Dear Larry:

Enclosed are two reports:

(1) Rebuttal: Technical Review of the Hartman Scenario: Implications for WIPP (Bredehoeft, 1997) by Swift, Stoelzel, Beauheim, Vaughn, and Larson, by John Bredehoeft; and

(2) The Hartman Scenario Revisited: Implications for WIPP, by John Bredehoeft and Walter Gerstle.

The first report was presented at the July 30, 1997 State-DOE quarterly meeting. The second report was partially previewed at that meeting and has since been completed.

The Hartman Scenario Revisited demonstrates the following main points:

(a) The occurrence at the Hartman #2 Bates well was caused by a hydraulic fracture (hydrofrac) extending from the Texaco Rhodes-Yates waterflood location to the Hartman #2 Bates well; other explanations are unsupported.

(b) BRAGFLO in its current implementation is an inadequate model to predict the extent of hydrofracs. Further, Stoelzel and Swift (1997) are nonconservative in their use of BRAGFLO to compute the radius of hydrofracs caused by leaking injection wells, in that they assume uniform permeability in the entire borehole annulus around the casing of the leaking well and allow fluids to enter all marker beds simultaneously.

(c) Linear Elastic Fracture Mechanics (LEFM) is a widely used and accepted model for fractures, including hydrofracs. Using LEFM, the three scenarios analyzed in
Bredehoeft (1997) are examined and found still to pose compliance problems for WIPP. These scenarios are: (1) a hydrofrac extending from a leaking injection well to WIPP at low pressure, (2) a hydrofrac extending from a leaking injection well to WIPP at high (lithostatic) pressure, and (3) a hydrofrac extending through WIPP and encountering a poorly plugged well that leaks upward. Under scenario (3) the containment limits are significantly violated in a few years.

We urge your careful consideration of these reports in connection with EPA’s forthcoming determination of WIPP’s compliance, based on DOE submissions which omit well injection from performance assessment.

For your convenience two copies of each report are included.

Very truly yours,

LINDSAY A. LOVEJOY, JR.
Assistant Attorney General

LALJr.:mh
EXECUTIVE SUMMARY

The Hartman #2 Bates well blew out during drilling in the lower Salado Formation with a large flow of brine. The consensus opinion of a number of geologists who examined the situation was that the cause of the blow-out was a hydraulic fracture that occurred in an anhydrite horizon in the lower Salado and extended from the Texaco Rhodes-Yates waterflood some two miles away to the #2 Bates well.

The Hartman Scenario

Van Kirk (1994) cited several empirical facts that pointed to a hydrofrac caused by the Texaco Rhodes-Yates waterflood:

1. the Bates #1 well (that was 100 feet from the #2 well) encountered no flows of brine in 1953;
2. water flows were reported in wells in the Rhodes-Yates waterflood starting in 1979;
3. the Rhodes-Yates waterflood was the only flood in the area with such high fluid pressure (the pressure gradient at the #2 Bates well was 0.966 psi/ft; the pressure gradient in the Rhodes-Yates field is mapped in excess of 1.25 psi/ft which is above the rock fracture pressure).

Dennis Powers pointed out that brine reservoirs in both the Castile and the Salado typically have pressures considerably less than 1 psi/ft, ranging down to 0.8 psi/ft, or less (Silva, 1996). Powers went on to suggest that the fracture gradient can be used to distinguish natural phenomena from man-induced. He too interpreted the blowout at the Bates #2 well to be caused by a hydrofrac induced by the Texaco Rhodes-Yates waterflood.

I estimated the permeability of the layer that blew out, using well established and documented methods, and showed that it was 5 orders of magnitude higher than the virgin anhydrite permeabilities measured at WIPP. This further indicated a hydraulic fracture. A court agreed and awarded Hartman damages based upon a hydrofrac extending from the Texaco Rhodes-Yates waterflood to the Hartman #2 Bates well.

The Hartman #2 Bates well blow-out was from a hydrofrac that occurred in a single anhydrite zone within the lower Salado Formation.

Implications for WIPP

The thrust of my report was to ask—suppose a leak occurs in an injection well in the vicinity of WIPP in such a way that brine is injected into one of the Marker Beds associated with the repository (Marker Bed 139, for example) are there serious consequences?
I identified three scenarios that pose problems:

1. **A hydrofrac extends from an injection well to WIPP, while WIPP is a relatively low pressure.** In this case I postulated that 1) fluid would flow into the repository, 2) the hydrofrac would close because of the low pressure, and 3) the process would then repeat—a frac would be recreated, it would extend to WIPP, fluid would flow into WIPP lowering the pressure in the frac, the frac would close, and the process would repeat. A pulsing flow to WIPP would be established. Pulsing flow is one of at least two possibilities. The in-situ hydrofrac experiments of Beauheim et al. (1994) and Wawersik et al. (1997) indicated that the experimental fractures in anhydrite did not fully close. The fracs after the pressure dropped retained significant quantities of the injected fluid and had high permeability. A second possibility is flow into WIPP through a higher permeability frac that does not fully close once the pressure is lowered. Still, the basic question remains—**suppose a hydrofrac extends from a leaking well through Marker Bed 139 to WIPP, what happens?**

2. **A hydrofrac extends from a leaking injection well to WIPP through Marker Bed 139 during a period when WIPP is at or near lithostatic pressure.** In this instance the situation is much clearer; the hydrofrac will remain open with a high effective permeability. **A large quantity of brine will flow to WIPP.**

3. **A hydrofrac extends through the region of WIPP and encounters an unplugged borehole.** In this instance there will be flow through the repository that transports contaminants to the unplugged borehole. **The transport of radionuclides in the brine away from WIPP will quickly violate the EPA certification standard.**

**Review Comments by Swift et al.**

Based upon an examination of the comments by Swift et al. the scenarios described above continue to pose a significant risk to WIPP. The reviewers did not address the salient points. They tended to dismiss the Hartman Scenario as one interpretation of the facts, but not their interpretation. Therefore, they choose not to address the Hartman case. They dismissed the remainder of the report by addressing the analysis and making a number of minor points that they argue invalidates the thrust of the report. They do not address the salient issue—**are there injection scenarios, stemming from what occurred at the Hartman # 2 Bates well, that could pose problems for WIPP?**

DOE has argued, based upon work by Sandia, that the consequences of a leaking brine injection well are insignificant and can be eliminated on that basis. (They do not argue that a leaking injection well is a low probability event—at least not as their primary reason to screen out the event.) There most recent analysis:

_Supplementary Analysis of the Effect of Salt Water Disposal and Waterflooding on WIPP, WPO # 44158, Stoelzel and Swift (1997)_

does not address what happened at the Hartman # 2 Bates well. In the recent investigation Stoelzel and Swift allow brine to simultaneously enter all the anhydrite beds in both the Castile and the Salado Formation. This is not what happened in the Hartman Scenario; brine was encountered in one anhydrite horizon. Stoelzel and Swift by allowing simultaneous flow into all the anhydrite marker beds minimize the distance it will migrate in any one layer. **Stoelzel and Swift have not tried to analyze what happened at the Hartman # 2 Bates well; they simply ignore it.**

**Below I answer point by point the review comments of Swift et al. In most instances I show that their comments are without substance; in some instance their remarks are simply wrong.**
REBUTTAL—SPECIFIC POINTS

Introduction

The reviewers, Swift et al., come to the following conclusions:

"...This report does not present a realistic or reasonable analysis of the phenomena it addresses. From the information available in the document, we conclude the conceptual models used in the analysis are incompletely explained, sometimes inconsistent with observed data, and sometimes inconsistent from one portion of the report to another. Reasonable alternative models are not considered. Important assumptions made in setting up the computational models, such as the choice of boundary conditions, are, in many cases, neither explained or justified, and some appear to be physically implausible. Insufficient information is provided to evaluate the adequacy of the computational modeling and simple checks of the physical reasonableness of the model results are, in general, not presented. Ultimately, the results of the modeling work and the conclusions drawn from them are not credible (Swift, et al., p 2)."

These are strong words that need rebutting. Swift et al. impugn the work by focusing on a number of minor points in the analysis; they then argue that these minor points invalidate the entire effort. They dismiss the major thrusts of the report. I will show that most of their criticisms are unfounded. The reviewers commented on each Chapter individually. I will respond to their remarks in the order in which they were made.

CHAPTER 1 and 2 (& CHAPTER 3) Introduction; & Background: Hydraulic Fractures

Comment: "...the statement on page 5 that water injection occurred "where it was not intended" at the Bates # 2 well presupposes that injection was the cause of the blow out. This should be identified as an interpretation, rather than a direct observation.

The reviewers raise this point again in commenting on Chapter 3:

"...The permeability derived for the Bates # 2 blowout zone "almost surely represents hydrofraced anhydrite in the Salado Formation". This is an interpretation, rather than an observation, and should be identified as such. The basis should be explained by which he has excluded reasonable alternative explanations, including the penetration of a local over-pressurized reservoir.

The reviewers raise the question of a reasonable alternative explanation. The Bates # 2 well was drilled approximately 100 feet east of the Bates # 1 well. The #1 well was drilled in 1953 and encountered no problems. The 1000 psi shut-in pressure measured at the surface in the #2 well indicated a pressure gradient of 0.966 psi per foot. Van Kirk (1994) suggested several empirical facts that pointed to a hydrofrac caused by the Texaco Rhodes-Yates waterflood:

1. the Bates # 1 well encountered no flows of brine in 1953;
2. water flows were reported in wells in the Rhodes Yates waterflood starting in 1979;
3. the Rhodes Yates waterflood was the only flood in the area with such high fluid pressure (the pressure gradient in the field is mapped in excess of 1.25 psi/ft which is above the rock fracture pressure).

Dennis Powers pointed out that brine reservoirs in both the Castile and the Salado typically have pressures considerably less than 1 psi/ft, ranging down to 0.8 psi/ft or less (Silva, 1996). Powers went on to suggest that the fracture gradient can be used to distinguish natural phenomena from man-induced. He too interpreted the blowout at the Bates # 2 well to be caused by the Texaco Rhodes Yates waterflood.
I added to that an estimate of permeability for the blow-out interval in the Bates # 2 well that is 5 orders of magnitude higher than the virgin anhydrite permeability at WIPP.

The reviewers quote Van Kirk on the amount of cement necessary to plug the Bates # 2 well. They were aware of Van Kirk’s report and his conclusions. A court agreed, and found in favor of the operator of the Bates # 2 well, Hartman—the cause of the blowout was the Texaco Rhodes Yates waterflood. At best, I may have not qualified my observations as interpretation. However, I did say almost surely represents, and in the penultimate paragraph in Chapter 2, I remarked: The consensus interpretation of what happened — these statements sound like interpretation to me.

**The reviewers discount the empirical facts of the Hartman—Bates # 2 well blowout.**

**Comment:** The relevance of the discussion in Chapter 2 of other examples of hydrofracturing is unclear, and should explained.

Other reviewers felt it was important to point out examples of hydraulic fractures that were more than a kilometer in extent. Massive hydraulic fractures at Wattenberg serve as examples of extensive hydrofracs. I took care to indicate that the fracs at Wattenberg were vertical; this fits the theory as presented by Hubbert and Willis (1957). The Henry Mountain laccoliths are examples of horizontal hydraulic fractures in which the fractures were filled by molten igneous rock. The largest of the laccoliths is more than 3 miles in diameter.

**Comment:** We note that Bredehoeft omits mention of the anhydrite hydrofracture experiments that were conducted in Marker Beds 139 and 140 at WIPP (Beauheim et al. 1993, and also Waversik et al., 1997, published in May of 1997 and not available at the time of Bredehoeft’s report).

This is an oversight on my part; I was not aware of Beauheim et al. (1993) a paper in the *International Journal of Rock Mechanics*.

**Comment:** The discussion in Chapter 2 of the mechanisms by which injection wells may fail lacks an adequate description of the construction of a fluid injection well.

The discussion in Chapter 2 was not intended as an in depth discussion of completion practices. The discussion of the connection of pipe in the hole to the intact rock is still correct. The pressure the pipe experiences depends upon whether it is intimately connected to the surrounding rock or the space between the pipe and the rock is filled with liquid.

The reviewers point to schematic diagrams of hole and pipe that are nicely symmetric with the pipe centered in the hole. This is not what happens in the real world; these schematic diagrams are at best idealizations of what happens in a borehole. Holes are invariably crooked. Pipe tends to rest against one side or the other of the hole. Cement fills the annular space, such as it is. The cement is often discontinuous, implaced in stringers, even when great care is taken. Cement bond logs are one of the tools used in the oil and gas industry to investigate how good cement jobs really are.

McKamey (1996) examined 74 boreholes in the vicinity of WIPP. He suspected 15 of the 74 to be in non-compliant with the State Engineer’s Regulations regarding plugging to protect water bearing horizons in the area. Another 49 of 74 boreholes were judged to be inconclusive. The information for these 49 boreholes was insufficient to determine compliance. Contrary to Sandia/DOE’s claim that operators abide by the regulations, these data indicate at least 20 % of the holes examined are not in compliance—the fraction could be much higher.

The remarks by the reviewers reflects an inconsistency with Performance Assessment (PA). In PA the corrosion rate of pipe is assumed to be very aggressive. Plugs are assumed to last 200 years. Statements are made in the CCA that plugs only generally last tens of years. On the other hand, Sandia argues that well
failures associated with reinjection are rare and state statistics to show this. Bailey in a Sandia memo states field observations indicate that casing failures in the Salado are well-known (La Venue, 1991—Bailey memo, p. B-20).

CHAPTER 3 The Hartman Scenario

Comment:  The permeability (of the Bates #2 well blowout zone) simply cannot be determined from the available data.

One of the common techniques for estimating permeability is the use of the specific capacity to estimate transmissivity and permeability. The usual practice is for a well driller to do an acceptance production test in which both the production rate and the drawdown in the production well are measured. The specific capacity of a well is the production rate divided by the drawdown, for example: gpm/feet of drawdown. Theis et al. (1963) showed on theoretical grounds that specific capacity could be used to estimate transmissivity and permeability. Bredehoeft et al. (1983) showed with a similar analysis that the method worked equally for flowing wells.

The Theis and Bredehoeft analyses are based upon transient flow theory. The method is quite robust; both Theis et al. and Bredehoeft et al. showed that the methodology is insensitive to the value of the storage coefficient. The results are not sensitive to the radius of the drill hole.

In the Hartman Scenario report I showed that three methods of estimating the permeability of the zone that blew out at the Bates #2 well provided similar results.

The results of using the specific capacity to estimate transmissivity and permeability is widely used and based upon sound theory. The reviewers have not bothered to examine the references cited. The comment by the reviewers is without substance.

Comment: Chapter 3 concludes with a brief description of a conceptual model for the relationship between porosity and permeability in fractures.

The appropriate fracture model has been a long-term debate within Sandia. PA uses the so-called Porosity-Model relationship. However, Sandia has numerous references to an Aperture Model that also fits the empirical data and gives quite different results. Larson and Davies (1993) in a memo to Martin Tierney point out there are two models to describe the permeability changes associated with hydraulic fractures; 1) the Porosity Model—used by PA, and 2) the Aperture Model. The Larson-Davies memo appears to be the first place where the relationship between porosity and permeability is plotted; for reference, I have included their Figure 2. I quote their memo:

"The difference between the Aperture Model and the Porosity Model is the degree of permeability change associated with the interbed dilation. A graphical comparison of permeability versus porosity was prepared to illustrate this difference in behavior under different assumptions about critical input parameters (Figure 2). As shown in Figure 2, the Aperture Model has a rapid increase in permeability once the fracture dilation begins regardless of the number of active fractures, whereas the Porosity Model has a gradual and nearly linear increase in permeability. The Porosity Model requires values of \( J \) of about 0.40 to attain permeabilities similar to the Aperture Model at a porosity of 0.02 if the initial porosity (matrix porosity) is 0.01. However, it is apparent from the graph that the two conceptual models are incompatible, i.e. no selection of parameters can make the shape of the porosity-permeability correlation in the Aperture Model look like that in the Porosity Model. Because of this difference, it is important to assess whether the effects of the difference warrant adopting a new correlation for use in Performance Assessment calculations."
Figure 2. Relationship between change in interbed porosity due to fracture dilation and interbed intrinsic permeability for two conceptual models. Because the behavior of the two sets of curves are inherently different, there is no parameter selection that can make one conceptual model behave as the other.

$h = 1.0 \text{ m}$

$\Phi_m = 0.01$

$k_m = 1 \times 10^{-19}$
In a report by Key et al. (1994) the value of n was set to a maximum of 22.95 \( (n = J) \), referred to above by Larson and Davies, 1994). At \( n = 22.95 \) a hydrofrac was computed to extend out 1800 m.

Beuheim et al. (1994) point out that there are two alternative models to describe the permeability of fractured anhydrite: 1) the Porosity Model; 2) the Aperture Model. They argue both models can be made to fit the empirical data. They suggested additional experiments to distinguish between the competing models. Freeze et al. (1995) discuss the alternative aperture model. They go on to state:

*Because of the higher predicted permeabilities the aperture model will propagate fracture-altered properties away from the repository, and will likely increase gas migration distance (Freeze et al., 1995).*

Larson (1997) in a memo to Lori Dotson gives Beuheim (1994) Figure 14 as a reference for changes in the porosity-permeability model. Figure 14 shows data less than an order of magnitude change in permeability with an 8 MPa change in confining pressure (Figure 14 is reproduced here).

As I examine the references neither Beuheim et al. (1993) nor Waversik et al. (1997) address the issue of which of the proposed models, 1) Porosity, or 2) Aperture, better fit the empirical data. A problem with the hydrofrac experiments that were done is that they were conducted close to the repository where the influence of the mined opening impacted the results.

The Aperture Model, as described by Larson and Davies (see their Figure 2—included here), predicts a change in permeability ranging from 5 to 10 orders of magnitude with a small change in porosity. The major change in permeability occurs when the hydraulic fracture is created. Beuheim et al. (1993) in plotting the pressure during hydrofrac experiment shows that 1) the pressure increases rapidly in the interval to be fractured once pumping is initiated, 2) the pressure reaches a point where the hydrofrac occurs, and 3) once the fracture occurs the pressure drops down and stabilizes with continued pumping—the pressure is such to accommodate continued injection, maintain the fracture open, and extend it. This is a typical pressure plot for hydraulic fractures. The Aperture Model more nearly reflects the empirical data.

The best explanation for why the Porosity Model was chosen by PA seems to be expressed by Larson and Fewell in a memo to Chu (dated March 12, 1997). The explanation is twofold. I quote the following questions and answers.

**Q. Why are there maximum porosity and permeability changes?**

**A.** These are set to prevent the possibility of unphysical destabilizing values being calculated for fluid flow parameters in BRAGFLO .......

**Q. Why isn’t a discrete-fracture model implemented in BRAGFLO?**

**R.** The problem posed by dynamic fracturing of interbeds due to high gas pressure is not amenable to solution by models in existence .......

In Chapter 4, I implemented a dynamic flow model that simulates discrete fractures in the flow domain. This model is stable, but only with small times steps, time steps of the order of seconds.

Other work using linear fracture mechanics (Mendenhall and Gerstle, 1993; Gerstle, Mendenhall and Wawersik, 1996) indicates that some model like the Aperture Model (if not the Aperture Model) is the appropriate relationship to describe hydraulic fracturing associated with WIPP.

*Returning to the comment, the empirical field data does not distinguish between the Porosity Model and the Aperture Model. PA has chosen to use the Porosity Model.*

Returning to my modeling, I used 4 % porosity for the Marker Beds; this is the highest value used in the Porosity Model. One can argue that I should have used 1.1 %, the measured virgin anhydrite porosity; I
Figure 14. Laboratory data showing reduction in permeability as confining pressure increases.

Figure 15. Field data showing increase in permeability as induced pore pressure increases.
chose the larger value. When it comes to transport, the higher porosity slows the transport of chemical constituents and gives WIPP the benefit of doubt.

I calculated the fracture aperture necessary to account for the permeability I estimated for the Bates #2 blowout zone. For one fracture the aperture is 0.02 cm, for 10 fractures it is 0.012 cm each. Wawersik et al. (1997) found similar results; they state:

*The corresponding average fracture opening during the last and most extensive pressure cycle in MB 139 was approximately 0.3 mm (0.03 cm).*

If I use 10 fractures then the increase in porosity is 0.12/4 or 12/400—a 3% change in porosity. If the virgin porosity is 1.1%, then the 10 fractures would increase the porosity by approximately 11%. If the porosity is 1.1% and there is only one fracture, then change in porosity is 0.02/1.1 or 2/110—less than a 2% change. In any case this change in porosity is small; neglecting this increase does not seriously impact the model results.

*The comment by the reviewers is based upon the Porosity Model they choose to implement. This model is subject to question by their own scientists.*

**Comment:** ..........the assertion that an increase in porosity is small does not necessarily mean that it is not important to the simulation results.

The reviewers go on to show that a value of the storage coefficient of $1 \times 10^{-5}$ should have been $2 \times 10^{-5}$. I accept their correction. Applying this to the results (Chapter 4), instead of a fracture extending 2.8 km in 100 days; it would take approximately 200-300 days to extend the same distance.

**CHAPTER 4 Modeling Hydraulic Fractures**

**Comment:** We do not believe that this chapter contains sufficient information to either evaluate Bredehoeft’s conceptual model for fracturing or its numerical implications. The chapter should contain a clear discussion of the numerical model and references to standard documentation of the numerical model and the computer code used to implement it.

The point of Chapter 4 was to introduce my hydrofrac model that then would be utilized in the various scenarios that follow in subsequent chapters. The numerical code was clearly identified; I quote:

*The model JDB2D/3D (Bredehoeft, 1990) modified to include hydraulic fractures is used to simulate the Hartman/Rohdes Yates Scenario. (Bredehoeft, 1997, p. 19).*

The code was published by the U.S. Geological Survey and is in the public domain. Bredehoeft (1990) contains a full description of the code including references to the basic theory; the appendices contain the FORTRAN listing of the code along with test problems. *The reviewers have simply not bothered to look at the references included in the report.*

The modeling procedure is clearly described on page 18; I quote:

*The modeling procedure used here is to initially simulate flow using the virgin permeability of the rock. During a time-dependent, transient simulation, when the model indicates that the pressure (or head) in any cell goes above lithostatic the permeability in that cell is increased dramatically. In my simulation, I increase the permeability as a single step to the permeability indicated by my analysis on the Hartman #2 Bates well.*

*This statement clearly describes my approach to modeling hydraulic fractures.*
Comment: The statement on page 19 that “cell dimensions were varied with the virgin permeability so that boundary effects were minimized” should be clarified.

I clearly stated that the code

“is finite difference, the grid used is 40 x 40. “

For the various simulations in subsequent chapters the cell dimensions were varied so that the impact of the far field boundary was minimized, as I stated.

Comment: A more complete discussion of the specific application of the model should be provided, including full description of initial and boundary condition and cell dimensions.

There is an extensive discussion of the model assumptions on page 17; these are summarized in Table 4.1. Table 4.1 provides the initial and boundary conditions to anyone examining it. The boundary conditions are the well head pressure and the far-field pressure—these are explicitly stated. The permeability for the virgin state and the hydrofraced state are explicitly stated. The initial condition is the far-field pressure, while not explicitly stated this is implied.

This same comment is repeated by the reviewers for each chapter following Chapter 4 where it is first made. The model used for all the analyses is simple:

1. one boundary condition is at the injection well where either: a) the pressure, or b) the injection rate is specified;
2. the second boundary condition is either: a) WIPP or a second well where the pressure is specified, or b) the pressure is specified for the far field (at the edge of the model domain);
3. the permeability of the model domain must be specified, the hydrofrac is given the permeability estimated for the Hartman # 2 Bates blowout zone; the zone beyond that hydrofraced is either given zero permeability (no flow) or the virgin permeability for anhydrite measured at WIPP.
4. flow during creation of the hydraulic fracture is transient with a storage coefficient, \( S = 10^{-5} \); flow after the fracture is created is steady, \( S = 0 \).

Each Chapter contains a table in which these conditions are explicitly stated. In most chapters there is an additional list of assumptions that describes the particular scenario being modeled.

The review comment is inconsistent with the statements and tables in the report.

Comment: The assumption contained in Table 4.1 on page 19 that specific storage does not change as the fracture opens should be discussed in detail and justified. This appears to be a key step in implementing the conceptual model introduced in the last paragraph of Chapter 3.

This a recurring comment by the reviewers; it results from their conceptual Porosity Model—a model I do not think is supported by definitive experimental results. I reviewed this comment extensively above, and will not discuss it again.

Comment: The assertion made on page 18 that “virgin permeability does not play much of a role in extending the fracture, as long as it is sufficiently low to allow the fracture to extend two miles” should be justified.

This is a result of the modeling.
CHAPTER 5 The Dilemma: Steady Flow

The point of this chapter was to show that in a system with a boundary that is at low pressure it is difficult to maintain a hydraulic fracture open. The low pressure boundary drops the pressure and the hydraulic fracture closes.

Comment: We do not believe that this chapter contains sufficient information to fully evaluate the conceptual model for brine flow in the marker bed or its computational implementation. As is the case for Chapter 4 and other chapters in this report, the text should contain a clear discussion of the conceptual model and its computational implementation. A more complete listing of initial and boundary conditions should be provided.

On page 20 a list of 9 model assumptions are explicitly discussed. The outer boundary condition is the far-field pore pressure—explicitly defined in statement 9. The model domain is explicitly discussed in statement 9. The other boundary condition at the well is explicitly discussed in statement 5. Since the flow field is steady, an initial condition is not required. On page 21, the model assumptions are summarized in Table 5.1.

Comment: The assumption on page 20 that this is a "steady flow analysis" needs further explanation and justification. As noted previously in comments on Chapter 2, it is not immediately obvious that it is appropriate to model fluid injection processes using steady-state assumptions.

Modeling a particular scenario was a two step process. First, a hydraulic fracture is created; this step is described in Chapter 4. It is a transient process. Once the fracture domain is established then flow was modeled through the fracture. Modeling the flow in the fracture is the second step in the modeling procedure used.

At the point at which the hydraulic fracture stabilizes (stops growing outward) there is a pressure distribution in the fracture. If one uses this pressure (or head) as the initial pressure/head distribution for flow then flow field quickly becomes steady—within a few days. For the flow period of the analysis, after the fracture has been established, steady flow is a good assumption.

Comment: The rate of flow reported into WIPP for the higher permeability case (88,000 m³/yr) is presumably smaller than the total injection rate; appears high compared to field rates. The most prolific injector in the region (the David Ross AIT Federal# 1) injected between 1991 and 1997 at an average rate of approximately 137,000 m³/yr (DOE, 1997), with most of this liquid presumably entering the target reservoir in the Bell Canyon Formation.

The reviewers make my point here—88,000 m³/yr is not out of the realm of possibility given the rate of injection of wells in the area. The repository contains approximately 50,000 m³; at a rate of inflow of 88,000 m³/yr the repository could be flooded in less than a year.

Comment: As a minor point, it appears that the vertical scale is incorrect on Figure 3. The text indicates that the injection well is maintained at 4 MPa above the far field pressure. Assuming a brine hydrostatic gradient of 0.525 psi/ft (consistent with the surface pressure simulated by the well), the injection well should appear in Figure 5.1 with a head approximately 340 m above the far field, rather than 100 m as shown.

There are several responses to this comment. Clearly the reviewers were able to interpret from the report the boundary conditions—the pressure at the well bore and in the far-field. The boundary conditions were not as obscure to them as their comments above would suggest.

Second, the plot in Figure 5.1 is taken directly from the model results. The pressure as computed in the finite difference cells is plotted. The pressure in the cell that contains the injection well is an average pressure over the block; this pressure is not the same as the pressure at the well bore. One has to make a
correction to go from a model block pressure to the pressure at the well. The plot is not wrong; it is the model pressures. I will grant that this might have been explicitly stated.

**Comment:** The plot also clearly shows the effect of maintaining WIPP at a constant, relatively low pressure. It becomes a dimple in the head surface, and functions unrealistically as a permanent sink.

I stated clearly the model assumption:

Assumption “8. (p. 20) the repository is at the far field pore pressure, 12.7 MPa, and remains at this pressure during the simulation. (Early in the history of the repository the pressure could be more or less atmospheric. As the repository receives significant flow the pressure will increase. For this analysis, I neglect the early pressure history.)

I believe the model assumptions are perfectly clear. These are after all assumptions that demonstrate the effect of the low-pressure boundary of the hydraulic fracture—the point I wished to make.

The fact that PA assumes well seals fail in 200 years has the effect of maintaining the repository at hydrostatic pressure once it has been penetrated by a drill hole.

**CHAPTER 6 The Stoelzel-O’Brien Cross-Section**

The point of this chapter was to demonstrate that a cross-section analysis significantly underestimates the flow. I did not attempt to implement the Stoelzel-O’Brien (1996) model. Rather, I showed that a cross-section will significantly underestimate the flow. Again the reviewers do not address the major point but rather focus on minor points. None of their comments refute the point that a cross-section underestimates the flow.

**Comment:** Based on the information provided, however, we do not believe that the comparison implied between Bredehoeft’s model and the Stoelzel and O’Brien model is adequately justified. Specifically, the text should address the geometry used by Stoelzel and O’Brien, which included an approximation of radial flaring around the injection wells as well as cross-sectional regions between the repository and the well.

Clearly, the Stoelzel and O’Brien model is a cross-section. Near the well-bore they flare the model, as they move further out the model becomes a cross-section. As suggested above one has to make a correction to go from the pressure in a model block to a well in that block. One can reduce that correction by making smaller grid blocks near the well. Stoelzel and O’Brien minimize the model correction by flaring the model near the well.

**Comment:** As a perhaps minor point, the vertical scale on Bredehoeft’s Figure 6.1 may have the same problem as Figure 5.1.

As pointed out in Chapter 5 above, Figure 6.1 is taken from the model. The pressure plotted represents average pressure in the grid block of the model and not the pressure at the well bore.

**CHAPTER 7 Transport Through WIPP**

**Comment:** The purpose of this chapter is unclear.

I believe the purpose is perfectly clear in the report. The first sentence states:
"In this chapter I examine another scenario that has been discussed; this is the potential for a single leaking injection well to move brine through Marker Bed 139 contacting WIPP waste and moving it. In other words, transport of contaminant away from WIPP by brine injected in a single well.

In the penultimate paragraph in the chapter I conclude:

"We have created a situation in which the pressure is sufficiently high to hold a fracture open, but the flow through the region is very small. The flow is controlled by the surrounding region of virgin permeability. This again suggests that this scenario is not one of major concern."

CHAPTER 8  The High Pressure Scenario

Comment:  We do not believe that this chapter contains sufficient information to fully evaluate the conceptual model for brine flow in the marker bed or its computational implementation. Specific justification of modeling assumptions, including initial and boundary conditions.

The report is structured so that the general procedures are specified in Chapter 4. It was not deemed necessary to repeat this information in subsequent chapters. The following statement was made:

"I conceptualize the model similar to what I did above."

A transient simulation was done to create the hydraulic fractures. A number of specific model conditions are listed—1-5 (p. 28). The model assumptions including the initial and boundary conditions are spelled out in Table 5.1. The extent of the hydrofraced domain is shown in Figure 8.2. The hydrofrac was simulated using the methods discussed in Chapter 4. Once the frac was created I modeled the flow. I stated:

"I model this scenario by 1) creating a hydraulic fracture between the leaking well and WIPP, and 2) examine the flow that might occur should this happen.

I believe the procedure is clear, especially in light of my list of assumptions and Table 8.1. The review comment is without substance.

Comment:  As shown in Bredehoeft's Figure 8.1, the earliest time in the 100 realizations at which the pressure in the waste panel reaches 14.7 MPa is approximately 2700 years. Thus one condition, of many, that must occur is that fluid injection in the vicinity of the WIPP must be assumed to persist many thousands of years in the future. The text of Chapter 8 should acknowledge this point, and discuss its conceptual basis.

I made the statement in the last sentence of the chapter acknowledging this point:

"Of course, this scenario does not occur until the pressure within the repository builds up to near lithostatic."

In the executive summary I stated:

"This only happens at some time following closure of the repository—approximately 1000 years or more following closure."

Figure 8.1, taken from WIPP PA several realizations in which the pressure exceeds 14 MPa in the period from 1000 to 2000 years.

EPA in its rules recognized that drilling would go on for the entire 10,000 years under consideration. Resource recovery associated with the drilling will undoubtedly require the disposal of brine, and may
Rebuttal: Swift et al.  

28 July, 1992

Involvement injection for recovery of oil and gas and other minerals. I recognize that the scenario under consideration in Chapter 8 is a problem for 1000 years and beyond.

Comment: What is the justification for assuming that the repository remains at constant pressure regardless of the brine inflow.

The point of examining this scenario is not to calculate the pressure history of WIPP; the point is to show that the scenario of a leaking injection well can put a significant flow of water into WIPP under certain conditions.

Comment: Although the text does not show not mention boundary conditions for the flow model. Bredehoeft's Figure 8.3 shows that no flow boundaries were imposed on this model.

The reviewers are correct in this instance. This is inconsistent with the earlier result in Chapter 5.

I did enough simulations to know that how one handled the far field boundary did not significantly impact the results. Whether the far field permeability was 5 orders of magnitude lower than the hydraulic fracture or impermeable does not significantly impact the results. The results shown in Chapter 7 demonstrate this. However, the reviewers are correct.

CHAPTER 9 The Pulsing Scenario

This is the most uncertain of the scenarios in the report. The point of the scenario is that one asks: what happens if a hydraulic fracture approaches WIPP when the repository is at a low fluid pressure? This scenario is an attempt to answer that question; it is one possibility.

Comment: If the phenomenon were plausible at WIPP, a more sophisticated modeling approach than that presented by Bredehoeft would be needed to develop meaningful results.

I agree with this comment. My model can be made to pulse, as I demonstrate. Because the phenomenon is highly non-linear, it exhibits chaotic behavior—the duration of the pulses change with time more or less at random. Because of the very high non-linearity the system is very difficult to model even with a simple model.

I accept the criticism that a more sophisticated model is needed. I still ask the basic question—what happens when a hydraulic fracture from a leaking well approaches WIPP? I believe this is a scenario that can pose a problem for the repository. The reviewers admit they have not analyzed this scenario.

CHAPTER 10 The Two Well Scenario

Comment: Many of our comments are similar to those raised in earlier chapters. For example, the text should provide a more complete discussion of the conceptual model and its implementation in the computational model. Assumptions made in setting up the computational model should be clearly stated and justified. Documentation of the computational model, including a more complete listing of initial and boundary conditions should be provided.

My response is similar to that in the other chapters. The assumptions including the boundary and initial conditions are summarized in Table 10.1. There are two relevant boundary conditions for the flow model—
a) the pressure at the inflow well; and 2) the pressure at the outflow well, both are clearly stated in Table 10.1. The table also indicates that I assume the permeability beyond the limit of the hydraulic fracture to be zero. The model parameter values are clearly stated in Table 10.1. The model domain and the grid cells are
shown on Figures 10.1, 10.2 and 10.3. In all of the scenarios, I created a model hydraulic fracture, and then simulated flow through the fracture. The comment seems again without real substance.

Comment: The transport model should be described in detail, because it is its first application in the report.

The transport model is clearly referenced; I state:

“For the transport calculations I use the code JDB-MOC, a method of characteristics code developed for PCs (Bredehoeft, 1994). This code is based upon an earlier code MOC (Konikow and Bredehoeft, 1994).”

The MOC code (Konikow and Bredehoeft, 1978) is public domain, published by the U.S. Geological Survey, with very extensive documentation. The PC version (Bredehoeft, 1994) is sold as a proprietary code. It too has extensive documentation. In both versions the FORTRAN code is included with the model. Again, the reviewers have chosen not to examine the references cited in the report.

Comment: If the pathway into the unplugged borehole is envisioned to be hydraulic fractures, as stated in the text, then the mechanism by which the pressure in the borehole remains high enough to keep the fractures open should be described. Without further explanation, this assumption that fractures will remain open appears to directly contradict the conceptual model for fracturing presented in Chapter 2, in which fractures close if pressure falls below lithostatic.

The conceptual basis for the specification of lithostatic pressure in the outflow well is not stated either, and we believe none can be offered. For an unplugged hole to maintain atmospheric surface pressure and lithostatic downhole pressure it would need to be filled with a liquid with the density of rock (approximately twice the density of brine).

The issue is how to maintain a pressure at the bottom of the outflow borehole at or above lithostatic. What is required is that the dissipation of head up the borehole be such that the bottom of the hole remain at lithostatic pressure. One way to envision this is to have the borehole filled with a material with finite permeability—sand, for example. Given the flow rate envisioned in the two well scenario of Chapter 10, if the material in the borehole has a hydraulic conductivity of approximately $10^{-7}$ m/sec (about the equivalent permeability estimated for the Hartman hydrofrac) the head dissipation in the borehole would maintain the bottom of the hole at lithostatic pressure. An open pipe of approximately 1 inch in diameter produces the same head dissipation, and would also maintain the bottom of the hole at lithostatic pressure or slightly above. The reviewers are simply wrong.

Comment: The conceptual basis should be provided for the assertions on page 36 that “the repository and the disturbed rock zone (DRZ) provide a high permeability pathway for fluids where it exists” and that brine in the marker bed will have “approximately the same concentration of radionuclides as the brine within WIPP”. What is Bredehoeft’s estimate of the permeability of the waste and the DRZ? How does it compare to the permeability of his fractured marker bed? If the permeability of the fractured marker bed is much greater than that of the DRZ and waste, most of the flow may be confined to the marker bed. We believe Bredehoeft’s assumption of instantaneous flow and transport between the marker bed results in an extreme overestimation of the quantities of radionuclides in marker bed brine.

CCA uses a permeability for the waste region of $1.7 \times 10^{-13}$ m$^2$. I estimated the equivalent fracture permeability for the Hartman # 2 Bates well as $3 \times 10^{-12}$ m$^2$. The repository is three meters in height; the transmissivity of the repository becomes $5 \times 10^{-13}$ m versus $30 \times 10^{-13}$ m for marker bed 139. There is a factor of 6 difference in transmissivity—less than an order of magnitude difference. Given the uncertainty in both these numbers, assuming the brine in the repository is available to the marker bed seems a conservative and reasonable assumption.
In the room Q experiment, brine flowed out beneath the room through the marker bed. Making a similar assumption again seems reasonable.

Comment: ... The report contains insufficient detail about the transport calculations to evaluate the accuracy of the results conditional on the modeling assumptions, but as Bredehoeft notes in the caption of the figure "the spread in the curves at later times is some measure of the error". Based upon a visual inspection, it appears that approximately 50 EPA units have passed a point 3 km from the repository at 13 years, but only 43 EPA units were reported at 2.75 km. The model appears to have created 7 EPA units of radionuclides in 250 m, which is a 16% mass balance error. Transport errors of this magnitude may not be insignificant. The report should discuss this error in more detail, and documentation of the reliability of the code should be provided.

The flow equation is generally parabolic in form, and relatively easy to solve numerically. With flow, mass balance errors can usually be made to be less than 1%, sometimes much less. The transport equation is known to be much more difficult to solve because it is parabolic—it involves approximating numerically a sharp concentration front. Nearly all the numerical schemes to solve the transport equation involve some compromise. Often the numerical scheme introduces numerical dispersion. The Method of Characteristics (MOC) is useful because it can handle the transport equation with a zero dispersion coefficient. However, MOC has other numerical problems, especially where the flow field is divergent.

In the MOC procedure a finite number of particles, representing chemical concentration, are moved in the flow field. In a divergent flow domain some finite cells can become vacant of particles. This can be fixed in several ways, none of which is fully satisfactory. For this reason material balance errors often range 10 to 20%, sometimes up to 30%. Most workers judge errors of this magnitude to be acceptable given the difficulties of solving the transport problem. The error, which I acknowledged, is not unusual in these problems. The reliability of the MOC code is discussed extensively in the code documentation which the reviewers choose not to inspect.

The transport code SECO has recently been compared to SWIFT and shown to produce different results explained as due to numerical dispersion. The width of the plumes generated by the two codes differed by 50% (Sandia, WPO # 44599). EPA Comment 5 (March 19, 1997) indicated mass balance errors exist in all but 2 of 600 transport simulations done for CCA. I quote from Sandia, WPO # 44700 in discussing the SECO error:

These results show that the mass balance error approaches 100% at the completion of the injection phase (first fifty years) for all the isotopes except the daughter product Th^{230}.

This discussion underscores the difficulty of computing transport.

Comment: Bredehoeft's Figure 10.4 which shows radionuclide concentrations through time at differing distances from WIPP through time, shows many second-order fluctuations in concentration, particularly at the outflow well. What do these fluctuations represent, and why do they appear to increase in magnitude through time?

As explained above, the Method of Characteristics involves 1) placing a finite number of particles that represent chemical concentration in the flow domain, and 2) then moving these particles in the flow velocity field. Because the particles are finite in number, and because the flow domain is approximated by a finite number of cells, the method produces second order fluctuations. The fluctuations are more pronounced where the flow is divergent as it is around the injection well. The second order fluctuations do not invalidate the method. One could smooth the output; but it seems more honest to report the model results without smoothing.
REFERENCES CITED


EPA Comment 5 (March 19, 1997) Enclosure 1, page2, 194.23 (a)(3)(iv)


15


The HYDRODYNAMICS Group

studies in mass and energy transport in the earth

The HARTMAN Scenario Revisited
Implications for WIPP

prepared for: NEW MEXICO ATTORNEY GENERAL

John Bredehoeft
Walter Gerstle

The Hydrodynamics Group
Civil Engineering, University of NM

August, 1997
EXECUTIVE SUMMARY

The Hartman # 2 Bates Well Blowout

Our review of the data continues to indicate that the blowout of the Hartman # 2 Bates well is best explained as a hydrofrac in the lower Salado Formation that extends from the Texaco Rhodes-Yates Waterflow to the well. This is the consensus of most investigators that examined the empirical data.

DOE and Sandia dismiss the interpretation that a hydrofrac happened at the # 2 Bates well.

LEFM versus BRAGFLO

Linear Elastic Fracture Mechanics (LEFM) is a widely used and accepted model for fractures including hydrofracs. BRAGFLO when compared to LEFM underestimates fracture radius by a factor of 5 times. Our review of the Sandia data does not present sufficient information to select the “porosity model” used in BRAGFLO over the “aperture model”. Numerous internal documents indicates Sandia’s concern about this problem. Use of a flow model such as BRAGFLO, whether it uses the porosity model or the aperture model, to estimate the extent of hydraulic fractures is at best an ad-hoc procedure. BRAGFLO in its current implementation is an inadequate model to predict the extent of hydraulic fractures.

Stoelzel and Swift (1997) use BRAGFLO to compute hydrofrac radius. Their analysis is non-conservative in that they:

1. assume uniform permeability in the entire annular space between the borehole rock wall and the casing; and
2. allow fluids to hydrofrac all the marker beds simultaneously.

These assumptions minimize the hydrofrac radius; they do not represent what happened at the Hartman # 2 Bates well.

Scenarios

We conclude that three scenarios described in Bredheoef (1997) still pose problems; these are:

1. hydrofrac extends from a leaking injection well into WIPP at low pressure;
2. hydrofrac extends from a leaking injection well into WIPP when it is at lithostatic pressure;
3. hydrofrac extends through WIPP and encounters a poorly plugged well that leaks upward.

Scenario number 3 has the highest negative consequences. It predicts that the containment criteria are significantly violated in a few years.
Chapter 1  INTRODUCTION

In March, 1997 Bredehoeft submitted a report to the EPA WIPP Docket entitled:

*The Hartman Scenario: Implications for WIPP.*

In the March report three scenarios were identified that involved hydrofracs created by a leaking injection well that pose problems for WIPP; these scenarios are:

1. hydrofrac extends from a leaking injection well into WIPP at low pressure;
2. hydrofrac extends from a leaking injection well into WIPP when it is at lithostatic pressure;
3. hydrofrac extends through WIPP and encounters a poorly plugged well that leaks upward.

Sandia in a memorandum by Swift et al. (June, 1997) reviewed the Bredehoeft March Report and found it not to be a sound analysis. They dismiss the Hartman Scenario that involves a hydrofrac as one interpretation, but not their interpretation. They choose not to address the Hartman case. They dismissed the remainder of the report by addressing the analysis and making a number of minor points that they argue invalidated the thrust of the report.

Bredehoeft rebutted the comments of Swift et al. in a memorandum to the EPA WIPP Docket dated July 28, 1997. In the rebuttal Bredehoeft argued that Swift et al. had avoided the salient issue—*are there injection well scenarios, stemming from what happened at the Hartman # 2 Bates well, that could pose problems for WIPP?*

DOE argued, based upon work by Sandia (Stoetzel and O'Brien, 1996; Stoetzel and Swift, 1997), that the consequences of a leaking brine injection well are insignificant and can be eliminated from consideration on that basis. (They do not argue that a leaking injection well is a low probability event—at least not as their primary basis to screen it out of consideration.) Their most recent analysis,

Stoelzel and Swift (1997), *Supplementary Analysis of the Effect of Salt Water Disposal and Waterflooding on WIPP: WPO # 44158,*

does not address what happened at the Hartman # 2 Bates well. In this investigation Stoelzel and Swift (1997) allow brine to simultaneously enter all the anhydrite beds in both the Castile and Salado Formations. This is not what we believe happened at the # 2 Bates well; there brine was encountered in one anhydrite layer in the Salado Formation.

WIPP Quarterly Meeting—Santa Fe, August 31, 1997

The Hartman Scenario was discussed at length in this meeting. Officials from both DOE and Sandia stated emphatically that they did not believe that the cause of the brine blowout was a hydrofrac that extended from the Texaco Rhodes-Yates Waterflood to the Hartman # 2 Bates well. A hydrofrac is a widely accepted interpretation of what occurred; a number of experts expressed this opinion. A court found in favor of damages for Hartman on the basis of the hydrofrac.

**DOE by denying that a hydrofrac explains the blowout at the Hartman well can ignore its implications for WIPP.**
Scope of this Report

What happened at the Hartman #2 Bates well happened almost ½ mile below the land surface; what exactly occurred is a matter of interpretation. The weight of evidence suggests that it was a hydrofrac that extended within the lower Salado Formation from the Texaco Rhodes-Yates Waterflood to the #2 Bates well. We review the data from the blowout in Chapter 2.

We have done additional work since the March Report utilizing Linear Elastic Fracture Mechanics (LEFM) that supports our earlier analysis. We believe LEFM is a good representation of the hydraulic fracturing process. We present this work in Chapter 3.

We also present a comparison in Chapter 3 of the LEFM model with the results of hydraulic fractures predicted by BRAGFLO. BRAGFLO when compared to the LEFM model underestimates the fracture radius by a factor of 5 times. We conclude that BRAGFLO as implemented is inappropriate for accurately estimating the extent of hydraulic fractures associated with WIPP.

We present analyses of the three scenarios of concern in Chapters 4, 5 and 6. We intend these analyses to satisfy some of the criticisms in the Swift et al. (1997) comment that Bredehoeft’s methods in the March Report were not adequately explained.

We do not address the probability of the scenarios we pose for WIPP. Rather our analyses assume that an injection well leaks in such a way that brine is injected into one of the anhydrite marker beds associated with WIPP—Marker Bed 139, for example. We ask—what might happen?
Chapter 2  The Hartman # 2 Bates Blowout

Introduction

Waterfloods are the most universal method of secondary recovery for oil; they are common practice throughout the world. Van Kirk (1994) did an analysis of the Texaco Rhodes-Yates Waterflood and its impact on the Hartman # 2 Bates well. He observed, after extensive analysis, that it is not uncommon for brine injected in waterflood operations to end up in strata other than the target zone. Texaco’s wells in the Rhodes-Yates Waterflood experienced water flows in the Salado Formation.

Ramey’s (1976) memo to the Secretary of the New Mexico Energy and Minerals Department records “numerous” severe water flows in Lea County, attributed to injected water escaping the target zone, penetrating the salt section, and moving laterally. An industry committee studied the problem in the Vacuum Field, and recommended shutting-in certain waterfloods for a period in an effort to identify leaking wells.

The Vacuum Field Geological Committee reported in 1987 on numerous instances of high-pressure, high-volume water flows encountered in the Salado Formation. A total of 48 flows of interest were examined. The Committee reported that fluid flow was facilitated both by dissolution and by mechanical fracturing—hydraulic fractures. The Committee concluded that large volumes of fluid can be stored in and along underclays and underclay-evaporite interfaces without the formation of large vertical solution cavities. Examples of Salado water flows include Texaco’s Central Vacuum Unit # 169 (Van Kirk, 1994).

NMOCDC records list 189 water flows encountered near waterflood operations in District 1 (Curry, Lea, Roosevelt, and part of Chavez Counties) in southeastern New Mexico during the period 1978 through 1992. The NMOCDC files also indicated severe water flow problems in the Eunice-Monument area in the 1970s.

Bailey’s (1990) memo reports, based upon the work of the Vacuum Field Salt Water Flow Geologic Committee, that there have been water flow drilling problems and numerous casing leaks in the Dewey Lake Red Beds, Rustler, and Salado Formations for many years. She goes on to state that large volumes of water travel laterally along bedding planes of the clastic-evaporite sequence; in many instances brine from the Salado water flows can be identified by chemical analysis as reinjected produced water. Bailey states: “casing leaks through the salt section are the most logical pathways for introduction of fluids into the Salado Formation.”

The evidence indicates that leaks are not uncommon associated with waterflood and reinjection operations.

The Hartman # 2 Bates Blowout—Empirical Facts

During drilling, the hole encountered a substantial flow of brine at a depth of 2240 feet in the lower part of the Salado Formation. The flow of brine was uncontrollable; the highest flow rate was 1200 barrels per hour (840 gpm). The well flowed for four days; the quantity of brine hauled away is given in Table 2.1:
Table 2.1  Brine hauled from the Hartman # 2 Bates blowout (Van Kirk, 1994).

<table>
<thead>
<tr>
<th>Date</th>
<th>Quantity Hauled (bbls)</th>
<th>Quantity Hauled (m³)</th>
</tr>
</thead>
<tbody>
<tr>
<td>16 January, '91</td>
<td>9,220</td>
<td>1466</td>
</tr>
<tr>
<td>17 January</td>
<td>14,130</td>
<td>2246</td>
</tr>
<tr>
<td>18 January</td>
<td>3,420</td>
<td>544</td>
</tr>
<tr>
<td>19 January</td>
<td>4,080</td>
<td>649</td>
</tr>
<tr>
<td>Miscellaneous</td>
<td>4,710</td>
<td>749</td>
</tr>
<tr>
<td>Total</td>
<td>35,560</td>
<td>5654</td>
</tr>
</tbody>
</table>

The shut-in pressure for the blowout zone was 1000 psig at the land surface. According to Van Kirk (1994): “this is a reflection of a very high down-hole pressure which equates to a pressure gradient of 0.966 psi per foot of depth.” The weight of overburden, the vertical lithostatic stress, is equivalent to a pressure gradient of 1.0 psi per foot. In the blowout zone the fluid pressure is approximately lithostatic.

The Experts’ Opinions

Van Kirk in his trial deposition reported that a number of newer wells in the Texaco Rhodes-Yates Waterflood encountered water flows in the lower Salado Formation.

Powers examined the Hartman # 2 Bates well data. He generally concurred with Van Kirk’s conclusions. He also commented on the pressure gradient and the possibility that the Bates # 2 well encountered a natural brine pocket (Silva, 1996). Powers states:

1. “Typically, the measured pressure gradients from brine reservoirs in either the Castile or the very low flows in the Salado are considerably less than 1 psi per foot, ranging down to 0.8 psi per foot or less. The difference in pressure can be used to distinguish between natural or human induced occurrences.”

Powers went on to interpret what occurred at Hartman # 2 Bates well; he states (Silva, 1996):

2. In the Hartman case, one has to either accept a natural cause or, if it is not natural, one must believe that fluid was transmitted along a bedding plane to the Bates lease perhaps for a distance of two miles. Transport along the bedding plane is the best explanation.
Transmissivity of the Blowout Zone

Bredehoeft (1997) in the March Report estimated the transmissivity of the blowout zone based upon specific capacity. A more accurate estimate of transmissivity can probably be obtained by using the record of flow as given in Table 2.1. (Transmissivity, T, is defined below.) Figure 2.1 is a plot of the average flow rate for the four days of data versus a calculated flow rate for a free flowing well (Lohman, 1972). The best fit value for transmissivity is $3 \times 10^{-5}$ m$^2$/sec. This value is lower than the earlier estimate in the March Report, $1.4 \times 10^{-4}$ m$^2$/sec, and is probably a better estimate for the equivalent transmissivity than that presented by Bredehoeft (1997).

We now believe the estimated transmissivity represents the equivalent transmissivity of a hydrofrac. Rather than divide the transmissivity by the thickness of the stratigraphic interval that produced the blowout in the # 2 Bates well to obtain a hydraulic conductivity, as Bredehoeft did in the March Report; we assume our new estimate represents an equivalent hydraulic conductivity (permeability) of the hydrofrac. In other words, we are assuming that the transmissivity and the hydraulic conductivity are equivalent in this instance, and describe the ability of the hydrofrac to transmit brine. The value for equivalent hydraulic conductivity is $3 \times 10^{-5}$ m/sec.

A value of hydraulic conductivity of $3 \times 10^{-5}$ m/sec (permeability—$3 \times 10^{12}$ m$^2$) is approximately 5 orders of magnitude higher than the highest in-situ permeability measured at WIPP. We assign this value to the hydrofrac in the calculations that follow—Chapters 4, 5, and 6.

![Figure 2.1](image-url)  
**Figure 2.1**  
Average flow from the Hartman # 2 Bates blowout zone fit to a theoretical calculation for a flowing well (Lohman, 1972).
Flow from the Yates Formation

One alternative explanation, suggested by DOE and Sandia officials, is that the blowout involved a flow path up the # 1 Bates well from the Yates Formation through the Salado anhydrite to the # 2 Bates well.

We made an attempt to analyze this scenario. In order to do the analysis we need a transmissivity for the Yates Formation. We obtained core permeability data for 3 boreholes in Lea County from the New Mexico Bureau of Mines and Mineral Resources, Office of the State Geologist in Socorro; the three wells are:

1. Gulf # 1-I Janda section 2-23S-36E, drilled in 1949;
2. Cities Service # 13-B Closson section 30-22S-36E, drilled in 1957;

There were 100 or more laboratory core permeability determinations in the Yates Formation in each well. Figure 2.2 is a plot of the core data. The data is referenced to the base of the Yates Formation. It is interesting how the same zones of permeability show up at the same elevations above the bottom of the formation; this is seen best on the linear plot of permeability versus elevation above the bottom (Figure 2.2).

The Gulf # 1-I Janda well has the highest Yates transmissivity. The transmissivity can be defined as (Lohman, 1972):

\[ T = \sum_{n=1,2,3} b_n K_n \]

where \( b_n \) is the thickness of a bed for which the hydraulic conductivity (permeability), \( K_n \), is measured. For the Gulf # 1-I Janda well the transmissivity of the Yates Formation is 1967 millidarcy-ft (an oil field unit) which translates to \( 6 \times 10^4 \) m²/sec. This compares to \( 3 \times 10^5 \) m²/sec indicated for the blowout zone above. The transmissivity of the Yates is lower than the blowout zone, but these estimates indicate there is less than an order of magnitude difference.

In Figure 2.3 we compare the calculated flow from the Yates Formation with a transmissivity of the Gulf # 1-I Janda hole with the Hartman # 2 Bates well blowout zone. The calculated flow from the Yates Formation is significantly lower than that observed for the blowout.

This analysis is only suggestive. We do not have core analysis for the Yates in the vicinity of the Bates lease. Permeability is known to vary widely in all natural materials. Our analysis suggests that the brine encountered in the Hartman # 2 Bates well did not come directly from the Yates Formation.

Conclusions

Dennis Powers (Silva, 1996) stated it best: “Transport along the bedding plane (in the Salado Formation) is the best explanation.” The weight of the evidence suggests fluid flow in the Salado Formation from the Texaco Rhodes-Yates Waterflood to the Hartman # 2 Bates well. We will show below that this probably involved a hydraulic fracture.
Figure 2.2  Plots of permeability versus elevation above the bottom of the Yates Formation for three wells in Lea County. The highest permeabilities are for the Gulf 1-J Janda well (diamonds on the plots).
Figure 2.3  Plot of calculated flow from the blowout zone and a Yates well with the transmissivity of the Gulf # 1-1 Janda well.
Chapter 3 LEFM—Hydraulic Fractures

Introduction

Linear Elastic Fracture Mechanics (LEFM) is a classical field of mechanics that has evolved during the twentieth century. It is by far the most well developed and clearly understood model for fractures. There are many textbooks on fracture mechanics. For an introduction to LEFM we refer one to Broek (1974) or Anderson (1989). A good summary of models for hydraulic fractures is found in Rubin (1993), which also contains an extensive list of references on the subject. Other examples of the literature on modeling of hydraulic fractures are given in Shlyapobersky and Chudnovsky (1994), Pollard and Holzhausen (1979), and Petitjean and Couet (1994).

Because of its relative simplicity, LEFM is the prevailing model for describing hydrofracs. Although LEFM does not provide good predictions of hydrofrac behavior in all situations, it provides reasonable predictions in many geological settings. It has been shown that LEFM is an excellent model for large hydrofracs in the WIPP setting (Arguello and Stone, 1994: Gerstle, et al., 1996).

Hydraulic Fractures from LEFM

We envision a simple system. A hydrofrac is created by injection into one of the Salado marker beds—Marker Bed 139, for example. In plan the hydraulic fracture is circular. It grows radially outward as fluid continues to be injected. As the fracture grows it encounters a second borehole, and flow can occur up the second borehole.

We use a finite element model to model the hydrofrac. A number of parameters, including the elastic properties of the rock, need to be specified. The model generates a number of outputs. Table 3.1 is a list of input and output parameters (Gerstle et al., 1996):

Table 3.1 Inputs and Outputs from the LEFM fracture model.

<table>
<thead>
<tr>
<th>INPUTS</th>
<th>Model Value</th>
</tr>
</thead>
<tbody>
<tr>
<td>Poisson's Ratio for the salt</td>
<td>0.25</td>
</tr>
<tr>
<td>Young's Modulus</td>
<td>31,000 MPa</td>
</tr>
<tr>
<td>Fracture Toughness</td>
<td>0.5 MPa m$^{1/2}$</td>
</tr>
<tr>
<td>Depth to the Fracture</td>
<td>659 m</td>
</tr>
<tr>
<td>assumed weight of overburden</td>
<td>2100 Pa/m (1.0 psi/ft)</td>
</tr>
<tr>
<td>2nd borehole when present distance</td>
<td>3000 m</td>
</tr>
<tr>
<td>hydraulic conductivity</td>
<td>variable m/yr</td>
</tr>
</tbody>
</table>

OUTPUT

- Crack Length
- Crack Opening Displacement—centerline
- Crack Volume
- Pressure in Excess of Lithostatic

The LEFM model treats the injected water as if it is incompressible. All the water is assumed to be stored in the fracture until there is some mechanism for outflow—another well, WIPP, etc. Without outflow the extent of the fracture is purely a function of the volume of water injected.
Figure 3.1 is a cross-section along the radius of one of the fractures produced by the LEFM model. Figure 3.2 is a plot of growth of a hydrofrac over time for a hypothetical fracture with an injection rate of 50,000 m$^3$/yr; it shows the fracture extending to 3 km in approximately 400 days at this injection rate. Figure 3.3 shows the vertical displacement at the center of this fracture.

Crack Length $a = 6000$ m

**Figure 3.1** Cross-section of a fracture along the radius showing the finite element mesh.
Fracture Growth

Figure 3.2   Plot of fracture radius versus time for a fracture with an injection rate of 50,000 m$^3$/year. At a radius of 3000 m the fracture encounters a leaking outflow well.
Figure 3.3  Plot of vertical displacement at the centerline versus time for the fracture illustrated in Figure 3.2. At approximately 400 days the fracture encounters an outflow well.
Quantities of Injected Brine

Since the fracture extent is controlled entirely by the quantity of fluid injected, it is of interest to examine the reported volumes, simply to bound potential quantities. Table 3.2 is a list of reported volumes:

<table>
<thead>
<tr>
<th>Reporter</th>
<th>Volume bbls/day</th>
<th>Volume m³/yr</th>
</tr>
</thead>
<tbody>
<tr>
<td>Ramey (1976)—typical waterflood</td>
<td>5000-6000</td>
<td>350,000</td>
</tr>
<tr>
<td>Bailey (1990)—typical injection well</td>
<td>1000-2000</td>
<td>115,000</td>
</tr>
<tr>
<td>LaVenue (1991)</td>
<td>414</td>
<td>23,700</td>
</tr>
<tr>
<td>LaVenue(1991)</td>
<td>960</td>
<td>55,800</td>
</tr>
<tr>
<td>David Ross AIT # 1 Federal (Silva, 1996)</td>
<td>3333</td>
<td>193,000</td>
</tr>
<tr>
<td>Todd 36 Federal # 3 (Silva, 1996)</td>
<td>1000</td>
<td>58,000</td>
</tr>
<tr>
<td>Pogo Oil (Silva, 1996)</td>
<td>1000</td>
<td>58,000</td>
</tr>
<tr>
<td>Cabin Lake Unit (Silva, 1996)</td>
<td>3069</td>
<td>178,000</td>
</tr>
<tr>
<td>Typical New Mexico (Kreitler et al., 1994)</td>
<td>3850</td>
<td>223,000</td>
</tr>
<tr>
<td>San Juan Basin (Kreitler, et al., 1994)</td>
<td>4399</td>
<td>255,000</td>
</tr>
<tr>
<td>San Juan Basin (Kreitler, et al., 1994)</td>
<td>6854</td>
<td>398,000</td>
</tr>
</tbody>
</table>

There was discussion during the Hartman trial of how much fluid was unaccounted for in the Texaco Rhodes-Yates Waterflood. The consultants for Hartman estimated that 20,000,000 barrels of brine (3,000,000 m³) were unaccounted for; Texaco argued it was not that much.

A Hartman #2 Bates Hydraulic Fracture

The most likely shape for a hydraulic fracture is circular; however the injection well is not necessarily located at the center of the circular fracture (Wawersik and Gerstle, 1996). In experimental fractures made in the laboratory the injection well is often located away from the center of the fracture, nearer the edge.

Figure 3.4 and 3.5 show the radius of fractures predicted by the LEFM model versus the quantity of brine injected. These figures were prepared for Marker Bed 139 at WIPP. The zone that blew out at the #2 Bates well is slightly deeper, by approximately 20 m. For our purposes Figure 3.4 applies equally well to the blowout zone.

The Hartman #2 Bates well is 2 miles from the Texaco Rhodes-Yates Waterflood. If the radius of the fracture is centered at the Rhodes-Yates Waterflood the fracture radius needs to extend approximately 3.33 km to reach the #2 Bates well. If the input well was off center near the edge of the fracture, as laboratory experiments sometimes suggest, the radius could be reduced to slightly more than ½ the distance between wells, perhaps as low as 1.7 km. This indicates that the volume of fluid that needed to be injected to create the fracture ranged from 15,000 to 95,000 m³ (94,000-600,000 bbls). As suggested above there may be 20,000,000 bbls (3,000,000 m³) of brine unaccounted for at the Texaco Rhodes-Yates Waterflood. The amount needed to create the hydrofrac is a small fraction of the amount lost.

If the loss of fluid at the Texaco Rhodes-Yates Waterflood is as high as Hartman’s consultants argued, 3,000,000 m³, then the LEFM calculations suggest a hydraulic fracture with a radius of 8 km could be generated.
Figure 3.4  Plot of hydraulic fracture radius versus volume of fluid injected.
Figure 3.5

Plot of hydraulic fracture radius versus volume of fluid injected—larger volumes.
Bredehoeft's Flow-Hydraulic Fracture Model

In the March Report, Bredehoeft (1997) used his flow model to approximate hydraulic fractures. The algorithm for approximating the effects of hydraulic fracturing was to increase the permeability (hydraulic conductivity) to that estimated for the Hartman #2 Bates blowout zone \((3 \times 10^4 \text{ m/sec})\) when the pressure in any cell in the model domain exceeded lithostatic. The permeability change was introduced as a step function. Bredehoeft did not change the porosity as the fracture was created; he argued that the aperture of the fracture is small and makes a negligible change in porosity. The fact that the porosity was not increased was the subject of criticism in the Swift et al. (1997) comment.

Using a flow model to estimate the extent of hydraulic fractures is an ad-hoc procedure. A flow model is not designed to model fractures; it is designed to model fluid pressures. The complexity of the fracture process can only be incompletely approximated by this procedure. The approximation is subject to discretization errors associated with both the spatial mesh and the time steps size. Of critical importance is how the storage of fluid in the fracture with an aperture with a few millimeters is approximated by the flow model.

A somewhat similar ad-hoc procedure to that used by Bredehoeft (1997) is used by Sandia in BRAGFLO; however, in BRAGFLO both a change in both porosity and permeability is introduced. The change in BRAGFLO is introduced gradually; a gradual change makes the numerical model more stable. Bredehoeft's model of hydraulic fracturing requires small time steps to insure numerical stability. We compared the Bredehoeft Flow-Hydraulic Fracture model to the LEFM model. Figure 3.6 shows the results of our comparison; the results compare favorably. The results are seen to be somewhat sensitive to the storage coefficient in the Bredehoeft model. Bredehoeft (1997) favored a storage coefficient of \(1 \times 10^{-3}\); Figure 3.6 indicates that this value works well. Contrary to the Swift et al. comment, the fact that the porosity is not increased in the Bredehoeft model does not impact the results significantly. Still it should be remembered that this is at best an ad-hoc method for representing hydrofracs.

This comparison suggests that Bredehoeft (1997) was probably justified in using his model to simulate hydraulic fracture in the scenarios he analyzed associated with WIPP. A better model of the hydrofrac process would couple the flow model with LEFM. We conceptualized such a model, however we did not feel it was necessary for this report. In this report we use 1) LEFM to model the hydraulic fracture, and 2) then calculate flow through this fracture using a flow model.
Figure 3.6 Comparison of the Bredehoeft Flow-Hydraulic Fracture model with the LEFM model.
In a LEFM model of a hydofrac, the extent of the fracture is purely a function of the quantity of brine injected, as shown above—see Figures 3.4 and 3.5. It is relatively easy to compare the extent of a fracture computed by BRAGFLO with that computed by the LEFM model. In the “radial models” analysis, results were presented for the extent of fracturing for both Marker Beds 138 and 139 (Stoelzel and Swift, 1997, Figure 16, p 45). Also included is the cumulative quantity of fluid injected (Stoelzel and Swift, 1997, Figure 8 and 9, p. 38). The results are compared to the LEFM model in Table 3.3:

<table>
<thead>
<tr>
<th>Radial Model R8—MB 138</th>
<th>Volume Injected (m³)</th>
<th>BRAGFLOW frac radius (m)</th>
<th>LEFM frac radius (m)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>1200</td>
<td>151</td>
<td>765 (calculated)</td>
</tr>
<tr>
<td>Radial Model R8—MB 139</td>
<td>1400</td>
<td>151</td>
<td>800+/-(Fig. 3.2)</td>
</tr>
</tbody>
</table>

**BRAGFLO when compared to the LEFM model underestimates the extent of the frac by a factor of 5.**

The appropriate fracture model has been a long standing issue within Sandia. PA uses the so-called Porosity-Model relationship, which is unsupported by experimental data. As shown above the Porosity Model underestimates the radius of the hydrofrac when compared to the established LEFM model. Through the years Sandia has numerous references to an Aperture Model that also fits the empirical data and gives quite different results. Larson and Davies (1993) in a memo to Martin Tierney point out there are two models to describe the permeability changes associated with hydraulic fractures, 1) the Porosity Model (now used by PA) and 2) the Aperture Model. The Larson-Davies memo appears to be the first place where the relationship between porosity and permeability is discussed. I quote their memo (Larson and Davies, 1993):

"The difference between the Aperture Model and the Porosity Model is the degree of permeability change associated with the interbed dilation. A graphical comparison of permeability versus porosity was prepared to illustrate this difference in behavior under different assumptions about critical input parameters (Figure 2—Larson and Davies memo). As shown in Figure 2, the Aperture Model has a rapid increase in permeability once the fracture dilation begins regardless of the number of active fractures, whereas the Porosity Model has a gradual and nearly linear increase in permeability. The Porosity Model requires values of J of about 40 to attain permeabilities similar to the Aperture Model at a porosity of 0.02 if the initial porosity (matrix porosity) is 0.01. However, it is apparent from the graph that the two conceptual models are incompatible, i.e. no selection of parameters can make the shape of the porosity-permeability correlation in the Aperture Model look like that in the Porosity Model. Because of this difference, it is important to assess whether the effects of the difference warrant adopting a new correlation for use in Performance Assessment calculations.

Beauheim et. al. (1994) again point out that there are two alternative models to describe the permeability of fractured anhydrite: 1) the Porosity Model; 2) the Aperture Model. They argue both models can be made to fit the empirical data. They suggested additional experiments to distinguish between the competing models. The definitive experiments involved measuring the strain during hydraulic fracture experiments. These experiments were not conducted. As a consequence, the porosity model used by PA is unconfirmed by experimental results.

Freeze et al. (1995) discuss the alternative aperture model; they go on to state:
Because of the higher predicted permeabilities the aperture model will propagate fracture-altered properties away from the repository, and will likely increase gas migration distance (Freeze et al., 1995).

Larson (1997) in a memo to Lori Dotson gives Beauheim et al. (1994) Figure 14 as a reference for changes in the porosity-permeability model. Figure 14 (Beauheim et al., 1994) shows data less than an order of magnitude change in permeability with an 8 MPa change in confining pressure. This is a small change associated with a large change in pressure. The experimental data were for loading in compression which is not the same as increasing the pore fluid pressure.

As we examine the references that describe the experimental anhydrite hydrofrac data, neither Beauheim et al. (1993) nor Wawersik et al. (1997) address the issue of which of the proposed models—Porosity, or Aperture—better fit the empirical data. A problem with the hydrofrac experiments that were done is that they were conducted close to the repository where the influence of the mined opening impacted the results. Wawersik et al. (1997) estimated the fracture opening as approximately 3 mm. This small fracture was sufficient to explain the several orders of magnitude difference in permeability between the hydrofrac and the intact rock. Wawersik et al. (1997) refer to the applicability of LEFM to estimate both pressure and fracture distances in the experiments. In order to interpret the experiments one has to assume a geometry for the fracture. The fractures near the tunnels are probably different than they would be in the far field.

The Aperture Model, as described by Larson and Davies (1993), predicts a change in permeability ranging from 5 to 10 orders of magnitude with a small change in porosity. The major change in permeability occurs when the hydraulic fracture is created. Beauheim et al. (1993) in plotting the pressure during hydrofrac experiment shows that 1) the pressure increases rapidly in the interval to be fractured once pumping is initiated, 2) the pressure reaches a point where the hydrofrac occurs, and 3) once the fracture occurs the pressure drops down and stabilizes with continued pumping—the pressure is such to accommodate continued injection, maintain the fracture open, and extend it. This is a typical pressure plot for hydraulic fractures. The Aperture Model more nearly reflects the empirical data.

The best explanation for why the Porosity Model was chosen by PA seems to be expressed by Larson and Fewell in a memo to Chu (dated March 12, 1997). The explanation is twofold. We quote the following questions and answers from their memo:

Q. Why are there maximum porosity and permeability changes?
A. These are set to prevent the possibility of unphysical destabilizing values being calculated for fluid flow parameters in BRAGFLO.

Q. Why isn't a discrete-fracture model implemented in BRAGFLO?
R. The problem posed by dynamic fracturing of interbeds due to high gas pressure is not amenable to solution by models in existence.

Other work using linear fracture mechanics (Mendenhall and Gerstle, 1993; Gerstle, Mendenhall and Wawersik, 1996) indicates that some model like the Aperture Model is a more appropriate relationship to describe hydraulic fracturing associated with WIPP. As mentioned above, using a flow model to approximate the hydrofrac process is at best an ad-hoc procedure.

A Peer Review group reviewed the porosity fracture model used in PA and gave it their approval, presumably based upon a review of the data. However, two facts were missing from the review:

1. The experiments in which deformation was measured during fracturing were proposed but never conducted. There is no data on changes in porosity during hydrofracing.
2. There was not comparison between the BRAGFLO predictions of fracture radius versus another model—LEFM, for example. As we show above, BRAGFLO greatly underestimates the radius of fracturing.
The Sandia empirical field data does not distinguish between the Porosity Model and the Aperture Model. The critical experiments to distinguish between the models were not done. PA has chosen to use the Porosity Model. When compared to a LEFM model, the Porosity Model implemented in BRAGFLO produces much shorter hydraulic fractures—by a factor of 5, as indicated above. BRAGFLO as implemented is inappropriate for accurately estimating the extent of hydraulic fractures associated with WIP. Using a flow model to approximate the hydrofrac process is at best an ad-hoc procedure subject to numerous errors.

Stoelzel and Swift (1997)

Stoelzel and Swift (1997) using BRAGFLO calculated the radius of hydraulic fractures associated with a leaking well. Their assumptions are non-conservative. They assume the borehole leaks near the bottom. The leaking fluid then goes into the annular space between the casing and the rock wall of the hole. They assume a uniform permeability for this space, and allow fluid to permeate all of the marker beds simultaneously. By assuming uniform borehole permeability and contact with all the marker beds they minimize the radial extent of fracturing. Boreholes in failure typically have higher permeabilities at the point of failure than elsewhere. Failure tends to focus the leaking fluid.

A more conservative approach is to assume that the fluid contacts one marker bed only. This will produce the maximum radius of fracture, the most dangerous case for WIPP.

As suggested above, we question that BRAFLO adequately represents the hydrofrac process.
Chapter 4  Flow to WIPP at Low Pressure

Introduction

In the Bredehoeft March Report (Bredehoeft, 1997) one scenario identified for concern was a hydrofrac that extended from an injection well just outside the WIPP land withdrawal boundary into WIPP. In this scenario WIPP was at low pressure. Figure 4.1 is a schematic diagram of the hydraulic fracture. The question was posed—what might happen?

Figure 4.1  Schematic diagram of a hydrofrac extending to WIPP.
The Pulsing Scenario

Bredehoeft (1997) postulated a pulsing flow to WIPP. The mechanism suggested was: 1) a hydrofrac would extend into WIPP, 2) flow would occur from the fracture into the repository, 3) the flow would lower the pressure in the fracture and it would close, and 4) the process would repeat. The process would continue to repeat as long as injection continued.

Bredehoeft modeled this process. The model generated pulses of flow just as was hypothesized. The period between pulses varied as one might expect with such a highly non-linear model. The model demonstrated elements of chaotic behavior. Whether the model created chaotic behavior in the classic sense of chaos theory was unclear.

Pulsing flow appears to be a possibility; we see no reason to change the earlier analysis (Bredehoeft, 1997). Nevertheless, pulsing flow is not the only possibility.

Initial Break Through into WIPP

A hydrofrac to encounter WIPP from outside the land withdrawal boundary needs to extend approximately 3.33 km (2 mi) from the well to WIPP. The size of the fracture depends upon whether the injection well is at the center of the fracture or out near the edge. Experiments suggest that an injection well location at the center or out toward one edge of the fracture is equally likely. In either case the fracture will contain a large quantity of brine.

In the case where the injection well is at the center of a circular fracture, the radius of the fracture will be approximately 3.33 km. If the injection well is near the edge of the fracture the radius of the fracture might be as small as 1.7 km. LEFM indicates that the brine volume in these fractures will range from a low 15,000 to a high of 95,000 m$^3$. WIPP mined openings are expected to consolidate quickly after filling; the pore volume within the filled and closed repository is approximately 50,000 m$^3$.

Most of the brine in the crack will flow into WIPP once the hydrofrac breaks through. The aperture of the fracture is such that the equivalent permeability of the fracture is high; there is almost no head loss due to flow in the fracture. While the physics of what happens on breakthrough into WIPP are not totally clear, one can anticipate a major amount of the brine in the crack to flow out and into WIPP. Depending upon the geometry of the fracture, the volumes are such that approximately one pore volume of brine can be quickly delivered as the hydrofrac encounters WIPP.

Continued Flow after Initial Breakthrough

The question arises as to what happens after the initial breakthrough. As suggested above, the initial breakthrough will place a substantial quantity of brine into WIPP; it may well fill the repository. There may be sufficient MgO to tie up some of the water chemically by forming the mineral Brucite. Our analysis (Bredehoeft and Hall, 1996) indicated the MgO probably could remove approximately one pore volume of brine (≈ 50,000 m$^3$). However, once the capacity of the MgO is overwhelmed by more than one pore volume of brine, then a large quantity of free water will be in the repository. As the repository fills the pressure will rise both from internal gas generation and from the pressure of the injected water. As injection continues the pressure in the repository will tend toward the pressure of the injected fluid.

The anhydrite hydrofrac experiments in WIPP suggested that a hydrofrac, once it is generated, does not fully close (Beauheim et al., 1994, Wawersik et al., 1997). Approximately one third of the injected fluid remained in the fracture after their experiment. This indicates that a higher permeability pathway is created by the hydrofrac. The experiments also indicated that with pressure differentials of 1 MPa flow rates following hydrofracing were over two orders of magnitude higher than the prefrc rates.
Even if the pressure drops in a hydrofrac that extends into WIPP, flow will continue in the higher permeability pathway created by the fracture. As long as injection into the Marker Bed continues we can expect flow to WIPP. As WIPP fills and pressure becomes lithostatic the hydrofrac will tend to continue to extend outward. The hydrofrac will extend beyond WIPP; this leads to the two-well scenario described in Chapter 6.

Conclusions

A hydrofrac extending to WIPP from a nearby injection well will:

1. Place a large volume of brine into the repository simply from the amount of brine stored in the fracture (15,000 to 95,000 m$^3$). This water may be of sufficient volume to totally flood the repository.

2. Continued injection into the Marker Bed will continue to flow water into the repository through the increased permeability pathway associated with the initial fracture whether the pressure is sufficiently high to hold the fracture open or is lower.

Whether the flow system pulses as postulated by Bredehoeft (1997) is academic. A significant quantity of brine will flow to the repository under this scenario.

If injection continues, WIPP will fill with brine, the pressure will rise to lithostatic, and the fracture will continue to extend outward beyond WIPP. Brine in the repository will lead to gas generation, tending to further increase the pressure.
CHAPTER 5 WIPP at High Pressure

As alluded to above, a different scenario occurs when the pressure in WIPP is at, or near, lithostatic. To those unfamiliar with WIPP, such high pore pressures may seem unusual. However, gas generation within the repository can raise the pressure. Figure 5.1 is a plot of pressure versus time for a set of realizations from PA. It shows a number of realizations in which the pressure was at or near lithostatic.

Pressure in Undisturbed Repository

Figure 5.1. Pressure in WIPP (from WIPP PA).
At high pressure, a leaking injection well can create a hydraulic fracture that will extend into the repository and remain open. Such a well can produce flow that will move a substantial amount of brine into WIPP. We model this scenario by 1) creating a hydraulic fracture between the leaking well and WIPP using LEFM, and 2) examining the flow that might occur should this happen using a flow model. We have made a few changes in the model from the Bredehoeft (1997) March Report. These changes are made to address some of the criticisms by Swift et al. (1997). Conceptually the model is similar to that proposed earlier.

The Model

We conceptualize the model in the following manner:

1. We start with the region with virgin anhydrite permeability—\( \approx 10^{-19} \text{ m}^2 \).

2. In creating the hydraulic fracture we use LEFM to define the region fractured. In this instance we will assume that the injection well is off center, nearer one edge of the fracture, and that the fracture has a diameter of 6 km which extends to include WIPP. We give the fracture the permeability observed for the Hartman #2 Bates well—\( \approx 3 \times 10^{-12} \text{ m}^2 \) (3 x \( 10^{-5} \text{ m/sec} \)). An alternative procedure would be to use the cubic fracture flow law to calculate an equivalent permeability for the fracture from the aperture predicted by the LEFM model. Use of this alternative procedure indicates an even higher equivalent permeability for the fracture.

3. We hold the pore pressure in WIPP at lithostatic—\( \approx 14.7 \text{ MPa} \). (We realize that as flow occurs to WIPP the pore pressure within the repository will increase. Our intent is not to predict the pressure in WIPP, but rather to see how much flow might occur through a hydrofrac.)

4. We hold the well head injection pressure at 9.7 MPa (1410 psi); (This translates to a pressure approximately 2 MPa above lithostatic at the depth of WIPP.)

5. For the flow calculations we use a storage coefficient of \( 10^5 \). (Swift et al (1997) criticized Bredehoeft's March Report for using steady flow for the analysis.)

The model assumptions are summarized as:

<table>
<thead>
<tr>
<th>Table 5.1</th>
<th>Model Assumptions</th>
</tr>
</thead>
<tbody>
<tr>
<td>Fracture Pressure Well @ WIPP depth</td>
<td></td>
</tr>
<tr>
<td>Hydraulic Conductivity K</td>
<td></td>
</tr>
<tr>
<td>Specific Storage S</td>
<td></td>
</tr>
<tr>
<td>3 x ( 10^{-5} \text{ m/sec} )</td>
<td></td>
</tr>
<tr>
<td>10^5</td>
<td></td>
</tr>
<tr>
<td>Well Head</td>
<td></td>
</tr>
<tr>
<td>WIPP Pressure</td>
<td></td>
</tr>
<tr>
<td>Fracture Initiation</td>
<td></td>
</tr>
<tr>
<td>Fracture Domain</td>
<td></td>
</tr>
<tr>
<td>9.7 MPa</td>
<td></td>
</tr>
<tr>
<td>14.7 MPa (lithostatic)</td>
<td></td>
</tr>
<tr>
<td>14.7 MPa (20 bars above far field)</td>
<td></td>
</tr>
<tr>
<td>6 km (diameter)</td>
<td></td>
</tr>
</tbody>
</table>

With these assumptions we model the system.
Model Results

The model suggests significant flow to WIPP; the results are not significantly changed from Bredehoeft (1997):

Table 5.2. Flow to WIPP—repository at lithostatic pressure.

<table>
<thead>
<tr>
<th>Distance from WIPP</th>
<th>Well-head pressure (psi)</th>
<th>Permeability</th>
<th>Flow rate</th>
</tr>
</thead>
<tbody>
<tr>
<td>Model</td>
<td>3.4 km</td>
<td>1410</td>
<td>$3 \times 10^{-12} \ (3 \times 10^{-4} \text{ m/sec})$</td>
</tr>
</tbody>
</table>

Figure 5.2 is a plot of the head distribution associated with this model after one year of injection. Within the domain of the fracture the flow has reached steady-state, outside the fracture the flow continues to equilibrate to the far field pressure boundary. However, the flow outside the domain of the fracture is insignificant.

Figure 5.3 is a plot of the flow to WIPP versus time. Starting with a flow field with no gradient at time zero the flow reaches a steady state in approximately 150 days. The rate is large, approximately 75,000 m³/yr. Since WIPP, once consolidation takes place, has a pore space of approximately 50,000 m³, this is a significant quantity of flow.

Once WIPP fills, the hydrofrac will continue to extend outward from the repository. This leads to the scenario described in Chapter 6.

Concluding Remarks

This analysis indicates that 1) if the repository were near lithostatic pressure, and 2) at the same time an injection well some miles away leaked in such a way that pressure above lithostatic were imposed on Marker Bed 139, a high permeability hydraulic fracture could connect between the well and WIPP. The pressure everywhere would be at lithostatic, or above, and the fracture would remain open. In this instance a steady flow field is created in approximately $\frac{1}{2}$ year, and it is maintained as long as the well continues leaking, or until the pressure in WIPP builds up to approximately that at the injection well. In the process WIPP will receive a large inflow of brine. If the injection continues the frac will continue to extend beyond the repository.

This is one of the Hartman type scenarios that poses a significant problem. This scenario is much like drilling into a Castile brine reservoir in terms of its impact on WIPP. Of course, this scenario does not occur until the pressure within the repository builds up to near lithostatic, which in the undisturbed state may not occur until 1000 years after closure, or longer.
Figure 5.2.  Head distribution after one year of injection. On this plot the area that is hydraulic fracture is clearly shown.
Figure 5.3 Flow to WIPP as a function of time. The flow becomes essentially steady-state in approximately 150 days.
Chapter 6  The Two-Well Scenario

Introduction

Bredehoeft (1997) identified a two-well scenario that has the potential to move a plume of contamination outward from WIPP across the regulatory boundary. In this scenario a well is injecting at pressures above lithostatic on one side of WIPP. On the other side of the repository is a poorly plugged well open to marker beds associated with the repository. The scenario is: 1) the injection well leaks and causes hydraulic fracturing of a marker bed associated with WIPP, 2) the repository fills with brine and comes to an ambient pressure approaching that of the injection well, 3) the hydraulic fracture continues to grow outward and reaches an unplugged well on the opposite side of WIPP, and 4) a coupled two-well, in-out, flow field is established.

This flow field, with injection on one side of WIPP and flow out on the other side, creates flow through the repository. This flow through the repository moves wastes to the regulatory boundary. Transport occurs quickly.

In this analysis, similar to Bredehoeft (1997), we assume that WIPP is passive. It fills with brine and reaches a pressure dictated by the two wells. Flow occurs through the marker bed, is diverted through the repository, and back into the marker bed. This occurs because the repository and the disturbed rock zone (DRZ) provide a high permeability pathway for fluids where it exists. Contaminants in solution in the repository are moved out and transported downstream in the marker bed.

This scenario will quickly move contaminants to the land withdrawal boundary with approximately the same concentration as the brine within WIPP. The concentration at the boundary is entirely dependent upon the solubility of the waste in the repository brine. Sorption in the marker bed can only slow the transport to the boundary. Since we are assuming flow in Marker Bed 139 which is approximately 1 m thick, it seems conservative to assume no dispersion and retardation. With a 10,000 year time frame the retardation associated with sorption, unless it is very large, does not matter so long as the injection continues for some period. The ultimate volume of contaminant reaching the boundary depends upon how long the injection persists.

Two-Well Model

We modeled this scenario, including the contaminant transport. We make a number of assumptions; these are summarized in Table 6.1:
21 August, 1997

Table 6.1 Model Assumptions

<table>
<thead>
<tr>
<th></th>
<th>Fracture</th>
<th>Far Field</th>
<th>WIPP</th>
<th>Well</th>
</tr>
</thead>
<tbody>
<tr>
<td>Hydraulic Conductivity $K$ (equivalent permeability of crack)</td>
<td>$3 \times 10^{-3}$ m/sec</td>
<td>0</td>
<td>same as fracture</td>
<td></td>
</tr>
<tr>
<td>Specific Storage $S_s$</td>
<td>0</td>
<td>(steady flow)</td>
<td>0</td>
<td></td>
</tr>
<tr>
<td>Porosity</td>
<td>0.04</td>
<td>(this value indicated in the CCA for hydrofraced anhydrite)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Pressure</td>
<td>14.7 MPa (lithostatic)</td>
<td>ambient</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Well Flow Rates</td>
<td>model result ($2.3 \times 10^{-3}$ m$^3$/sec; in = out, steady flow)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Fracture Domain</td>
<td>8 km diameter</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Dispersion Coefficient</td>
<td>0</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Sorption$'$</td>
<td>0</td>
<td>(no retardation)</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

With pressures at both wells specified (inflow & outflow wells) we adjusted the flow rate of the outflow well until the head remains above lithostatic throughout the flow domain. The resulting flow rate is approximately that determined for the scenario in Chapter 5.

Swift et al. (1997) commented that one could not maintain a lithostatic pressure in the Marker Bed at the outflow well. This is not true; one simply has to have a head loss in the well equal to the difference between 1) hydrostatic head at the depth of the marker bed, and 2) lithostatic head at this point. A well with an equivalent open diameter between 1 and 2 inches provides the necessary head loss to maintain the bottom of the well at lithostatic pressure.

WIPP Contaminant Concentration

The WIPP Sensitivity Analysis (CCA Appendix SA) contains plots of contaminant concentration, for a full set of PA realizations, in both Salado and Castile brine with magnesium oxide backfill. In the first 2000 years, typical concentrations for the set of PA realizations ranged from $10^3$ to $10^3$ EPA Units/m$^3$. Figure 10.1 is the plot of contaminant concentration in WIPP in EPA Units versus time. For the purposes of this analyses we use a concentration of $10^3$ EPA Units/m$^3$—at the low end of the typical PA calculations.
Figure 6.1 Radionuclide concentration in EPA Units/m³ in WIPP with MgO backfill (from WIPP Appendix SA).
Model Results

For the transport calculations we use the code, JDB-MOC, a method of characteristics code developed for PCs (Bredehoeft, 1994). This code is based upon an earlier code MOC (Konikow and Bredehoeft, 1978).

Figure 6.2 is a plot of the hydraulic head showing the two wells. Flow is 1) in at the injection well, 2) through the fractured marker bed, 3) through WIPP, 4) back into the marker bed, and 5) to the outflow well. WIPP is positioned between the two wells. The repository is passive; its pressure is dictated by the flow field between the two wells. As flow moves through WIPP, contaminants are moved by the flow field.

Figure 6.3 is a plot of contaminant concentration near the outflow well; concentration is expressed as a percent of the concentration in WIPP. One sees the first arrival of the contaminants in approximately 7.5 years, that is followed by a build-up in concentrations. Concentrations at the outflow well are reduced by the radial flow mixing of uncontaminated with contaminated brine at the well.

The system is modeled without either dispersion or sorption in the marker bed. The marker bed is only one meter thick; both effects may be limited in such a thin bed, especially since it is hydraulically fractured. Dispersion will spread the plume reducing the maximum concentrations; the total mass transported is the same; it is simply spread out by dispersion. Sorption creates retardation, the plume moves slower than the fluid velocity predicts. With sorption, the plume still gets there; it moves more slowly.

Figure 6.4 is an isometric projection of the plume. Again, the concentration is plotted as a percentage of the concentration in WIPP. The plume is as wide as the repository, approximately 750 m. One can see that a substantial volume of contaminated brine crosses the regulatory boundary.

Figure 6.5 is a plot of the integrated releases from WIPP, in EPA units, at differing distances. One must integrate the mass of contaminate to produce Figure 6.5. This plot indicates when the transport of contaminants would violate the EPA standard. The modeling suggests that 50 EPA Units cross the regulatory boundary in approximately 13 years.

Concluding Remarks—Back of the Envelope Check

The transport associated with this scenario is so large that we wish to check the model with a simple calculation. The velocity of flow is given by Darcy's Law:

\[ v = (K/n) \frac{\partial h}{\partial l} \]

where
- \( v \) is the velocity of flow;
- \( K \) is the hydraulic conductivity;
- \( n \) is the porosity;
- \( \frac{\partial h}{\partial l} \) is the hydraulic gradient.

The flux of fluid is given as:

\[ Q = v n \]

To obtain the flow into or out of the WIPP, we integrate the flux in the marker bed either upstream or downstream from the repository. Since the flow field is simple and one dimensional in the vicinity of the repository, the flux through WIPP is easily derived:

\[ Q_{\text{total thru WIPP}} = Q * W_{\text{normal to flow}} * H_{\text{marker bed}} \]

where
- \( W \) is the width of WIPP normal to the flow field; and
H is the height of the maker bed.

We can substitute numbers in this equation:
\[
\begin{align*}
v &= 0.8 \times 10^{-3} \text{ m/sec}; \\
n &= 0.04 \\
W &= 750 \text{ m} \\
H &= 1 \text{ m}
\end{align*}
\]

therefore:
\[
Q = 2.4 \times 10^{-4} \text{ m}^3/\text{sec}; \text{ or } 21 \text{ m}^3/\text{day}; \text{ or } 7600 \text{ m}^3/\text{year}.
\]

The flow through the repository is approximately 10% of the well flow. At this rate one repository pore volume flows through WIPP in 6.6 years; this flow has the potential to totally replace the fluid in the repository (the repository after consolidation has a pore volume of approximately 50,000 m³). One pore volume of repository brine with a concentration of contaminants of \(10^3\) EPA Units/m³ contains 50 EPA units; in this scenario this dissolved waste is transported to the land withdrawal boundary. (One EPA Unit of contaminant, with a probability of 10%, violates the EPA licensing criteria.) It is easy to see that this scenario poses problems for WIPP.

It is hard to predict the concentration of contaminants in the repository in a scenario in which it is rapidly flushed. With rapid flushing the contaminant concentrations will be limited by the solution reaction kinetics. We have not extended the release plots beyond the initial 50 EPA Units in solution for this reason.

![Figure 6.2](image)

**Figure 6.2**  Head distribution for the two-well scenario. The area hydrofraced area is apparent on this figure.
Figure 6.3 Plot of contaminant concentration at differing distances from WIPP. Concentrations are plotted as a percent of the concentration in WIPP.
Figure 6.4  Isometric projection of the contaminant plume in the two-well scenario.
Figure 6.5  Plot of the contaminant mass released from WIPP. The contaminant concentration in WIPP is $10^3$ EPA Units/m$^3$ for this result. The three curves should converge with time; the spread in the curves at later times is some measure of the model error.
Chapter 7 Conclusions

The Hartman #2 Bates well Blowout

Our review of the data continues to indicate that the blowout of the Hartman #2 Bates well is best explained as a hydrofrac in the lower Salado Formation that extends from the Texaco Rhodes-Yates Waterflow to the well. This is the consensus of most investigators that examined the empirical data, including us.

*DOE and Sandia dismiss the interpretation that a hydrofrac happened at the #2 Bates well.*

LEFM versus BRAGFLO

Linear Elastic Fracture Mechanics (LEFM) is a widely used and accepted model for fractures including hydrofracs. BRAGFLO when compared to LEFM underestimates fracture radius by a factor of 5 times. Our review of the Sandia data does not present sufficient information to select the “porosity model” used in BRAGFLO over the “aperture model”. Numerous internal documents indicate Sandia’s concern about this problem. *BRAGFLO in its current implementation is an inadequate model to predict the extent of hydraulic fractures.*

Stoetzel and Swift (1997) use BRAGFLO to compute hydrofrac radius. Their analysis is non-conservative in that they:
1. assume uniform permeability in the entire annular space between the borehole rock wall and the casing; and
2. allow fluids to hydrofrac all the marker beds simultaneously.

*These assumptions minimize the hydrofrac radius; they do not represent what happened at the Hartman #2 Bates well.*

Scenarios

We conclude that three well injection scenarios described in Bredehoeft (1997) still pose problems; these are:

1. hydrofrac extends from a leaking injection well into WIPP at low pressure;
2. hydrofrac extends from a leaking injection well into WIPP when it is at lithostatic pressure;
3. hydrofrac extends through WIPP and encounters a poorly plugged well that leaks upward.

*Scenario number 3 has the highest negative consequences. It shows that the containment criteria are significantly violated in a few years.*
References Cited


21 August, 1997


References, WPO 39830, 11 p.


