40-8027/C-12

UNITED STATES ATOMIC ENERGY COMMISSION

IN THE MATTER OF:

KERR-McGEE CORPORATION

Docket No. SUB-1010

Place - Washington, D. C.

Date - Tuesday, 16 October 1973 Pages 217 - 317

Telephone:
(Code 202) 547-6222

ACE - FEDERAL REPORTERS, INC.

Official Reporters

415 Second Street, N.E.
Washington, D. C. 20002

CR 2803
Haines
Ro

UNITED STATES OF AMERICA

ATOMIC ENERGY COMMISSION

- - - - - - - - - - - - - - - - X
:
In the matter of: :
: Docket No. SUB-1010
KERR-McGEE CORPORATION :
:
- - - - - - - - - - - - - - - - X

Courtroom No. 1
U. S. Tax Court
1111 Constitution Avenue, N.W.
Washington, D. C.

Tuesday, 16 October 1973

The hearing in the above-entitled matter was reconvened, pursuant to adjournment, at 9:00 a.m.

BEFORE:

JOHN FARMAKIDES, Chairman, Atomic Safety and Licensing Board

LESTER KORNBLITH, Member

DR. DALE BABCOCK, Member

APPEARANCES:

(As heretofore noted.)

Ro

C O N T E N T S

E X H I B I T S

P R O C E E D I N G S

CHAIRMAN FARMAKIDES: We will begin.

May we have the same panel back on the witness stand?

Whereupon,

JOHN S. RODGERS, WILLIAM J. SHELLEY, H. K. VAN POOLLEN, AND H. J. GRUY

resumed the stand and, having been previously duly sworn, were examined and testified further as follows:

CHAIRMAN FARMAKIDES: Mr. Kinsey, I think you were interrogating yesterday. I think you said you had one last question that you might or might not pose.

You may proceed, sir?

MR. KINSEY: May I expand on my one question, sir?

XXXXXX

CROSS-EXAMINATION (Continued)

MR. KINSEY: Mr. Shelley, getting back to your typical analysis, Figure 1, I believe you indicated yesterday that 340 times 10^{-8} microcuries is the average or a typical concentration in the waste stream.

With an application granted, what limit, what upper limit, would you propose on radium concentration in the raffinate stream, assuming there was the need for some standard?

MR. SHELLEY: We believe that we ought to adhere to the 2,100 because of the factors I explained or described

yesterday in the absence of some standard in a relation that might be proposed in the future. This is not being released to unrestricted use, or we start with that presumption, and we feel we ought to hold to the higher limit.

MR. KINSEY: In other words, you would set the 2.1 times 10^{-5} as the upper limit above which the radium concentration would not go?

MR. SHELLEY: Based upon what I know now.

MR. KINSEY: In the 1972 application, as I recall the word "storage" is used as opposed to disposal; am I correct?

MR. SHELLEY: I believe so.

MR. KINSEY: Does the word "storage" to you connote retrievability?

MR. SHELLEY: It certainly suggest it. Yes, I would say that it certainly suggests it.

MR. KINSEY: Do you at any time envision retrieving?

MR. SHELLEY: In the absence of a demonstrated accident, no.

MR. KINSEY: In the event of a demonstrated accident, how would you propose to retrieve the waste water already injected?

MR. SHELLEY: As the geologists have suggested, we would backflow the well until the loss of fluid stopped.

DR. BABCOCK: What do you mean by "backflow"?

MR. SHELLEY: If I understand their terminology correctly, sir, they merely take the pressure off the well, the pressure that has developed, change the connections on the pumps and remove the fluid until loss of fluid from the reservoir ceases.

DR. BABCOCK: The pressure where?

MR. SHELLEY: At the wellhead.

DR. BABCOCK: You are taking the pressure at the surface?

MR. SHELLEY: Right.

DR. BABCOCK: And letting it flow out to the surface?

MR. SHELLEY: Right, back into the pumping system which you have reversed.

DR. BABCOCK: The normal water level is not now at the surface?

MR. SHELLEY: That is true.

DR. BABCOCK: Therefore you would not be taking the pressure completely off.

MR. SHELLEY: You would not be taking the pressure completely off unless you pumped it down.

DR. BABCOCK: Right.

MR. SHELLEY: Which I understand you could, and it may be necessary. At least you would remove fluid until the indicated loss of fluid from the reservoir ceased.

MR. VAN POOLLEN: To me the term "backflow" is not limited to just merely taking off pressure at the surface. It just means you reverse the flow, take it out.

DR. BABCOCK: As distinguished from putting a second well in a mile away and pumping a fluid into that second well and washing it out? That is not what you are talking about by "backflow"?

MR. VAN POOLLEN: Backflow as we use the term in the petroleum business on our water injection, where we inject water for a certain length of time and then we say let's backflow the well for a while to clean it out.

MR. KINSEY: Mr. Shelley, along this line of questioning feel free to consult any other members of the panel as necessary.

Would you envision any instance where it would require a pumping out of the entire amount which had been injected?

MR. SHELLEY: As you know, I am not -- I have no experience with reservoir engineering. I have never heard of an instance requiring that measure of retrieval.

CHAIRMAN FARMAKIDES: Excuse me, Mr. Shelley.

Anyone on the panel can answer the question.

MR. VAN POOLLEN: Could the question be restated, please?

MR. KINSEY: Can you envision any instance where

it would be necessary to pump out everything that had been pumped down?

MR. VAN POOLLEN: No, I cannot envision that.

MR. RODGERS: No.

MR. KINSEY: Would it be possible to bring back up everything that had gone down?

MR. GRUY: It would be possible to get a large part of it but it wouldn't be possible to get absolutely all of it because some of it is mixed. Some of it wets the sand grains and if you wanted to go to the expense, you could produce a lot of the Arbuckle water that was partially diluted with it but you could never get the last trace of it.

MR. KINSEY: In other words, to get all of it you would have to pump out a lot more than you put down.

MR. GRUY: Right.

DR. BABCOCK: Could I interrupt just one second? Let's suppose that you pumped out as large a volume of water as you pumped in. What would be your expectation as to recovery of the material of the salts that you put in?

MR. GRUY: 85 percent?

DR. BABCOCK: Thank you.

MR. SHELLEY: May I add something there, sir. When you say "the salts you put in," as I stated yesterday, the uranium and thorium is going to precipitate with the neutralization of the acid solution. From what I understand of the

mechanism of this precipitation in that media, I would not expect to get any of the uranium or thorium back.

MR. VAN POOLLEN: My answer circumvented that question. I was just assuming that it was all the same. If there has been a reaction and some has grouped out, of course, you wouldn't get that.

DR. BABCOCK: I thank you for your addition, Mr. Shelley. It is very helpful.

CHAIRMAN FARMAKIDES: Mr. Shelley, I am not clear on this. Your supplement to Mr. Gruy's answer to Dr. Babcock's question -- I think you stated there would be precipitation. I assume from what you said that the precipitation would occur in the well.

MR. SHELLEY: Yes, sir.

CHAIRMAN FARMAKIDES: Why in the well?

MR. GRUY: In the formation.

CHAIRMAN FARMAKIDES: Why in the formation?

MR. SHELLEY: The mechanism is that it is now dissolved because of the highly acidic state of the raffinate as produced. The acid concentration is approximately 1 molar. A 1 molar concentration is a pH at zero, which is a stronger acid than most folks are exposed to, ever.

CHAIRMAN FARMAKIDES: What causes the precipitation?

MR. SHELLEY: When that degree of acid hits the dolomite in the formation rock, it will produce a mixture

probably of calcium nitrate and calcium bicarbonate.

CHAIRMAN FARMAKIDES: It is the reaction to the dolomite?

MR. SHELLEY: Right. That removes the acid thus permitting uranium and thorium to precipitate probably as hydrous oxide.

CHAIRMAN FARMAKIDES: Excuse me, Mr. Kinsey.

MR. KINSEY: Mr. Shelley, in the event your application is granted for deep well disposal, what would you intend to do with raffinate retention ponds which are presently in place?

MR. SHELLEY: As I say in the monitoring section in my written testimony, following the advice of Mr. Cruy and Dr. van Poollen, we would use about 3 million gallons of the neutralized raffinate to confirm the reservoir limits.

As you know, we have been using some waste -- some available heat to warm the raffinate ponds and secure a degree of evaporation. In addition, we are installing a submerged combustion burner for the purpose of evaporation.

Now, you add this all together and assume that these five sequences are done, let's say, arbitrarily next appear. At that point you have probably got -- oh, at most 2 to 5 million gallons left in those raffinate ponds. You would stop adding because you are now using the well for the acid raffinate.

What we do -- I would have to suppose a specific condition of each pond. Is it uniformly distributed? Is one pond down lower than the other so that the solid that has precipitated is available and drying? Is next spring going to be as wet as this spring was in Oklahoma?

At some point we would permit this material in the pond -- it is essentially some fluid and a lot of precipitate and there is some sand in there and some calcium precipitates. We would deliberately take it to dryness in some manner and remove -- well, when I say "to dryness," it is not going to dryness. It is going to be a wet cake and we would remove it and examine it for radionuclides and, if possible, bury it on the site. If not, either arrange for commercial burial or return it to our mill for addition -- for rework and eventually recycle back to use, probably.

MR. KINSEY: Would you, then, say, fill in the ponds?

MR. SHELLEY: No, sir.

MR. KINSEY: The ponds would still be in place?

MR. SHELLEY: Yes, sir. In the event that something stalled out on the well, we could always go back to using the ponds.

MR. KINSEY: In your own estimate, what is the expected plant longest for the Sequoyah facility?

MR. SHELLEY: I answer to that question, in the environmental report we put in 30 years, to the year 2000.

It is problematical. The plant itself can operate longer. It depends upon the market and the availability and the need, I think.

MR. KINSEY: How long do you propose to monitor in the manner set forth in your testimony?

MR. SHELLEY: I don't think we have covered that with these gentlemen here.

MR. GRUY: You would have to monitor as long as you inject it.

MR. KINSEY: Not after?

MR. GRUY: If you quit injecting, you would still monitor, yes.

MR. KINSEY: Are we talking about monitoring the well for the half life of the radium 226, which is 1,600 years?

MR. GRUY: I don't think so. I think you would want to monitor for several years after you quit injecting to periodically test what your pressure condition is in your reservoir and inspect the surface.

MR. KINSEY: Is there any way with certainty that you could walk away from the well in 40 or 45 years and be assured that there might not be a later escape in 200 or 300 years?

MR. GRUY: Certainly you can walk away from the pits and be sure in 200 or 300 years it wouldn't be somewhere, either -- there is no way you could do that.

CHAIRMAN FARMAKIDES: Why is that in view of the fact you are going to precipitate out most of the radionuclide?

MR. SHELLEY: We are only precipitating the uranium and thorium, not the radium.

MR. KINSEY: I would like an answer to my earlier question, Mr. Gruy.

MR. GRUY: I realize I answered your question indirectly. This has been -- this area is no doubt not within the circulatory pattern of the groundwater with this high salinity. It is 50 miles updip in this formation before it comes to the surface, or more. Then from here it goes down-dip and it doesn't come to the surface until up in the Arbuckle Mountains. This area is surrounded by faults and we think that we have proved and will continue to prove with the monitoring that it is isolated there.

I don't think there is any way for it to get out except that the pressure be built up in there high enough to rupture it. By monitoring while you are injecting and after you quit injecting, you could assure yourself that you haven't ruptured that tank. I think it would be safer than it would be anyplace else in the world.

End #1

lmil

MR. KINSEY: Mr. Shelley, does Kerr-McGee own all the proposed disposal formation, or have control of all the acreage which is over the proposed disposal formation?

MR. SHELLEY: It has control over the aerial extent to which we expect the raffinate to progress. For instance, to the north boundary and to the east one and well out toward the west one. We do not own the land or the entire fault block.

MR. KINSEY: Such that another company could come in and drill a well and conceivably inject something into a hole?

MR. SHELLEY: Yes, sir.

MR. KINSEY: Could this not conceivably build up pressure within the formation, additional pressure?

MR. GRUY: If somebody injected into the ground in this fault block, it would build up additional pressure. Of course, we would object very strongly if somebody asked for a permit to inject into this fault block.

MR. KINSEY: Supposing they were given a permit and allowed to inject. What effect could that possibly have on the waste water in the Kerr-McGee?

MR. GRUY: Well, it would build up the pressure more rapidly than this injection would build it up and it would force the waste water injected in the Kerr-McGee well back from the -- there would be an interference from the two

2mil

injection points and it would hold the progress of that front back along the side toward the well.

MR. KINSEY: Could that increase in pressure lead to a possible significant build-up such that it might rupture or fracture that fault?

MR. GRUY: It would be directly proportional to the amount injected.

MR. KINSEY: In other words, you are saying if additional pressure were built up through some other source, conceivably a fault could be fractured within that block?

MR. GRUY: Conceivably it could be fractured if pressure was built up from any source. Another well would certainly increase the rate at which the pressure would be built up and increase the danger, there is no question about that.

MR. KINSEY: I just have, I think, one more series of questions. On page 4 of your testimony, the first full paragraph, the first sentence. I think you have indicated before -- you state that initially a program to confirm the model would be conducted. Then on page 5 you say -- which is the last sentence in the incomplete paragraph on the top of the page -- "in the vicinity of significant mismatch of actual and model predictions additional consultation would be arranged to involve the ongoing program." At this point if such a mismatch occurred, would you cease pumping operations?

3mil

MR. SHELLEY: I would expect so. That would depend upon the type of mismatch.

MR. KINSEY: What would you do in the event that the initial testing program was completely mismatched to the model, in the event such an occurrence happened?

MR. SHELLEY: We don't even believe that is within the realm of credibility. If our understanding of what these two people can do with this information is correct, this sequence of injections will meet their need for additional information to build a model that fits the reservoir exactly, more exactly. Then when we switch to the acid injection, as I said yesterday, you are going to change that reservoir.

MR. KINSEY: Could such confirmation of the model be had without injection of raffinate? What I am saying is, couldn't you inject a substitute for the raffinate and get a clear confirmation on the model?

MR. SHELLEY: As you know, the proposal to inject pond raffinate is almost -- we are cleaning up the formation water. We are diluting the radium concentration in the formation. Of course, you could use straight water, river water. I suppose you could use oil.

MR. VAN POOLLEN: It would be the same.

MR. KINSEY: One final question, again going back to the upper limit which we discussed for the typical radium concentration, which would be 2.1 times 10 to the minus 5.

4mil

In light of the upper limit and in light of, I think, you believe that a conservative approach need be taken here, would you not consider Dr. Sternhagen's analysis of the dilution in either the Arkansas or the Illinois River? Do you not believe that should be based on an upper limit as opposed to the typical other limit if a conservative approach were to be used?

MR. SHELLEY: I hadn't given that any thought. I would expect to monitor it. I think the upper limit would be restricted to a situation that I do not now foresee. As I am sure you know, the future pattern of the source of feed materials for a conversion plant is completely one of crystal ball gazing. Sooner or later, people inform us -- the AEC has made studies. At some point, we are going to have to import uranium. The quality of milling carried on in Australia or South Africa or other places is problemmatical. From what we have seen of domestic mills and the mills in Canada, we will never approach that upper limit. I do not know how to answer your question.

MR. KINSEY: I think you have answered it sufficiently.

Mr. Chairman, I have no further questions.

CHAIRMAN FARMAKIDES: Thank you, Mr. Kinsey.

Dr. Babcock has a series of questions.

DR. BABCOCK: Mr. Gruy, I would like to go over with

5mil

you the mechanism whereby you concluded that there were no leakage -- that there was no leakage through the various boundaries that you described. I am not interested in great detail. Would you please give the major steps along the line by which you reached that conclusion.

MR. GRUY: As you know, we built a mathematical model of this reservoir with properties assigned to each tiny block over the thing with impermeable blocks put at various distances --

DR. BABCOCK: This was your first assumption, that the blocks were impermeable?

MR. GRUY: At a certain position. Then after determining the position of those impermeable blocks that most nearly matched the conditions that we had observed, the inflow and the back flow and the fall-off in pressure, we then assigned those limiting blocks for permeability and found that when we assigned them permeability we didn't get as good a match. Now, with .01 of a thousandth of a darcy, we got a pretty close match. When we came to .1 of a thousandth of a darcy, there was a significant difference in the pressure. That is the basis on which we say that these boundaries are not only impediments, but they are seals.

DR. BABCOCK: And I believe that you showed in your graph that the points that you calculated after you had assumed a porosity always fell below the points that you were

6mil

calculating when you assumed no leakage?

MR. GRUY: Yes, sir.

DR. BABCOCK: Is that not an inevitable result?

MR. GRUY: Yes, because if there is leakage off, the pressure has to be low.

DR. BABCOCK: Now, then, let's leave that model and see if we can construct another route that you could have gone. As I understand your model, you went down the route of first assuming no leakage. Then you introduced leakage as a calculated parameter and found you got a poor fit?

MR. GRUY: Yes, sir.

DR. BABCOCK: Let's go the other way around. Let's first assume that the boundary has a leakage. You then get your best fit by moving the boundaries to a new -- to wherever they are. Then having gotten the best fit, you could have then assumed, I believe, that the boundaries had no leakage and gotten an answer in the opposite direction and would then either have proved or disproved that there had been a leakage. Did you do the alternate route?

MR. GRUY: We did not.

DR. BABCOCK: Would that have been an acceptable route to go down?

MR. GRUY: Well, we -- the truth of the matter is that faults are nearly always sealing. They are not always sealing and you can't assume that they are. But most, or a large

7mil

part, of the oil field traps that we work with are fault traps and have faults. these faults are sealers to gas, gas being, of course, more fugacious. So I think the mere fact that we are showing the kind of defects we are showing in the curve that we presented, or a change in flow shows that there is not flow through that. Now, I can see your point, but that is not the way that we operate because of the fact that the faults are most generally sealing.

DR. BABCOCK: I accept your answer that that is not the way you went. Would it have been an acceptable way to get the answer, whether there was leakage or not leakage?

MR. GRUY: I never have tried it that way. I don't know what problems we would run into.

DR. BABCOCK: Mr. Rodgers?

MR. RODGERS: I might be able to offer something that is helpful in regard to the approach of determining boundaries and determining whether the boundaries were leaking or not in regard to your question. That is this. The sequence of events are interrelated. That is to say, the radioactive tracer survey data were conducted and this gives our first key or clue in regard to the counterflow that is mentioned in our testimony as to an abnormal pressuring of the lower layer. Now, this indicated to us that this was layer five and it is a limited layer, because of the counterflow evidence. Then in the sequence of events we next started examining our early time

8mil

pressure data as we show on Exhibit G-5 and trying to detect evidence of the nearest impediment reflection which would have to occur through the most permeable layer, number five, then at that point this was our fulcrum point or our anchor point in pursuing the model analyses in regard to boundary description. In other words, this gave us our first boundary to anchor on, which is the nearest boundary in layer number five, and then we proceeded from that point to add additional no-flow or impervious boundaries in layer five and then accordingly in the other layers.

The logic of the solution to the problem was such that we did consider the impervious condition in order to match in with what we saw in the counterflow data on radioactive tracer surveys.

CHAIRMAN FARMAKIDES: What you just said, sir, wouldn't that just as well have supported the question posed by Dr. Babcock, that your first assumption could have been that you assumed there was leakage rather than assuming there was no leakage? What you just said, Mr. Rodgers, I think would support that.

MR. VAN POOLLEN: Yes, because he moves the boundaries back in. So you started off by having leakage in one layer.

CHAIRMAN FARMAKIDES: That's right.

MR. VAN POOLLEN: So you brought it back?

MR. RODGERS: Yes.

9mil

CHAIRMAN FARMAKIDES: Excuse me, Dr. Babcock.

DR. BABCOCK: Go ahead. It makes no difference.

MR. GRUY: Actually to some extent we did what he says.

MR. VAN POOLLEN: That is what we did.

MR. RODGERS: That's right. I understand your point. That's correct. I was thinking more in terms of the layer number five, which is very small and limited in area as indicated.

MR. GRUY: First we assumed they were all leaking and then we moved the boundaries in.

DR. BABCOCK: Could we now turn to Exhibit A, Mr. Gruy's report of May 1, 1972. I would like to speak with reference to Figure 14. I will consider in this next question only the points which are labeled "X," which are the calculated best fit for impermeable boundary. I will also speak about the points that are labeled with triangles and are the calculated east boundary with a leakage that is related to the number 0.1. A little while ago we established that the triangle fits, which are the points related to leakage, of necessity must always be below the "X" fixes and we find that they are always related. Isn't the inevitable result that we automatically get a poorer fit to the measured data by that method?

MR. GRUY: That's right.

10mil

DR. BABCOCK: Now, then, how can we say it is not the logic of your argument that inevitably, the way you calculated it, you are going to get zero leakage. That was the assumption you started out with and you merely confirmed it.

MR. GRUY: No, sir. You see, the actual pressures are higher than we checked.

DR. BABCOCK: Say that again.

MR. GRUY: The thing that stands out here is that any leakage has got to make these pressures lower than they would have been without the leakage. Our actual measured pressures, the circles, are always higher.

DR. BABCOCK: Not always.

MR. GRUY: Well, they are higher in this poor fit end down here.

DR. BABCOCK: They are higher in one end and lower in the other. You said they were always.

MR. GRUY: I mean in the late time region -- you wouldn't have a boundary leak in the early time region when it is right close to the well. You wouldn't have a boundary leak until the pressure moves out to your boundaries. So you would be looking for a leak out here. Well, we couldn't match this late time thing, but anything we do to put a leak on it makes it worse.

DR. BABCOCK: Inevitably.

MR. GRUY: Right.

DR. BABCOCK: All right. Let me take a different line of attack.

When you established your original boundaries you assumed a boundary. You then made a calculation. You found that the calculated result was not an acceptable one. You adjusted the boundaries. You got a new calculated result. You kept on adjusting your boundaries until you got the best fit boundary location.

MR. GRUY: Yes.

DR. BABCOCK: All right. Now, then, when you put a new parameter in the situation you were calculating, which was leakage, why did you not then search for a best fit the next time?

You have already said that inevitably you were going to get a poorer result. Why didn't you then search for a best fit?

MR. GRUY: Because it appeared to us that anything -- that any leakage is going to make the pressures lower. We already can't calculate them as high as they are.

DR. BABCOCK: But we know how to adjust that. You merely adjust your boundaries.

Mr. Rodgers?

MR. RODGERS: If I may add, Dr. Babcock, the problem that revealed itself in the procedure which we took was that we did not find total closure of all boundaries. In

Layers 3 and 4 I think the best we could say during the test time was that the diffusivity allowed us to -- at that point we realized we had a system in which we had almost total closure, but we couldn't substantiate in Layers 3 to 4.

I believe we still had an open direction insofar as the pressure analysis was concerned. So we were in kind of a situation where we would desire additional -- testing additional transient pressure time as we recommended or planned to do in the monitoring program during the initial phases and further substantiate the boundary conditions.

DR. BABCOCK: I don't know whether this is quite along the line you are going down or not, but I want to inject another question and I believe it is along the line you are speaking about.

After you had gotten your best fit, assuming no leakage, do you then retain those boundaries in their identical position?

You then assume leakage and still retain those boundaries where they were. Had you adjusted those boundaries to some new best fit, would you not have gotten a better fit than the triangles that you indicate on Graph 14?

MR. RODGERS: I don't believe we would have, frankly. There are two things which will influence our fit and that is the pore volume and pore volume distribution

from an aerial aspect.

The other influencing factor on our fitting technique or fitting procedure is the shape of the reservoir system or the shape of each layer with respect to the well location.

Based on -- quite frankly, based on our pursuance of fit, we did find that we were getting into a very sensitive realm of boundary adjustments as we do discussing this test.

In other words, we find that some of these boundaries could not be manipulated over large distances in trying to obtain the fit.

We get into a very narrow distance range of possible boundary positions in the fitting.

DR. BABCOCK: I take it that this is your statement as a professional in the field, that adjusting the boundaries for the triangle points in the case of leakage would not give a better fit than the fit that you have gotten there?

MR. GRUY: That is our opinion.

DR. BABCOCK: Could I ask other technical experts here the same thing?

Mr. van Poollen, do you agree with that?

MR. VAN POOLLEN: Yes, I agree with that. You could move the boundaries some, but the concept is there. There are sealing boundaries. If you make them leak you will

not get the same response.

MR. KORNBLITH: Mr. Gruy, I would like to ask you a question or two. You mentioned a few minutes ago in response to one of Dr. Babcock's questions that you would expect the effects of leakage to show up during the latter part of the shut-in curve.

Why would you not expect them to show up early?

MR. GRUY: Because early you haven't established any pressure difference at your boundaries.

MR. KORNBLITH: You have a larger difference, I would think, because this is a shut-in curve.

MR. GRUY: Well, but your difference -- you see, you have a transient situation. You build up a pressure rate at your wellhead. Then that pressure moves out. I have maps in here that shows how much the pressure has increased at various distances from the well at times.

Please refer to Figure 5.

MR. KORNBLITH: This is in this report?

MR. GRUY: Yes. The pressure distribution and injected fluid front after five years in Layer Number 1. These pressures are at 2650 feet.

MR. KORNBLITH: What was the situation immediately before this run that is shown on the shutdown measurements? Was not the thing under static conditions or had you just stopped pumping?

MR. GRUY: We had been pumping into it so that we had built up an elevated pressure right at the well.

MR. KORNBLITH: How long had you stopped pumping when you started these measurements?

MR. GRUY: We started to measure immediately to get the way the pressure falls off as it, of course, moves out into the reservoir.

MR. KORNBLITH: And you say that whether or not it was leaking at the boundary would not affect the pressure immediately and it would take some hours or days before the effect of this would show up?

MR. GRUY: Yes, down in your later time. Your initial part of the curve reflects conditions right around your well bore.

MR. KORNBLITH: But certainly at the initial stages you have higher pressures at the boundary than you have later on?

MR. GRUY: No.

MR. KORNBLITH: No?

MR. GRUY: No, because the pressures don't move that fast. We are not in a tank. We are in this -- this thing is a very tightly packed sand body and that pressure moves out slowly.

MR. KORNBLITH: So if you are pumping on a well and then you shut it in, after you shut it in the pressure of

the boundary continues to rise before it starts to fall?

MR. GRUY: Right. It will never fall. It will continue to rise, slower and slower and slower, until it averages out all over, but the pressure to boundary won't fall unless the it leaks.

MR. KORNBLITH: Now, let me change areas a little bit.

Dr. Babcock originally asked you to describe your procedure for demonstrating that the boundary didn't leak and you described this curve-fotting process. Doesn't that refer only to that east boundary? Maybe I have missed something in your explanation. How do you tie down the impermeability of the other boundaries?

MR. GRUY: Well, this applies to all of them. We made all boundaries permeable at the same time within this model so that any time the pressure was built up inside of the -- at the boundary, if there was permeability there there would be an outgrowth of fluid.

Now, this -- at this time this is all really complicated, but when we once get the pressure out to all the boundaries and we filled our tank, we get out of what is called the unsteady state thing into a semisteady state sort of situation where you will have a material balance.

Each barrel of injected fluid is going to run up the pressure in the same amount and you will begin to get --

(Demonstrating on the blackboard) -- well, you have a sytem of some length, a distance here, and the well in the middle. If we have pressure increasing this way and distance down here from your well, then we say this is the static pressure.

When you begin to inject you raise the pressure here at the well and around the well.

Now, when you stop injecting, this drops. This moves out. T his drops and moves out and finally you end up with a little higher pressure than you had before. But if you wait long enough, it will be uniformly over the whole tank.

CHAIRMAN FARMAKIDES: In other words, Dr. Gruy, you have indicated on the board here that what we have got as a system is a cup sitting on its base and then in the middle you have got a well coming in with a cone extending from that -- from the bottom of the well, which would be the point of the well closest to the base of the cup. That cone would become more shallow as your pressure drops.

MR. GRUY: Right, just like if you had some jello that sank down with time to fill the whole pan that it was in. That is the way the pressure moves out.

Now, then, after we have effected all of these boundaries, when we start to plot we shut down and we calculate this average pressure from the way this falls off. Then you will have a -- if you make a plot of cumulative injection versus pressure, now this plot has got to be a

straight line rising in this manner, and the amount that the pressure rises per unit of injection will be a function of the size of this tank and its volume.

If this curve begins to do this, falling off, it is leaking.

CHAIRMAN FARMAKIDES: Please articulate that so that the record can pick it up. That is what I was trying to do previously.

MR. GRUY: Once you have pressured all the boundaries so that your fluid is no longer escaping into the wild blue yonder but it is confined and you have a confined system, then your average pressure will go up as a function of injection at a constant amount.

In other words, you will have a pound pressure increase per "X" number of barrels injected, in your average pressure in your reservoir. That will be constant. The only way that that can vary from that is for something to happen to the size of your reservoir. If it begins to leak your pressure per barrel injected won't go up as much because you haven't increased the volume in your tank that much. So it will be completely obvious that the leakage occurred.

MR. KORNBLITH: I think we are getting a little away from the topic that I started out to ask about.

MR. GRUY: I thought that was your question. I am

sorry.

MR. KORNBLITH: You say the leakage does not show up until late in the shutdown time versus pressure curve. How late is late? Is it a function of how far away the boundary is?

MR. GRUY: Yes, sir.

MR. KORNBLITH: So if there was leakage of the boundary 5000 feet away would it show up later than leakage at an 1100 foot distant boundary?

MR. GRUY: Yes, sir.

MR. KORNBLITH: Would it show up if it were at 5000 feet on this curve at all or would it be someplace out way beyond the end of the page?

MR. GRUY: This is 150 hours. We were effecting some 29,000 feet on this test. But if there was a leakage close by the profile would be lower.

MR. KORNBLITH: You say that you learned from this experiment that you could detect a leakage equivalent to a tenth of a millidarcy but not one equivalent to a hundredth of a millidarcy.

MR. GRUY: Yes, sir.

MR. KORNBLITH: That applies, then, only to that nearest boundary. The sensitivity would be lower, would it not, at more distant boundaries?

MR. GRUY: Yes, but in the model we have put the

permeability on all boundaries when we ran the thing and anyplace -- there would be some leakage all along as soon as the pressure began to increase.

MR. KORNBLITH: But at the east boundary, which is the one at 1100-odd feet, if it were impermeable and there were leakage at one of the more distant boundaries, what would you know about it from the runs that you have made so far?

Would you know -- would you still be able to say that it couldn't be in excess of a tenth of a millidarcy?

MR. GRUY: It would take more time, more injection history, to absolutely make that statement.

MR. KORNBLITH: So your statement about how tight the boundary is really only applies to that first boundary?

MR. GRUY: It applies more strongly to the closest boundary than to the more distant boundaries, yes, sir.

MR. KORNBLITH: I don't quite understand the "more strongly" adjective. Your sensitivity, taking it from this test, is lower at the more distant boundary; is that correct?

MR. GRUY: You are getting me into things that are so complicated that there is not any way I can explain them or even understand them myself because all I can tell you is how it happened in the model.

We put the permeability in all of them. Now, where

the permeability was in there, more of the leakage occurred at the closest boundary because the pressure difference was greater. But there was some leakage occurring at the more distant boundaries because there was some pressure difference.

When there is some pressure difference and some permeability there will be some flow, but most of the flow occurred at the closest boundary.

MR. KORNBLITH: That is because you assumed equal permeability at all the boundaries.

MR. GRUY: Right.

MR. KORNBLITH: Did you make any runs where you assumed the nearest boundary was impermeable and look at the effects of varying the permeability of the others?

MR. GRUY: No.

CHAIRMAN FARMAKIDES: Let's recess for ten minutes.

(Recess.)

end3
2803

lmil

CHAIRMAN FARMAKIDES: We will proceed with our line of questioning. I want the Applicant to understand that the Board is trying to develop an understanding of the Applicant's analysis of the configuration of the reservoir. I think it would be most helpful if, for example, the answers to the questions are more direct. I would appreciate that. We haven't said anything in the past, but I would appreciate if a question is asked that the response be directly to the point. It would save us all a lot of time. I think Dr. Babcock had some questions he wished to ask now.

DR. BABCOCK: This line of questioning is going to be slightly different from the last line, but related. I would like now to turn to Figure 13 of the May 1972 report, which has as its description Exhibit A. I believe it is Exhibit A of the transmittal letter.

MR. IRVINE: I am sorry, Dr. Babcock, I didn't understand the reference to Exhibit A.

DR. BABCOCK: It is Exhibit A that was attached to the transmittal letter.

MR. IRVINE: Thank you, sir.

DR. BABCOCK: My questions are relating to the line that extends vertically from the point labelled 1.0 at the bottom of the photograph. What is the significance of the points that are on that line?

MR. GRUY: Those are the points, starting at the

2mil

bottom, at 115 hours. The next one at 65 hours. The next one at 5 hours. The top one at .23 hours. They are calculated in the model for the permeabilities that we used in the model for our determination.

DR. BABCOCK: In other words, this line represents the actual measured points?

MR. GRUY: No, sir, there are no measured points on Figure 13.

DR. BABCOCK: It represents your best fit to the measured points?

MR. GRUY: The best fit to the measured points are the .01.

DR. BABCOCK: Let's take the point at about 1207, which is 4.23 hours. Can you show me where that point came from?

MR. GRUY: The only points on Figure 13 are at the hours I read.

DR. BABCOCK: Can't you find a place in your report that you can get that number for me?

MR. GRUY: I thought you were referring to Figure 13.

DR. BABCOCK: I want you to find that figure at some other place in the report.

MR. GRUY: I am sorry, sir, but I do not understand the question.

3mil

DR. BABCOCK: Is it 1267 pressure point as given on Figure 13 found someplace else in the report?

MR. GRUY: I don't see a 1267 pressure point on Figure 13.

DR. BABCOCK: 1267. Well, it is approximately 1267, on the .23 hour curve.

MR. GRUY: The .23 hour curve at one. You want to find that point somewhere else?

DR. BABCOCK: Yes, please.

MR. GRUY: If you go to Figure 2, it is above the points that we could plot.

DR. BABCOCK: All right, let's take the next point, for five hours.

MR. GRUY: A little over 1262. That is the point right under the 1263 line, or the fourth circle coming down from the top left. No, the fifth circle.

MR. KORNBLITH: These should correspond with Xs rather than circles, shouldn't they?

MR. GRUY: That's right.

DR. BABCOCK: Will you change the record, please.

MR. GRUY: It would be the fifth X.

DR. BABCOCK: All right, let's take the next point just below the 1262.

MR. GRUY: The 65 hour point?

DR. BABCOCK: Right.

4mil

MR. GRUY: That is the point where the X is shown at 65 hours on Figure 2.

DR. BABCOCK: Would you read that number, please.

MR. GRUY: Well, I can read it a little bit less than 1253.

DR. BABCOCK: And what did you read it on the graph on Figure 13?

MR. GRUY: A little bit less than 1252.

DR. BABCOCK: Is there a discrepancy there?

MR. GRUY: Yes, it would appear that there is a little bit of a plotting error there, yes.

DR. BABCOCK: All right, let's go down to the next point, 1247.8, we will say. Would you find that point on Graph 2, Figure 2?

MR. GRUY: That would be the point a little under 1249. This may be on a different run because I am sure we didn't misplot that much.

DR. BABCOCK: The first points that you were telling me were from this same run. Have you changed runs in between?

MR. GRUY: Let me see. John, this Figure 2, is that the same runs these are supposed to have been taken from? We have got a calculated value of 1249. There is some difference in the runs here because I know we didn't misplot the things that much. These were runs we made for permeability

5mil

variations in layers three and four on the west side at distances of greater than 1164 feet from the injection point. These are not -- this is not a comparable thing. This Figure 13 was put in to show the effect of changing the permeabilities.

DR. BABCOCK: But the --

MR. GRUY: But apparently we have not used as the one, the final and best fit numbers here. The curve does show that you have a greater effect from reducing the permeability from what was used than you do from increasing the permeability from what was used.

DR. BABCOCK: I believe in your text you drew rather important conclusions from the fact that these lines were all essentially similar in form?

MR. GRUY: Yes, sir.

DR. BABCOCK If you plot the curves for 115 hours and 65 hours through the points that are established other places in the report, you do not get the same shaped curve?

MR. GRUY: Well, these points were for the same series of runs with this difference, whereas it might not have been the same runs that were plotted on these other graphs.

DR. BABCOCK: Can you find these points you have plotted in this set of data?

MR. GRUY: Well, I haven't looked and I don't imagine that they are in here because if they were in here, they would be the ones I was looking at and obviously they are

6mil

not.

CHAIRMAN FARMAKIDES: Do you still have confidence that Figure 13 has value?

MR. GRUY: Yes, sir.

CHAIRMAN FARMAKIDES: In what way?

MR. GRUY: In showing the effect of changing the permeability at distances greater than 1164 feet away from the injection point.

CHAIRMAN FARMAKIDES: What is your data supporting Figure 13? Where is it?

MR. GRUY: We would have to get our runs. Do you have those runs with you, John?

MR. RODGERS: No, I don't.

MR. GRUY: We made a great number of runs and we made one run with permeabilities at one and then we increased the permeability to show the effect on the pressures at this time and then we decreased the permeabilities at these distances to show the effect.

CHAIRMAN FARMAKIDES: You don't have such evidence right now, sir?

MR. GRUY: I don't have such evidence with me.

DR. BABCOCK: I am now talking with relation to Figure 2 of the Gruy report of May 1, 1972. I am going to ask questions concerning the circled points which the legend indicates are the measured data. Has Kerr-McGee given evidence

7mil

as to what the actual measured data were that these points came from? In other words, would it be found in Exhibit D of the transmittal letter which is entitled, "Sperry Sun Precision Pressure Gauge Report"?

MR. GRUY: Yes, sir.

DR. BABCOCK: I will pick one point and see if you can find that point for me in that report. Let's take the point at about 125 hours. Or it might be easier to find the point at 145 hours. Either point.

MR. GRUY: At 145 hours here, we show slightly over 1249 pounds.

DR. BABCOCK: That is on Figure 2, yes, sir. Now, would you find 1249 for me in the report that I just spoke of?

MR. GRUY: Mr. Rodgers tells me that there was a run at 75 hours and a recalibration made and they didn't make a new report, so that that is not -- that number is not in the report submitted. I was not aware of that.

DR. BABCOCK: That is a surprising statement to me. I thought that we had been given the correct data.

MR. GRUY: I thought so, too.

MR. VAN POOLLEN: May I try to help out?

DR. BABCOCK: Yes, sir.

MR. VAN POOLLEN: This is a very rough looking graph.

8mil

CHAIRMAN FARMAKIDES: What are you pointing to, sir, for the record?

MR. VAN POOLLEN: This is a draft showing the pressure as a function of time on one of my worksheets. During the early --

CHAIRMAN FARMAKIDES: Excuse me, Dr. van Poollen. Could you identify it further? Is that in the evidence?

MR. VAN POOLLEN: No, sir.

CHAIRMAN FARMAKIDES: Do we have that in the record?

MR. VAN POOLLEN: This is not in the record.

CHAIRMAN FARMAKIDES: Do you intend to put that in the record?

MR. VAN POOLLEN: May I ask --

CHAIRMAN FARMAKIDES: Let's understand one thing. It is the job of this Board to make a decision here and we want the details. We want the facts. We want both sides to put on the facts of their case. Now, you just can't make general statements and then expect us to simply accept those without some data. If you intend to support Figure 2, Dr. van Poollen, were those worksheets then you have to discuss that with your attorney as to whether you want that worksheet put into the record, sir? If that does support it, I suggest to you that it might be very valuable.

(Discussion off the record.)

9mil

MR. VAN POOLLEN: My only apology is that it looks so bad.

CHAIRMAN FARMAKIDES: If he can read it, sir, that is good enough. If your thought processes are on that paper, that is fine with me.

MR. VAN POOLLEN: All right.

MR. IRVINE: We will have copies made and submit it.

CHAIRMAN FARMAKIDES: Would you want to introduce that as part of your record, sir, and would you want to show it to counsel, Mr. Kinsey? We can cite it as Applicant's Exhibit 4.

MR. IRVINE: Yes, sir.

MR. KINSEY: We have no objection.

CHAIRMAN FARMAKIDES: No objection?

MR. KINSEY: No, sir.

CHAIRMAN FARMAKIDES: You are offering it into evidence, Mr. Irvine, and we will accept it into evidence as Applicant's Exhibit 4.

(The document referred to was marked Applicant's Exhibit 4, for identification, and was received in evidence.)

xxx

MR. VAN POOLLEN: On this Exhibit 4, I have plotted the pressure versus the log of time function. This shows the raw data that was in the Sperry Sun report which is

10mil

an attachment to an exhibit.

MR. GRUY: Well, now, John tells me that the Sperry Sun corrected that report and never gave us the correction to Kerr-McGee and the old uncorrected one got in the record. Now, in your study, I don't know if you got the right one or not.

MR. VAN POOLLEN: This is the uncorrected.

DR. BABCOCK: Isn't this a little unusual, to have this --

MR. GRUY: It is entirely a surprise to me.

DR. BABCOCK: It is a surprise to me.

MR. GRUY: It is entirely a surprise to me. I thought the basic data was in the record.

MR. VAN POOLLEN: I have the same data that Dr. Babcock has on this graph.

CHAIRMAN FARMAKIDES: We will be very pleased to have that, sir. We want a complete record and if that can assist us, fine. If you have data supporting the --

MR. GRUY: You do not have the corrected data with you?

CHAIRMAN FARMAKIDES: The problem, Dr. van Poollen, is that the data that you used is not the corrected data, as I understand Mr. Gruy.

MR. VAN POOLLEN: I want to point out that I think it is.

CHAIRMAN FARMAKIDES: You think it might be?

11mil

MR. VAN POOLLEN: Yes.

CHAIRMAN FARMAKIDES: Perhaps we should recess for 10 minutes and I think the panel should discuss this among themselves and come in with the position of the Applicant as to what is the correct data. We will recess for 10 minutes.

(Recess.)

CHAIRMAN FARMAKIDES: Mr. Irvine, can we proceed?

MR. IRVINE: Yes, sir.

Mr. Chairman, at this time we would prefer to withdraw Exhibit No. 4. I think that Dr. van Poollen will be able to explain because the exhibit itself has his rough notes and has so many other things on it that it would tend to be more confusing than helpful at this time. At least we are going to attempt to do it this way.

CHAIRMAN FARMAKIDES: You know it has been introduced into evidence. Is there any objection from the Staff to having it withdrawn?

MR. KINSEY: We would have no objection to their withdrawing it.

CHAIRMAN FARMAKIDES: We would like to see it before we consent to withdrawal.

(Document handed to Board.)

(Discussion off the record.)

CHAIRMAN FARMAKIDES: We have had a short bench conference and I think what we will do is defer ruling on withdrawing Applicant's Exhibit 4 at this time pending an explanation by Dr. van Poollen which could be articulated into the record and also, Mr. Irvine, I understand that Dr. van Poollen will be in a position to give us a graph of the main points involved.

MR. IRVINE: Yes, sir.

CHAIRMAN FARMAKIDES: All right, Dr. van Poollen.

MR. VAN POOLLEN: On Exhibit 4 --

MR. IRVINE: Dr. van Poollen, without reference to Exhibit 4 at this time, can you articulate what you have found and then it can be determined whether Exhibit 4 will be helpful or not?

MR. VAN POOLLEN: I have made a plot of pressure versus a time function and I have noticed that during two different gauge fronts --

CHAIRMAN FARMAKIDES: I assume that time is the most important variable.

MR. VAN POOLLEN: That is correct.

CHAIRMAN FARMAKIDES: What else is in that function?

MR. VAN POOLLEN: The time function is the injection time plus the shut-in time, that quantity divided by the shut-in time. Consequently, the overriding factor is the shut-in time.

On this plot, I can see that the trends of the two gauge runs were identical. However, when the gauge was rerun an apparent discrepancy of 2.2 pounds occurred. I consequently adjusted the pressures downward to the second gauge run which I assume was done in it.

CHAIRMAN FARMAKIDES: That was your assumption, sir?

MR. VAN POOLLEN: Yes, sir.

CHAIRMAN FARMAKIDES: Did you contact Dr. Gruy?

MR. VAN POOLLEN: I contacted -- that specific item I contacted Mr. Rodgers.

CHAIRMAN FARMAKIDES: Did Mr. Rodgers at that time tell you if that was a valid assumption?

MR. VAN POOLLEN: Yes. He told me that this was more than likely what they had done.

CHAIRMAN FARMAKIDES: But you have no other explanation for that assumption?

MR. VAN POOLLEN: No, sir. This is something that happens very often. As a matter of fact, all of the people in my organization, when they plot their data, have to mark where the gauge run was, when the new pressure gauge was run. At that time you frequently see funny things on your curves.

DR. BABCOCK: I don't believe that is shown on Figure 2.

MR. VAN POOLLEN: That is correct.

DR. BABCOCK: In other words, Figure 2, taken by itself, indicates that there was a continuous gauge reading, or I guess I should say gauge setting, starting from zero hours and ending at 145 hours.

MR. VAN POOLLEN: That is correct. However, the raw data was given to me and by plotting it I could see that this practical phenomenon was taking place again like it does so often.

CHAIRMAN FARMAKIDES: So your graph, then, differs from that of Figure 2?

MR. VAN POOLLEN: Not following adjustment.

DR. BABCOCK: After adjustment it is the same?

MR. VAN POOLLEN: That is correct, sir.

DR. BABCOCK: I note in the Sperry Sun exhibit that I was talking about a little while ago, each one of the runs is started with a calibration run. Should not this calibration run eliminate the effect that you speak of?

MR. VAN POOLLEN: Not necessarily because there is a very high precision instrument run on a wire and a tape that there could be some minor depth difference of where the gauge was run again.

For example, 2 pounds is approximately equal to 4 feet. I don't recall the exact depth at which we are running but it is in the order of a few thousand feet. It is conceivable to have a few feet of difference between runs.

DR. BABCOCK: They say on this chart that the psig is at 2,650.

MR. VAN POOLLEN: That is correct.

DR. BABCOCK: That would to me indicate that it is 2,650 plus or minus maybe a foot.

MR. VAN POOLLEN: I think there could be some more leeway than that.

CHAIRMAN FARMAKIDES: How much more?

MR. VAN POOLLEN: I think you could go up to 2 feet or 3 or 4 feet, if both gauges have that. That is what I was referring to earlier in my testimony, that the 5-1/2 pounds -- which, by the way, had I not corrected this number, would have been 7.7 pounds -- so I corrected it to be what I though was in ine. That is where I say that 3.4 or 5.5 pounds are very difficult between runs to accept as precise numbers.

DR. BABCOCK: I note in the calibration that is given in the Sperry Sun report, as I understand it, they got the same calibration forthe last gauge setting as they did for the gauge setting before.

Did I read the numbers correctly?

MR. VAN POOLLEN: I believe you did.

DR. BABCOCK: Why would we not find a difference in those two calibrations if there was an actual difference of 2 pounds per square inch in the numbers that were coming out?

MR. VAN POOLLEN: I am not sure I understand your question precisely.

DR. BABCOCK: Sperry Sun calibrated their gauges before the last run, we will say. They calibrated them before the next to the last run. The calibration data, as I understand it, are identical in the two cases.

MR. VAN POOLLEN: Yes, sir.

DR. BABCOCK: As reported in here.

MR. VAN POOLLEN: Yes, sir.

DR. BABCOCK: But you tell me that there is an actual difference in the readout of about 2 pounds per square inch. In other words, the readout is 2 pounds per square inch high in the last run.

MR. VAN POOLLEN: That is correct, but the calibration will still be the same. This calibration was run in the laboratory.

DR. BABCOCK: I didn't realize that.

MR. GRUY: Maybe the bond gets 2 feet deeper on the second run. We checked this with Sperry Sun and it is my understanding that they said they had made an error in that report of 2.222 pounds. We asked them to submit a corrected report to Kerr-McGee. We subtracted 2.222 pounds from each of the readings after 75 hours. I was not aware that the corrected report was not submitted and part of this record.

MR. VAN POOLLEN: I would like to state that I did not know that, either, but I did it personally after I made my graph and I found 2.2 difference and corrected it by 2.2, independent of any other information from Sperry Sun.

DR. BABCOCK: I found 2.2 pounds also.

I would like now to turn to page 7 of the May 1, 1972, yearly report. I am talking about the general subject of the validity of the model. It is reported here a column of figures which is entitled "Pressure by Analytic Solution,"

and another column of figures is "Pressure by Model Solution."

Would you say a few words to explain that difference that you are trying to bring out there, Mr. Gruy?

MR. GRUY: Well, the analytic solution is a solution by a continuous equation rather than using the model divided into blocks.

Now, I want to call your attention to a typographical error in the fourth column there. There should be a zero before -- between the decimal point and the 5 in the last three numbers on that column.

DR. BABCOCK: I noted that. Go ahead.

Do you have more explanation?

MR. GRUY: No. That is the difference in the solution.

DR. BABCOCK: The near identity of these two columns of figures suggests to me that you were using almost the identical equation for the one calculation as the person did in the other calculation. Is that correct?

MR. GRUY: No, sir. We used the same physical parameters but a different mathematical equation.

DR. BABCOCK: Was the difference only a finite difference method of calculation or was it a basic difference in the formula that you were using?

MR. GRUY: Well, the model solution is the finite difference method, treating each block in the others. The

other is a continuous --

DR. BABCOCK: What is the difference? If you use the finite difference method and make your finite differences very small, what is the difference between that and the continuous method?

MR. GRUY: This is just done to show that they were small enough and that it was checked.

DR. BABCOCK: In other words, this is the same basic equation that you are using.

MR. GRUY: Yes, the same basic reasoning.

DR. BABCOCK: In other words, differences between the true geological formation that we are talking about and the model that you have made would not show up in these two calculations at all.

MR. GRUY: No, sir.

DR. BABCOCK: Fine. In other words, you are merely proving here that the finite difference method used small enough increments so as to eliminate the errors caused by that method.

MR. GRUY: That is correct, sir.

DR. BABCOCK: I have a question on an entirely different area. I don't know who on the panel this should be properly addressed to so I will address it to you, Mr. Irvine, and you can reassign it if necessary.

MR. IRVINE: You may be sure that I will be glad

to reassign it.

CHAIRMAN FARMAKIDES: Or if you want to designate a chairman of the panel and then we will address it to the chairman and he can answer it or whoever else he would suggest.

MR. KORNBLITH: I was --

CHAIRMAN FARMAKIDES: Do you want to do that, sir? It might be easier since these people are all technically qualified and you are a lawyer as I am. Perhaps one of them can be better routed.

MR. IRVINE: I will be happy to advocate Mr. Shelley.

MR. KORNBLITH: Mr. Shelley, has any consideration been given to the possible effects of earthquakes on the reservoir, and, if so, what consideration? I don't recall seeing anything in the material concerning this matter.

MR. SHELLEY: We know, Mr. Kornblith, that the area in which the plant is located and this fault block is located is not a seismic-active area.

I am sorry I can't quote from memory -- I don't have it with me. This was described in our environmental report. It is a Zone 2, which is a very slight chance of earthquakes. If my memory is correct, I think it's a tremor every 10,000 years. The geologists tell me that this block has remained stable at the disposal horizon for a period of some 250 million years or more because of the lack of

ty 10

apparent dilution and general stability of this area. This is about the extent of the examination of this problem.

We feel that in the event of a quake that released fluids from these underground formations the fluids that are there are already to some degree containing radionuclides and they contain strong salt solutions. It would undoubtedly be hazardous unless diluted. We don't believe that the injection of raffinate in such a large reservoir would add measurably to this risk.

End #5

MR. KORNBLITH: There have been some cases -- and I am thinking particularly of a recent one in the Denver area -- where the injection of fluids has led to earthquakes. Is the situation here greatly different.

MR. SHELLEY: As I understand it, it is and perhaps some of the geologists that have made studies of this -- it is my understanding that the Denver injection was into igneous base rock, into fractures that were already existing.

I recollect that the explanation is that the liquids were injected under very high pressure. They were injected into these fissures or fractures and therefore they lubricated the movement or the relief of stress in these base rocks.

CHAIRMAN PARMAKIDES: Dr. Shelley, you may, of course, consult with other members of your panel or ask one of them to respond.

One of the reasons, sir, why we admitted your supplemental no. 3 which I think was objected to by the Staff was because we thought it was relevant evidence from a safety viewpoint and this is one of the areas we wanted to explore. What you have given us so far, sir, is not as helpful as if you could tell us where you discuss this, as Dr. Kornblith asks, where have you discussed it? You yourself are not a geologist so I would suggest you might want to ask one of the members of your panel to respond to Mr. Kornblith's question.

MR. VAN POOLLEN: I would be in a position to answer

that because I did a considerable amount of work on the Arsenal well where the earthquakes occurred and study that data in considerable detail.

MR. KORNBLITH: Please do.

MR. VAN POOLLEN: At the Arsenal well injections took place at the essentially very finite reservoir which could be shown in subsequent data. As a consequence pressures were increased rapidly to the point where it would start to extend certain fractures. Then once it started to extend these certain fractures -- and this was in igneous rock -- the pressure on the fracture plane increased and consequently movement could take place.

As a consequence we say tremor rather than earthquakes. It never has been of any serious nature except it was alarming to people that the continuation could conceivably result in a bad earthquake. Consequently it has been recommended by various people on the panels that worked on these -- and they were not all in perfect agreement -- that whenever a waste disposal project takes place that you should, as part of the monitoring from an environmental point of view, have seismic information.

If you find that the seismicity of the area tends to increase you stop immediately. So this answers, hopefully, the question on can you start an earthquake. This has been the only one that we know of that ever happened. There are more

bits of information available on the injection of fluids into reservoirs, and a cooperative effort took place between the people in Mendel Park and I think with the environmental agency -- I don't know the exact name -- that is part of the USGS.

And the Chevron Oil Company in Colorado have been injecting fluids and measuring the seismicity, very precise measurements, and they have observed that noises rather than even tremors occures as a result of injection. When they ceased injection then the noise disappeared.

MR. KORNBLITH: Are seismic measurements planned here?

MR. SHELLEY: We had not considered it to this point, sir. All of the information I had available from the Oklahoma Geological Society led me to believe that this area was much more stable than the area in Denver that had this experience. It certainly could be added to our monitoring program.

MR. KORNBLITH: I don't think the Board is recommending that you do it or don't do it. I am sure it is not the sort of thing one would want to make an instant decision on.

But the basis for this being different than the Denver situation is, as I understand it, two-fold. One, the injection pressures are much lower, and second, the geology is

very significantly different.

MR. VAN POOLLEN: Yes, sir.

MR. KORNBLITH: We have heard some discussion of the fact that these boundaries could rupture if the pressure were raised high enough. Can someone give me an order of magnitude of the pressure increase that might be expected to be necessary?

MR. GRUY: I think that something on the order of a thousand pounds would certainly be the range of hazard. We had proposed only to go up to 150 pounds in this three-year period.

MR. KORNBLITH: So at a thousand pounds your estimate that the rupture would take considerably longer than the estimated lifetime plan to be reached?

MR. GRUY: It would depend upon the injection rate but we certainly would not raise the pressure that high.

MR. KORNBLITH: The point was brought up this morning when Dr. Babcock was questioning about the effect of other people drilling other wells that go into this fault block. Has any consideration been given to whether this is really a potential problem and whether so, would be desirable to prevent --

MR. GRUY: It does not appear to be a potential problem because this fault block has been explored. It is not productive, probably because of the fact that it has been sealed

so long that oil or gas could not get into it. There is no reason to suspect that anyone would want to drill a well in here in search for oil or gas or any other known minerals.

There is no reason to suspect that any plant in the area is going to want to dispose in this same block. Certainly it could be prevented before the regulatory bodies if someone should request permission to do so.

MR. KORNBLITH: Do you happen to know who would have authority to permit it.

MR. SHELLEY: I will ask Mr. Irvine to confirm this, but I believe in the state of Oklahoma the --

MR. IRVINE: The Corporation Commission of Oklahoma has control of the disposal of materials, particularly salt water. That is oil and gas brines. Then of course the Oklahoma Water Resources Board would control anything that had to do -- anything that had to do with disposal of other matters that might affect surface waters, or I mean underlying fresh waters.

MR. KORNBLITH: So there are agencies that have appropriate authority to prevent this?

MR. IRVINE: Yes, sir.

CHAIRMAN FARMAKIDES: Does the Staff counsel -- did you confirm on that?

MR. KINSEY: We will accept the statement of Applicant's counsel.

CHAIRMAN FARMAKIDES: It is a little unusual to have the legal arm testify but we assume that this is correct.

DR. BABCOCK: I have one final question of Mr. Gruy. If you will turn to page 12 of the May 1, 1972 report. Near the bottom of the page I will read only one sentence but it may be that more than one sentence should be read for a complete understanding of it. That sentence is: "All calculated points in this case are from zero point four to zero point six pounds per square inch below those calculated without leakage."

Mr. Gruy, can you tell me what that means?

MR. GRUY: That means that with the leakage at one-tenth of a millidarcy you would calculate a lower pressure of point four to point six pounds lower than you calculated with no permeability at the barrier.

DR. BABCOCK: Now let's turn to figure 14. Let's take the 115-hour point. I believe the record is talking about the different between the point labeled "X" and the points labeled with a triangle, is that not correct?

MR. GRUY: Yes, sir.

DR. BABCOCK: What is the difference for the 115-hour point?

MR. GRUY: About half a pound.

DR. BABCOCK: Now let's turn to the 15-hour time. What is the difference there?

MR. GRUY: The 15-hour time?

DR. BABCOCK: Yes, sir.

MR. GRUY: That looks like it is a little in excess of a pound.

DR. BABCOCK: Maybe about a pound and a half?

MR. GRUY: Right.

DR. BABCOCK: In other words, that point is severely outside the zone that you said that all points were within?

MR. GRUY: Yes, sir, but we were considering the late-time region after the break in the curve. The statement is inaccurate as to meaning all. In the early-time region we did have this greater difference.

DR. BABCOCK: I infer that that that word "all" then is not correct.

MR. GRUY: Not correct, yes, sir.

DR. BABCOCK: Now you get down to the next sentence and it says to the 115-hour should the -- in time the standard deviation and so on. In other words, I infer that that you were completely happy with all points up to the 115-hour point.

MR. GRUY: Our calculation of the standard deviation at that point -- I don't know why that statement was made and why the 115-hour point was used. I think we stopped the run there and I don't know why. But the standard deviation between the curves was calcualted as it says there.

DR. BABCOCK: But just a little while ago you told me that this point 4 to point 6 was supposed to apply to the

jeri 8

late times but in the next sentence it says it applied more appropriately to the early times.

MR. GRUY: This next sentence just gives the facts of the standard deviation fit of the curves themselves.

DR. BABCOCK: But Mr. Gruy, the sentence above that says "All calculation points." Then you selected a range within the "All" calculated points to say "This is a better range" for some reason.

MR. GRUY: This is just a statement of how the curves fit and what the deviation of the curves was under the two conditions, the standard deviation of the two curves. I admit that the "all" shouldn't be there. It should say "all" in the late-time region.

DR. BABCOCK: I think that is all I have, thank you.

MR. KORNBLITH: Mr. Gruy, I refer you to your curve having -- 8 in your testimony, exhibit G-8. Looking at the points that you have plotted from your calculations, they all seem to follow a smooth curve which differs a little from the crosshatch-measured curve but nonetheless there is a smooth curve except for the 15- and 25-hour points. Is there some explanation for why a calculated curve that I suppose was calculated from some well-behaved function would behave like this?

MR. GRUY: That is some near-boundary effects that we could never figure out exactly what it was. We didn't work

on it long enough to know what that is. But the things that we did to try to bring that part of the curve further in line through the other part of the curve further out

MR. KORNBLITH: One other question. In one of your earlier curves, or I guess in your original submission, you talked about the standard deviation of your best fit curve being point 55 or point 75 psi different from the measured curve. I have always in the past seen standard deviations associated with numbers that are in some respect random whereas in this case there seems to be a systematic difference between the two curves in that one is lower in one region and higher in the other and they are both well-behaved curves.

What is the significance of using standard deviation in this respect?

MR. GRUY: Well, I think that the lower the standard deviation the better agreement you have overall and it is the method that we use. We attempt to get the standard deviation within the accuracy of the instrumentation if possible. If you have two solutions, both within the accuracy of the instrumentation, then you don't know which one is right.

DR. KORNBLITH: If you had wanted to you could have adjusted your curve so that the standard deviation in the early time period was much less at the sacrifice of the later time period or vice versa. Is there some reason for taking the standard deviation of the whol curve rather than the part that

of most interest in focusing on that?

MR. GRUY: Well, we think it is all important for different things. You see different things at different parts of it. We try to fit the whole curve to the lowest standard deviation.

CHAIRMAN FARMAKIDES: Do you have redirect, Mr. Irvine?

MR. IRVINE: No, sir, I have none.

CHAIRMAN FARMAKIDES: Gentlemen, thank you very much. You have been most helpful. You can see we are trying to explore in depth your analysis and we will do the same with that of the Staff. At this point I think what we will do is to proceed with -- that ends your direct case, Mr. Irvine, am I correct?

MR. IRVINE: Yes, sir.

CHAIRMAN FARMAKIDES: We will proceed, then, with the Staff. Thank you again.

MR. IRVINE: Do you want to rule as to exhibit 4 now or do you wish to reserve that further?

CHAIRMAN FARMAKIDES: Yes, I think we will rule. The Board does not need it and, of course, the Applicant may withdraw it. I would appreciate it very much, however, if the panel would stay until after the Staff's direct case if that is at all possible.

MR. IRVINE: Yes, sir.

CHAIRMAN FARMAKIDES: Thank you.

(Witnesses excused.)

MR. KINSEY: Mr. Chairman, may we have a five-minute recess?

CHAIRMAN FARMAKIDES: Yes.

(Recess.)

e 6
cr 2803

#7 mml
cr2803
Dennis (H.)

MR. KINSEY: Mr. Chairman, if there is no objection the Staff would like to present all four of its witnesses as a panel.

MR. IRVINE: No objection.

MR. KINSEY: Beginning from my left, the panel consists of Mr. John Robertson of the U.S. Geological Survey; Dr. Donald Warner, Professor of Geology at the University of Missouri at Rolla; Mr. Donald Nussbaumer of the U.S. Atomic Energy Commission; and Mr. George D. DeBuchananne of the U.S. Geological Survey.

At this time, Mr. Chairman, may I request that you swear the panel.

Whereupon,

JOHN ROBERTSON

DONALD WARNER

DONALD NUSSBAUMER

GEORGE D. DE BUCHANANNE

were called as witnesses on behalf of the Regulatory Staff, and having been first duly sworn, were examined and testified as follows:

DIRECT EXAMINATION

XXXX MR. KINSEY: Mr. Nussbaumer, I hand you Staff Exhibit A.

Would you please identify this?

MR. NUSSBAUMER: This is my testimony in the matter

mm2

of the Kerr-McGee Corporation application for amendment of its source material license.

MR. KINSEY: Are the statements contained therein true to the best of your knowledge?

MR. NUSSBAUMER: Yes, they are.

MR. KINSEY: Dr. Warner, I hand this to you as Staff Exhibit B.

Would you identify it, please?

DR. WARNER: This is my testimony in the matter of the Kerr-McGee Corporation application and in addition includes several evaluations which I had done previously for the Atomic Energy Commission.

MR. KINSEY: Are the statements contained therein true to the best of your knowledge?

DR. WARNER: Yes, they are.

MR. KINSEY: Mr. Robertson, I hand you Staff Exhibit C.

Would you please identify that?

MR. ROBERTSON: This is my written testimony in the same hearing regarding analysis of the use of numerical methods.

MR. KINSEY: Are the statements contained therein true to the best of your knowledge?

MR.ROBERTSON: Yes.

MR. KINSEY: Mr. DeBuchananne, I hand you Staff

mm3

Exhibit D.

Would you identify it, please?

MR. DE BUCHANANNE: This is a statement of qualifications of George DeBuchananne of the U.S. Geological Survey.

MR. KINSEY: Mr. DeBuchananne, have you read the testimony of Dr. Donald Warner?

MR. DE BUCHANANNE: I have.

MR. KINSEY: Do you agree with the statements contained therein?

MR. DE BUCHANANNE: I do.

MR. KINSEY: Would you adopt those statements as your own?

MR. DE BUCHANANNE: Yes.

MR. KINSEY: Do all four of you adopt the statement contained in your respective testimony as your own in this proceeding?

MR. ROBERTSON: Yes.

DR. WARNER: I do.

MR. NUSSBAUMER: Yes.

MR. DE BUCHANANNE: Yes.

MR. KINSEY: Mr. Chairman, I move the testimony and attachments be admitted into evidence at this time.

CHAIRMAN FARMAKIDES: Any objections?

MR. IRVINE: No, sir.

CHAIRMAN FARMAKIDES: They will be so received.

mm4

(The documents referred to were marked Staff Exhibits A thru D for identification, and received in evidence.)

MR. KINSEY: Mr. Nussbaumer, would you briefly describe your testimony for the Board?

MR. NUSSBAUMER: My testimony summarizes the geologic evaluations made by the consultant to the AEC in this matter. It discusses the radiological nature of the wastes to be injected into the Arbuckle formation as well as the geological characteristics of the formation water itself, and states the conclusion of the AEC Staff in this matter, which is in essence that we do not believe enough is known about this complex formation to know how the waste will behave in the formation in order to support a conclusion that the waste will be adequately contained.

We feel containment is necessary because of the concentrations of radium 226, which is the isotope in principal concern, which concentrations exist at levels significantly in excess of the standards for such concentrations in unrestricted areas.

Also, the concentrations of activity, again radioactivity in the brine water, are in excess of the standards for unrestricted area concentrations of radium. This information being necessary to demonstrate that the

mu wastes must be contained in order to protect public health and safety.

Finally, that in the absence of being able to find that the wastes will be contained, we are unable to support a position of approving the application for the Kerr-McGee Corporation.

MR. KINSEY: Dr. Warner,would you briefly summarize your testimony?

DR. WARNER: In my testimony specifically prepared for this hearing, I discuss some general aspects of the mathematical modeling of subsurface hydrologic systems, the site geology of the Kerr-McGee site, the reservoir model developed by the Kerr-McGee Corporation consultants, the question of recovery of the injected waste water and some aspects of monitoring of the injection well if it should ever be operated.

In addition to this specific testimony, my testimony which has been incorporated into the record, includes three previous reviews of material concerning the application which were not prepared specifically for this hearing.

MR. KINSEY: Mr. Robertson, would you briefly summarize your testimony?

MR. ROBERTSON: My testimony starts off with a description of mathematical modeling, computer modeling, as applied to underground water systems, the usefulness of this tool, and also the limitations.

It then goes more specifically into the specific model used for the Kerr-McGee well and an evaluation of some of the shortcomings that may be inherent in such a use on a specific problem of this nature.

The principal shortcoming or source of limitation in the application of this type of modeling in this problem would be a lack of data at any other point other than the disposal well. There are many ~~sources~~ types of data used in this model and only one collection point.

MR. KINSEY: Mr. Chairman, the panel is now available for questioning.

CHAIRMAN FARMAKIDES: All right.

CROSS-EXAMINATION

MR. IRVINE: Mr. Nussbaumer, you have written your report here and apparently from what I can see and from what you have stated, the information that you have put into this report is based upon Staff information, or checking by other parties which you have merely assembled and put together in a report.

Would that be a fair statement?

MR. NUSSBAUMER: Some of the information, the data on the radium concentrations and so on, are taken from the Kerr-McGee application which was filed.

MR. IRVINE: So far as you know, neither you nor anyone under your supervision and control has made any

mm7

independent checks of this data?

By that I mean you have not made any independent -- you have not put it through a laboratory or checked it in that fashion?

MR. NUSSBAUMER: No, to the best of my knowledge they haven't.

MR. IRVINE: I call your attention to page 6 of your testimony there and in the only full paragraph that appears on that page, you have indicated in the second sentence there, "For example, Applicant assumes the five disposal zones comprising the Arbuckle formation to be homogeneous, isotropic and constant in thickness, porosity and permeable. Therefore, the movement of the waste water from the well bore can be accurately calculated. It is the Staff's position based on the analysis of core samples from the Arbuckle performed by the Applicant that there is a significant range of permeabilities within the Arbuckle which would preclude accurate calculation of fluid migration."

Then you quote a statement from a Mr. Schneider, apparently in a letter dated July 13, 1970.

Now this information concerning the core samples apparently is that which was submitted in the original application which was withdrawn, isn't it?

MR. NUSSBAUMER: I believe that may be the case, yes.

MR. IRVINE: Have you or your staff examined the

mm8

later information that has been submitted in connection with the radioactive tracer or radioactive tracing which would determine the permeabilities and the places where the water entered the formations.

MR. NUSSBAUMER: Yes, I believe Dr. Warner has evaluated that information.

MR. IRVINE: But you apparently did not secure that evaluation prior to your testimony here or you did not consider it in preparing your testimony?

MR. NUSSBAUMER: I think it was considered, but the earlier information was used merely to show the range of the permeabilities that had been previously reported.

MR. IRVINE: When you speak of the range of permeabilities within the Arbuckle, are you speaking of the range of permeabilities throughout the entire Arbuckle zone or formation?

MR. NUSSBAUMER: I think as the statement indicates, the range is determined from the core samples that were taken, so whatever they would represent would be what the range is supposed to cover here.

CHAIRMAN FARMAKIDES: I am sorry, sir. I didn't hear that last remark. You sort of fell off and I missed the last three or four words.

MR. NUSSBAUMER: I was saying the range of permeabilities indicated in the statement were determined from

mm9

core samples taken in the Arbuckle range so that to the extent that the core samples were representative of that range then they would represent the entire range.

MR.IRVINE: So that you are not aware of whether the range of permeabilities are limited only to those formations which have been testified to as having available permeability for the entry of water, or whether these not only include those but also more impermeability zones than the Arbuckle which were also cored?

MR. NUSSBAUMER: I think the assumption here is that the cores that were taken were meant to be representative of the Arbuckle range so that one would get an idea of what the permeabilities would be.

MR. IRVINE: But you are aware of the testimony, are you not, that there are a limited number of zones which water would enter into?

You have heard the testimony of the other witnesses.

MR. NUSSBAUMER: Right.

MR. IRVINE: So that as a matter of fact, the fact that there may be a significant difference in the permeabilities in the Arbuckle formation really has little or not bearing insofar as the formations into which the water would enter, isn't that correct?

MR. NUSSBAUMER: I guess I can't answer that question. It is a little beyond me in terms of the geological aspects.

mm10

DR. BABCOCK: May I interrupt to ask a question?

You are using the term "Arbuckle formation."

You, Dr. Nussbaumer, are using the term "Arbuckle Range."

Is there any divergence between the two? Are those terms synonymous?

MR. IRVINE: I think in the sense that both Mr. Nussbaumer and I are using them, they are synonymous. We are speaking of the overall Arbuckle formation or range.

DR. BABCOCK: Which is 1000 feet thick?

MR. IRVINE: Yes, sir.

CHAIRMAN FARMAKIDES: I would like to have an answer to that question. Perhaps the panel can answer it, and there is no reason why the panel cannot consult.

I think it is a fair question and it certainly goes to the testimony put in by Dr. Nussbaumer.

MR. KINSEY: Mr. Chairman, could we have the question read back?

(Whereupon, the reporter read from the record, as requested.)

CHAIRMAN FARMAKIDES: Did you understand the question, sir?

Did the panel understand the question?

DR. WARNER: I will be glad to try to answer the question, but I am not exactly sure what it is that you are

mm11

interested in, Mr. Irvine.

I understand the problem in the fact that Mr. Nussbaumer constructed his notes including data which are not presently part of the record, but I don't understand exactly what the problem is with regard to that.

MR. IRVINE: Well, Mr. Nussbaumer's statement --

end #7

8
2803

CHAIRMAN FARMAKIDES: Perhaps you could reframe your question, sir. Evidently there is some concern that they don't understand the question.

Could you reframe it?

MR. IRVINE: That is what I am trying to do now.

Mr. Nussbaumer has referred to the fact in his statement here that the analysis of core samples from the Arbuckle performed by the applicant -- that there is a significant range of permeabilities within the Arbuckle which would preclude accurate calculation of fluid migration.

My question is, is he speaking of the core samples and the permeabilities that have reference only to the zones into which it has been established that water will enter by radioactive tracing, or is he speaking about permeabilities revealed by cores taken throughout the Arbuckle zone, of which it has been testified there is a significant permeability?

DR. WARNER: May I try to make an interpretation of what Mr. Nussbaumer has said here and then ask for his concurrence if it is correct?

CHAIRMAN FARMAKIDES: Well, I think it --

DR. WARNER: Well --

CHAIRMAN FARMAKIDES: Excuse me, sir. I think the question is posed in the alternative. You have an option of A or B. The question goes: is it A or is it B, as I under-

stand Mr. Irvine.

If you cannot answer it, I would be curious why not. It is one of two options that the Council has proposed. Unless, of course, you are suggesting that neither of these two options is correct.

If that is the case, then so state.

MR. NUSSBAUMER: May I have the question back?

(The reporter read from the record as requested.)

MR. NUSSBAUMER: I believe the statement is speaking about the Arbuckle zone in general.

MR. IRVINE: So that insofar as the -- there is no comparison here in your thought with the limited zones which have been shown by the radioactive tracing, it would be the ones which would accept the water?

MR. NUSSBAUMER: That's correct, sir.

MR. IRVINE: I would like to direct your attention to page 8 of your testimony, Mr. Nussbaumer, please. In the first full sentence appearing on that page, which in the third line starts "The concern here is that this brine containing radium being incompressible will be displaced in equal amounts to the amount of waste injected."

Now, is it still your testimony in view of what you have heard today that the brine or the water or liquid in the formation, the liquid being put into the formation, are still incompressible?

MR. NUSSBAUMER: No. I think there apparently is some compressibility. The statement was intended to indicate that the brine would have to be displaced by the injection fluid and, therefore, if the boundaries do not exist to control the migration of the fluid that the brine would then move out in advance of the injection fluid, and that this brine already has a radium concentration in excess of the standards for unrestricted areas and, therefore, if it did leak out you could have a problem from this material in the absence of any problem from the raffinate wastes.

MR. IRVINE: Do you have anything in the Staff reports that you have received or the material that you have received that would indicate the probability of such an escape of this material or such a migration of this material within the confines of the fault block which is under discussion here?

MR. NUSSBAUMER: I don't believe the application has any information on the probability of the material leaving the fault block.

MR. IRVINE: Have you or have any of the people on your staff or any of your consultants made such a study?

MR. NUSSBAUMER: No, we have not attempted to determine independently the probability of this material leaking at the fault block.

MR. IRVINE: In view of the additional information

that has been submitted as evidence in this case, and which I believe you and your staff were furnished copies of ahead of time, do you still believe, as you state at the bottom of page 8, that there has not been adequately shown that faults exist within 1200 feet northeast of the well and approximately five miles southwest of the well?

DR. WARNER: If I were being asked to answer in this case, the testimony which I have already introduced in writing and the general content of the reviews and reports that I have previously submitted would stand unchanged.

MR. IRVINE: Dr. Warner, I would like to call your attention to the December 22, 1972 letter which you wrote to Mr. Mallaro.

In the fourth paragraph thereof you state as follows:

"After considering all of the presently available information it is my opinion that in the absence of any more as a practical alternative Kerr-McGee should be allowed to use the well that they have constructed for injection of up to 50 million gallons of waste water with no foreseeable significant hazard to the environment or public health."

MR. KINSEY: Mr. Chairman, could the record reflect that the word pronounced as "should" was "could"?

MR. IRVINE: I am sorry. Kerr-McGee Corporation could. I beg your pardon. Could be allowed.

Dr. Warner, in fairness, thereafter you make several recommendations about monitoring wells.

DR. WARNER: That's correct.

MR. IRVINE: Will you assume, please, that either monitoring by wells as you suggest or monitoring by model as Kerr-McGee suggests is a satisfactory solution and one of them is in effect?

Based upon what you now have heard, do you still subscribe to that statement?

DR. WARNER: Do I still subscribe to the conclusion that I reached in that letter?

MR. IRVINE: Yes, sir.

DR. WARNER: That particular paragraph that you quoted?

MR. IRVINE: Yes, sir.

DR. WARNER: Yes, I do.

MR. IRVINE: Then you feel that it would be safe for Kerr-McGee provided proper monitoring is done, to insert some 50 million gallons of waste water with no foreseeable significant hazard in the environment or to the public health?

DR. WARNER: Yes, that's correct, but I would like to point out -- and, as you read in that paragraph -- that I qualified that by indicating that in the absence of any better practical solution to the problem.

MR. IRVINE: Do you know of a better practical

solution at this time?

MR. IRVINE: Do you know of a better practical solution at this time?

DR. WARNER: No, sir. I am not suggesting anything. That is not within my area of expertise to look at the alternatives. I was simply stating that the intent -- the general tenor of the letter was that if there were no better alternative I felt that this could be done without any significant hazard, as I indicated.

However, my feeling is -- and it is expressed in a number of other places -- that the geological conditions there are sufficiently complex so that in the long term there is some potential for movement of the fluid from the particular zone in which it is intended to be confined or in fact the brines which are presently confined within the reservoir.

Therefore I qualified that -- the feeling that they could be allowed to use it by relating it to a specific volume of water rather than saying in general they could be allowed to use it without any foreseeable hazard and so forth.

MR. IRVINE: The 50 million gallons figure that you used was in fact the request of Kerr-McGee for the amount they could put into their -- or the amount that they should be allowed to put in for a five-year period, was it not?

DR. WARNER: Well, my feeling was it is not

related to time but rather to volume. So I am, therefore, qualifying my recommendation or my belief with regard to volume rather than time.

MR. IRVINE: Would the time of five years to insert 50 million gallons of waste water in there be one which would be acceptable as far as you are concerned?

DR. WARNER: Insofar as I am aware, yes. The only thing that could change that, of course, is -- and I believe if you would examine the entire letter that it further recommends that this monitoring program be in effect and if the situation would appear to change during this five years then, of course, my opinion would change.

I believe this has also been the testimony of the Kerr-McGee Corporation witnesses as well.

MR. IRVINE: In other words, Kerr-McGee agrees with you that if there is any change there would need to be some readjustment; right? That is your understanding of the testimony?

DR. WARNER: Yes.

CHAIRMAN FARMAKIDES: Let's be clear that the record shows that the letter thatyou have reference to, Mr. Irvine, is attached to the testimony of Dr. Warner, as are two other letters.

We have assumed that all those letters are part of the testimony. Is that correct, Mr. Kinsey?

MR. KINSEY: That's correct.

CHAIRMAN FARMAKIDES: All right.

MR. IRVINE: Mr. Nussbaumer, have you or your staff ever suggested to anyone, or to Kerr-McGee, I should say, permitting the use of the well with adequate monitoring as suggested by Dr. Warner?

MR. NUSSBAUMER: To my knowledge, we have not, no. That was not part of the application that was filed.

MR. IRVINE: What do you mean it was not a part of the application, sir?

MR. NUSSBAUMER: I don't believe the application for the deep well disposal suggested a limited disposal period or limited volume of waste material.

MR. IRVINE: Would it not have been within your purview, however, to have granted the license so limited, or within your authority to do so?

MR. NUSSBAUMER: Yes, we have the authority to impose any conditions we think are appropriate on licenses we issue.

MR. IRVINE: Dr. Warner, along this same line, in your written testimony you have now, as I see it, at least, indicated a change in your monitoring. As I read your letter of December 22 and the previous information that you had put in, which is attached to your testimony, you had indicated three monitoring wells or three wells to be drilled in

addition to the one already drilled, one some several thousand feet to the north, to determine the existence of the northwest fault -- northeast fault, the one we call the Webbers Falls fault, a second one to be drilled to the top of the Arbuckle some 500 feet or so to the north of the presently existing well, and a third one to the southwest several thousand feet away which would be drilled and completed in the same fashion as the present well.

As I understand your present testimony, you have dropped the need for a well -- the first well that you would suggest drilling. I see nothing in the testimony that would require that now.

DR. WARNER: That was unintentional. I didn't intend to convey that impression. I think I have repeatedly said that there isn't anything in any of my written testimony so far that I wouldretract.

If I gave that impression it was incorrect. My recommendation as it was given in the letter of December 22 is -- still stands. In other words, the one well is not for monitoring purposes. The well that was to be drilled for stratographic information was simply to determine if a fault in fact existed between that well -- between that stratographic test and the injection well.

Now, the other two wells were for monitoring purposes.

It was my concept, although it is perhaps not expressed, that this stratograhic hole would -- well, it might -- if someone wished to it could be left open and used for a monitoring well besides, but I don't believe I used that as part of the recommendation in my December letter.

In any event, I have not changed. That was a very thoughtful recommendation and I haven't changed my opinion.

MR. IRVINE: In your letter of December 22 you recommend in this so-called strathole, the one that would be drilled some 2000 feet to the north of the present swell, that that be drilled on the first marker bed.

That, of course, could be one of several beds which are well known in there which would give you the information, the wapanucka, the viola or something of this nature is what you had in mind, wasn't it?

DR. WARNER: I was leaving the option open to the Kerr-McGee Corporation to select whichever one they felt was adequate.

MR. IRVINE: You indicated also in there that if that marker bed were not found at the location predicted by Kerr-McGee's geology, that this well should be drilled on to full depth. Is this still a part of your recommendation?

DR. WARNER: It could be. Again I would have left that open as an option, too, as to whether or not to complete

jon11

that well as a deep test or whether to simply plug that and drill a deeper well at the third site that I recommended.

MR. IRVINE: I would like, if I may, at this time, to call on Dr. van Poollen as the expert to question Mr. Robertson about several matters in his testimony.

CHAIRMAN FARMAKIDES: Does the Staff have any objection?

MR. KINSEY: No objection.

CHAIRMAN FARMAKIDES: The Board will accept that.

MR. VAN POOLLEN: Mr. Robertson, I would like to refer to your testimony on page 4, the second paragraph. There you make certain statements about the use of the equations and consequently the use of the model that was used by Gruy and Associates. I wonder if you would like to expand on that a little further.

MR. ROBERTSON: The complete second paragraph?

MR. VAN POOLLEN: Well, the whole thig is related. First of all, you state that "Gruy's report states that one side of the equation solved by the model is 'a certain equation" which I presume was meant to be "Another equation" put "P" was mistakenly left out in the Applicant's equation.

MR. ROBERTSON: That particular matter was cleared up in yesterday's testimony and I do not agree with the way I worded that statement at this point.

CHAIRMAN FARMAKIDES: How would you change your testimony, sir, so that it is now accurate?

MR. ROBERTSON: All right, just a minute here. I would strike the first two sentences.

CHAIRMAN FARMAKIDES: The first two sentences?

MR. ROBERTSON: Of that paragraph. I would strike the second sentence, at least.

MR. VAN POOLLEN: You are only striking the second sentence?

MR. ROBERTSON: Just a minute. Let me think a

minute here.

The second sentence would be enough to strike.

MR. IRVINE: I beg your pardon, sir?

MR. ROBERTSON: The second sentence, I would strike that. There was a misunderstanding relating to the common ground water term used in these type of equations to common terms used in the petroleum industry.

CHAIRMAN FARMAKIDES: All right, the Board will strike the second sentence of that paragraph, which is the second full paragraph on page four of the testimony of John Robertson. Proceed.

MR. VAN POOLLEN: Let's refer back to the first sentence, then. "Another questionable error involves the mathematical equation used by Gruy and Associates for the model." What do you mean by "questionable"?

MR. ROBERTSON: The question is what is the factor "B" and how is it determined and how was it varied in the model.

MR. VAN POOLLEN: But the way I would read this might imply possibly that it is incorrect yet? Do you still feel this to be the case?

MR. ROBERTSON: I don't feel it is incorrect, no. I don't feel that the equation as stated is incorrect.

CHAIRMAN FARMAKIDES: Your point now, sir, as I understand it -- let me clarify this for my own thinking. Your

point now is that with that second sentence stricken the problem here is ambiguity of the equation? And you are then expounding on that in your paragraph?

MR. ROBERTSON: My problem is in the Gruy report or in the Gruy testimony or in Dr. van Poollen's testimony it is not clear what factor "B" is nor how that equation -- how you can get a pressure direction from that equation, since that factor does not directly show a pressure term. These equations are used to determine pressure directions with the points used to plot the curve.

CHAIRMAN FARMAKIDES: What is your conclusion from that.

MR. ROBERTSON: Where is a pressure term in that equation?

MR. VAN POOLLEN: Did I explain this to you yesterday or did I not?

MR. ROBERTSON: Yes, that has been explained to me.

MR. VAN POOLLEN: But you would still like to have this as part of your question, that you question all of this?

MR. ROBERTSON: Well, let me say it is not important. My testimony is not -- if it ended on that sentence, if it would expedite the matter --

CHAIRMAN FARMAKIDES: No, we don't want to expedite the matter. If that is your testimony then stick by it. If there is a problem with it and you want to change it, change

it. There may well be a disagreement between yourself and Dr. Van Poollen. So be it. But I don't want the expediency aspect to get in to the hearing.

MR. ROBERTSON: At this point I accept the equations as written in the Gruy testimony, the mathematical equations, which I suppose would -- to eliminate any ambiguity or inference by the first sentence I would strike the first sentence.

CHAIRMAN FARMAKIDES: So you would strike both the first and second sentences?

MR. ROBERTSON: Yes.

CHAIRMAN FARMAKIDES: I assume, Mr. Kinsey, this is satisfactory to you?

MR. KINSEY: So long as the sentences are stricken with the explanation as given by the witness.

CHAIRMAN FARMAKIDES: Yes. All right. We will strike sentences one and two of the second paragraph of page 4. Dr. van Poollen?

MR. VAN POOLLEN: I would like to ask you what is a good number for "S" which you called the storage coefficient?

MR. ROBERTSON: I can't answer that question for the Arbuckle formation.

MR. VAN POOLLEN: Would you give this for a formation approximately 10 percent porosity at a depth of approximately 3000 feet?

MR. ROBERTSON: A number in the magnitude of

point zero zero zero five would be --

MR. VAN POOLLEN: Thank you very much.

MR. ROBERTSON: This could vary by at least a factor of ten or more from very similar formations.

MR. VAN POOLLEN: Thank you, I am aware of that. That is all I had.

Now, you state that it is not clear to you how "S" relates to the quantity 5 over "B" nor how "S" varies in the Gruy model. Is this still a problem?

MR. ROBERTSON: Yes. It is not clear to me how "S" or the factor phi over "B" was there in the Gruy model nor how "B" would be determined at all.

MR. VAN POOLLEN: Do you know what "B" is?

MR. ROBERTSON: Yes. I can't give you a number for it. I know what it represents at this point.

MR. VAN POOLLEN: Would you state to me what it represents?

MR. ROBERTSON: It is called the formation volume factor, I believe, or something like that. It represents the ratio -- let me think for a minute. As I understand it, it is ~~the volume~~ the ratio of the volume of material of water in the formation to the volume of water that would be on the surface if that volume was brought to the surface.

MR. VAN POOLLEN: Could you give me a definition of the word "storage coefficient"?

MR. ROBERTSON: The storage coefficient is a number which represents the amount of water that would be produced, as an example, in a pumping well if you reduced the pressure in that well a unit of reduction. That would produce a certain unit of water out of the well, out of the formation. That is the number. That is what it is. It represents the elastic properties of both the water and the solid matrix of the aquifer ~~effluent~~ as well as the porosity.

MR. VAN POOLLEN: Consequently can you see from your own explanations of the quantity "B" and the quantity "S" that there is a considerable analogy between the two.

MR. ROBERTSON: Yes, there is. The factor of ~~"P"~~ phi over "B" I would say -- there is an analogy between that factor and the factor "S" although they are not equivalent because you are working in different units.

MR. VAN POOLLEN: That is all for me right now.

CHAIRMAN FARMAKIDES: Mr. Irvine?

MR. IRVINE: Mr. Robertson, I would like to call your attention to page 5 of your testimony, please. In the second full paragraph there --

CHAIRMAN FARMAKIDES: Excuse me.

(Discussion off the record.)

CHAIRMAN FARMAKIDES: Let's go back on the record.

MR. VAN POOLLEN: May we refer to your page no. 5, the second full paragraph, where you say "Therefore it also

appears that there might be a significant difference in the two curves over a much longer injection time of perhaps five years. Longer pre-injection tests, perhaps two or three weeks, could provide more meaningful data. It is dangerous to extrapolate a 150-hour injection test to the five-year performance of the well."

In relation to this I would like to ask you if you agree with the recommendations made in the Kerr-McGee panel's monitoring section that we would inject for a certain period of time with neutralized waste materials?

MR. ROBERTSON: Yes, that is what I am saying here. A longer injection period and a longer observation of pressure falloff period would be more meaningful. I understand that is one of the proposed tests you had.

MR. VAN POOLLEN: And you don't foresee any harm would come to people in the whole situation as a result of this?

MR. ROBERTSON: I --

MR. KINSEY: Mr. Chairman, I don't think the witness would be qualified to answer that question.

CHAIRMAN FARMAKIDES: I agree, unless the witness -- Do you feel qualified on that question?

MR. ROBERTSON: No, sir.

CHAIRMAN FARMAKIDES: Reframe your question, sir, or you might like to pose it to the entire panel.

MR. VAN POOLLEN: Could this question be read back for the entire panel?

(The pending question was read as requested.)

CHAIRMAN FARMAKIDES: Is anyone on the panel able to respond to that question?

MR. ROBERTSON: I would like to make one clarification. When I speak of an injection test I am thinking of water, not waste. That is what I was referring to as a long-period injection test, water but not necessarily waste water.

MR. VAN POOLLEN: But the question is, would even water do any harm by causing leakage of the brines which apparently we are concerned about?

CHAIRMAN FARMAKIDES: You have reframed your question now to make it specific to water?

MR. VAN POOLLEN: Yes.

MR. ROBERTSON: I can't answer that because I don't know when there would be leakage.

MR. VAN POOLLEN: How about the panel?

DR. WARNER: Well, it was my conclusion in 1972 and essentially still is that if it were felt it was absolutely necessary and essential to operate the well because of lack of other alternatives that this could be done with a reasonably small amount of risk element.

So essentially the further testing of the well is nothing more than initial operating phase so my opinion with

with regard to the testing of it would be essentially the same as that with regard to the operation of it.

It could be done if it is felt that this is the best way to proceed, all things taken into account.

MR. VAN POOLLEN: Therefore, Dr. Warner, you would not object to the fact that a neutralized waste material would be used rather than water?

DR. WARNER: Well, I think that it is fairly obvious that if you inject the waste for a few days it is not going to go more than a few feet away from the well and there is really no way that this waste water itself could have any possible harmful effect unless it moved directly up the well bore around the casing. If the cement were inadequate and it moved directly up along the well bore it could conceivably get out of the immediate Arbuckle formation. That is the only possible way it could in that short period of time.

e 9
2803

lmil

MR. VAN POOLLEN: I would like to ask one more question of Mr. Robertson. On your page 4 again, the third full paragraph, the last sentence. "A larger ~~or~~ error in estimated storage coefficient or the factor five over B could yield drastically different long-term pressure build-ups in the formation."

Could you clarify this?

MR. ROBERTSON: The pressure build-up is dependent or partly dependent upon compressibility of the water as well as the rock matrix. That is what the factor ~~P~~ phi over B and the factor S would incorporate. So a large difference in the number used versus a different number would change the pressure build-up.

MR. VAN POOLLEN: What would you suggest could be done to determine this kind of a coefficient in more detail?

MR. ROBERTSON: Well, for one thing additional wells drilled in the area could provide better porosity distribution data.

MR. VAN POOLLEN: And you would get this kind of porosity distribution from cores or from what?

MR. ROBERTSON: From standard methods, cores as well as injection tests, methods Kerr-McGee has used on their wells.

MR. VAN POOLLEN: Do you feel these methods would

2mil

work in the presence of various boundaries?

MR. ROBERTSON: What do you mean, the presence of various boundaries? Do you mean right immediately on a boundary or ~~in addition to~~ in the vicinity of a boundary? I don't understand your question.

MR. VAN POOLLEN: Do you feel that the mathematical methods to determine the storage coefficient will work in the presence of faults located several hundred feet from the well?

MR. ROBERTSON: There are major problems in determining -- are you speaking of determining a storage coefficient or porosity?

MR. VAN POOLLEN: Well, we assumed that they were analogies, didn't we?

MR. ROBERTSON: Porosity and storage ~~facility~~ coefficient are not an analogy.

MR. VAN POOLLEN: The term ϕ over B is related to the storage coefficient. If we sat down, we could conceivably calculate one from the other.

MR. ROBERTSON: Yes.

MR. VAN POOLLEN: So I limited it to S.

MR. ROBERTSON: Okay.

MR. VAN POOLLEN: Do you feel you can determine this from observation wells in the presence of barriers?

MR. ROBERTSON: This is a difficult problem in the presence of barriers. I do not say in my testimony that it could

3mil

be determined.

MR. VAN POOLLEN: So consequently, are you not stating that you should have an observation well to determine the storage coefficient, but it is very difficult to get it?

MR. ROBERTSON: No, I did not say that you should have an observation well to determine the storage coefficient.

MR. VAN POOLLEN: Didn't you say that by tests --

MR. ROBERTSON: I said to determine porosity distribution you can do it from tests in the wells. You can determine porosity which is one factor involved in this. There are also the compressibility of water and the compressibility of the rock matrix involved in this, which are more difficult to determine.

MR. VAN POOLLEN: But you do need an additional well for that, or don't you?

MR. ROBERTSON: This would depend on the particular hydrogenic conditions.

MR. VAN POOLLEN: In this reservoir.

MR. ROBERTSON: I haven't tried to determine "S" in this reservoir and I am not sure -- it is difficult in a complex boundary problem like this to determine "S" from a well, yes.

MR. VAN POOLLEN: But I heard you say that you could do this by tests and now I hear you say that you cannot do it.

MR. ROBERTSON: I only said you could determine

4mil

porosity from tests in a well, not "S".

MR. VAN POOLLEN: Could you show me how you would do that, please?

MR. ROBERTSON: Determine porosity in a well?

MR. VAN POOLLEN: Yes.

MR. ROBERTSON: It is explained very clearly in the Gruy testimony, with tracer injections and taking the cores to the laboratory.

MR. VAN POOLLEN: We have to get it from cores?

MR. ROBERTSON: No, I said with tracer injections in the well itself.

MR. VAN POOLLEN: Tracer injection tests indicate what the porosity is?

MR. ROBERTSON: No, excuse me. With bore hole geophysical techniques, density logs, a combination of several bore hole geophysical techniques can yield porosity data in a well, in a bore hole, without the use of cores.

MR. VAN POOLLEN: Consequently are you telling me that by having twice the amount of data I can improve materially on the knowledge of porosity?

MR. ROBERTSON: I am saying -- yes, you can improve it somewhat. I didn't say just one well. I said from the other observation wells.

MR. VAN POOLLEN: But from a practical point of view, how many observation wells would you want to have before you would say that you know the porosity is sufficient?

5mil

MR. ROBERTSON: Well, that is also a difficult question. It would depend partly on the results -- presuming you analyzed one well at a time, if every well yielded the same results you would become more and more confident with each well. If each well yielded drastically different results, you would become less and less confident and would require more. This is a statistical problem and I am not a statistician.

MR. VAN POOLLEN: What additional information would I need to calculate the storage coefficient if I knew the porosity quite accurately?

MR. ROBERTSON: You would need the compressibility of the formation you are injecting into as well as the compressibility of the capping formations, the bounding formations, and the compressibility of water.

MR. VAN POOLLEN: And would then we be able to get all this information from a single well with injection, with subsequent pressure measurements?

MR. ROBERTSON: This can be done, but in your testimony I did not see that it was done.

MR. VAN POOLLEN: How would you be able to do that, sir?

MR. ROBERTSON: There are standard techniques in the analysis of pressure fall-off or injection build-up providing the system meets the assumptions of all --

6mil

providing that the hydrologic or geologic system meets adequately the assumptions involved in applying these techniques, this can be done by analysis of pressure fall-off curves or injection curves.

MR. VAN POOLLEN: Do you recall the graph which was described and placed on the blackboard shown by Mr. Gruy where he showed the cumulative injection and the extrapolated reservoir pressure to determine the size of the confined reservoir?

MR. ROBERTSON: Vaguely, yes. Is this the graph you are referring to?

MR. VAN POOLLEN: Do you feel that that kind of method would render information about the total storage capacity of the reservoir?

MR. ROBERTSON: Yes, if the reservoir was sealed.

MR. VAN POOLLEN: Consequently, one well could give you that information?

MR. ROBERTSON: If the reservoir is sealed, yes, and the well is in good condition.

MR. VAN POOLLEN: If the reservoir is not sealed, what would happen?

MR. ROBERTSON: Well, you would have leakage.

MR. VAN POOLLEN: Correct, but what would happen to the curve?

MR. ROBERTSON: The cumulative curve would take on

7mil

a different shape. I can't read it -- is that pressure on that side of the board?

MR. VAN POOLLEN: I think it is sufficient to say that it will have a different shape so something would show up. Consequently, wouldn't a single observation well give you the information we are seeking?

MR. ROBERTSON: Not if you don't know already what the curve should look like if the reservoir is sealed.

MR. VAN POOLLEN: Wouldn't we know this after we have injected a certain amount of fluid, then allowed the reservoir to come to equilibrium?

MR. ROBERTSON: You can get some approximate magnitude.

MR. VAN POOLLEN: How approximate?

MR. ROBERTSON: Well, I don't have a number in my head right now. It depends on the accuracy of all of your measurements, particularly the pressure measurements. You can have a small amount of leakage and it may not significantly affect your pressure measurement within 2 psi, but it might be a significant -- .

MR. VAN POOLLEN: Thank you very much.

CHAIRMAN FARMAKIDES: Mr. Irvine, anything further?

MR. IRVINE: Yes, sir.

Dr. Warner, on page 4 of your testimony, in the first full paragraph there, in the last sentence, you say,

8mil

"The equations discussed do not directly yield information on the rate and direction of movement of fluid." Now, as I read the testimony of Mr. Robertson appearing on the first page of his testimony here in the next to the last sentence in the second paragraph, it reads as follows: "Furthermore, the models can predict rates and direction of ground water movements within the reservoir." It appears to me that those statements are in diametrical conflict. Are they or are they not?

DR. WARNER: Well, the models that I referred to are those which I discussed previous to that particular statement and the type of models that I am discussing previous to that particular statement are not useful for determining rate and direction of fluid movement. I haven't studied Mr. Robertson's testimony to be able to reconcile the two statements. There is no doubt about what I am saying. I don't think there is any question whatsoever about what I am saying and I can't explain what he is saying.

MR. IRVINE: What kind of model are you speaking of, sir?

DR. WARNER: Well, the models that I discussed previous to that particular paragraph are models which describe the pressure response of a reservoir or aquifer during either injection or withdrawal of water and it has nothing to do with the rate and direction of movement of the

9mil

fluid.

MR. IRVINE: Are you talking about what we would refer to as a pressure fall-off test and Mr. Robertson may be speaking of a modeling technique?

DR. WARNER: I am talking about the type of a model that could be constructed from a pressure fall-off test which only tells you the pressure distribution but doesn't tell you the fluid distribution. It may be assumed -- it could be assumed that the -- that -- in making a mass balance that the fluid was moving through the reservoir in a certain way to create this pressure, but it is not necessary. In other words, the general -- the general utility of the type of models that I have discussed above are with regard to the -- with regard to the pressure response of the aquifer and not with regard to the rate and direction of fluid movement. In other words, this -- those equations don't incorporate into them anything with regard to the porosity of the aquifer or the rate at which the water is moving away from the well. They are talking about the rate at which the pressure is moving away from the well, not the rate at which water moves.

MR. IRVINE: Is it your contention that the model used by Mr. Gruy and his associates would not render the rate and direction of movement of the fluid?

DR. WARNER: Yes, that's correct. They would have

10mil

to make separate calculations.

MR. IRVINE: Are you sufficiently aware of Mr. Robertson's testimony to know whether you are using the term modeling in the same concept that he is?

DR. WARNER: No, I really wasn't concerned with his testimony and I can't comment on that. My statement is with regard to the material that precedes it in my testimony and it is not with regard to Mr. Robertson's. If you will read the material which leads up to that statement, I believe that it is an accurate statement and it stands. There is no -- I --

MR. IRVINE: But there is no question that there is a form of modeling which will render the information on rate and direction of movement of fluid?

DR. WARNER: Well, yes, there is, if you take into account the necessary variables in the -- in making this calculation. But there are a number of variables which I list in here which were not incorporated into the Kerr-McGee -- incorporated into the Kerr-McGee model. Into their calculations of the rate and direction of fluid movement.

MR. IRVINE: Which are those, sir? I don't seem to follow you.

DR. WARNER: Well, if you will read the material that follows that particular statement, it says, "The most elementary calculation used to estimate the rate and distance

llmil

of movement from injection wells assumes uniform radial movement away from the well," with the uniform displacement of water and no mixing of the two. This is a type of model Gruy assumed. I further go on to say that more complex models include hydraulic gradients, density differences and so forth and these were not incorporated in the Kerr-McGee model.

CHAIRMAN FARMAKIDES: Perhaps this might be a good place to recess for luncheon. We will reconvene at 1:45.

(Whereupon, at 1:00 p.m., the hearing was recessed, to reconvene at 1:45 p.m., this same day.)

jonl

11
2803

AFTERNOON SESSION

(1:45 p.m.)

MR. KINSEY: At the time of the break Dr. Warner advised me that he had intended to expand further on the answer to the question that was proferred just prior to the break and I was wondering if there was no objection whether he could continue.

CHAIRMAN FARMAKIDES: Let's go off the record for a moment.

(Discussion off the record.)

CHAIRMAN FARMAKIDES: Back on the record.

Mr. Irvine, I understood you to say that your cross-examination is complete?

MR. IRVINE: Yes, sir.

CHAIRMAN FARMAKIDES: All right. Does the Staff have redirect?

XXXXX

REDIRECT EXAMINATION

MR. KINSEY: Dr. Warner, just prior to the break you had indicated that there were certain parameters that were not included in the Gruy model and I believe you did want to expand further on that. Would you please do so?

DR. WANRER: I thought it might be helpful to the Board if I very briefly clarified my discussion relative to modeling to indicate that in the construction of the analysis that Gruy has made or that anyone else would make presumably

there would be two fundamental steps that you would do, or two fundamental analyses which you would make.

One would be that of the pressure distribution in the reservoir. During the computation of the pressure distribution you are not simultaneously, at least ordinarily that I am aware of, also analyzing the rate and direction of movement.

Once the pressure distribution has been determined you can take this information and use that to determine the rate and direction of movement.

The fact is that in the equations which describe pressure distribution they do not include such factors as porosity, dispersion coefficient, they make no provision for fluid movements as affected by density diffrences in the fluids.

When I say that porosity is not included, it can be lumped in one term and you need never see it. It is in there in some way or another, but you don't necessarily ever have to see the term porosity itself in the pressure equations.

CHAIRMAN FARMAKIDES: How do you accommodate it, then?

DR. WARNER: In either ground water work or in oil field reservoir work you can get out a parameter, which was discussed extensively by Dr. van Poollen and Mr. Robertson,

which is called storage coefficient.

Porosity is in that term, but you never need see it. It can be obtained from an analysis of pressure falloff curves and you never need to independently determine porosity in order to get that number.

But for the determination of the rate and direction of fluid movement you must have porosity. You must, in order to make an order or complete estimate, also include any dispersion coefficient, which was discussed yesterday. If there were sufficient density differences between the two fluids you would also have to accommodate those things.

It is an additional step and in detail it can be very complex.

In a simplified form it is only calculated on the basis of a straight interface between the two fluids and density differences, dispersion and so forth are not taken into account.

Now, in the calculation of their -- in making their calculations of the rate and direction -- rate and distance -- direction and distance of fluid movement, the Gruy people or the Kerr-McGee consultants did not take into account mechanical dispersion, hydrodynamic dispersion. They did not take into account density differences, things of this sort, which all tend to make the fluid boundary extend

further away from the well than it would otherwise.

Yesterday Dr. van Poollen commented that this might make a difference of perhaps ten percent in the calculation Last evening I took advantage of a few spare moments just to make a very quick calculation, using dispersion parameters that have been determined in other limestone reservoirs, and determined that it would not be unreasonable at all to presume that the fluid front might extend at least 300 feet beyond the calculation that the Gruy people made.

This is only taking into account the mechanical dispersion effect. That would change its position from 900 to 1200 feet from the well.

Then one could further take into account the fact that there are -- where there are rapid changes or there are, let's say, large differences between permeabilities in narrow zones, that individual zones may in fact be the ones which the fluids choose to move along.

Dr. van Poollen mentioned this himself yesterday. I have cited an example of such a case in my testimony relative to another injection well installation in which it was calculated that the fluid movement would be on the order of magnitude of 500 feet and actually in the course of drilling three additional wells it was determined that the fluid movement exceeded 2700 feet.

We don't know how much further it may have moved.

In other words, the calculations were off by five times at least and may have been off considerably more than that. We simply don't have wells any further away to know.

CHAIRMAN FARMAKIDES: Is there anything further, Mr. Kinsey?

MR. KINSEY: No further questions.

DR. BABCOCK: Could I ask one small question concerning the last information you gave? Was this situation that you described in which the fluid moved 2500 feet, was that a bounded or nonbounded reservoir?

DR. WARNER: The reservoir was not bounded by faults. There were several permeable and porous zones in the reservoir. It is a very similar situation to that of the Kerr-McGee well and in my testimony I discuss this.

In the Knox dolomite which is an equivalent of the Arbuckle formation.

The individual reservoirs in each of the wells, or the individual porous permeable zones, were bounded by permeability pinch-outs but not by faults.

Apparently the majority of the waste has traveled in a single narrow band which was not bounded, whereas the other reservoirs, the other porous and permeable zones, appear to be bounded and thus the waste chose to move in a narrow zone.

But the point with regard to this example was that

it was not possible to predict, even though with large amounts of evidence were available -- all the same kinds of tests were madeon these wells that were made on the Kerr-McGee well and in spiteof this large volume of evidence it was not possible to determine accurately into which zone the waste was going to go.

Injective tests in fact were run on individual zones separated by straddle packers rather than the entire formation itself.

So, in some ways the data we had were more amenable to analysis than some of the Kerr-McGee data.

I am not intending to say that the two cases are completely analogous and that the analysis of the duPont Johnsonville well couldn't have been improved upon, but every effort was made to make the best analysis possible.

In spite of all that, it still was not possible to accurately predict where the waste was going to go. It was only found out later where in fact it did go.

DR. BABCOCK: Is the thrust of your argument that it is possible that this same thing could occur in Kerr-McGee?

DR. WARNER: Yes; in fact, I have stated that in my testimony, that this is the type of thing you find in this type of formation. It is a common occurrence to find this type of thing in formations.

I can't, certainly, contend that that is what is

going to happen in the Kerr-McGee well. I can only feel that itis a reasonable possibility that it could because it has happened in other cases in similar circumstances.

CHAIRMAN FARMAKIDES: Is there any further examination, Mr. Irvine?

MR. IRVINE: May I have just a moment?

RECROSS-EXAMINATION

XXXX

MR. IRVING: Dr. Warner, did you in this new Johnsonville well -- I assume you had some part in it -- did you make a numeric model of the information that you had here and was this the basis for your projection?

DR. WARNER: No, a numeric model was not constructed. The only thing that was attempted to determine was the rate and distance of movement, independently of pressure distribution.

MR. IRVINE: That is all we have, your Honor.

CHAIRMAN FARMAKIDES: The Board has some questions.

DR. BABCOCK: I have a question that was related to the question that I was giving to the panel and I don't remember who was asked that question. The question related to the porosity of the dolomite formation. It was suggested that radioactive material was injected into the formation and that was a mechanism of getting some information as to the porosity or the uniformity of porosity.

I don't know whether you are the one to answer this

question or not, but what I was interested to know is when the radioactive material was injected into the formation, approximately how far did this radioactive material go in and then, of course, come back out again?

DR. WARNER: I think that question would be better asked of the people that had the test performed.

In general, it only goes a very short distance, the purpose being to simply determine which of the zones has accepted it and then a radioactive -- a gamma ray log is run which indicates which zones this material has gone into, but it usually has only gone in a few feet.

DR. BABCOCK: Then the purpose of injecting radioactive material was to determine that there was a porous zone and not its extent?

DR. WARNER: If I interpret it incorrectly, I expect they will correct me, but it is to determine where in the bore hole the porous and permeable zones are and in this case it allowed them to make a comparison of the relative permeabilities of the zones but determining which one the larger amounts went into.

MR. KORNBLITH: If you have a situation where the upper and lower boundaries of a permeable zone are not paralle, they are still planar, but have a different slope so that the zone is not as thick on one side as the other, how would you expect this to affect your distribution of

injected material?

DR. WARNER: Well, in formations of this type generally the reservoirs are porous -- reservoirs of porous and permeable zones are not bounded above and below by strictly planar boundaries.

The thickness of the porous and permeable zone tends to vary in a general area in which it exists.

If you are specifically talking about a wedge-shaped boundary --

MR. KORNBLITH: Yes, just to give us a general picture of it.

DR. WARNER: If you have a wedge-shaped boundary, of course as your wedge narrows the permeability and porosity would decrease and so this would tend to inhibit fluid movement.

MR. KORNBLITH: Why would they decrease?

DR. WARNER: Because your permeability might remain constant, permeability being the measurement at the point of the ability of your porous material to transmit it. But if you multiply it by the thickness, which is a thing of concern, this would increase as the thickness of your aquifer increases.

MR. KORNBLITH: Would the permeability or porosity per unit volume remain constant or would the fact that the zone is narrower indicate there has been some increase in pressure

on that side which would tend to reduce it?

What would you expect?

DR. WARNER: Perhaps it would help me if you were to give me the context in which you are trying to understand this. Are you talking about these permeability pinch-outs, for example, that we discussed?

lmil

MR. KORNBLITH: We have been discussing a model which is based on the waste moving out radially. What I am after is what this shape would be in the case of this hypothetical wedge-shape. Would the cross-section of the material that was injected still be a circle?

DR. WARNER: I think the easiest general answer I could give would be that any changes you get, any significant changes that you get in the thickness, porosity, permeability of your layers does tend to affect the rate and direction in which they move. The direction in which your permeable layer is narrowing would tend to be one in which the porosity and permeability would be decreasing and, therefore, the waste water would tend to move in the opposite direction if it didn't thin in that direction as well. I think perhaps it would be better stated that less would move toward this decrease in porosity and permeability than would move in the direction in which it remained constant.

MR. KORNBLITH: So the maximum distance to which it extended would be in the thicker direction?

DR. WARNER: Not necessarily, because, of course, as your formation narrows the amount that it takes to move a certain distance is less and, therefore -- and as your formation thickens, although the total volume going in that direction may be greater, it has a greater cross-section in which to accommodate itself. In fact, we never try to make analyses as

2mil

sophisticated as that simply because you don't normally have the information in that degree of detail, or let's say it seldom would be tried, to make an analysis that sophisticated simply because you don't normally have the kinds of information that would allow you to make that.

MR. KORNBLITH: All right, thank you.

CHAIRMAN FARMAKIDES: If you were, Dr. Warner, going to create your own model here, what are the factors that you would consider or the assumptions that you would make?

DR. WARMER: Well, I don't think that the -- I would have approached the model any differently than any other person working in the field would approach it. In the first place, in general, everyone works generally the same way. You have to. You are constrained by the constants that go in, or the parameters that go into the equation. You all have to determine those in order to make any calculations. So everybody's procedure would essentially be generally the same. The place that you might differ is in the details or perhaps not details -- in the interpretation of the data that you have to work with. Certainly when there is a small amount of data there is a maximum amount of interpretation required in the sense that you don't have as good a quality information to work with as if you have more. So, therefore, you are required to extend your -- what information you do have andutilize it to the maximum.

3mil

In other words, you are saying, sir, that you would accept the model created by Kerr-McGee, but your interpretation of some of the parameters or some of the data generated would be different?

DR. WARNER: I can very well imagine if I had started out with the same data that Kerr-McGee had originally and constructed a model that I would not have come up with the same kinds of boundaries that they have, necessarily. Some of them perhaps you would have. It is quite clear in some cases that the reservoir is bounded. In other cases the reason for the boundaries is less clear.

Specifically, there is no reason at all to assume that the reservoir is bounded to the southwest because their analysis never extended to that, far enough in that direction to show that it was bounded. And I believe that their analysis of the boundaries as far as 28,000 feet away to the northwest has to be very weak because the data at that point were not extensive because this is the far-out -- farthest out limit at which they were able to make any analysis. So I feel that there -- it necessarily has to be a questionable interpretation. Not that it is incorrect, but certainly wouldn't be necessary for someone else generating the same model to have come to the same conclusions that they did.

CHAIRMAN FARMAKIDES: That is one assumption you would not accept?

4mil

DR. WARNER: Not necessarily. I hope you don't have the impression that I tried to in fact go through the same procedure they did. This is a very time-consuming thing. There is an enormous amount of effort involved. However, it is quite easy to look at what was done and make judgments with regard to the general validity and the alternatives that might have existed in the interpretation as opposed to generating an original model oneself and going through all of the effort that is required and time that is required to do that. One can make simple calculations, look at the data and form judgments, which is essentially what I did.

CHAIRMAN FARMAKIDES: How many judgments did you form, sir? Could you clarify this?

DR. WARNER: Those kinds of judgments are all listed in my testimony and I tried specifically to show examples of where there are weaknesses in the Kerr-McGee --

CHAIRMAN FARMAKIDES: You described them in your testimony insofar as you weren't to discuss them?

DR. WARNER: So far as I felt it was necessary. If the Board is interested in discussing any particular examples further, I would be very glad to.

CHAIRMAN FARMAKIDES: Are there any other judgments that you have not placed in your testimony that you would like to express at this time in view of the record to date?

5mil

DR. WARNER: Well, I did make one further calculation last night which I think might be of interest. There was discussion yesterday of the usefulness of a monitoring well which would lie 600 feet away from the Kerr-McGee injection well and midway between the possible Webbers Falls fault and the Kerr-McGee injection well. The usefulness of such well for monitoring any leakage in the Webbers Falls fault -- in fact, any leakage that might occur through any mechanism. First of all, the purpose in suggesting such a monitoring well was my feeling that if there were vertical leakage in the Arbuckle formation that it is going to be evidenced in all of the super-adjacent formations in which there is sufficient porosity and permeability for it to be observed on its way up. The time at which this response would occur is something that no one can predict. But if one were seeking a location for additional ways to monitor for potential leakage, vertical leakage from the Arbuckle, this, in my opinion, is the next most logical thing to do aside from what Kerr-McGee has already proposed to do, and that is monitoring of the injection well itself. We asked the Kerr-McGee panel about their feelings with regard to the response of this Simpson formation, if in fact this monitor well were there. They, of course, were not able to make any quantitative response simply because they hadn't performed any calculations and neither had I at that time. I simply

6mil

felt that the formation would respond to any leakage which occurred into it if, in fact, it is porous and permeable so that it will transmit fluid pressures at some reasonably -- at some reasonable rate. Well, in order to make these calculations I made assumptions of permeability for this formation, that is, the Simpson formation, of one darcy, a tenth of a darcy, and a hundredth of a darcy. I made the assumption that the rate of leakage into the Simpson formation would be one gallon per minute, which is one-twentieth of the rate that Kerr-McGee proposes to inject into it. I made the assumption that the time at which we were attempting to detect this leakage would be 10 days from the time at which it began. I made the assumption that the observation well that I am remembering would be 600 feet away from the point of leakage. I made an additional assumption of the compressibility of the Simpson formation which one has to have to make this calculation and I essentially assumed that only the water in the formation was compressible, which is a conservative assumption.

In any event, the number comes out -- it really doesn't make any difference. For a matter of interest, the number would be about 15 times 10 to the minus 6, that is, for the storivity or the compressibility of the formation. This is the storivity, not the number commonly used by the old field people. The response at the monitoring well after

7mil

10 days in the case of a permeability of one darcy would be a rise in water level of 1.3 feet. The response in the case of a tenth of a darcy would be 11 feet and the response in the case of a hundredth of a darcy would be 86 feet. My conclusion from these rather simple calculations is that there would be no problem in detecting this leakage under the circumstances that I have described, and I still believe that this type of monitor well would be a useful addition to the monitoring which Kerr-McGee has recommended by themselves. I am not recommending that the Kerr-McGee monitor not be used. I think they certainly should use it. I recommend the additional monitoring to detect any vertical leakage that could occur or might occur at this nearest inferred fault.

CHAIRMAN FARMAKIDES: Thank you.

DR. BABCOCK: Does your analysis assume that the entire fault area had the one darcy and a tenth darcy porosity?

DR. WARNER: No, this is in the Simpson formation. I am assuming that leakage is occurring at the rate of one gallon per minute into the Simpson formation. Now, of course, one could make any number of possible assumptions. I just simply picked one as an example of the effect that it would have if you had a monitor well in the formation.

DR. BABCOCK: That therefore assumes a certain size path or leakage route from the formation up.

DR. WARNER: No, it doesn't. It doesn't make any

8mil

difference what the size of the leakage path is. I am only assuming that one gallon per minute is getting into the Simpson formation. I don't really care how it is getting there.

DR. BABCOCK: I recognize that. On the other hand, it could get there via a crack that would have a width of a tenth of an inch or whatever. That is what I was getting at.

DR. WARNER: Right. I am simply assuming that it is a point source of leakage. However it got to the Simpson formation is not important.

DR. BABCOCK: I would like to go to an entirely different subject now, if I could. You heard the questioning that the Board gave to the Applicant's panel concerning the analysis of the pressure fall-off curve when there was leakage and when there was not leakage.

DR. WARNER: Yes, sir.

DR. BABCOCK: I would like to take the technical experts of this panel over much of the same ground and see if we come out with the same answers, or nearly the same. The route that Kerr-McGee went down was to first assume that the boundaries had no leakage. They then assumed that these boundaries were located in a certain position which best matched the fall-off curve that was presented when the water injection was stopped. After they had done this, giving the

9mil

full mathematical treatment to get the best fit, they then assumed that one of the boundaries had a certain leakage factor. They then fed this into the computer and they got a fall-off rate that always gave lower pressure than had been computed without it. Now then, I believe I asked whether the inevitable result of having a porous boundary was that all pressure measurements or pressure calculations with this assumption would give a lower pressure than the pressure calculations for the non-leakage case. Do you agree with that?

DR. WARNER: Yes, sir, they would have to.

MR. ROBERTSON: Yes.

DR. BABCOCK: That is inevitable?

DR. WARNER: Yes.

DR. BABCOCK: I then asked if they had picked the case where leakage was assumed and had refigured the boundaries in order to get the best fit for the leakage case, whether they would have gotten a result that more closely approximated the experimental data. Would you answer that question, please?

MR. ROBERTSON: I think I could answer that. It is not only the boundaries that can be adjusted to obtain a good fit. There are several other things, including the permeability and the storage characteristics of the reservoir as well as the location of the boundary. The question is, yes --

10mil

DR. BABCOCK: Would you limit yourself first to the question I asked? I will then allow you to expand in any way you want.

MR. ROBERTSON: Possibly the boundaries could be adjusted to obtain a better fit to the leaky boundary case. Then assuming you got a good fit, then you could assume that the boundaries were not leaky and that you would inevitably get a higher pressure curve, which would be not as good a fit as the other one. So you again work either way.

DR. BABCOCK: I would like to rephrase the answer that you gave me to your question as I understand it. If I did not understand it correctly would you correct me.

The answer that you gave was, in the case where a computer has been fed with the assumption that one of the boundaries is porous, readjustment of the position of those boundaries will give you or could give you a better fit to the experimental data than was gotten before the readjustment.

MR. ROBERTSON: Yes, it could. Not necessarily in all cases but it could. As I say, there are other things you generally adjust besides the boundaries.

DR. BABCOCK: In the original calculation did Kerr-McGee adjust anything other than the position of the boundaries?

MR. ROBERTSON: I believe all that I remember them adjusting was the boundaries. They later manipulated other things like porosity. For the original fit I think they confined it to the boundaries.

DR. BABCOCK: Now I would like to take you down the other route. Let's first assume that the boundary has a leakage. Let's now go to the computer and say "I want to know the location of the boundaries that will give the best pressure falloff data compared to the experimental data." Can you tell us whether the boundaries gotten by that method would have been the same or would they have been different from the boundaries

given by the computer for the no-leakage case?

MR. ROBERTSON: I would say they would probably be different.

CHAIRMAN FARMAKIDES: Is that the answer of the panel? You can consult if you would like, gentlemen.

MR. ROBERTSON: Would you restate your question, please.

(The pending question was read.)

MR. ROBERTSON: I would stay with my answer, that they would probably be different.

CHAIRMAN FARMAKIDES: Did you say different?

MR. ROBERTSON: Yes, they would be different, probably.

DR. WARNER: Could I just make my own comments, besides answering yes or no? I agree with you that if one wanted to adjust the model one should be able to obtain whatever answer you want from the within reason. In other words, yes, if you found leakage and it gave you poor fit and you readjusted -- or gave you less good fit and you readjusted -- you should be able to readjust the boundaries to get essentially the same answer you had in the first place. Except that so many boundaries and so many variables in this particular case that it is possible that when they shifted a boundary it did cause them a problem. But the general answer to your question would be yes. You could shift the boundaries to

cause a change in the response of the model. Or you could shift other variables as well as the boundaries.

DR. BABCOCK: I am going to attempt to rephrase the answer that you gave. If I do not rephrase it correctly I would hope you will help me out.

I believe you answered if the computer is first told that there is a leakage and then the computer is asked to compute a new set of boundaries that best fits the experimental case, those boundaries will be given the boundaries that the computer had for the no-leakage case.

DR. WARNER: That is not exactly what I said but the result would be the same.

DR. BABCOCK: Do you agree with the statement I make?

DR. WARNER: I agree with it.

DR. BABCOCK: Will you accept that statement?

DR. WARNER: I will accept that.

CHAIRMAN FARMAKIDES: That is the statement I understand Dr. Robertson to have made, too. Is that correct, sir?

MR. ROBERTSON: Well, slightly different. First of all, the computer doesn't compute where the boundaries are.

DR. BABCOCK: I recognize that.

MR. ROBERTSON: The operator shifts it.

DR. BABCOCK: The computer plus a helper.

MR. ROBERTSON: Yes.

DR. BABCOCK: But the computer is the one that furnishes the data. The operator then looks at the data and says "I am not happy with that data. I am going to feed it a new question."

MR. ROBERTSON: New set of boundaries.

DR. BABCOCK: New boundary locations. It comes back with a boundary that is different than it gave before. You either like that answer better or you don't like it as well. And that gives you then an opportunity to make a new selection for the boundary. This is what I call the computer finding, the computer with the help of a steering wheel.

DR. WARNER: Might I just comment on the analysis that Kerr-McGee consultants made with regard to the leakage of the boundaries, or would you be interested in such a comment?

DR. BABCOCK: Yes.

DR. WARNER: It seems to me -- I am unable to understand how so much credence can be lent to this analysis of leakage when first of all the model figures themselves differ from the observed data so far. I mean the variation between the no-leakage case and the leakage case is much less than the variation between the basic data to begin with and any one of the cases, whether it is leaky or non-leaky.

DR. BABCOCK: I recognize that and I was going to get in to that next.

What I would like to do is to get this question pinned down as tightly as I can.

The next question I would like to ask you is if you had the data that Kerr-McGee had for no-leakage and the boundaries as predicted by calculational methods, you then made the test, a single test on the computer, assuming leakage and got a different falloff rate. Would you then have been able to conclude that the boundaries were therefore tight.

DR. WARNER: Not in this case. I can't see how in this case they can because the data fit the curve -- the calculated values fit the observed values less well but they are already so far off the observed data curve that I can't see the significance of the difference.

The difference shown is very small in comparison with the divergence of any of the data from -- or calculated data from the observed data that I can't see it -- I can't see the significance that is attributed to the calculated values.

DR. BABCOCK: Would you have felt any more sure if you had taken the Kerr-McGee data, assuming leakage, and run additional computations to see if you could improve the fit and if you got a fit that was equal to or better than the fit of data without leakage would you then have made the same conclusion?

MR. ROBERTSON: I might speak on that.

DR. BABCOCK: Sure.

MR. ROBERTSON: I think it is apparent from the curve we are talking about that the model as set up is not really very sensitive to the amounts of leakage indicated. So that you cannot put much confidence in whether the boundaries are leaky or not leaky, at least in the order of magnitude that Gruy talked about.

The difference between a leaky and non-leaky boundary is so small that you can't put confidence in whether the system -- the system is very insensitive to that amount of leakage.

DR. BABCOCK: What you are saying is regardless of the route by which you come to the answer to this problem, your conclusion is that the Kerr-McGee data is not adequate to tell whether there is leakage or not leakage, is that a correct statement?

MR. ROBERTSON: Of the magnitude of leakage that they have talked about, yes.

DR. BABCOCK: Does the rest of the panel agree with that?

DR. WARNER: Yes, I agree with it.

DR. BABCOCK: I would like to ask one other question, and that has to do with the model that Mr. Gruy gave, which was a square boundary that Mr. Chenoweth gave. His boundaries, just to look at the map, looked quite different than the ones that Mr. Gruy gave. Do you consider that there was a

significant difference between those two, or was the difference just more picturesque than anything?

MR. ROBERTSON: I have not looked at that particular comparison so I don't think I could answer the question.

DR. WARNER: Well, the geologic -- in making the geologic maps Mr. Chenoweth made the features appear to be -- they don't appear to be exactly a straight line. They will be somewhat curved or irregular. In making the computer analysis, the difference between a line curved that much and a perfectly straight line isn't apparent. It really doesn't make any difference.

They simply -- it is simply assumed that the barrier is a linear one, if it is not so non-linear that it is obviously giving you some different response than a lineal barrier would.

DR. BABCOCK: In other words, what you are saying is that geological data support the model that Gruy Associates prepared.

DR. WARNER: I am not certain that is the case. There is a good correlation between the model which Gruy and Associates prepared and the geology of the areas interpreted on the geologic maps that have been submitted. But as I previously stated, I wouldn't have felt the same compulsion or -- that the Gruy people did in putting -- in putting in some of these boundaries that are a long distance away from the injection

well based on the data that they had to work with. I can't help but believe that the interpretation was a combination of modeling plus use of the geologic material. I can't -- I couldn't possibly have arrived, myself, at the conclusion that some of those boundaries existed without having some reason to do that, because the pressure falloff data are not -- essentially the divergence of the pressure falloff data from a straight line is simply not so -- is not great enough to make one believe independently that there has to be a barrier there.

There is one very sharp break in the pressure falloff data. It is an apparent answer shown on the data they prepared in their report. The other divergences which I saw in Dr. Van Poollen's analysis which was passed around today are much less sharp. That doesn't mean they are not correct.

His interpretation is certainly as likely to be accurate as anyone else's and perhaps even more so. But the data upon which those interpretations are based seem to me to be very few.

There are very few points to work with and the divergences are not sharp ones.

DR. BABCOCK: Do you consider that it is significant that the data given in the May 1, 1972 Kerr-McGee report -- looking at figure 2, the calculated points do not fall off as rapidly as do the measured points during approximately the first 60 hours of pressure falloff? After 60 hours the

calculated points are always below. Do you place -- is this merely a part of what you said the fit of the data is inadequate or is this a special item that you wanted to make any comment about?

MR. ROBERTSON: Well, I think that is why a longer injection test would provide more meaningful information on that. There is a significant difference between the two curves. What the significance is in a long-term pressure injection of, say, waste, you can't determine at this point. That is the question I raised. There is a difference. How much difference this would be -- let's say a five-year waste injection may be very significant or it may not be. It is hard to say from this curve.

There is enough difference in the two curves to raise a question.

MR. KORNBLITH: I asked the Applicant's panel this morning about what the data that they showed demonstrated about the permeability and leakage of the boundaries other than the nearest one. Could I get the views of this panel on that? It is figure 14, I believe.

MR. ROBERTSON: Well, they did not look at that problem of the leakage of other boundaries, apparently, so there is no data available.

MR. KORNBLITH: In other words, you say that figure 14 -- in running the curves for -- there is a little confusion

jeri 10

in my mind. It says these are the calculated ones for adding permeability to the east boundary. I was under the impression that the statement this morning said that it was -- that the leakage was placed at all the boundaries.

MR. ROBERTSON: Yes, there is some confusion on that.

MR. KORNBLITH: Is the new testimony any different in that respect?

MR. ROBERTSON: I was under the impression that leakage was for all boundaries, but looking at the curve it does say the east boundary only.

MR. KORNBLITH: All right, I guess my question has disappeared.

DR. BABCOCK: I was anxious to know the depth that you attempted to go in to the Kerr-McGee results in order to satisfy yourselves that everything was correct.

Did you for example take the data as prepared in the Sperrison report, which is pretty close to the raw data, and then proceed from that to the various graphs that were made and then proceed from the graphs to the statements that were made, or did you go in to the subject that thoroughly?

MR. ROBERTSON: I did not take the raw data.

DR. BABCOCK: What was the mechanism you had of reviewing the Kerr-McGee data?

MR. ROBERTSON: My mechanism was to evaluate the Gruy testimony and attempt to see how the model was manipulated

to arrive at the indicated boundaries and other parameters and to see if the conclusions made there were valid in my opinion.

My conclusion was that the techniques, mathematical techniques, used were accepted and valid techniques based on the equations they had, and that the way boundaries and so forth are manipulated is an accepted way to manipulate the parameters but it is not the only way.

The solution to a problem like this, to a complex problem like this with many variables, is not unique. You can get a black box. That is a term we use to indicate something that will generate and manipulate a curve. That does not necessarily mean that the black box is representative over a longer period of time. That is the general overview I took of it.

CHAIRMAN FARMAKIDES: We are going to permit further examination and further redirect if necessary.

e 13
2803

Mr. Irvine?

CHAIRMAN FARMAKIDES: We will take a five-minute recess.

(Recess.)

CHAIRMAN FARMAKIDES: Mr. Irvine, do you have any questions?

MR. IRVINE: We had about one or two more, but Dr. Warner has raised a new tchnical subject matter about which I am considerably uninformed and that was his example that he used here.

I would like to have Dr. van Poollen ask the questions, if I may.

CHAIRMAN FARMAKIDES: The Board wants very much to elucideate the entire subject and we think the records needs development.

MR. VAN POOLLEN: Dr. Warner, I would like to ask you -- first I would like to state that you used the compressibility of water only --

DR. WARNER: That's correct.

MR. VAN POOLLEN: And what would happen to the pressure or the 86 feet water rise in the observation, what would happen to that number if you also included the rock compressibility?

DR. WARNER: It would be smaller but the term is -- it is in a logarithmic term so it wouldn't be much smaller.

DR. BABCOCK: Ten percent maybe?

DR. WARNER: I really don't know. I could quickly make a calculation. It is involved in the -- if you took -- well, let's put it this way: if the term were taking the log of say a thousand, the number is 3. If you divide that in half, if you doubled the compressibility, the number would be 500 and the log would be 2.6 or something like that instead of 3. That is the difference.

MR. VAN POOLLEN: Could I also ask you if that would be the same thing as if instead of measuring it at 600 feet you had measured at 400 feet?

DR. WARNER: At 400 feet the pressure increase would be higher, of course.

MR. VAN POOLLEN: What I am suggesting, if you doubled the compressibility, it is the same as changing the whole location from about 600 feet to about 400 feet?

DR. WARNER: The well term is a squared term, so it is not exactly the same. Yes, moving the observation -- the point I was simply trying to make was that quite observable differences in water levels would occur within the Simpson formation if you made these simple assumptions that I was making. That is the only point I was trying to get across.

I was trying to get some quantitative figures into an argument which previous to that had simply been a qualitative one.

MR. VAN POOLLEN: I appreciate that, and I would like to ask you one more question in this regard. That is, you stated that you had one gallon per minute of leakage into the reservoir?

DR. WARNER: Yes.

MR. VAN POOLLEN: At one single point only?

DR. WARNER: Yes.

MR. VAN POOLLEN: Consequently you used the point source solution?

DR. WARNER: No, I used the general solution for a recharged well so it would have been the same as if it were distributed across the entire formation rather than a point, but the answer wouldn't be materially different.

MR. VAN POOLLEN: Wouldn't it be quite different if this leak were not at a single point but if this leak were distributed over a few hundred feet along a fault plane?

DR. WARNER: Of course the fault plane -- I really can't -- I can't comment on that. It would be different, but the -- I can't comment on the magnitude of difference. I don't know what the magnitude of difference would be.

I would agree with you it would be different, but I don't have -- didn't make any trial calculations on that basis so I don't know.

MR. VAN POOLLEN: That is all.

MR. IRVINE: That is all we have.

CHAIRMAN FARMAKIDES: Anything further, Mr Kinsey?

MR. KINSEY: No further questions, sir.

CHAIRMAN FARMAKIDES: We will excuse the panel. Thank you very much.

(Witness excused.)

CHAIMRAN FARMAKIDES: We would like the Applicant, if it will, to allow us to ask a couple of other questions of the Applicant's panel.

MR. IRVINE: Yes, sir, that is why they remained here.

MR. MURRAY: Will the Board want our panel back?

CHAIRMAN FARMAKIDES: I would hope that they would stay for another ten minutes or so. We may, although I don't know.

While we are getting organized, let me throw out a couple of miscellaneous matters that I think we should tend to. We haven't gotten the transcript yet, but I am sure we will and I would appreciate very much if the transcript corrections could be submitted to me within a period of time.

I will leave it to Mr. Kinsey and Mr. Irvine. Two weeks?

MR. IRVINE: That will be fine.

MR. MURRAY: We do have to not only obtain our transcripts, Mr. Chairman, but we have to send them out to

Idaho and to Missouri respectively to have them gone over and then get the responses back. If you would give us maybe a little leeway on that.

MR. IRVINE: I had not considered that time. We have the same problem, your Honor.

CHAIRMAN FARMAKIDES: All right. Let's make it four weeks.

MR. MURRAY: Thank you, sir.

CHAIRMAN FARMAKIDES: When are the proposed findings due? I guess from the Applicant in about fifteen days. The record won't close until you have supplied all the material that you were going to supply.

MR. IRVINE: Yes, sir.

CHAIRMAN FARMAKIDES: Then I think the Staff gets another additional period of time so that we have roughly a month from today before all proposed findings need be in, both the Applicant's and the Staff's. Then I think the Applicant has an opportunity of responding further, five more days. I think four weeks would do it for the transcript corrections. You won't have the benefit of knowing what the other parties' corrections will be in the preparation of your proposed findings.

All right. Then we will follow the rules with respect to the proposed findings. That rule is 2.754 or something like that.

MR. MURRAY: In that connection, Mr. Chairman, could we approach the bench on this?

CHAIRMAN FARMAKIDES: Yes.

(Discussion off the record.)

CHAIRMAN FARMAKIDES: We were just discussing dates for porposed findings. The Applicant's proposed findings will be due on or before November 16, 1973. The Staff's will be due on or before November 30, 1973. The Applicant will get five more additional days beyond that in which to comment further.

Now, I have two other miscellaneous matters.

Well, let's go ahead. Iwill bring these up at the end.

Whereupon,

JOHN S. RODGERS,

WILLIAM J. SHELLEY,

H. K. VAN POOLLEN and

H. J. GRUY

resumed the stand and, having been previously duly sworn, were examined and testified further as follows:

MR. KORNBLITH: I wnated to get back once again to this question of leaking boundaries. I have assumed, perhaps incorrectly, that one of you had assumed that all the boundaries leaked. Is that incorrect?

MR. GRUY: You had assumed correctly. I was

incorrect. I had said that we had assumed that all the boundaries leaked. However, I have now checked and find that we only made the eastbound permeable. That is the only one. I was in error. You understood me correctly, but I misstated the facts.

MR. KORNBLITH: What then is the basis for your assurance that the other boundaries don't leak?

MR. GRUY: It would be the most sensitive to the closest boundary. These models are very sensitive to the total volume in them. If you changed the boundaries you would change the total volume. If you changed the total volume of the thing you would get quite a different picture.

Of course, there can be no definite finite answer to your question without having made more runs with different assumptions, which I have not done. But I believe that the runs that we have made fairly describe the system.

MR. KORNBLITH: Let me ask you a little more specific question. If I recall the numbers you stated on the basis of these runs that at the east boundary you could detect forty barrels a day but not four. Is my recollection correct?

MR. GRUY: Right.

MR. KORNBLITH: What would the comparable numbers be, if you can make an estimate, for a boundary 13,000 feet away?

MR. GRUY: There would be a good bit of difference. I don't know what the comparable number would be.

MR. KORNBLITH: Would they increase faster than the distance to it faster?

MR. GRUY: Once you are filled up it wouldn't make any difference.

MR. KORNBLITH: That doesn't come for a hundred years?

MR. GRUY: No, we would be filled up pretty quick. We are saying 29,000 feet and this is 150 hours. I don't mean filled up. I mean once your pressure is affecting your boundaries, onceyou have had a pressure increase at your boundaries, which we haven't had a pressure increase at our farthest boundaries as of the end of this test --

MR. KORNBLITH: I was not looking now on the 30,000 foot boundary. I am still at a closer one, 13,000 feet. If the distance to the boundary instead of being 1100 feet is 11,000 would the sensitivity go down by a factor of 10 or more or less? Is that a linear or nonlinear function?

MR. GRUY: Do you know, Dr. van Poollen or Mr. Rodgers?

lmil

MR. GRUY: I can answer the question. It is dependent on the shape factor and so many things.

MR. KORNBLITH: All right, if you can't answer the question, there is no point in trying to repeat yourself. Thank you.

DR. BABCOCK: I have no further questions.

(Discussion off the record.)

CHAIRMAN FARMAKIDES: Thank you very much for your patience. This ends the Board's questions.

(Witnesses excused.)

CHAIRMAN FARMAKIDES: I think we are clear now on the transcript corrections and other proposed findings. Also I would say that we have received in the mail, addressed to the Atomic Safety and Licensing Board, a letter from Mrs. M. E. Arnold, President, League of Women Voters of Oklahoma, 808 South Peoria Avenue, Tulsa, Oklahoma. In essence, she says that she would like to have this -- as we understand the communication, she in essence says she would like to have this be her limited appearance. We will read it into the record.

"Statement to the Licensing Board of the AEC.

"RE: Kerr-McGee Corporation.

"October 10, 1973.

"The League of Women Voters of Oklahoma is concerned about the Kerr-McGee Corp. request to dump radioactive

2mil

wastes into a 3,000 ft. disposal well at their plant near Vian, Sequoyah Co., Oklahoma. We feel that a decision of this nature is of such importance as to warrant the full publication and open hearings that any real or potential source of surface water pollution would receive.

"Deep well disposal systems which dispose of wastes too concentrated and dangerous for surface disposal are not perfect systems. Geological factors which make the difference between safe conditions and hazardous ones are not always possible to predict and any error or accident produces pollution lasting for generations. There can be no provision for cleanup.

"The League has long supported publicizing all data on pollution and pollution control programs as well as increased opportunities for citizen participation in hearings and on decision making bodies. Basic to this is good communication between industry, regulatory agencies and citizens.

"Mrs. M. E. Arnold, President

"League of Women Voters of Oklahoma"

I think insofar as the Board is concerned, an issue similar to this with respect to the intervention of the parties was introduced at the August 14 hearing. I think the Board met that by allowing additional time for intervention, additional time for limited appearances from that time,

3mil

August 14. The notice was published on August 16, as I recall. We gave 30 more days, which would expire September 17. I think we have had a full public hearing as requested by the Applicant and I think it has been properly noticed not only in the Federal Register, but as I understand it, it has been noticed through other means.

I have, in addition, one other miscellaneous matter to raise. I have a filing submission for filing submitted by the Applicant through his attorney in which he wishes, I understand, a letter to be entered into the record. The letter is dated August 8, 1973. It is to Mr. W. J. Shelley, Director, Regulation and Control, Kerr-McGee Corporation. It is from the Oklahoma State Department of Health, Northeast 10th and Stonewall, Oklahoma City, Oklahoma, signed by Mr. Robert L. Craig, Engineer.

It goes to the application filed by the Kerr-McGee Corporation. Is there any objection to this letter being received into evidence?

MR. KINSEY: There is no objection.

CHAIRMAN FARMAKIDES: The letter so be received as Applicant's Exhibit No. 3.

(The document referred to was marked Applicant's Exhibit No. 5, for identification, and was received in evidence.)

4mil

CHAIRMAN FARMAKIDES: Dr. van Poollen was going to briefly sketch on paper his extrapolation of the last two frames of the worksheet that he had earlier identified as Applicant's Exhibit 4, and then that Exhibit 4 was withdrawn. So the new Applicant's Exhibit 4 will be that short graph.

Also I want to be certain that copies of the Staff's testimony have in fact been filed with the Commission.

MR. KINSEY: They have.

(Discussion off the record.)

CHAIRMAN FARMAKIDES: Yes, is there anything else that you would like to raise at this time in this proceeding? Mr. Irvine?

MR. IRVINE: In order to complete the record, may we now offer the copies of Exhibit No. 3 that we spoke about earlier that we have now had copies and are prepared to introduce?

CHAIRMAN FARMAKIDES: This will be Applicant's Exhibit 2. All right. Do you also have a copy for the Public Proceedings Branch?

MR. IRVINE: Yes, sir, as many as we need.

CHAIRMAN FARMAKIDES: Give those to the Court Reporter. He is the one who will te sending those in.

Is there anything else, Mr. Irvine?

MR. IRVINE: No, sir, I believe not.

CHAIRMAN FARMAKIDES: Mr. Kinsey, do you have

5mil

anything remaining?

MR. KINSEY: Other than when you indicated filing testimony with the Commission, you mean one copy for you for inclusion as an exhibit in the record?

CHAIRMAN FARMAKIDES: That has not been done so far as I know.

MR. KINSEY: I have it here.

CHAIRMAN FARMAKIDES: As I understand it, on your testimony you indicated that the Public Proceedings Branch has copies.

MR. KINSEY: As does the Appeal Board.

CHAIRMAN FARMAKIDES: All right, that is sufficient.

Gentlemen, this is all that I have. Thank you very much. I think we have had a fruitful day. It is still early enough for you to catch your planes. Thank you again. The proceeding is complete.

(Whereupon, at 3:20 p.m., the hearing was adjourned.)