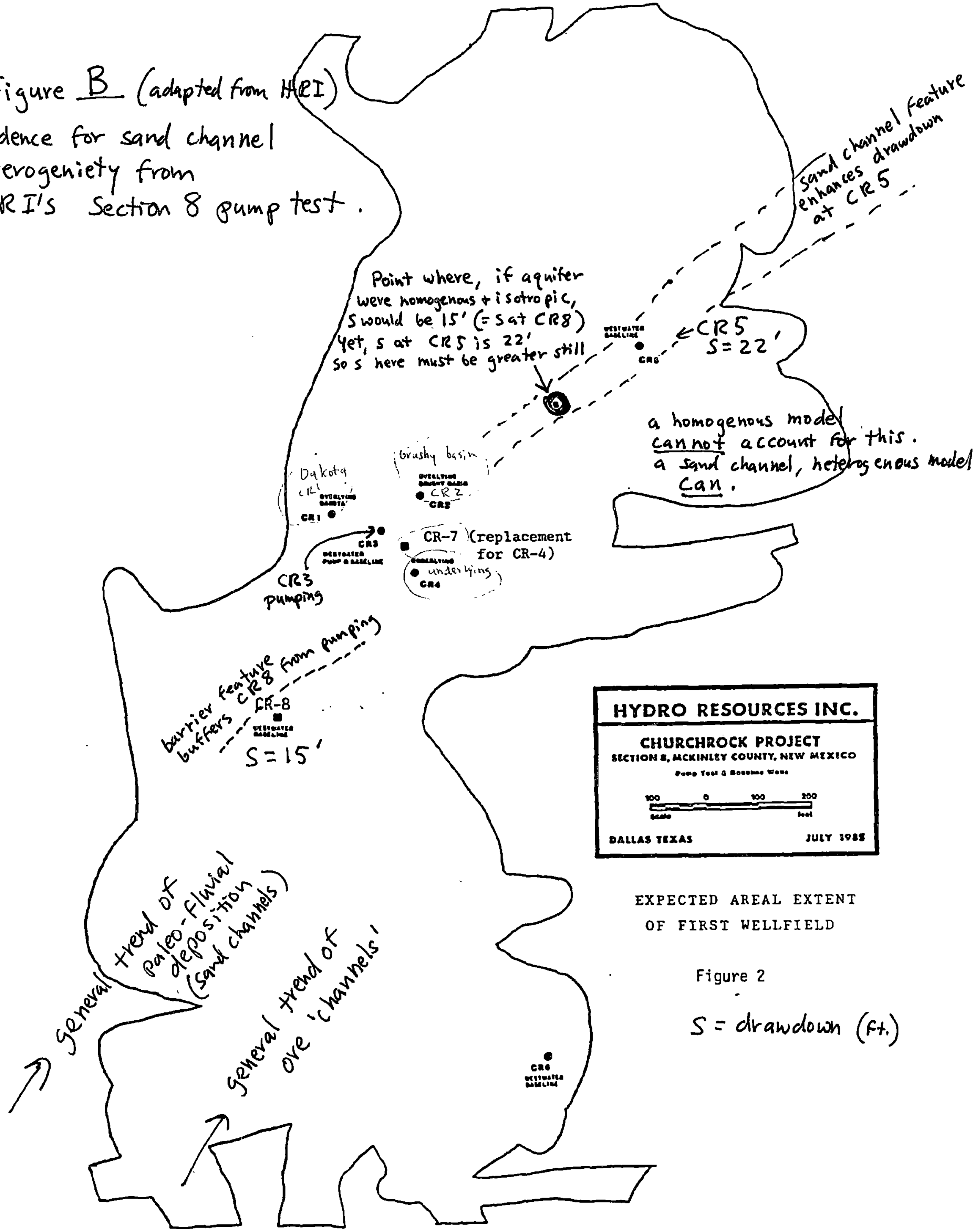
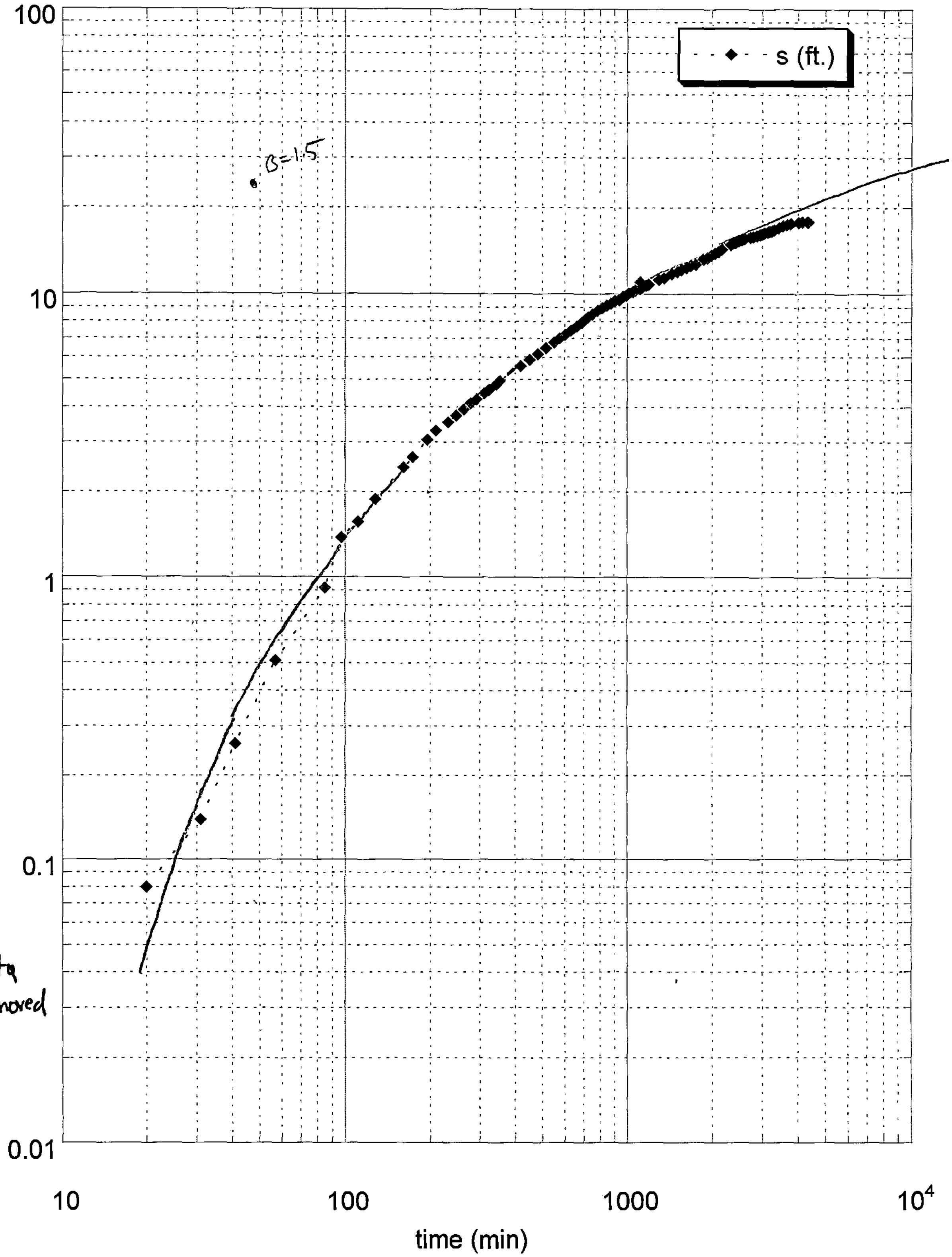


FIGURE C

CP2 Manual re-match to Hantush type curve

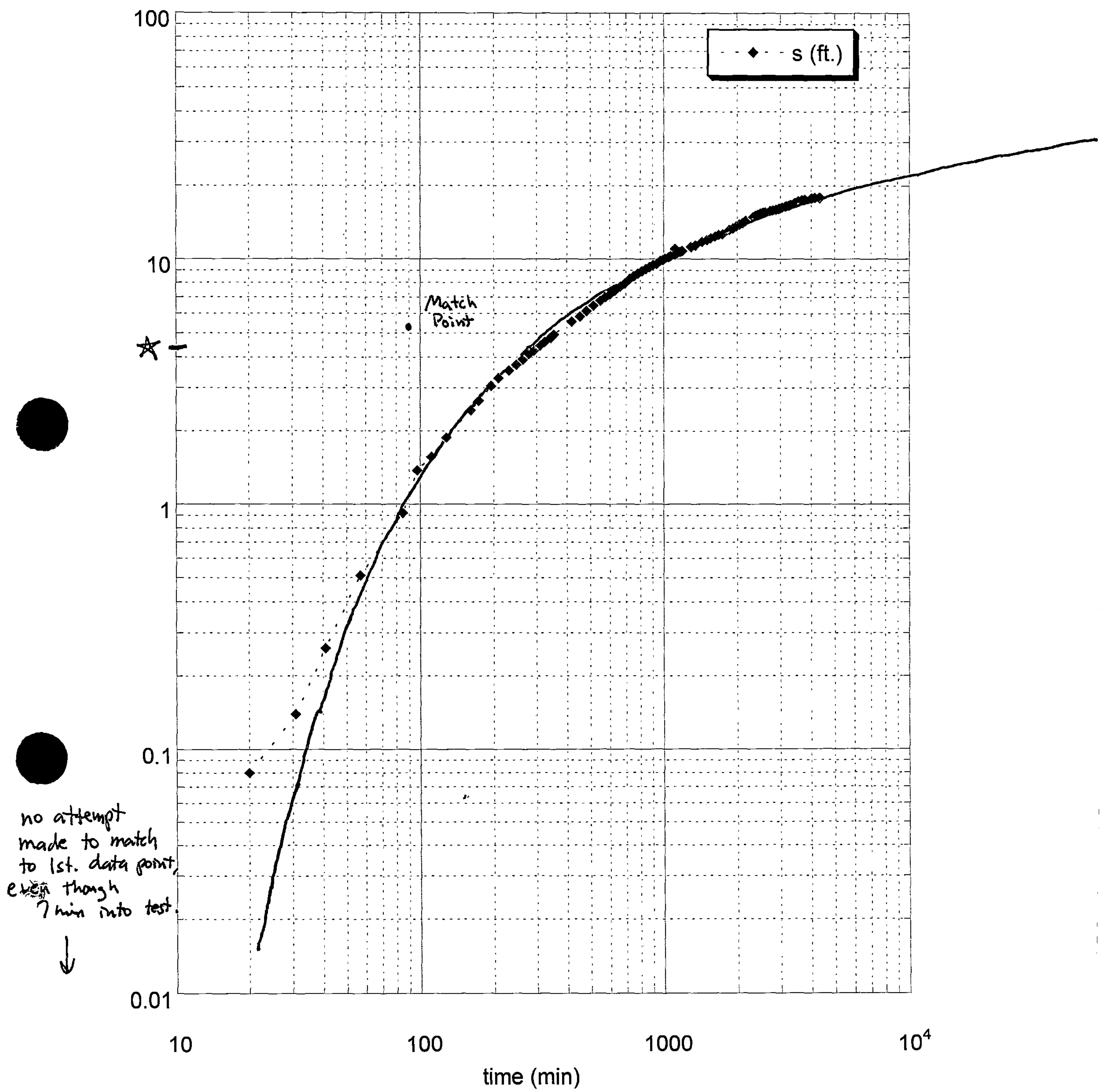


1st data point ignored

$T \cong 504$ gpd/ft $\quad S \cong 1e-5$

FIGURE D

CP2 manual match to Theis type curve



T = 2186 gpd/ft. , based on match point above (at s = 5.3 ft)

☆ required drawdown match point level (s = 4.3 ft)
for T to equal 2641 gpd/ft. (as claimed by HRI)
Yet, a match to Theis is not possible at this level

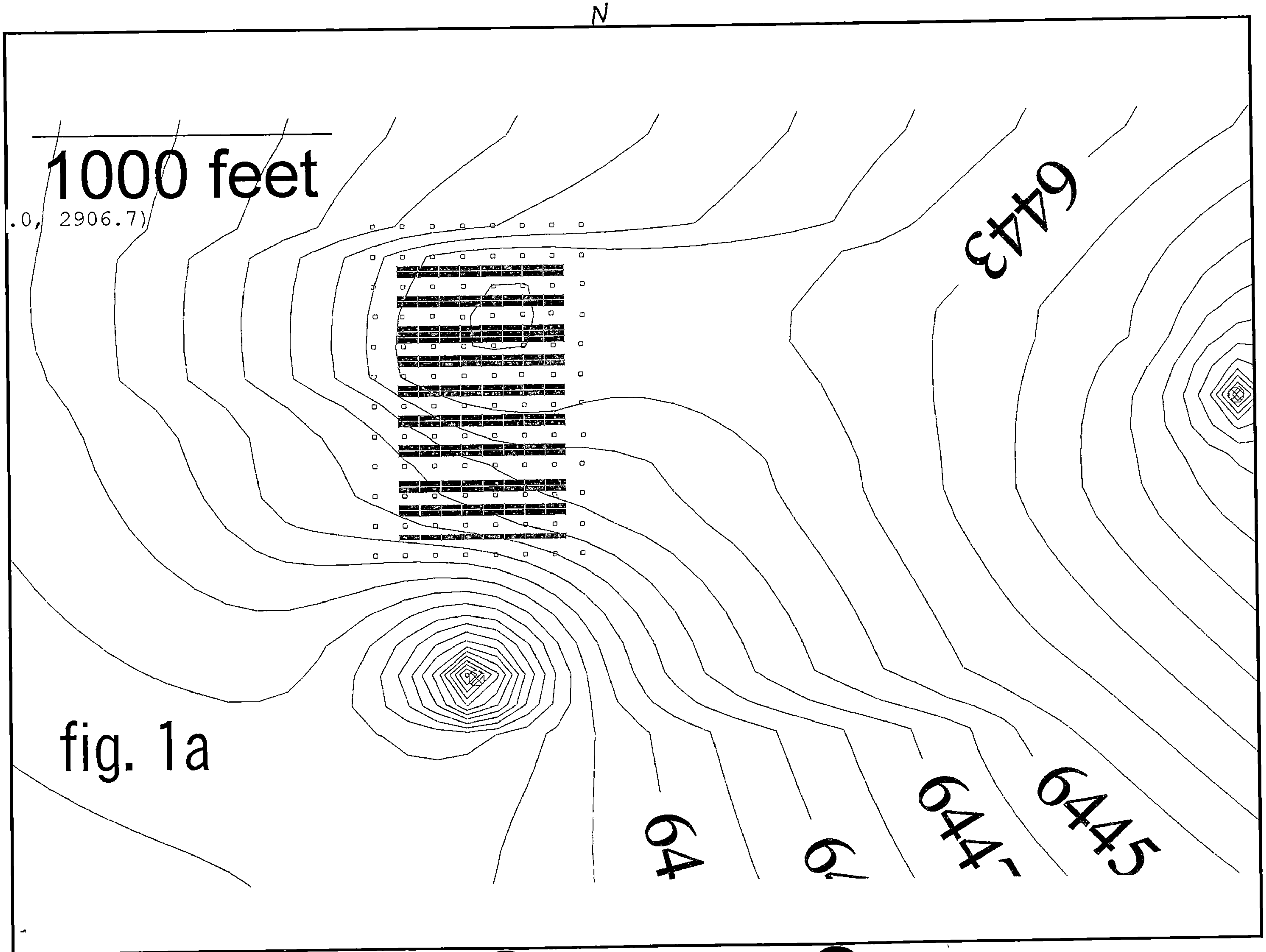


fig. 1a

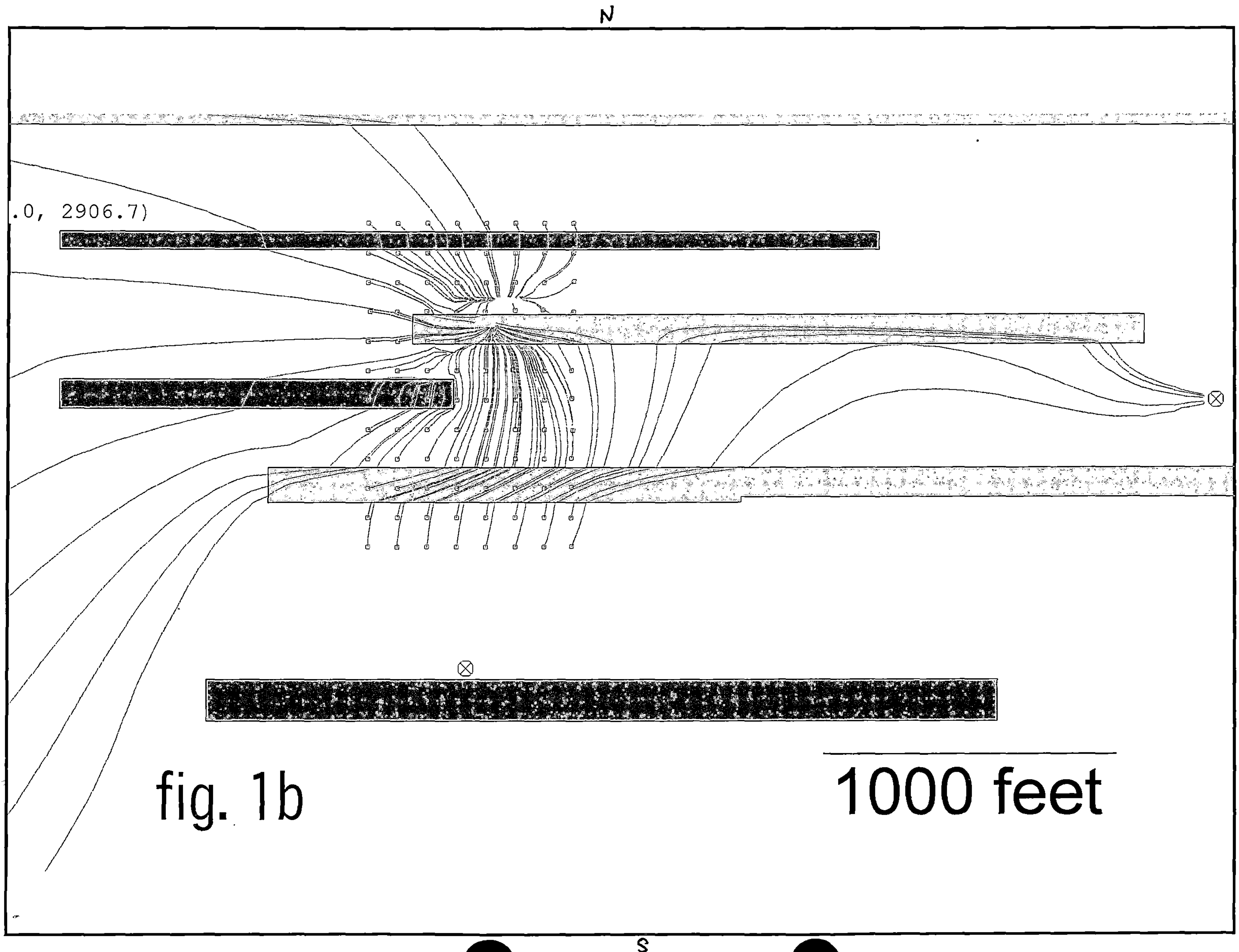


fig. 1b

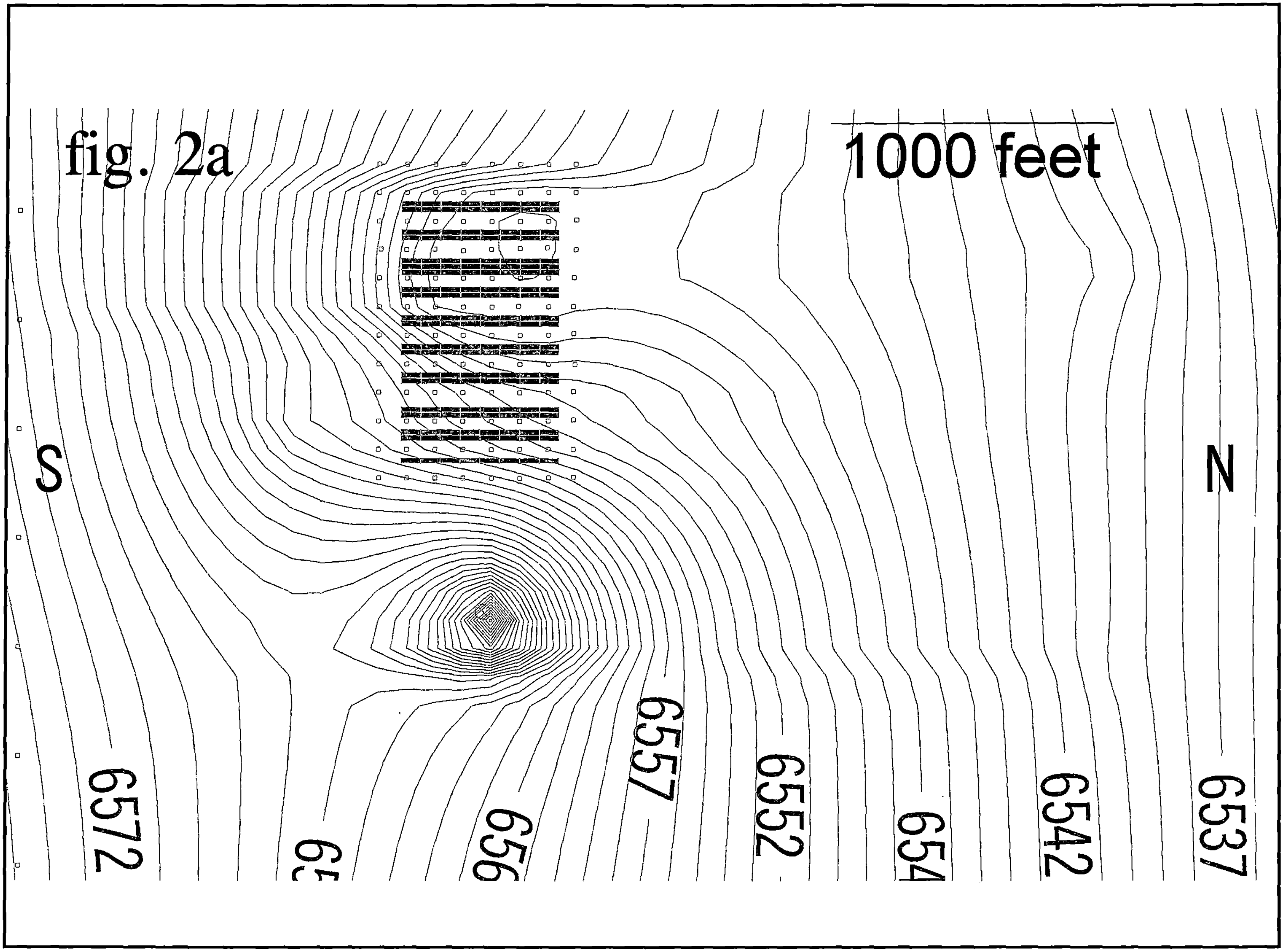


fig. 2a
1000 feet
S
N
6572
65
656
6557
6552
654
6542
6537

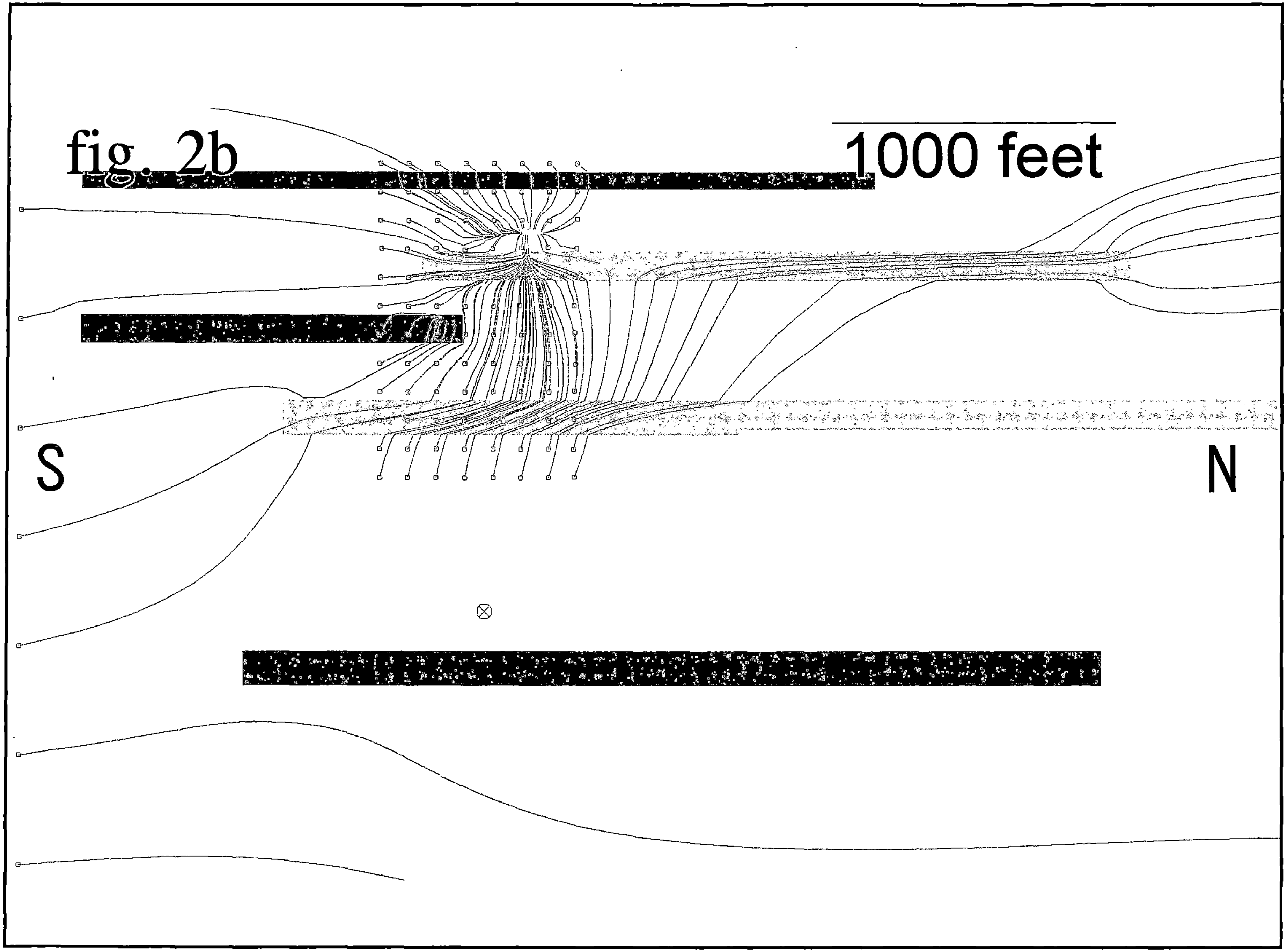


fig. 2b

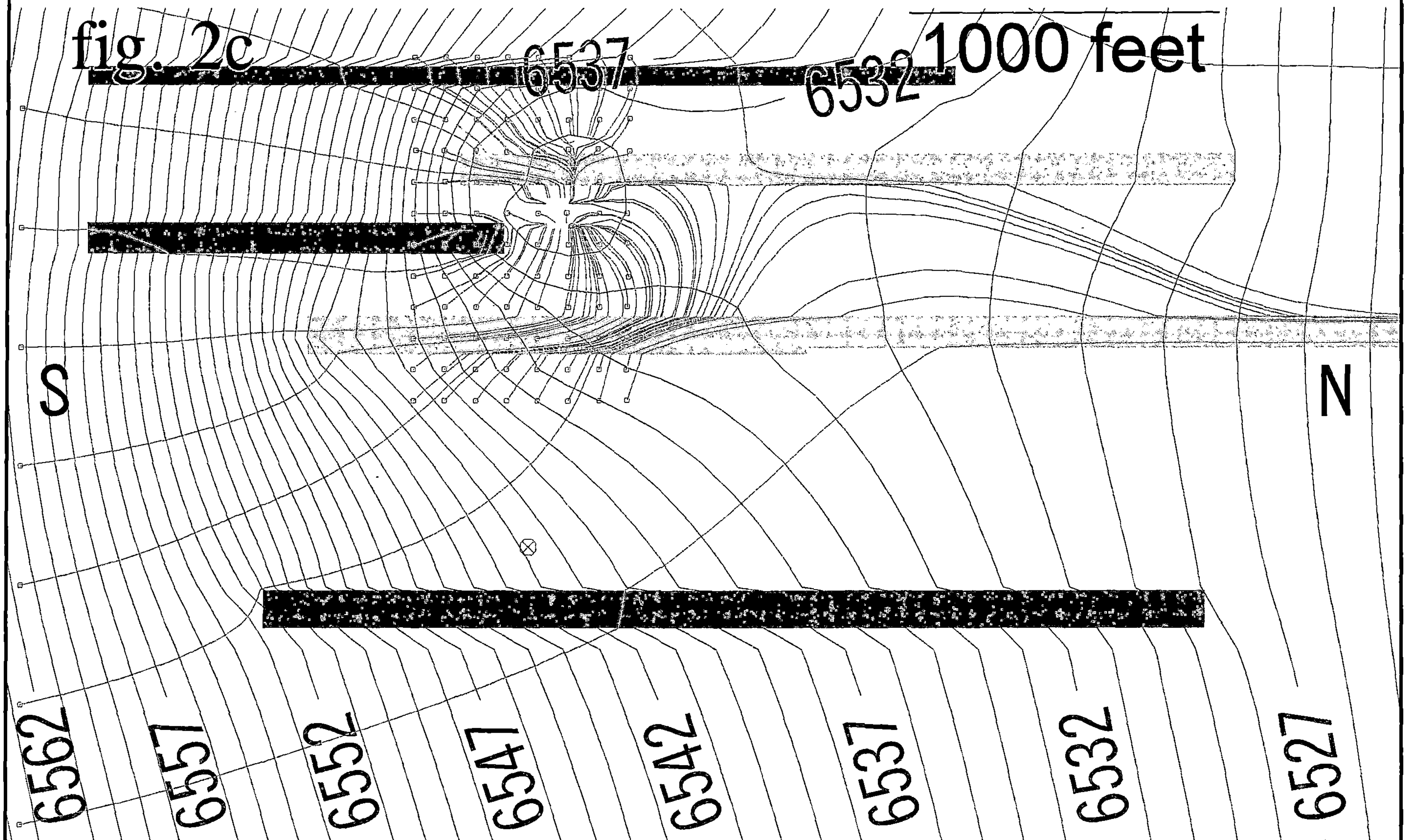

1000 feet
6537
6532
S
N
6562
6557
6552
6547
6542
6537
6532
6527

fig. 2c

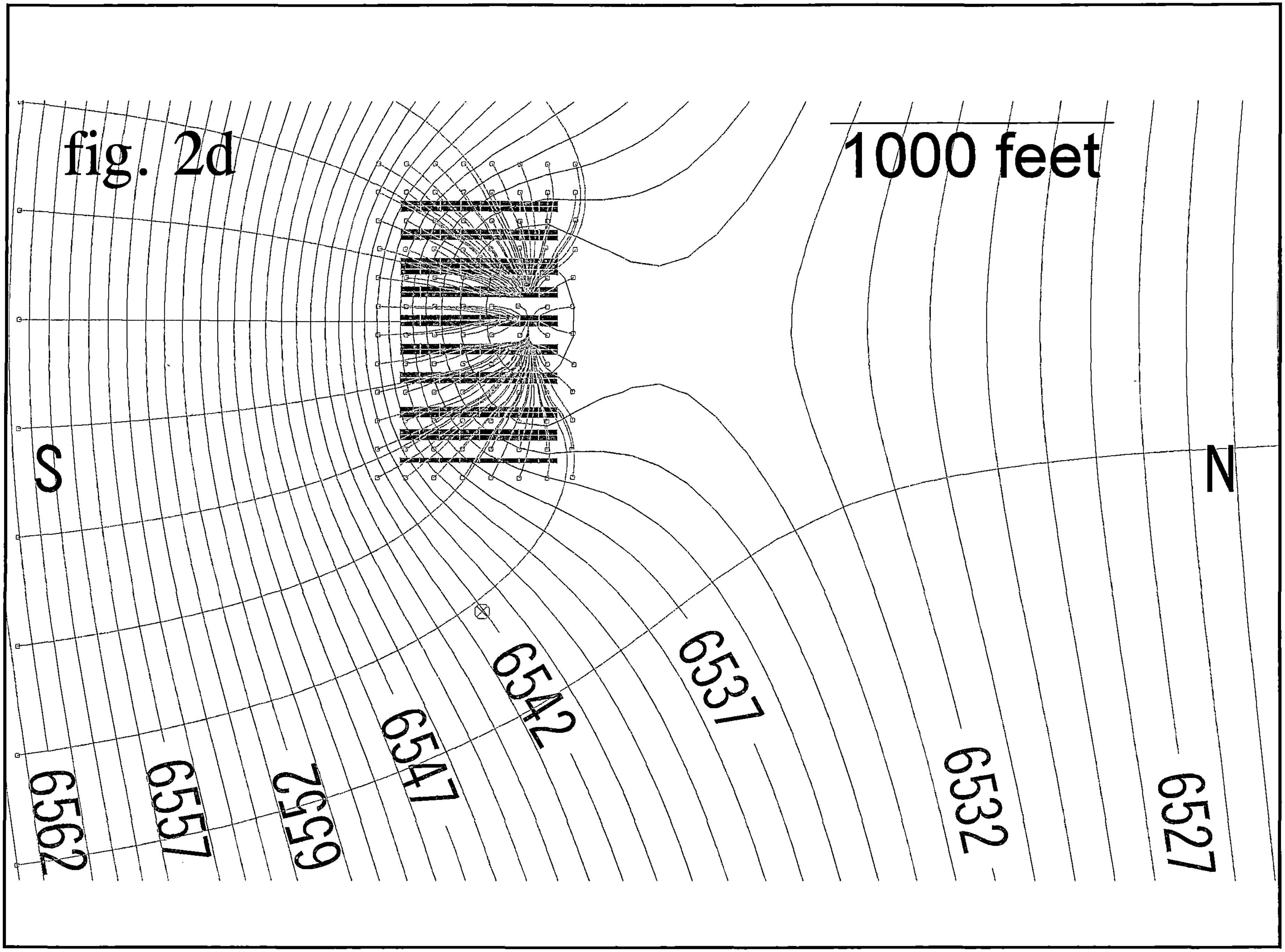


fig. 2d
1000 feet
S
N
6562
6557
6552
6547
6542
6537
6532
6527

Ground-Water Hydraulics

GEOLOGICAL SURVEY PROFESSIONAL PAPER 708 mination of T and S by use of the family of type curves described in this paper has advantages over that by use of the Theis type curve alone.

HANTUSH MODIFIED METHOD

Hantush (1960) presented an important modification of the theory of leaky confined aquifers in which the storage of water in the semipervious confining bed or beds is taken into account. His main equations are:

$$T=\frac{Q}{4\pi s}H(u,\beta) \qquad [L^2T^{-1}], \tag{90}$$

where

$$H(u,\beta)=\int_u^\infty \frac{e^{-y}}{y}\operatorname{erfc}\left(\frac{\beta/\sqrt{u}}{\sqrt{y(y-u)}}\right)dy \quad [\text{dimensionless}], \tag{91}$$

$$u=\frac{r^2S}{4Tt} \qquad [\text{dimensionless}],$$

as in the Theis equation, and

$$\beta=\frac{r}{4b}\left(\sqrt{\frac{K'S_s'}{KS_s}}+\sqrt{\frac{K''S_s''}{KS_s}}\right) \qquad [\text{dimensionless}], \tag{92}$$

where

K = hydraulic conductivity of main aquifer,
K', K'' = hydraulic conductivities of semipervious confining layers,
$S=bS_s$, $S'=b'S_s'$, $S''=b''S_s''$ = Storage coefficients of the main aquifer and of the semipervious confining layers, respectively, and
S_s, S_s', S_s'' = specific storage (storage coefficient per vertical unit of thickness) of the main aquifer and confining layers (b, b', and b''), respectively.

The versatility of equations 90 through 92 lies in the fact that they are the general solutions for the drawdown distribution in all confined aquifers, whether they are leaky or nonleaky. Thus, if K' and K'' approach zero or are made equal to zero, β approaches or equals zero, and equation 90 becomes equation 46, the Theis equation for nonleaky confined aquifers. Hantush (1960, p. 3716–3718) gives general solutions for three different configurations of aquifers and sets of confining beds. If K'', S', and S'' approach zero or are made equal to zero, two of these solutions become equal to equation 85 of Hantush and Jacob (1955)—the equation for leaky confined aquifers for which release of stored water from the confining beds is considered negligible.

Plate 4 is a logarithmic plot of $1/u$ versus $H(u,\beta)$ for various indicated values of β, copied from a plot made by E. J. McClelland, U.S. Geological Survey, Sacramento, Calif., in 1961 from tabulated values by Hantush (1961). Time-drawdown or time-recovery data from tests in aquifers whose confining bed or beds are suspected of releasing water from storage are plotted (as s versus t) on 3×5-cycle logarithmic paper having the same scale as plate 4 (such as K & E 359–125G or 46–7522), and this is superposed on plate 4 until a fit is obtained on one of the type curves by the usual curve-matching procedure. From values of the four parameters at a convenient match point, T and S may be determined from equations 90 and 47, respectively.

Thorough knowledge of the geology, including the character of the confining beds, should indicate in advance which of the two leaky-aquifer type curves to use, or whether to use the Theis type curve for nonleaky aquifers.

EXAMPLE

Table 12 gives the time-drawdown measurements in an observation well at Pixley, Calif., 1,400 ft from a well pumping 750 gpm, supplied by Francis S. Riley (U.S. Geological Survey, Sacramento, Calif., written commun., March 5, 1968). The pumped well, which is 600 ft deep, obtains water from gravel, sand, sandy clay, and clay of the Tulare Formation in an area where considerable land subsidence has resulted from prolonged pumping from confined aquifers containing appreciable amounts of clay.

TABLE 12.—*Drawdown of water level in observation well 23S/25E-17Q2, 1,400 ft from a well pumping at constant rate of 750 gpm, at Pixley, Calif., March 13, 1963*

[Drawdown corrected for pretest trend. Data from Francis S. Riley (written commun., March 5, 1968)]

Time since pumping began, t (min)	Drawdown, s (ft)	Time since pumping began, t (min)	Drawdown, s (ft)
6.37	0.01	90	0.75
8.58	.02	100	.82
10.23	.03	137	1.04
11.90	.04	150	1.12
12.95	.05	160	1.17
14.42	.06	173	1.24
15.10	.07	184	1.27
16.88	.08	200	1.35
17.92	.10	210	1.40
21.35	.12	278	1.68
21.70	.13	300	1.76
22.70	.14	315	1.83
23.58	.15	335	1.87
24.65	.17	365	1.99
29	.21	390	2.10
30	.22	410	2.13
32	.24	430	2.20
34	.26	450	2.23
36	.28	470	2.29
38	.30	490	2.32
41	.33	510	2.39
44	.36	560	2.48
47	.38	740	2.92
50	.42	810	3.05
54	.46	890	3.19
60	.52	1,255	3.66
65	.56	1,400	3.81
70	.60	1,440	3.86
80	.65	1,485	3.90

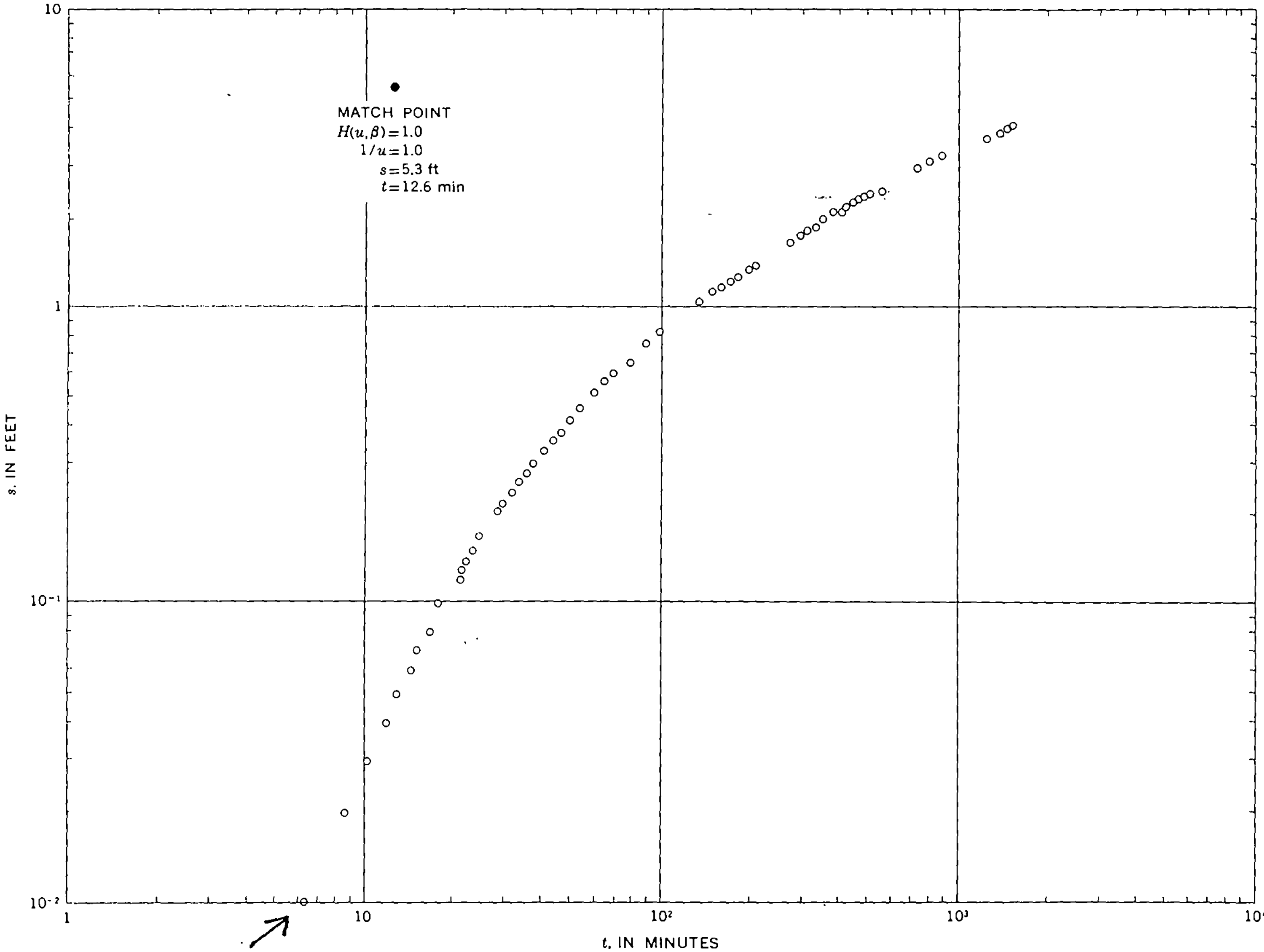


FIGURE 20.—Logarithmic plot of s versus t for observation well 23S/25E–17Q2 at Pixley, Calif.

(See table 3, "Tulare-Wasco area.") The aquifer is confined by the Corcoran Clay Member, about 6 ft thick, above which is an unconfined aquifer about 200 ft thick. A logarithmic plot of s versus t from table 12 is shown in figure 20, which shows also the match-point values of the four parameters obtained by superposition on plate 4. From these data, T and S are computed from equations 90 and 47, as follows:

$$T=\frac{Q}{4\pi s}H(u,\beta)$$

$$=\frac{(750 \text{ gal min}^{-1})(1{,}440 \text{ min day}^{-1})(1.0)}{(4\pi)(5.3 \text{ ft})(7.48 \text{ gal ft}^{-3})}$$

$$=2{,}170 \text{ ft}^2 \text{ day}^{-1},$$

rounded to 2,200 $\text{ft}^2 \text{ day}^{-1}$, and

$$S=\frac{4Ttu}{r^2}=\frac{(4)(2{,}170 \text{ ft}^2 \text{ day}^{-1})(12.6 \text{ min})}{(1{,}400 \text{ ft})^2(1{,}440 \text{ min day}^{-1})(1/1.0)}$$

$$=3.9\times10^{-5},$$

rounded to 4×10^{-5}.

Preliminary attempts to fit both early and late data from table 12, and similar drawdown and recovery data from two other observation wells at $r=650$ and 1,220 ft, to the Theis curve gave apparent values of T from 5 to 20 times the more realistic value computed above, and apparent values of S from 17 to 25 times the value computed above.

CONSTANT DRAWDOWN

Hantush (1959) derived an equation for determining T and S for a well of constant drawdown that is discharging by natural flow from an infinite leaky confined aquifer, and he also gave solutions for a circular leaky confined aquifer with zero drawdown on the outer boundary and for a closed circular aquifer. The equations for the infinite leaky confined aquifer follow:

$$T=\frac{Q}{2\pi s_w G(\alpha, r_w/B)} \quad [L^2T^{-1}], \tag{93}$$

where

$$\alpha=\frac{Tt}{Sr_w^2} \quad \text{[dimensionless]}, \tag{94}$$

$$r_w/B=r_w\sqrt{T/(K'/b')} \quad [L^2], \tag{95}$$

and

$$G\left(\alpha,\frac{r_w}{B}\right)=\left(\frac{r_w}{B}\right)\frac{K_1(r_w/B)}{K_0(r_w/B)}+\frac{r}{\pi^2}\exp\left[-\alpha\left(\frac{r_w}{B}\right)^2\right]$$

$$\cdot\int_0^\infty \frac{u\exp(-\alpha u^2)}{J_0^2(u)+Y_0^2(u)}\cdot\frac{du}{u^2+(r_w/B)^2} \quad \text{[dimensionless]}, \tag{96}$$

where

K_1 = Modified Bessel function of second kind, first order,
K_0 = Modified Bessel function of second kind, zero order,
J_0 = Bessel function of first kind, zero order,
Y_0 = Bessel function of second kind, zero order, and
u = variable of integration.

The integral in equation 96 cannot be integrated directly but was evaluated numerically, and values of the parameters are given by Hantush (1959, table 1) from which plate 5 was drawn after Walton (1962, pl. 4). When $B=\infty$, r_w/B and $K'b'$ (equal to T/B^2) $=0$, so that the parent-type curve on plate 5 is the same as on plate 1—the nonleaky-type curve of Jacob and Lohman (1952, fig. 5)—except, of course, that the values of the parameters differ.

On translucent logarithmic paper of the same scale as plate 5 (such as Codex 4123) values of Q are plotted on the vertical scale against values of t on the horizontal scale, and the data curve is superposed on plate 5. From the match point obtained by the usual curve matching procedure, preferably at $G(\alpha, r_w/B)$ and $\alpha=1.0$, values of the four parameters $G(\alpha, r_w/B)$, α, Q, and t are obtained. T is then determined using equation 93, and S is determined by rewriting equation 94:

$$S=\frac{Tt}{r_w^2\alpha} \quad \text{[dimensionless]}. \tag{97}$$

Unfortunately, I have no field data with which to illustrate this method.

UNCONFINED AQUIFERS WITH VERTICAL MOVEMENT

Boulton (1954a) derived an integral equation for the drawdown of the water table near a discharging well before the flow approaches steady state, which is founded partly on a consideration of vertical flow components, such as those that prevail near the well during the early stages of a pumping test in an unconfined aquifer. (See Stallman, 1961a.) In our notation, his partial differential equation describing the head (h) at the water table is

$$\frac{\partial h}{\partial t}=\frac{K}{S}\left[\left(\frac{\partial h}{\partial r}\right)^2+\left(\frac{\partial h}{\partial z}\right)^2-\frac{\partial h}{\partial z}\right] \quad [LT^{-1}]. \tag{98}$$

As equation 98 is nonlinear and cannot readily be solved, he assumes that the head gradients are small enough that their squares may be neglected, whence

$$\frac{\partial h}{\partial t}+\frac{K}{S}\frac{\partial h}{\partial z}=0 \quad [LT^{-1}], \tag{99}$$

where h = pressure head ($p/g\rho$) plus elevation head (z).

Subject: **[Fwd: FYI, with regard to a characterization by NRC and others of your opinion on an ISL mine]**
Date: Wed, 03 Mar 1999 19:57:13 -0700
From: michael wallace <mwallace@thuntek.net>
Reply-To: mgw@anacrolith.com
Organization: michael wallace
To: "nmelc@nets.com" <nmelc@nets.com>, SRIC <sric@igc.org>

EXHIBIT
tabbies
6

please forward this important email to Johanna Matanich and Chris Shuey
respectively
at the earliest possible opportunity.
thank you,
Mike Wallace

Subject: **Re: FYI, with regard to a characterization by NRC and others of your opinion on an ISL mine**
Date: Wed, 3 Mar 1999 12:10:15 -0700
From: "Shlom P. Neuman" <neuman@hwr.arizona.edu>
To: <mgw@anacrolith.com>
CC: "Tom Nicholson" <tjn@nrc.gov>

Dear Michael:

It was good to hear from you, and I want to thank you for your candid note regarding the uranium ISL licensing issues associated with HRI's intentions in New Mexico's San Juan Basin, and the lingering controversy regarding my position on these issues.

Allow me to clarify some points regarding my "position" on these issues:

1. I have never formulated either a formal or an informal position regarding the above site or issues.
2. In the context of a generic research project on conceptual hydrogeologic models, on which I am working under the auspices of the US Nuclear Regulatory Commission (NRC), I reviewed NUREG-1508 titled Final Environmental Impact Statement to Construct and Operate the Crownpoint Uranium Solution Mining Project, Crownpoint, New Mexico. The purpose of my review was merely to use this case as an example of a hydrogeologic framework, and its conceptual modeling, by the NRC.
3. My opinion about the manner in which hydrogeology at the site is conceptualized in NUREG- 1508 is summarized in overheads I presented to the NRC staff at the agency's headquarters in Rockville, MD, on January 29, 1998. My opinion is amplified in an internal letter report I submitted to the NRC, concerning my research for the agency, at about the same time.
4. In preparing my opinion, I was completely unaware of any controversy or litigation concerning the site.
5. To date, I have not reviewed any additional material concerning the site.
6. Based on the information in NUREG-1508, I stand behind every statement that I made in my talk to the NRC staff on January 29, 1998, concerning the site.
7. On march 19, 1998, I participated in a teleconference concerning my above opinion about the hydrogeologic conceptual framework for the site with NRC staff. This teleconference has not changed my opinion about hydreogelogic conceptualization of the site in any way.
8. Any statement or statements made by the NRC concerning opinions that I allegedly voiced during this teleconference are those of the agency, not mine. I have never been given a chance to review and/or comment on such statements.
9. In particular, an NRC memo by Joseph J. Holonich, Chief of the Uranium Recovery Branch, addressed to Peter B. Bloch, Presiding Officer of the Atomic Safety and Licensing Board, dated April 20, 1998, misrepresents my association with the NRC and my opinions about the site.

Feel free to bring these points to the attention of anyone concerned with this matter.

Best regards,

Shlomo Neuman