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convenes the

EXPERT PANEL MEETING

Analysis and Historical Reconstruction of Groundwater Resources and Distribution of Drinking Water at Hadnot Point, Holcomb Boulevard and Vicinity, U.S. Marine Corps Base, Camp Lejeune, North Carolina

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EXPERT PANEL

Analysis and Historical Reconstruction of Groundwater Resources and Distribution of Drinking Water at Hadnot Point and Holcomb Boulevard and Vicinity, U.S. Marine Corps Base, Camp Lejeune, North Carolina.

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Glossary of Acronyms and Abbreviations

ASCE	American Society of Civil Engineers
AST	above ground storage tank
ATSDR	Agency for Toxic Substances and Disease Registry
AWWA	American Water Works Association
BTEX	benzene, toluene, ethylbenzene, and xylenes
CAP	community assistance panel
CD-ROM	compact disc, read-only-memory
CERCLA	Comprehensive Environmental Response, Compensation,
and Liability Act	
CI	cast iron
DCE	DCE: dichloroethylene
	1,1-
DCE:	1,1-dichloroethylene or 1,1-dichloroethene
	1,2-
DCE:	1,2-dichloroethylene or 1,2-dichloroethene
	1,2-
cDCE:	*cis*-1,2-dichloroethylene or *cis*-1,2-dichloroethene
	1,2-
tDCE:	*trans*-1,2-dichloroethylene or *trans*-1,2-dichloroethene
DHAC	Division of Health Assessment and Consultation, ATSDR
DOD	U.S. Department of Defense
DON	U.S. Department of Navy
EPANET or EPANET 2	a water-distribution system model developed by the EPA
ERG	Eastern Research Group, Inc.
gal	gallons
gpm	gallons per minute
HPIA	Hadnot Point Industrial Area
HUF	hydrologic unit flow
IRP	installation restoration program
LGR	local-grid refinement
MESL	Multimedia Environmental Simulations Laboratory, Georgia Institute of Technology
MGD	million gallons per day
µg/L	micrograms per liter
MODFLOW	a three-dimensional groundwater flow model developed by the U.S. Geological Survey
MODPATH	a particle-tracking model developed by the U.S. Geological Survey that computes three-dimensional pathlines and particle arrival times at pumping wells based on the advective flow output of MODFLOW
MT3DMS	a three-dimensional mass transport, multispecies model developed by C. Zheng and P. Wang on behalf of the

	U.S. Army Engineer Research and Development Center, Vicksburg, Mississippi
NAVFAC	Naval Facilities Engineering Command
NCEH	National Center for Environmental Health, U.S. Centers for Disease Control and Prevention
NTD	neural tube defect
PCE	tetrachloroethylene, tetrachlorethene, PERC® or PERK®
PEST	a model-independent parameter estimation and uncertainty analysis tool developed by Watermark Numerical Computing
ppb	parts per billion
PVC	polyvinyl chloride
SGA	small for gestational age
Surfer®	a software program used for mapping contaminant plumes in groundwater
TCE	trichloroethylene, 1,1,2-trichloroethene, or 1,1,2-trichloroethylene
TechFlowMP	a three-dimensional multiphase multispecies contaminant fate and transport analysis software for subsurface systems developed at the Multimedia Environmental Simulations Laboratory (MESL) Research Center at Georgia Tech
TTHM	total trihalomethane
USEPA	U.S. Environmental Protection Agency
USMC	U.S. Marine Corps
USGS	U.S. Geological Survey
USPHS	U.S. Public Health Service
UST	underground storage tank
VC	vinyl chloride
VOC	volatile organic compound
WTP	water treatment plant

P R O C E E D I N G S

(8:15 a.m.)

HOUSEKEEPING RULES

DR. CLARK: Morris has got a couple of things that he wanted to go over, sort of general issues. One thing that we had talked about is ~~if I don't know~~ [–ed.] whether Scott can finish his presentation perhaps during lunchtime if that would be possible.

How long would it take? About 15 minutes or so to --

DR. BAIR: Ten or 12.

DR. CLARK: Okay, we'll try to work that out because I think you were right at the point, sort of the punch line, and we sort of missed that, very interesting.

Morris, you have a couple things you want to say?

MR. MASLIA: First of all I wanted to thank Barbara for bringing in the biscuits and all that this morning. That was a welcome treat, and Rene, ~~and Rene,~~ [-ed.] and also our staff, Kathy Hemphill, Rachel Rogers and Liz for administrative help in getting things going.

Second of all, for those who are turning in or traveling on ERG’s money, you can mail in your receipts to Liz when you get back or e-mail them or however you want to do that. Then thirdly, and perhaps this was a misunderstanding but hopefully we can clear it up to this morning. I wanted to make sure everyone understood that the notebooks and the materials that were sent to you were not intended to imply they were anywhere near completion.

I think that impression may have been observed because we gave a time schedule and it showed we were planning originally to be finished by December of 2009. So that was not the intent. I apologize if that message sort of came about to appear and to sort of demonstrate we talked a lot about Table C-7 through C-13 yesterday so I printed them out for you.

And if you go to any one of the tables, even the last table, you’ll see that it takes seven, eight, nine, ten, 12, a dozen, couple dozen files just to compile the data for one table. So the files are massive to go through, and so this was sort of our compilation of data that we had completed.

And it was not intended to imply that we are ready to send this thing out for clearance or peer review or anything like that. It was really to get your feedback, and in fact, feedback in terms of the timeline and everything else. So hopefully, that clears that up, and I think that is about all.

We really want to try to stick to the schedule. We did pretty good yesterday. Today, because I know some people have some near five o'clock or six o'clock flights, so we do want to do the final round of input from the panel, which we're looking forward to the recommendations to the Agency that, I believe is scheduled to begin at 2:30.

So with that, that's all I have to say, and Mr. Chair, I will -- oh, and they have asked us, we are having audio problems if you're watching it on streaming, and they've asked that you clip the remote onto your belt and the lapel up here, not hang this in a shirt or in your pocket or anything like that. So with that, we're up, is Jason ready?

RE-INTRODUCTION OF PANEL AND SUMMARY OF DAY 1 ISSUES AND DISCUSSION

DR. CLARK: One thing I want to do just for the record is go around the room and have everybody give their name so we know who's in attendance officially. So I'll start with Randall.

DR. ROSS: Randall Ross, U.S. EPA.

DR. KONIKOW: Lenny Konikow, U.S. Geological Survey.

DR. GOVINDARAJU: Rao Govindaraju, Purdue University.

MR. HARDING: Ben Harding, AMEC Earth and Environmental.

DR. CLAPP: Dick Clapp, Boston University.

DR. POMMERENK: Peter Pommerenk.

DR. WARTENBERG: Dan Wartenberg, Robert Wood Johnson Medical School.

DR. BAIR: Scott Bair, Ohio State University.

DR. ASCHENGRAU: Ann Aschengrau, Boston University.

DR. DOUGHERTY: Dave Dougherty, Subterranean Research.

DR. HILL: Mary Hill, U.S. Geological

Survey.

DR. GRAYMAN: Walter Grayman, Consulting Engineer, Cincinnati.

DR. CLARK: And I'm Bob Clark.

We're going to start off this morning with a discussion of water distribution system modeling. Heard a lot about groundwater yesterday.

Jason, you're up.

WATER-DISTRIBUTION SYSTEM MODELING

MR. SAUTNER: Can everyone hear me? Is this on? Is that better?

Today I'm going to talk about the historical reconstruction of the water distribution systems, and just as an overview I'll go over some background. I think many of you have a good idea about the background from discussions yesterday, and then I'll go into more of the water distribution system modeling. It's going to be an all-pipes calibration. I'll go into the interconnection, which is going to be a big topic, of transfer of water between systems. And then I'll go into some historical reconstruction and talk about some preliminary scenario results.

Overall, the water treatment plant service areas, we have Hadnot Point, which everyone knows about. It’s 74 miles of pipelines. Approximately 71 percent of it is PVC. There’s four elevated tanks. The controlling tank is SFC-314, which is right down in here in this area. All of the elevated tanks are 300,000 gallons. Delivered water is approximately 2.3 million gallons per day in 2004.

And then we have the Holcomb Boulevard system up here. It’s about 73 miles of pipelines, approximately 67 percent cast iron. There’s three elevated tanks. The controlling tank is right up here. It’s Paradise Point S2323. It’s a 200,000 gallon tank. And the delivered water in Holcomb Boulevard was approximately one million gallons per day in 2004. And there’s two interconnections which we talked about. The Wallace Creek, which I guess now we’re going to call the Marston Pavilion to avoid confusion. And that’s the bypass valve located right here. And then we also have booster pump 742, which is a 700 gallon per minute booster pump.

Some significant events that occurred between 1941 through 1987: In 1941, the Hadnot Point water treatment plant comes online, which is located right here. In 1952, the Tarawa Terrace treatment plant came online. I don’t have the Tarawa Terrace water distribution system model on here, but it’s located right up here. And in ’72, the Holcomb Boulevard water treatment plant, located right here, came online in June of ’72.

From November of ’84 through February of ’85 is when most of the several supply wells were shut down due to VOC contamination. And January 27th through February 4th of ’85, there was about a nine-day period where the Marston Pavilion bypass valve was open continuously. In 1987, the Holcomb Boulevard water treatment plant was expanded to provide water to the Tarawa Terrace and Camp Johnson areas. And in 1987, the Tarawa Terrace water treatment plant was taken out of service.

As for the Hadnot Point water distribution model, it’s an all-pipes model.

We used EPANET. I think many of you are aware with EPANET and its capabilities. It simulates spatially distributed contaminant concentrations throughout the network, and it can perform extended period simulations of hydraulic and water quality behavior within the network.

The Hadnot Point model consists of about 3,900 junctions, about 4,000 pipes. And what we did was we conducted a hydraulic and water quality field test May 24th through 27th of 2004. During this test we collected and recorded hydraulic data, such as pressure and flow. And we also injected a calcium chloride and sodium fluoride at the water treatment plant, which was our source location. And we measured this continuously throughout locations in the distribution system.

Here are some calibration results. The Hadnot Point, the model was initially run using a single demand pattern. And this was obtained from a water balance on the distribution system. Eventually what we did was we used the PEST model to estimate eight different well, we aggregated eight different demand patterns throughout using the Water Conservation Analysis Report from 1999 in which they estimated water usage in different zones, and we allocated eight different groups. And by using PEST we estimated different 24-hour demand patterns.

The blue dots on this graph show the SCADA data, which is what we recorded in the field. It's actual water level data at SFC-314, which is the controlling tank at Hadnot Point. The red line is simulated data from the water balance, and the green line, which is a little difficult to see here, is the PEST water level simulation data. And you can see that the fit got much better by using PEST.

Over here we have some concentration graphs.

DR. HILL: With PEST what was it you were estimating? What values were you changing to create that fit?

MR. SAUTNER: The 24-hour demand patterns, and it was actually a colleague of ours, Claudia Valenzuela that did the PEST modeling. So we have a full report on it and details of how she conducted it.

Here is fluoride concentration just at a random logger that I chose in the system. You can see the blue line is what our continuous monitor recorded, and the red line is what we're simulating. And the same down here with the chloride concentration. The blue line still is field data from what we recorded on the continuous monitor, and the red line is the simulation.

MR. HARDING: Jason, can I ask you a question?

MR. SAUTNER: Yes.

MR. HARDING: On that, was that a four- or five-day period that you, yeah. Did you, if I recall what you said, you said you had eight different classifications for water demand --

MR. SAUTNER: Correct.

MR. HARDING: -- diurnal patterns, right? Did you use the same pattern? Did you calibrate one pattern that was used on the 24th, 25th, 26th or did you calibrate a five-day pattern that -- you see what I'm saying?

MR. SAUTNER: Yeah, that's what Claudia did. I'm not exactly sure of how she did the calibrations for the PEST.

MR. HARDING: What I'm getting at is if you calibrate an exact pattern for these five days, that's the best fit for those five days, you're not going to be able to extrapolate that to other periods of time when you don't have calibration data. You're going to have to have a pattern that you can use going back in time, and typically you have one 24-hour pattern for each category of use.

MR. SAUTNER: Right, and I'll get into this a little later. We assume that generally throughout both the distribution systems that the demand patterns didn't really change much. There was, I mean, historically. While there were significant changes that I showed you in that list of significant changes throughout the systems, overall demand in the systems didn't change much.

MR. HARDING: Yeah, that's fine, but I guess what I'm getting at is, is that if you are going to take a single 24-hour pattern for each of eight categories of use, then that's the way the calibration results ought to be shown. In other words the same pattern should be used on the 24^{th}, 25^{th}, 26^{th}, so on and so forth.

MR. SAUTNER: Okay, you're saying a 24-hour average of this.

MR. HARDING: Well, I don't know. You said you didn't know how she did it. Because you could fit it both ways. You could fit it to look at the, what is it, the five days -- I haven't done the math -- yeah, five days altogether or you could fit it to a single 24-hour period and then replicate that period. And that's what you're going to have to do --

MR. SAUTNER: Right, for historical extended simulations.

MR. HARDING: Right, so you just need to -- I don't know how you did it, and it sounds like you don't know, but the way you should do it is to do your calibration exactly the way you're going to do your extrapolated simulations.

MR. MASLIA: (off microphone) But the way the PEST model was run, because we've got all the files and stuff like that is we ran it for the entire period of the test. We put in what we thought were the initial ^ [diurnal patterns –ed.], and we did that based on five

^ [days –ed.]. Then we ran a test based on continuous water levels throughout the entire test period to go in and adjust ^ [the diurnal –ed.] patterns and we got a five-day length of time ^.

MR. HARDING: Yeah, and the problem with this is it violates Mary's first law, which is it looks scary. And it's too good a fit, right? And the reason is, is that you've fitted every hour of the water demand to the, and so what you should do, because you're not going to be able to do that in 1969 and '70. So what you should do, at least this is my recommendation -- Walter can weigh in -- but you should fit a 24-hour pattern for each category of use just like you started out with. But you're going to get one that's fitted, and then replicate that over the five days and see how your calibration works. That's what I suggest.

MR. MASLIA: But you have your data that you're measuring will vary over, during the test.

MR. HARDING: Right, it's going to vary. I mean, people don't behave exactly the same way each day, and when you look at, when you compare your idealized pattern to the actual pattern, it's not going to be the same in life. But this five-day pattern isn't going to be the same five-day pattern you see on May 24th of 1972, for example.

DR. GRAYMAN: Yeah, I agree with you, though what I'd like to see is that graph and then do the next step which take what would be the best repeating 24-hour pattern and see how that works. And I guess the other question on it is what does, the resulting best-fit demand patterns, do they look reasonable or are they, in effect, just --

MR. SAUTNER: Do you mean the demand patterns in terms of diurnal demand patterns?

DR. GRAYMAN: Yeah.

MR. SAUTNER: Yeah, they're all reasonable.

DR. GRAYMAN: But in the end you do want to come up with a repeating 24-hour pattern, which you can then use for future or past modeling.

DR. HILL: So on these other years when you don't have so much data, what data do you have?

MR. SAUTNER: Well, I'll get into the historical reconstruction later in the discussion --

DR. HILL: Okay, as you go. And just one thought about, you might do instead of a daily pattern repeated, you might do a weekly pattern.

MR. SAUTNER: That's one thing I also thought of because for the Holcomb Boulevard, which I'll show you next, we have a longer period of time.

So the Holcomb Boulevard system has about 4,800 junctions, 4,900 pipes, and we did a field test in which we just shut off the fluoride feed at the water treatment plant, at the Holcomb Boulevard water treatment plant. We shut it off and watched it drop down to background levels to about 0.2 micrograms per liter, and then we turned it back on and watched it go back up.

This test was, we did about a 21-day test with continuous monitors out there. You can see the date here is about September 23rd, 2004 through -- oh, I only have four days showing here, but the test did go from

September 23rd 'til October 11th or 12th. On this graph I just represented four days of data. And similarly, the blue dots are the SCADA data, which is what the operation rooms recorded. The red line was simulated from the water balance, and the green line was simulated from PEST.

DR. HILL: I'm sorry. I may have missed it. But how do you get the water -- what --

MR. SAUTNER: Water balance?

DR. HILL: The water balance, where does that come from?

MR. SAUTNER: That's just, it's taking what's stored in the tanks, how much water's delivered to the system, what the demand is on the system and during, you know, adding, subtracting and determining how much water was used in the system basically.

Is that an easy way to describe it, Walter?

DR. GRAYMAN: (off microphone) And then use a single common pattern, ^ [diurnal -.ed] pattern for all ^[days –ed.].

MR. HARDING: Yeah, that's the difference, Mary. They have one pattern, and then they're going to break it down to different categories of use.

MR. SAUTNER: Right.

MR. MASLIA: Jason, I think it's important to point out, and Mary, initially, where the patterns were derived from is each military installation had a water use survey done. They used a, a program was developed to really see how they could conserve, it was a conservation study. And the conservation study basically provides a gross amount on the average daily usage, what showers are being used, what swimming pools are being used. And so to start this effort off we derived initial estimates from those values to get the model going.

MR. SAUTNER: Thank you, Morris.

I know again it looks like this file lacks Mary's first law; however, I guess I should have chosen a different graph. This one is located close to the source so you're going to get better results right near the source.

You can see the, so now we have the date here from September 23rd through October

11^{th} around. And you can see the fluoride concentration's starting out around one microgram per liter dropping down to about 0.2 and then going back up to one.

Here's some, I guess this is a little misleading. It says PEST-derived demand factors is actually the allocations, the different categories that we used. The red is bachelor housing. There's a gray, which is the cooling system. The light blue is family housing. There's a heating plant, vehicle washing, office and work areas. And unfortunately, I don't think this is in the packet that you have of my slides. I added this one.

Now I'll get into some interconnections discussions.

MR. HARDING: Jason, so how did you then allocate spatially to the nodes, the base demand that you varied with your diurnal pattern? How did you allocate across the categories? Did you do a separate demand pattern for each node?

MR. SAUTNER: No, no, no. There's eight different patterns, so depending on what location, you know, each node was identified as, it would get a certain pattern.

MR. HARDING: Those were in actual use. I see what you're saying.

MR. SAUTNER: So now interconnections, which I guess is going to be a big discussion. As you know there are two interconnections, the Wallace Creek, which we're calling Marston Pavilion now, and the booster pump 742.

It was originally thought that Marston Pavilion bypass valve and the booster pump 742 were operated only on very rare occasions and solely for emergency situations. However, additional data discovery and discussions with both former and current water utility staff have led us to believe that historically water was transferred from Hadnot Point to Holcomb Boulevard more frequently than originally thought.

As previously mentioned, the Marston Pavilion bypass valve was not easily accessed so it was not typically open long enough to be considered a significant source of water transfer. Basically, the historical scenarios that I've constructed, I don't open the bypass valve; however, through suggestions we can open it and run different scenarios just to see how the water reacts going through there.

As Ben pointed out, I think he alluded to yesterday, if you were to turn on the 700 gallon booster pump, and you had that bypass valve open, water is simply just going to go right back down. And I saw that. I ran a scenario. Exactly what you said happened.

However, there was that about a nine-day period from January 27th through February 4th where that Marston Pavilion bypass valve was open for about nine consecutive straight days, and from the logbooks and discussions with the water utility staff, we determined that booster pump 742 was generally used during late spring and early summer months to account for irrigating the Scarlet Golf Course.

There was actually two golf courses loaded up, located in Holcomb Boulevard, and that created such a demand on the Holcomb Boulevard system that water needed to be sent from Hadnot Point to Holcomb Boulevard.

DR. GRAYMAN: Jason, can I ask a question?

During that long period, what was it, nine days?

MR. SAUTNER: Uh-huh.

DR. GRAYMAN: Was the booster pump running, too?

MR. SAUTNER: That's another thing, I'm not sure of. Logbooks, we were told that whenever the bypass valve was open, the booster pump was always running first. If the booster pump couldn't supply enough water, they would open the bypass valve. I don't understand, as what I just discussed your scenario of if you have the booster pump pumping and you open the valve, water's simply going to go back down.

MR. HARDING: Well, nobody could see the way the water's flowing. There's no instrumentation or anything to reveal this, so people misunderstood the value of opening the valve, and it actually was a counterproductive action. So it would cause the penetration of the water from the booster pump to happen much faster. Right, Walter?

DR. GRAYMAN: Well, I think we need to establish, I assume there are pumps at each of the treatment plants essentially that are pumping the water from the treatment plant up to the tanks, which is the gray line in those two. And I'm guessing that the gray line is probably fairly similar between the two or the normal water levels in the tanks are they the same in Hadnot Point as they are in Holcomb Boulevard?

MR. SAUTNER: I believe they're fairly the same.

DR. GRAYMAN: And so then they'll put the booster pump on just essentially it's dedicated to moving the water from the treatment plant in Hadnot Point into the Holcomb system. And so whether the direction the water's going to be going if they open the bypass is really going to depend on what the water levels are in the two tanks and what the demands are. So you may not necessarily get a circulating system.

MR. SAUTNER: Right, if you had lower levels in the Holcomb Boulevard, you would have higher levels, and higher levels in the Hadnot Point tanks, you would have water pressure --

MR. HARDING: For any sustained operation eventually you'll get to the point where the flow is coming back through the valve. I can't imagine any other --

DR. GRAYMAN: Except when they turned off the Holcomb Boulevard treatment plant which is what they did right there in this case.

MR. HARDING: If there was an enormous demand, that's right. But your model will tell you this. The model will answer this question pretty well.

MR. SAUTNER: And to answer your initial question, logbooks indicate that the bypass valve was open. They never mention anything about the booster pump during this nine-day period. Typically, logbooks were pretty consistent and had good information on what was open and what was closed. However, during this nine-day period it does not indicate whether the booster pump's on. I can run different scenarios for both open, just the bypass valve open, you know, see how it reacts.

MR. HARDING: It should be fairly clear because if your tanks are really, if your heads are going down, if your grade's really low, it would probably not be tolerated and so they were probably running the booster pump, and that seemed like that was their normal mode of operation.

And where's the second, I found one of the golf courses. Where are the two golf courses? One was in Hospital Point --

MR. ENSMINGER: Both of them are there.

MR. SAUTNER: Both located in there.

MR. HARDING: Oh, okay.

MR. ENSMINGER: One's on one side of the street, and the other one's on the other side.

MR. HARDING: Okay, I didn't count the holes.

MR. SAUTNER: So as far as the interconnections, from the Camp Lejeune logbooks. We have information from 1978 through 1986. There are a few data gaps. You can see here in '79 we have no information, in '81, '82 we have no information.

The booster pump 742 operations, it's a 700 gallon per minute rated capacity during the study timeframe. That was later replaced with a 300 gallon-per-minute pump, and it's currently out of service. It was operated mostly in late spring to early summer, April, May, June, July, and it was operated more frequently in the mid-‘80s as you can see here than it was in the early ‘70s.

I’m sorry, this is the number of days that it was operated for each month. You can see in the early ‘70s it was operated seven days, one day, three days in 1980. And then towards the middle ‘80s you can see it operating a lot more.

MR. ENSMINGER: I have a question. What is the, we understand that there was a valve right there at Building 670, the Holcomb Boulevard plant, that could be opened right into the treated water in the water treatment plant that was inter-tied to the Hadnot Point system. And from the discussion I had with a former water treatment plant operator, he said they could transfer water from the Hadnot Point system without running the booster pump from the elevated tanks, just gravity flow.

MR. SAUTNER: I don’t believe that there’s a --

Joe, you might be able to help me answer this question.

-- I don’t believe that there was an

interconnection directly to the Holcomb Boulevard treated tank.

MR. HARTSOE: There's check valves in the ^ [Holcomb Boulevard –ed.] pump room that would prevent it from going back to the treated water reservoir. The only connection I know that he's talking about would be the 12-inch line coming from the booster pump. There was a bypass --

MR. SAUTNER: But that doesn't run directly to the treatment plant. It runs to the intersection but not to the treatment plant. It runs into the distribution system and not directly to the treatment plant.

MR. ENSMINGER: Where was that valve that opened and closed that 12-inch line?

MR. HARTSOE: Well, you had cut-off valves between the booster pump and Holcomb Boulevard, but if you have the valve shut off in the booster pump itself, then the pump was off. So there was no way to go back. Somebody had to either go in there and open up a valve inside the building itself and cut the ^ [valve to –ed.] booster pump ^ [742 –ed.].

MR. ENSMINGER: Well, would it be possible for somebody to take a short cut and leave that valve open at the booster pump and just shut the valve up at the plant off at the intersection there?

MR. HARTSOE: We never messed with that valve. I don't know of anybody messing with a valve there. It would still have to go through the pump, some way it would have to gradually feed through the pump and --

UNIDENTIFIED SPEAKER: And the flow would be so low that it probably wouldn't really make a big difference ^, because that's the reason why you have a booster pump that's to transfer a large amount of water.

MR. HARTSOE: I don't know who would have cut the valve~~,~~ ^[on –ed.].

MR. SAUTNER: So the next graph is going to be occurrences of the bypass valve openings, the number of days. As far as the logbooks are concerned, there's no openings all the way until a first occurrence which was the nine-day continuous opening on January of '85. And then beyond that nine-day period it's opened only a handful of times. One day here, four, three and one day here.

This is kind of just an overall summary graph of the hourly operation of booster pump. It’s a little difficult to see on this scale since it goes from ’78 all the way through ’87. It’s zero hours to 24 hours, and this is just simply when it was turned on or when it was turned off. To zoom in and get a little bit better of a picture this graph right here to the right is May of ’86, and you can see this is the one that was used most frequently. I think it was used about half the amount of days of the month. And we averaged, it was used from about nineteen hundred hours to twenty-four hundred hours.

So we came up with some different scenarios. As I said, it was operated most frequently in May of ’86. The hours of operation according to the logbook are nineteen hundred to 24 hours, and it operated about half the days during the month, and that was in May of ’86. Then we also came up with just a typical May of 1980 case. The average hours that it was operated was seventeen thirty through twenty-three forty-five, which is about 5:30 p.m. to 11:45 p.m. And it operated about three days during the month. And we confirmed with Camp Lejeune former and current water utility staff that they would typically shut the valve off at twenty-four hundred hours when the operator's shift was over.

MR. ENSMINGER: You mean the pump.

MR. SAUTNER: What did I say?

MR. ENSMINGER: Valve.

MR. SAUTNER: Valve, yeah, sorry. Booster pump 742. Sorry about that.

Just to refresh your memory on the water distribution systems now. On the Hadnot Point system, the treatment plant's right here, the controlling tank down here. And then we have the Holcomb Boulevard system with the water treatment plant right here, the controlling tank over here. Golf courses. We have Berkeley Manor, which will become important in terms of the historical reconstruction simulations. Berkeley Manor is right here with an elevated tank right here.

And another important thing is to know that the golf courses during this timeframe were irrigated with potable water which is what created the big demand on the water distribution system. And we also have our two interconnections, which is the Marston Pavilion bypass valve, and the booster pump 742. So again, remember these are all preliminary results, nothing's finalized.

We have our first scenario which is no interconnection. This was done as the May 2004 extended period simulation so there'll be no water transfer between Hadnot Point and Holcomb Boulevard. This is controlling tank S-2323, which is the Holcomb Boulevard controlling tank. And you can just see extended period simulation simply fluctuates all the way out 744 hours, which is 31 days.

Now, we did some interconnection scenarios. This is May of '86 where it's open every other day. The booster pump was pumping every other day, nineteen hundred to twenty-four hundred hours, and you can see it cycling every other day. And we also have our third scenario which is May of 1980 which is the green line. And you can see fluctuation three days in the middle of the month which is when we planned it to operate.

So now our concentrations in the controlling tank for Holcomb Boulevard, no interconnection, there’s obviously no transfer of water from Hadnot Point to Holcomb Boulevard. But it was open every other day in May of 1986, there was still no transfer of water to the controlling tank. And then obviously if it was only three days, there was no transfer of water. So no concentration was making it to the controlling tank in Holcomb Boulevard from the Hadnot Point water distribution system.

Now however, if you look at Berkeley Manor tank with no interconnections you can see the water level fluctuating. With the interconnection open every other day in May of ’86 you can see it fluctuate every other day. And when it was open three days in the middle of the month, similarly just three days of fluctuation right here.

When we look at the concentrations, and this is assuming just 100 micrograms per liter or 100, I guess it would be considered units, just to get a percentage-wise, to get a feel for how much water from Hadnot Point went into Holcomb Boulevard, with no interconnection no water transfer, zero concentration.

With the interconnection every other day you can see concentrations build up in the tank at Berkeley Manor. When it was open three days in the middle of the month, the green line, you can see the three steps in the very middle of the month, and then there's no more water transfer so the tank has concentration in it and then you just see it start to dilute out.

Interesting thing is, is that this is for May of 1980. If you were to do, go ahead and simulate June of 1980, you would have to put this concentration in as a starting point.

Overall this is just a figure to look at the distribution of the concentrations throughout the systems. With no interconnection all the water stays down in Holcomb Boulevard -- I'm sorry, in Hadnot Point. And there's zero water transferred into the Holcomb Boulevard system. With the interconnection -- again, these are all just averaged out. So instead of running, well, with running the extended period simulation, instead of looking at over time, every value was just averaged.

So with water connection in May of 1986 conditions, you can see no water in these areas. Again, the yellow dots are zero-to-five percent and the orange dots are five-to-20 percent. So you can see on average in the Berkeley Manor about, it actually comes to about 22 percent water, well, 22 percent was averaged in the tank. Overall the system it's about 20 percent around these nodes.

And then with the three days in the middle of the month when it was open in May of 1980, you see no water transferred in this area. You see a few areas in here where you're going to get between five and 20 percent of water from the Hadnot Point system.

So future considerations that we have for this are to try and develop some historical trends, explore using climatic data which is directly related to when the golf courses were irrigated along with the known booster pump 742 operating conditions from 1978 to 1986 to try to estimate historical booster pump operations from 1973 to 1977. Remember, we don't need operations from '68 to '72 because Holcomb Boulevard received all of its water from Hadnot Point. So it was really only a five-year period that we're missing data right here on booster pump operations.

Some other considerations for historical reconstructions, we have actual data so instead of maybe doing an average condition for May of '86 and saying that the booster pump opened at nineteen hundred hours and closed at twenty-four hundred hours, we have the actual data on a daily basis and an hourly basis of when the booster pump was open and when it was closed. We could actually put this into the model and still run it as an extended period simulation.

We also want to run some scenarios where I include Marston Pavilion bypass valve opening into the historical reconstruction. As I was discussing with Ben, I've run some preliminary simulations. It appears that there's little influence in the Holcomb Boulevard area when the bypass valve is open.

And that's mainly because there's, I

guess it would be more influence in the Hadnot Point area. Water kind of goes from Holcomb Boulevard to Hadnot Point rather than going from Hadnot Point to Holcomb Boulevard. This can be changed also as we discussed with varying tank levels to create different pressure variants.

And also want to run the scenario where the nine-day event from January 27th through February 4th of 1985 with the bypass valve open continuously. And with that I'll leave it open to questions.

PANEL DISCUSSION: WATER-DISTRIBUTION SYSTEM MODELING

DR. DOUGHERTY: Remind me about 1972 and why there's no consideration in the second half of 1972.

MR. SAUTNER: In 1972 that is when -- correct me if I'm wrong -- isn't that when Holcomb Boulevard, in June of '72, Morris?

MR. MASLIA: June of '72 is our best estimate of when the Holcomb Boulevard water treatment plant came online.

DR. DOUGHERTY: So the assumption is that --

MR. SAUTNER: Prior to '72 it was receiving all of its water from Hadnot Point.

DR. DOUGHERTY: I understand, but there was no interconnection you had to worry about between the start up, which probably would be ~~pre-transferred~~ [pre-transfer –ed.] to the Department of Defense, and --

MR. SAUTNER: And so you're speaking the actual June of 1972, July of '72. Yeah, I suppose I could change that figure to be '72 through '77 and use, there would be no transfer, well, it would be all Hadnot Point water for April, May of '72. June/July we might want to also find historical --

DR. DOUGHERTY: Right because it does generate an additional exposure potential.

MR. SAUTNER: Correct.

DR. POMMERENK: Jason, for these very short-term interconnections in your illustrations here, you used 100 micrograms per liter as the mass and as coming across the interconnection. What are you planning on using for the historical reconstruction? Are you going to use the monthly mean that you get from your groundwater model or, because, you know, obviously these concentrations can change on a daily basis in the system.

MR. SAUTNER: You're talking about concentration input for the model?

DR. POMMERENK: Yes.

MR. SAUTNER: Well, we're not at that point yet, but one way to do it is to whatever number they get from the groundwater model, whatever number they give me, I put it in as a simple, we have a start, you know, they will give me a date, a time when the concentration was like that, and that will go into the model as is.

DR. POMMERENK: Okay, but I want to caution because we're going to have a monthly average concentration. In reality, of course, the concentrations can change on a daily basis. And if you look at Table C-13, it nicely illustrates how Building 20, which is the Hadnot Point plant is 900 micrograms per liter TCE, another day several days later 430 and then another day later non-detect which means within the distribution system there will be also considerable fluctuation.

Now, I guess from an epi standpoint, if you're using the mean that's fine for Hadnot Point. But for the short-term interconnection, you need to have some idea of how much is going, how much mass is across going across that interconnection during the six hours or whatever that pump was on in order to determine what the exposure will be downstream. Because you cannot simply assume it was mean concentration because it may have been zero or may have been a thousand ^ [micrograms per liter during –ed.] interconnection.

MR. SAUTNER: I don't think that there's any way we can tell that though. I mean.

DR. POMMERENK: That's my point.

MR. SAUTNER: Well, it's going to end up being an average. I understand that you're talking about a short period interconnection. We have what information we have. So I can run different scenarios and --

DR. POMMERENK: Yeah, I mean, I think it's going to be a stochastic problem though. Of course, you don't know but that's my question. How are you going to approach this in terms of uncertainty which is again what, I guess, the epi study's looking for since you don't know but you need to provide some kind of measure of how certain is your, of your exposure modeling results. How are you going to account for the fact that it could have been during the six hours of interconnection that the source could have had non-detect or 2,000, that’s what I’m --

MR. SAUTNER: Yeah, I guess we’ll cross that bridge when we get to it and discuss more later. That’s probably a discussion for the panel to help determine. Maybe we could run some Monte Carlo simulations or --

DR. GRAYMAN: You’re right in terms of there’s both stochasticity due to the source term at the treatment plant plus a great deal in terms of when the booster pumps were on. And I think you do have to consider both of them. But it’s, I mean, the amount of information you have in terms of exactly what the source concentrations are going to be at any given time, how they’re varying around the mean and also when the actual booster pump was turned on and off, especially in this three year period where you have no information. You’re really going to have to do it in a probabilistic manner.

DR. CLARK: We had a question from the audience back here I think.

MR. HARTSOE: Let me clarify something. I may have to get back with you on some of it. I was thinking about what Jerry said about a valve. I was thinking about what Jerry was saying about a valve at 670 cut on. And during that timeframe when the reservoir was contaminated with the gas leak, 670 was shut down, but water was still supplied through that 12-inch line.

Jerry is talking about to 670. I mean, it was being delivered water to 670, but 670 was not pushing any water out because the reservoir was cut off. The water would not go back to the reservoir because of the check valves on the high-lift pumps, and I'm wondering if what they were talking about when they say a valve, during that time when we put the, when we were putting the reservoir back online and having to fill it up and took all sorts of tests after that to make sure the water was good enough to drink before we sent it out.

We did have times when they probably

had to backwash a filter. And there is a valve on the outside of the reservoir that you had to, you could cut on, and that would be coming from Building 20. So that may be what valve -- I'm not sure and I'll have to get back with you. I could see where they would open that valve just to backwash the filters.

UNIDENTIFIED: And that's what I recall as well.

MR. HARTSOE: I mean, I can get back with you --

MR. SAUTNER: We'll get together in the future and discuss the --

MR. HARTSOE: And, Jerry, that may be, I don't know of any other valve they could cut on but that one. So I'll be glad to get back with Jason on that.

MR. ENSMINGER: And this other question about the contaminant levels when the booster pump was running and whether what the contaminant, the idea that you didn't really know what the levels were of the contamination. Well, we only have one test that shows what those levels were, and that was the split samples taken by the state which

I gave all of you in your packet of documents there. The analytical results showed the levels in the Holcomb Boulevard system.

DR. CLARK: Dave, you had a comment.

MR. ENSMINGER: And that was one of them that showed 1,148 parts per billion of TCE at the Berkeley Manor housing area's elementary school.

DR. CLARK: Dave, you had a comment?

DR. DOUGHERTY: It was just a question on, and I'll reference Table C-13 kind of as an example. Do we know the sampling protocol for this 1985 data? These, just to get it right.

MR. FAYE: What was that question again? I'm sorry.

DR. DOUGHERTY: Do we have a sample protocol for the 1985 data from taps and those sorts of things? In other words are these --

MR. FAYE: Protocol as to what?

DR. DOUGHERTY: The sampling protocols, how the samples are actually taken.

MR. FAYE: No, but I suspect from earlier information that in terms of the sampling, which is not really that definitive, in late 1984 samples were collected in glass bottles, iced and shipped to the laboratory.

DR. DOUGHERTY: How were they transmitted into the bottle?

MR. FAYE: I think it was just you open up the tap. You fill up the bottle.

DR. CLARK: You're thinking of the volatilization issue I presume and the loss of contaminant because of that sampling.

MR. FAYE: Oh, yeah.

DR. DOUGHERTY: I'm thinking of that and then in terms of for using these as part of the calibration targets that these may be considered somewhat less than an actual --

MR. FAYE: Sure, and also I think the issue that, the main issue is determining at the beginning of this process, when Hadnot Point was actually turned on to supply all of Holcomb Boulevard, we don't really know what the concentrations of the various, TCE for example here, were at Hadnot Point at that time.

But we know, number one -- well, first of all, we know all the wells that were pumping at this time. We know all but one of the contaminated wells was turned off at this time. And we do have concentrations in the contaminated well at this time at the beginning, which would be 651. So actually, you could just do a simple mass balance. And we know the pumping rates.

So we could just do a simple mass balance and estimate what that source concentration was at the beginning of this intervention. So I don't really think that's an insurmountable problem.

DR. CLARK: But I think you're correct. As I recall at that time sampling was an issue particularly for inexperienced utilities who were just beginning to learn how to take volatile samples of THMs and the VOCs as well. It's a good point.

MR. HARDING: What's absolutely critical about understanding the sample is the time of day and the, really what's important, it's 100 feet from one of the tanks. I can't remember the number, I think. Looking at it on Google maps. Whether that tank's filling or emptying has a profound impact on how you interpret the sample.

If you remember Scott's little diagram

of how the plumes move, well, it happens the same way in a water distribution system. I mean, water flows downhill or down gradient, however you want to think about it, but it happens much faster. Your divide shifts can happen in a matter of minutes, you know, the switch from flow direction can change in a matter of moments.

And so the exact moment you took this, the snapshot of conditions at that moment matters a lot. And we can't ever get that exactly right, so you have to keep that in mind when you're trying to calibrate a water -- you have way more measurements out in the system than I have ever had. I've got the luxury of maybe two or three samples out in the system most of the time. You've got this wonderful fluoride calibration stuff.

I mean, you should be able to do a pretty good job of getting a model that's reasonable. You shouldn't try to fit it perfectly because -- I'm going to talk about this a little bit later -- you're over-fitting your water demands right now, and we have to back off from that.

But what I wanted to do was address Peter’s comments about the variability, and in part it’s this how incredibly dynamic a water distribution system is, and how you could have a sample at 8:00 a.m. and a sample at 2:00 p.m., and they could be completely different depending on which source happened to be supplying that node.

But just thinking out loud conceptually what you need to do is you need to have a, you’re going to have a groundwater model that gives you wellhead concentrations. This is a term I use. This is that average, vertically average, concentration on a monthly basis. And then you have to have a model of your well dispatch -- I’ve talked about this several times -- that will bring the water together into your unpressurized tank that then is at the water treatment plant. And this may or may not require a hydraulic model because of the differences in head at the different wells and the pump curves. You have to decide that.

And then you’re going to have the rest of your water distribution model which you’ve seen. And you're going to have to model this concentration all the way through. You're going to have one model that's integrated together and it'll have to be stochastic because you don't know how they operated the wells absolutely, and you're going to have to make a model.

But you can inform that model with standard operating procedures or human tendencies. And we've done the same sort of thing before, you just have to do your best, but you have to recognize the uncertainty and quantify it. So I don't know, Walter may want to add to that.

MR. MASLIA: Ben, can I just clarify something because what you've said is absolutely correct, but we're not going to be getting that complex. From the start we made a decision not to model the actual transfers of water within the distribution system or from the different wells in other words. If the wells mixed in a single tank we would get that single concentration. If not, we would take the concentration on the finished water side of the treatment plant. Now, in this particular, a case like in Table C-13, and I agree with you, I mean, throughout all the data we have, except for the data that we collected, we have no time data. This is, if you put that together with the fluoride data that we gathered, I think we've got a very rich set to calibrate and test to. In other words so you've only got one well pumping during this period, and that's 651.

UNIDENTIFIED: Only one contaminated well.

MR. MASLIA: Only one contaminated well pumping. To me it would seem to be, to use this if you want to either verify the calibration that we already have based on our current field data and then try to model this and see what it would take in terms of either well combinations or opening-closing valves to try to duplicate this.

MR. HARDING: Just as a general comment, you guys focus too much on calibration and not enough on the practical question of how you're going to go back and extrapolate out the periods when you don't have enough information. It's wonderful to get your model to fit and then you violate Mary's first law.

But you have to think about how you're going to get a realistic model, a reliable model that goes back in time to 1972 and 1976 when you're not going to have any information.

And that's why I'm saying, and which well is on. I mean, obviously -- I can't remember all the numbers, but 651 was the real bad boy here, right? If 651 isn't on, no problem, right? Well, let me step back and say something about that in a second. But if it's on, then you've got big problems.

Now, one of the things that Jason illustrated up here is the reason why you have to do really long-term, extended-period simulation because that trace went off the end of the month. And typically what we would do is we would run a year at a time, continuous simulations, and then we would initialize the next year with our tank concentrations and even our pipe volume, the mass that was in the pipes, because the pipes can store a substantial amount of water and contaminant.

And so you'll have a memory in those tanks. It is the memory of the system, and you really have to respect that. If the tank at the school there was discharging at the time you took that measurement, that means your tank had a milligram per liter in it. If it was filling it, and it was getting initialized with a milligram per liter. So I just want to make that point.

But you really have to think about how you're going to go back and not worry so much about getting a trace that looks really, really nice. But figuring out how you're going to get a realistic and reliable model and go back.

DR. HILL: In order to do that, and in order to get an analysis of uncertainty it would be really nice to use the dataset you do have and do cross-validation where you'd leave off the, use your different, but instead of leave one out, leave a whole period out. And then go ahead and calibrate however you want to to your one set, and then look to see how well you do when you come back to the set that's not included in your calibration.

And you're going to want to use, for those periods you don't have information, you're going to want to use the method that gives you the best power in that cross-validation test. And that cross-validation test will give you a measure of how well you do when you don't have data.

And that's your uncertainty analysis so you don't go back and do Monte Carlo, you actually have an evaluation of how well you do when you don't have data for the period of interest. So it'll probably be faster than what you're doing now in terms of an uncertainty analysis, and it will have a better statistical background.

DR. GRAYMAN: I just had a comment on what Ben said. First of all, I'd turn it around a little bit. What I'd say is you're probably in a much more fortunate situation in terms of having a better intrinsic model of the distribution system than is normally the case in any of these. So what it's done is it's reduced the uncertainty in that part of the model, so that's good.

But then carrying on that's a starting point. We still have all of this probabilistic analysis has to be done for the source concentration for the operations. In terms of what Mary said, I'm a little concerned, and I guess I don't fully understand what information you have, what water quality information you have in the distribution system. It just seems to be very anecdotal still.

And so anything where you did an analysis, where you tried to calibrate the model and match this, and I'm not talking about today's --

DR. HILL: I wasn't talking about the concentration data. I was talking about the pumping schedules. In terms of your concentration data, I mean, what was done at Tarawa Terrace is to just throw all this raw data at the groundwater model and say fit it, when, if you looked at the data, there was absolutely no, you weren't providing a function that was consistent with the data.

Now, what the inconsistency was there I don't know, but you need to think about the concentration data in the context of some of the things people have brought up. Because it's pretty clear, I mean, things change so much day-to-day, there's something going on with the collection activity or, and I don't know those processes enough, but this data needs to be evaluated with that in mind first and altered.

So if these are all biased low because of processes you know occurred, there has to be some adjustment to those. If you throw this into the regression, it just tries, I mean, the models just try to match it, so you have to, that was one aspect that was presented by Professor Aral yesterday is that you need to really look at your data and try to develop, figure out what trends, your underlying trends, are involved there, not just throw the raw data at the model.

MR. HARDING: Let's be very clear --

DR. GRAYMAN: When you say this data, let's be very clear which data we're talking about.

DR. HILL: That was the concentration data I was talking about.

DR. GRAYMAN: The concentration data in the distribution system or from the sources?

DR. HILL: Well, I mean, you can calibrate the groundwater model on both of those. I think individual well data has been dealt with more frequently, and in either period -- I can't remember -- are there periods of time when we have distribution, we have finished water concentrations, and we don't have individual well concentrations?

MR. FAYE: I can answer that. The data to the best of my knowledge that we collected at several intervals, May of '84 was one where we were all out there, these were when we were injecting various --

Go ahead, Walter.

DR. GRAYMAN: Two thousand and four.

MR. FAYE: I'm sorry, 2004, yeah. We were all injecting the fluoride and some other, calcium chloride, into the distribution system. That was strictly an effort to calibrate the distribution system models. And then similar things were done for Holcomb Boulevard and Tarawa Terrace.

Now, there was no interest in collecting any well data at that time. There was, to the best of our knowledge, there were no contaminated wells active at that time. So this was strictly an effort to collect data to calibrate the water distribution system models, EPANET 2.

Now, to the only data that we have where a contaminated well or wells were operating and where contaminant concentration data were actually collected within the distribution system. Those data are all presented with respect to the distribution system on Table C-13, which you have in front of you now. The --

Excuse me, Mary, go ahead.

DR. HILL: I think that the issue is that if you have concentration -- I was going to say, if you have concentration data into the individual wells, I would think it would be better to use that even if at the same time you have finished water concentrations. But then I was thinking, well, maybe that's not the case because of the, there are so many contentious problems with the samples. Maybe it's not a bad thing to have duplication.

MR. FAYE: Let me just finish my thought, and then we can address what you're trying to say I think.

The only time that we actually have data coincident in time where contaminant concentration data were collected within the distribution system and when we have knowledge of the contaminated well or a well or wells being pumped, was for this nine- or ten-day period in late January and early February of 1985.

And those data in terms of the distribution system are presented on Table C-13. And the contaminant data at the individual wells are also in tables, well, it'd be Table C-7, basically, just Table C-7. And in terms of the actual WTP, that would be on Table -- help me here, folks, if you looked at it. That would be on Table C-11.

And we also have daily records of which wells were being pumped during this time and which were not so we can actually, but there was only one contaminated well at the time and that was HP-651. So whatever was going on, the other wells that were pumping were actually diluting HP-651. I mean, whichever ones they were, they were not contaminated or were very minimally contaminated, you know, as far as detection limits were concerned.

So those are the only data that we have where well data and distribution data were collected relatively simultaneously.

DR. HILL: And you don't have the pumping schedule. They destroyed those records, right?

MR. FAYE: Well, we know which wells were pumped on a daily basis, and because of the extreme conditions that existed at that time, it wouldn't be unreasonable to assume that those wells were just pumping 24 hours a day. They had to get the water into the system to maintain, to supply demand. So if those wells, you know, I think that would be a reasonable assumption.

DR. HILL: If you really, I mean, given that two-week period of time where you have this, you have measured concentrations at the wells, delivered concentrations, pretty good knowledge of the flow system, so you could use that as a test period, a really good test period for your entire system of modeling.

MR. FAYE: Yeah, to demonstrate the validity of the accuracy, precision, all the other terms that were used, we could demonstrate it as a test for that particular period of time.

MR. SAUTNER: And, Bob, also just to note. We have pumping schedules not just for that ten-day period. We have, I believe it's for two months, right around there, isn't it? December, January and February.

MR. FAYE: Right. So the whole process, I want to make a point again, the whole process is highly simplified because of the extraordinary condition that existed, that the wells were going full bore, full out to meet demand. We know the pumping rates at the wells, and there was only one contaminated well at the time that was pumping.

And that turned out to be one that was a real mess in terms of contamination. So it is sort of a fortunate situation where all this information happened to be -- and it was totally accidental as far as I can tell -- but it just turned out that that was the case.

UNIDENTIFIED: What were those days?

MR. FAYE: Basically from about January 27 or so of 1985 to February 11th, 12th, 13th, 1985. Something along those lines.

DR. GRAYMAN: I think it would be extremely useful to take that period and it's almost -- I'll call it an exercise, but that's a little bit pejorative -- but that you go through the exercise of seeing that the model can realistically match what happened during that one-month period. But unfortunately, it's such an unusual period that I'm not sure you're going to be able to gain much in terms of using that to simulate the other periods.

So it's almost going to be, it's going to be necessary that you be able to reasonably match it, but I'm not sure that that's going to be that useful in extending it for the rest of the 15-year period or 12-year period.

DR. HILL: You could use it as a test period, as a check period. Don't use it as calibration and do daily time steps.

DR. CLARK: We have a question back here in the audience.

MR. PARTAIN: Just an observation, on the May 1982 Grainger Lab report, actually, not the report is going to have that, but there was a sample taken from a point within the Hadnot Point distribution system. I believe it was Hospital Point and came with a reading of 1,400 parts per billion within the system. Can that not be a snapshot of what was going on in that system so you can compare it to what you got in 1985?

So you’ve got two different points separated by three years. One with a 1,400 parts per billion reading at the hospital and then later on the January ’85 testing within Holcomb Boulevard, and you’ve got the school at 1,100 parts per -- 1,148?

DR. HILL: You can. The thing about this other situation is you have a pretty good handle on every piece. You have the pumping, the -- and that’s what makes it so unusual. So the one you’re talking about I’m not sure that it’s a similar set of circumstances or not. I mean, maybe there is. I don’t know.

MR. PARTAIN: That was a ^ [water-quality – ed.] sample that they were doing and the lab technician took it upon himself to actually quantify the levels, and he came up with a 1,400 part per billion reading for ^[TCE – ed.].

MR. ENSMINGER: Yeah, and three years later you get 1,148 parts per billion of TCE in another sample, and it's about 300 parts per billion less than the '82 sample. Well, you had some other contributing wells that had been already taken offline, but you still had that one hot one online, 651.

MR. PARTAIN: And that same technician also noted that they had, they did that sample, went looking again, and it dropped off, and then several months later the technician has a conversation with the base supervisor chemist and says, hey, the peaks are back and they're high again, but it doesn't quantify ^.

DR. CLARK: We'll let Morris get a point in here.

MR. MASLIA: No, I've got a question actually for both the epi people and the water modelers.

Since the case or the set of data as has been pointed out for the January '85 date seems to be our most complete in terms of all parts of the supply and delivery system or distribution system that we've got information on, and we know one contaminated well, 651, was pumping being diluted by other wells, which we know were pumping going in there, could we not use that from the epi side, would you not consider that potentially a worst case scenario?

MR. HARDING: How could that be the worst? Oh, for Holcomb Boulevard.

MR. MASLIA: Did they pump all the contaminated wells at the same time?

MR. HARDING: I couldn't even --

MR. FAYE: No, you wouldn't consider that in terms of the groundwater pumping. You wouldn't even come close to considering that as a worst case scenario. Because you could have a situation easily where 651 prior to 1984, 651 -- or July '84, actually -- 651, 602, 608, 634 -- what others, could all be pumping at the same time, and they'd be dumping contaminants into the Hadnot Point WTP like there's no tomorrow, so that would be more of a worst case than just one contaminated well pumping.

DR. DOUGHERTY: The entire 1968 through '72 period which --

MR. FAYE: Yeah, from 19, yeah, and prior to, actually, 651 came online in I think 1970, but prior to that you certainly had a good number of contaminated wells that existed, pumping into Hadnot Point WTP and being distributed through the Holcomb Boulevard pipe system. So, no, I wouldn't --

MR. ENSMINGER: If you use just the January samples that would not be, another reason it wouldn't be your worst case is because all your benzene contaminated wells were offline by that point.

MR. FAYE: Oh, yeah, I mean, considering your individual constituents, yeah. You can go right down the line and be indicative of that. I'd say this 1982 sample that was brought up that's on Table C-11 at the hospital, 5/27/82, 1,400 micrograms per liter TCE, that -- I'm just kind of blowing smoke here -- but probably 651 was pumping then.

We don't really know, but that concentration is comparable to some of the January '85 concentrations. So there might have been a similar situation going on. But, yeah, in terms of worst case we really don't know, but I wouldn't say January of '85 was the worst case, just my thought.

MR. HARDING: You need to know to be able to make a statement like that, you need to know a lot, and you'd need to know where the water was coming from that was at -- I can't think of the name of the point, but the school.

MR. FAYE: Berkeley Manor.

MR. HARDING: You'd have to know, and it could be coming out of the tank. It could be a blend. And it's really hard to know. At Hospital Point it's going to be a little more stable I would think because it's sort of out on the --

MR. FAYE: Out at the end of the distribution system.

MR. HARDING: And I can't see well enough to see if there's a tank between it and the water treatment plant.

MR. SAUTNER: There is because here's Berkeley tank right here.

MR. HARDING: I'm color blind too so I can't see the pointer. So anyway, you can't make a blanket statement like that. This is why you build the model is to make this evaluation. And you have to -- I want to make a little editorial comment here -- you have to comfortable going out on a limb and making some subjective judgments about whether this is a reasonable model or not. You're going to have to do that because you just can't do everything based on data analysis, as Mary said. You're just going to have to test and come out with, it's a great tool I think, but you're just going to come out with something that's over-fitted.

DR. DOUGHERTY: Just a quick question on this early '85 data. So they have the measurements at the treatment plant, and we have measurements at wells, and we have pumping rates.

MR. FAYE: Right.

DR. DOUGHERTY: Have you just done the mixing calculation to see if the well concentration and the treatment plant concentration match?

MR. FAYE: No, as Morris hopefully clarified earlier this morning, I mean, this work that you all have in your notebooks here is very, very preliminary work, very early in the process of the project in terms of getting some definitive results. So we just haven't got to that point yet.

DR. CLARK: So there is a point, I think Ben's got a good point. You could use the one scenario to validate and calibrate the model and then add in other wells as you think they might have occurred during some of these maximum contaminant mixing scenarios. You can get a pretty good picture, I think, of what might be going on within the system.

MR. FAYE: Absolutely. And whether we want to use it as a sort of a test as Mary suggested or as part of a full-blown calibration, I mean, I think those points of view just need to be worked out in a dialogue amongst the staff and you folks and whatever. But, yeah, it is the only time, it is the only time where we actually can integrate the complete system, pumping wells and their respective models, the distribution system and their respective models and then look at the results.

DR. DOUGHERTY: I really encourage you to take the ten minutes and do the calculation to see if the mixing of the well data to the treatment plant in that period of time is self-consistent, and if not, it may give you some sense of some response error and hence a measurement error.

MR. FAYE: I agree, and it's neat because it is a fairly simple thing to do.

DR. CLARK: But one thing I haven't heard discussed is the potential for degradation. Has any of that been factored into the calculations at this point? We haven't really done those simulations either, I know, but it seems to me some of that could be important.

MR. FAYE: Absolutely. We know from Tarawa Terrace as far as the groundwater's concerned that probably degradation is a major issue. Within the distribution system, that I don't know.

DR. CLARK: Well, there's some pretty long residence time in some of those tanks. I haven't done the calculations, but if you're given vinyl chloride as an endpoint then you have a very serious issue.

MR. FAYE: Right, right.

MR. HARDING: I think the residence times are ^[important –ed.].

DR. CLARK: It could be degradation also. Well, like also, well, some of it may be degradation within the system, but I don't know. The times might be sufficient for degradation.

DR. KONIKOW: Well worth looking at it, but the residence time in the groundwater much, much, much longer than the residence time in the tank.

MR. HARDING: If this is a matter of triage I wouldn't spend very much time on worrying about degradation in the water treatment system. You've got lots of other good stuff you could spend time on here that's way more important than that. Don't focus on the details, focus on the big picture.

I want to ask some more questions about water use, because water use, you have continuity, and you have energy that balance in these models, and some of us think in terms of continuity, and some of us think in terms of energy, and the systems are different, sensitive in different ways. But in this particular case where you've got this big old golf course out there, and that's what's driving some of these interconnections. You know, understanding the pattern of water use is going to be important.

And I'm concerned that I haven't heard enough, I don't quite understand exactly what you've done during your calibration period, but more than that I don't understand your plan for going back and modeling this during the periods for which there are no data. And the way I've approached it, and I think Walter's done it the same way.

We first sort of load the nodes with a kind of a fraction of the water use on a daily basis. And then apply a unit-less pattern of diurnal water use. I'm sort of getting the sense that what you've done is you've fitted both the total daily water demand and the diurnal pattern, using PEST, and again, it makes a beautiful chart, but it isn't going to help you when you go back in time. I don't know if you have daily records of water production at the water treatment plant, do you?

MR. SAUTNER: Daily? Daily records?

MR. FAYE: Yeah, we do have daily records I think in terms of production. That was on one of my slides the other day, yesterday.

What is it, Jason, 2004 to 2008 and then there's '95 through --

MR. HARDING: No, I meant back in the time that matters.

MR. FAYE: No.

MR. HARDING: So you're going to have to come up with a pattern of use on a total system use and then you're going to have to disaggregate that to the nodes spatially. And then you have to disaggregate it with your diurnal pattern. And so those are some of the conceptual steps. I mean, you can throw up your hands and say we can't do it, but I've done it. Walter's done it. You have to do it.

DR. GRAYMAN: I'm not clear. I think you weren't sure either in terms of when PEST was done. Was it done just to give you these representative eight diurnal, say, normalized patterns? Or was it also to try to determine the quantity of water that was used, say, over that period?

MR. SAUTNER: No, I believe it was just done for the diurnal.

DR. GRAYMAN: Yeah, that was my

understanding.

MR. SAUTNER: The quality, we used the water conservation analysis study.

MR. HARDING: How does that get water to the individual nodes? How do you know how much water was used at or near the school in Berkeley Manor, for example, just as an example? How did you understand that from the water balance?

MR. SAUTNER: Well, from the water conservation study we had different categories of demand, whether they were bachelor housings, family housings, so we know Berkeley Manor is a family housing area. Most of the demand nodes in that area were assigned.

DR. GRAYMAN: Okay, so the equivalent of having a meter, an annual meter.

MR. HARDING: That's good. That's good.

DR. BAIR: That's great, and I misunderstood that because I thought you were fitting --

MR. SAUTNER: I'm sorry. I wasn't clear, I guess.

DR. BAIR: No, that's the way, that's conceptually the way it should be done. And then but you're going to have to come up with a set of patterns that are either constant or respond to certain rules. For example, Mary suggested doing it every day of the week. It's probably not going to help you much, but you definitely want to take into account weekend days, for example.

On your golf course you know they're not going to water the golf course at two o'clock in the afternoon, right? You know they're going to water it at night --

MR. MASLIA: Actually, that's not correct. Ben, seriously, they water it when the general calls up and says he wants to have a tee-time, and then they turn it on.

MR. SAUTNER: We were told anywhere from early morning to afternoon to late at night it could have been watered.

MR. ENSMINGER: Having lived there I have some resident knowledge of the water usage on that base. Wallace Creek separates those two areas right there. The Hadnot Point and Holcomb Boulevard system -- that's Wallace Creek. It separates, this is Hadnot Point. This is the Holcomb Boulevard system. At eighteen hundred every evening, the water demand down in here where all the troops are at would drop off dramatically.

MR. PARTAIN: The Officer's Club?

MR. ENSMINGER: No, no, the Officer's Club was up here. It was up in here, right in here. All these housing areas, Midway Park, Berkeley Manor, Paradise Point, those demands in the evenings would go up because the people were coming home.

Now the troops, after we got off work we had PT, and then we'd secure the troops. They'd go back to the barracks and they'd either, well, they'd get their showers, and then they would put their civvies on and go to chow hall or head out to town to the bars. So the water demand over here would drop off. Then in the morning about 0500, the water demand here would start picking up again and level out. You know you had morning PT, showers, chow hall, formation, back to work, and then you had that same cycle.

On the weekends, the weekends the water demand here was low. On Hadnot Point the water demand here would be high because everybody would be home.

DR. CLARK: What about light industrial use or lawn watering in residential areas?

MR. ENSMINGER: You didn't have many people watering their lawns in base housing unless you had a few people that were trying to get yard of the month or something. I never did. But industrial, most of your industrial, all of your industrial use water would have been at Hadnot Point.

DR. GRAYMAN: Right. I think one step you want to take is take a look at those patterns as you develop from a PEST modeling and really to check them for being reasonable based on what he was saying.

MR. MASLIA: We actually, if you go back when we were, when we tested like the Hadnot Point system and injected the calcium chloride, you actually saw that exact diurnal pattern. It jumped up at 5:00 or 6:00 a.m. in the morning and then leveled off and then Hadnot Point went down around four or six or whatever. That we saw when we did the test. And so I mean from that standpoint, the PEST just confirmed that. It was just trying to optimize the tank water level

UNIDENTIFIED: And the different patterns for the different types of units.

MR. MASLIA: Yes, yes, that's correct.

DR. HILL: One thing on the, just thinking about those patterns and looking at like one of the figures -- it's Figure 8 in the text -- but this is, it's May 24^{th} through May 28^{th}. That's a Monday through Friday. And if you look at the different days, there's not, Monday and Tuesday it looks like they're kind of similar in pattern. But then the other days look, Thursday and Friday look similar. But to my mind there's not a lot of diurnal similar patterns in this.

MR. HARDING: This is real life.

DR. HILL: Well, yeah, so I guess any patterns we think about could be compared against this data and that could be part of what goes into the model testing.

MR. HARDING: Let me make a comment here that you can't expect under normal sort of modeling extrapolation conditions to be able to predict what happened at 2:00 p.m. on Tuesday, June 12^{th}. You can't do that so you have to average things after, you've got to run these models on an hourly or shorter time step because you don't get the dynamics of the system. But then you've got to average things up.

And your goal is to get good statistics that support the epidemiology study over these sort of windows of three months, right? So you probably have a rolling average of over three months because that's your resolution need.

For these case studies where you've got a critical case, like this case we're talking about here at Berkeley Manor and maybe the Hospital Point, yeah, that would be great diagnostics to go down and just really detail this down and lock everything down and see if it's all consistent, but I wouldn't put too much stock in it. You've got to set your error bars. You've got to be comfortable with the fact that you're going to have some error bars in this.

MR. SAUTNER: I just want to add one thing also for the calibration procedures. We had other hydraulic information and we put some water meters out to record flows. So we have that as another calibration measure. We had -- Walter was in with us when we conducted some fire flow tests. So we do have shorter period of times that we can go in and look at more specifically for our calibration.

MR. PARTAIN: When we were talking about the golf course, I did want to show you all this memo here, and this is, if you look at the date, July 1985. So this is post -- I'll put quotes around it -- post discovery of the contamination. And this is a memo from the Base Maintenance Officer to the Assistant Chief of Staff Facilities. If you look on here, let's see, they currently have two 250 GPM booster pumps to provide pressure for the pump and sprinklers on the north course. It's one course.

MR. ENSMINGER: The whole course.

MR. PARTAIN: One course, which when operating do draw a considerable amount of water. We really need to pursue this. And looking at the rounding slip, let's proceed with vigor -- I can't read from here.

MR. ENSMINGER: Info from PWO.

MR. PARTAIN: Public Works Officer. Can you read that for me, Jerry? I can't see that from this side.

MR. ENSMINGER: When do you think we'll have -- incorporated?

MR. PARTAIN: Information, and that's Mr. Price, his comments.

MR. ENSMINGER: He was the head ^.

MR. PARTAIN: And then on the back, "Yeah, thanks, Bill, this is good idea. We should push hard." So the golf course is an issue here. I mean, they're, yeah, this is priority. They realize they've got to drain the system. And keep in mind now we've got wells offline. There's water problems.

We have documentation that there's water issues at this point, and there's a concern here. So the golf course evidently is drawing a lot of water somewhere. And one course, we've got two, basically, two 250 gallon pumps -- I'm sorry, two 250 gallon per minute pumps pumping out and what kind of draw is that going to put on the system.

MR. ENSMINGER: And this plan was actually realized and initiated in 1987. They drilled separate wells alongside of some of the water hazards on the golf course. They were pulling the water from the water hazard and replenishing the water hazard with water from the wells.

DR. KONIKOW: Would the recharge rate onto the golf course be higher than everywhere else? Was that in the groundwater flow model?

MR. FAYE: No, except for a couple of isolated areas out there, Lenny, what we call the Brewster Boulevard aquifer system is essentially a sand pile with some disconnected clays and lenzoidal clays in that system, which we call the confining units, respective confining units, but it's basically a sand pile. So what you basically got is whatever there's left over after ET goes, is infiltrated probably. And the water table's ten, 15-to-20 feet depending on the contours, the land contours. So that's essentially conceptually what I think is going on there.

DR. BAIR: Aren't you surcharging it with the golf course irrigation water in addition to the rainfall?

MR. FAYE: Yeah, that was the question that he asked.

DR. BAIR: So is that area given more recharge than other areas in the model?

MR. FAYE: Sure, well, like I said, there is no model right now. The work that Jason talked about yesterday is very preliminary, and so that represents, what he was doing represents a long-term, average condition. For the transient model, yes, there would have to be some higher rates of recharge for that area.

DR. DOUGHERTY: (Off microphone; indiscernible).

MR. FAYE: Yeah, yeah, and as somebody mentioned yesterday, it actually might even be what they call a SWAG, which is a Scientific Wild Ass Guess.

DR. BAIR: I guess I have a bad idea that I'd like to pass along. As we talk about golf courses, I'm a golfer. I hate the trees, but I think the trees might provide you with a surrogate for some information you're looking at on a longer average than what we've been talking about on the water distribution system.

But some types of trees take up TCE,

and if you were to core some of the trees on the golf course in Berkeley Manor and other places, I suspect you can find a laboratory that could analyze the annual growth rings for the amounts of TCE. Now, it won't tell you a microgram per liter, but it will tell you a high, low, none. And you could use that timeframe as a surrogate for what's being distributed across the base by looking at different trees across the base. So that's my bad idea.

DR. GRAYMAN: I was just going to comment it's either brilliant or totally off the wall.

DR. KONIKOW: I'll go for off the wall.

DR. GRAYMAN: I think it's a good idea, Scott. At least look at it.

MR. ENSMINGER: I saw that capability. I saw exactly what he's talking about. They do test and they can help.

DR. GRAYMAN: So what are the trees like on the course.

DR. BAIR: Are there trees on the course?

MR. ENSMINGER: Oh, yeah.

UNIDENTIFIED: But they're not watering the trees. They're watering the --

MR. ENSMINGER: Yeah, but those roots go way down.

DR. BAIR: They're watering the fairways, too, aren't they? They have to be.

MR. FAYE: Well, that's probably what we need to do (off microphone).

DR. BAIR: Right, and then you could go to the yard of the month and get tree rings from that.

MR. ENSMINGER: Don't be cutting all the trees down, Scott.

DR. HILL: You don't have to cut the tree down. You just core it.

DR. BOVE: This is an interesting idea, but aren't we talking about from '72 to '85, we're talking about a few days a month during the summer months. That's what we're talking about. We're not talking -- and before '72, yes, Hadnot Point is serving this area. But after '72 we're talking about a few days in a few months during the summer so I don't see the point. Am I missing something?

DR. HILL: You're getting data for the period you don't have any information on.

MR. HARDING: Yeah, I think the button is on the golf course. I'm sorry, but I thought it was a good idea for Hadnot Point in general, and I forgot that the golf course was outside of Hadnot Point probably because it was such a small event it may not show up. But other, it's an interesting idea for Hadnot Point. The thing is is that sort of the anecdotal evidence indicates there was a lot of TCE a lot of times there probably in Hadnot Point itself.

DR. BAIR: Anywhere there's an irrigation system on the base. Are they keeping the Headquarters' petunias nice?

MR. PARTAIN: There are sources of TCE within Hadnot Point, too.

MR. ENSMINGER: I don't know that would find anything that was a confounding factor.

DR. BAIR: It was just an idea. I mean, as an academic it's my job to come up with something that uses my time and other people pay for it.

MR. ENSMINGER: But in the Hadnot Point system I don't think you'd find anything that had a constant irrigation in it.

DR. GOVINDARAJU: I just wanted to go back to this question of calibration. So the test that was conducted in 2004, was the purpose of that test to back calculate the demand pattern? Because that means there’s an expectation that that demand pattern is going to be repetitive of what happened in ’84.

MR. SAUTNER: I’m sorry. So this test right here?

DR. GOVINDARAJU: Yes.

MR. SAUTNER: This was a test we did -- let’s do this test here. We actually injected fluoride and chloride into the systems. This was to help us calibrate the model, and we gathered different hydraulics on the system and pressures and water levels, flows.

DR. GOVINDARAJU: True, but when you are fitting, you are saying I will ~~assimilate~~ [simulate –ed.] by fitting let’s say the demand patterns or demand factors from test. So it looks like the purpose of this test was to basically get the demand patterns out. Was that the goal of the test then?

MR. SAUTNER: Yeah, well, we did not have demand patterns except for a water balance, so we used the water conservation analysis to get a general demand allocation.

DR. DOUGHERTY: So did you fit only the water patterns or other parameters, too?

MR. SAUTNER: Well, we did other sensitivity analysis. We tried to change pipe frictions and stuff like that.

DR. DOUGHERTY: Tank mixing?

MR. SAUTNER: Tank mixing, yeah.

DR. GOVINDARAJU: So basically, my feeling is that system parameters ^[including –ed.] perhaps tank mixing and all, those have been ~~formatted~~ [fitted –ed.] because with that you can perhaps get an estimate of what the friction factors were back in ’84. The demand pattern is going to be, even if you prepare it very correctly with this, the chances of being able to reproduce it for ’84 are very difficult. Already I think we have heard about what you are going to get are monthly averages which you have to somehow fractionate or disaggregate into much smaller intervals.

MR. MASLIA: Can I make a couple of comments to maybe hopefully clarify what we have and what we did and why we did it? We came in there in 2003 and there was, from a model standpoint, a description of the distribution system. There was no information available as to daily demand patterns and things like that.

What we had, as I said previously, as most military bases have done, they've got a conservation study that was done. Not only for Lejeune, the Air Force has done it. The Army's done it at all their military bases. The purpose of that really was to study on an average basis the water use and see how they might reduce or conserve water.

And so it identified different water outlets, swimming pools, showers, latrines and so on and so forth. That was really our -- and then we knew the volumes of the tanks obviously. That was the only real, you know, that type of information that we needed. And when we summed up the water balance from the conservation study, we were off -- I mean, I say we, I mean taking the numbers from the study, off by about 30 percent from if you added up the storage in the tanks and the stuff the wells were pumping and all that sort of stuff. So there was a discrepancy in information there.

So one of the purposes in conducting the distribution system test was to see if, in fact, we could account for this discrepancy because we knew we would have to have a more robust -- I won't use the word accurate -- description of the distribution system.

We also made the assumption, and I believe it's still a correct assumption, is that the distribution system, with the exception of obviously separating off Holcomb Boulevard from Hadnot Point, but the activity patterns would have been the same whether the troops were there when we were doing the test or the troops were there in 1968 or whatever. And as Jerry correctly pointed out and we did in the test, they get up, run the shower at 6:00 a.m. or whatever and then it goes on in the Hadnot Point area.

In doing the test or gathering the data, we then were able to, as we had suspected, were able to, through using PEST, determine that the friction factors were insensitive. The system, the changes to that were basically insensitive. That left a demand pattern and water levels that were measured in the tanks through the SCADA available. And so we adjusted the demand patterns. In fact, we were able to match what actually was flowing through the system based on our measured data.

What was interesting also was at the end of the test, and I believe, was that, that may have been a Thursday or a Friday, as troops left for the weekend or whatever, because we got folks at the Hadnot Point to flow the system, I think it was, what, 2,100 gallons per minute, something like that. They came to us and asked if they'd cut that back because they were spilling water out of the controlling tank, French Creek tank was spilling water because they were pumping it at an average rate of what we had gone through the data and figured that the average flow was.

So he's correct. Over the weekend it drops. But our entire concept was that from average operational sense what we saw when we were doing the field test, which is what our goal was, that we could use that at any typical period historically to provide input to the epidemiological study. And hopefully, that clears where we got initial information from.

DR. CLARK: Was the pipe material ~~the same, had been~~ [–ed.] pretty much the same over the years or was there a switch from, say, cast iron to vinyl chloride at some point?

MR. MASLIA: Joe can probably give you a better idea, but at least now when they replace it they use PVC, don't you -- right, when they replace it presently, they're replacing it with PVC. But to give you an example, Tarawa Terrace was basically the same as it was, and it's got a mix of cast iron and PVC currently.

And even though C factor was not very sensitive, it was much more sensitive to PVC than it was to cast iron. And I've got those plots in Chapter I report under the water distribution part or the sensitivity of the water distribution system. It really was the purpose of the test or our concept going in is that there was, in terms of where the pipes went and all that, it would be no significant changes from the historical system.

And that's why we felt or why we justified that we could go out and get some field data. But it was basically what the primary driving factor was this big discrepancy of 30 percent between what the water conservation study said summing it up and what we knew presently was the volume that they were, you know, having.

MR. HARDING: The water conservation claim was summing it up from estimates of individual either categories of use or -- I'm not alarmed by a 30 percent difference then. Those are the same number. You've got to think in astronomical terms sometimes.

Yeah, I mean, if you had measurements coming out of the water treatment plant, those obviously would be your best piece of information which you don't have.

DR. KONIKOW: You're talking about historically, right?

MR. HARDING: Yeah, if you had the flow meter and you had the daily records, those, I've had cases like that, then that's great. We've had situations where all we had were monthly data. You don't even have that, but you're going to make an assumption about your stress periods, right?

And the assumptions you make should be the best you can make. Then they should be consistent with the water distribution model, and then you're going to have to disaggregate that down to a daily pattern. There's a variety of ways to do that. You know, you have to understand and be comfortable with this, it's going to be wrong. But as Locke said it will be useful. And that's the comfort you have to have. You have to be willing to be wrong but provide a useful piece of information.

MR. FAYE: We do have monthly data back to, into the 1950s and also into the '70s and '80s and '90s. So we do have a lot of monthly data to deal with.

DR. GRAYMAN: Can I broaden this a little bit? We can bring it back, but looking at the schedule where we're scheduled to talk about distribution system really for the rest of the morning, I think at some point the group should be looking at a little more broadly and that we really have by my count at least five different areas we're trying to simulate what we're going to be giving to the epidemiologists.

We have to be looking at wellhead concentrations, which we talked a lot about yesterday in terms of the groundwater flow models. We have to look at the well operation scenarios. How were the various wells combined at any given time. The interconnection scenarios, how was the booster pump operated and the Wallace Creek valve. The water use demand scenarios, which we have ideas from the present study, but these are still a lot of unknown. And then there's a system operation scenario and that's primarily how did they operate the system not from the wells but once from the treatment, when would the treatment plant pumps come on, how were the tanks operated.

And I think it would be useful as a group to try to discuss how are we going to bring all these together. I've heard the idea of using Monte Carlo simulation or some kind of partition hypercube, but we're talking about a large number of scenarios in all these different dimensions. And I hope we can at least start addressing that at some point.

DR. KONIKOW: Well, I don't think the epidemiologists want all of that information. They want -- correct me if I'm wrong -- they don't want to know the details of the groundwater flow model or the details of the groundwater transport model or even the wellhead concentrations. They want to know the outcome. What went through the distribution system.

DR. GRAYMAN: No, exactly what gets delivered to the customer.

DR. KONIKOW: Exactly.

DR. GRAYMAN: But all of those things bear upon making that vital decision.

DR. KONIKOW: Exactly, yeah.

DR. ASCHENGRAU: Just to add to that, I mean, to me there were lots of issues that came up yesterday that are similar of this sort, right, on the groundwater modeling. So it has to go even further than that, and it's just to me we would consider all those sensitivity analyses. And so we would want to know sort of the bounds of the estimates, the monthly estimates, that we are trying to get.

DR. KONIKOW: Let me add that there were quite a few, I think, important issues causing uncertainty and error in what predictions could be made that we didn't get to discuss yesterday. I mean, it's really much more complicated and uncertain than we even, we just began to scratch the surface.

DR. GRAYMAN: Right, and what's complicated here, when we were dealing with Tarawa Terrace, we were at the point where we really weren't that interested in the distribution system because it wasn't one of the factors or wasn't a primary factor or even a major factor in contributing how much was delivered to the customers. Here we're now having to, everything that was said about Tarawa Terrace and complicating it by the fact that Hadnot Point and Holcomb Boulevard appear to be significantly more complex situations. We then have to overlay that with the water being delivered to the customers primarily in this interconnection phase.

DR. HILL: This is actually just going back to something that Bob mentioned earlier, and it’s coming back to the groundwater model study. I apologize for that. But the idea of this is just a pile of sand, I would like to back off from that a little bit.

From the Castle Hayne downward it’s been there for 20 million years, and it’s a deposit that has some structure to it and some information that we can take advantage of. And the idea of representing, thinking of it as just a pile of sand, I’d kind of like to back off, thanks.

DR. KONIKOW: Maybe it was mentioned, it probably was and I just forgot, but what is the present situation at Camp Lejeune? Where is the present water supply coming from? And on a related issue, were the wells that were shut off and abandoned, how were those plugged? How were those sealed? Did we, was the annulus ~~crowded~~ [grouted –ed.]? So really two separate questions: one, what’s going on there today for the water supply? And second, what was done with the abandoned wells?

MR. FAYE: There are some slides showing the well locations, the historical wells and the modern wells. I’m not sure if Jason has any handy there or we can flip something up. But the well, the modern wells, the modern, active wells, Lenny, have been distributed along Brewster Boulevard and then through the, sort of the eastern extension of Brewster Boulevard and down North Carolina Highway 24. So they're well north of -- we'll see here hopefully in a minute. You can look on the posters as well. Just a second. And down Sneeds Ferry Road, and these are all well away from points of known contamination and indeed the sampling indicates that there's no additional contamination happening. Here we go.

MR. ENSMINGER: Unless it's munitions.

MR. FAYE: There you go. Lenny, these are the modern wells right through here in this area and then down here, down Sneeds Ferry Road down in this area. These are the modern wells.

DR. KONIKOW: Aren't those down gradient? If you look at the head distribution, isn't that down gradient from the contamination?

MR. FAYE: Sure, but you're looking at a relatively small radius of influence here for most of these modern wells out here. There's not any influence in terms of contamination unless there's an unknown source out there.

DR. KONIKOW: Well, what's the slope direction?

MR. FAYE: Pardon me?

MR. HARDING: Yeah, I'd like to see a head map, I guess for the side gradient.

MR. ENSMINGER: It flows toward the New River.

MR. FAYE: What's your question in terms of the regional flow patterns? They would be toward the streams, Wallace Creek and then toward the New River.

DR. KONIKOW: Well, it certainly isn't shallow, but as you go deeper is there -- in the upper Castle Hayne, is the flow direction the same as in the shallow system?

MR. FAYE: Pretty much, yeah, left undisturbed by pumping wells, yeah, it would be very, very similar, very similar, just like Tarawa Terrace actually. That goes back to my comment that Mary objected to that it's kind of a big sand pile out there. You see very little head difference.

Actually, there's some -- and this is discussed in one of the Tarawa, I think Chapter C, Tarawa Terrace report. There's an excellent set of observation wells out here from the lower Castle Hayne aquifer all the way up to the Brewster Boulevard aquifer. This is observation well clusters by the North Carolina folks, the State folks.

I think there's maybe like a three-foot head difference between -- and this is undisturbed -- three-foot head difference or four-foot head difference between the lower Castle Hayne aquifer and Tarawa Terrace aquifer.

DR. BAIR: That's huge. That's enormous.

DR. HILL: That's up or down?

MR. FAYE: Well, of course, it's upward because it's right next to Wallace Creek. You have an upward flow pattern. So we have about a four-foot head difference here.

DR. BAIR: Yeah, but that's an enormous head difference. For a pile of sand you shouldn't have any head difference.

MR. FAYE: I beg to differ. If you're by a regional drain, I don't care whether you've got a pile of sand or not. If you've got 300 feet of sediments or so, you're going to have a vertical upward --

DR. BAIR: You won't have a vertical drain without a head difference.

MR. FAYE: Pardon me?

DR. BAIR: If won't flow vertically unless there is a head difference.

MR. FAYE: Well, if you have a highland area here where you have recharge, and then you have discharge down to your main drains, which is the New River, Wallace Creek or whatever, you're going to have a diffuse upward leakage in the vicinity of the drains, and that's going to be vertical.

DR. DOUGHERTY: It means that the best technical data's a turning point.

MR. FAYE: Yeah, I mean, all you have to do is look at what ~~Hubbard~~ [Hubbert –ed.] did back in the middle '40s. You can look at what ~~Tote~~ [Toth –ed.] said in '55. And you've got, that's typical regional flow patterns.

DR. HILL: You've got three head maps in the material that I have. One is in Report Chapter B. It's on page B-30 and it's estimated pre-development, and so this is contour measured. But the points aren't on here so I can't say what's controlling the contours, but these are these contours.

Okay, then you have one in the material we were sent in the notebooks. It's Figure 1, page 8 under Tab 6 after the, in the second section of that. And that's also contoured measured. And then you also have the contoured simulated values later in that section if I can find it. And that's Figure 3.

In every one of these maps, the contours next to the streams imply a completely different hydraulic connection between the groundwater system and the stream. And that's true for the Northeast Creek and the Wallace Creek. So I mean, you're talking now about that the three-foot head difference and what that means in terms of interconnection with the stream.

And really, without the groundwater flow model, I don't know. I don't know if what you're saying is correct or not. But I can say that your potentiometric surfaces in these three figures imply, each of them implies, I mean, there are some similarities, but there's some drastic differences.

And I don't know if you have these in front of you. We haven't seen them in any of the slides, but the one from B-30, the Tarawa Terrace report, but that figure goes down into part of Holcomb Point.

MR. FAYE: If you look in Chapter C of the Tarawa Terrace reports, there's a discussion in there of the simulated potentiometric surfaces, and you can't quite see the upland areas of Tarawa Terrace here, but they would be here. Where you have recharge in the upland areas in layer one.

DR. HILL: I'm not talking about that. These are really dramatic differences. I mean, it didn't come up yesterday and I don't have slides, but in Chapter B the Northeast Creek shows that it's highly gaining like this. The contours look like this indicating water coming into the stream.

MR. FAYE: Right.

DR. HILL: But the contours on Figure 1 that we were given show the contour is going directly across the stream like this as if the water was really just going --

MR. FAYE: No, that's a boundary for -- well, it may be true, but what I'm saying --

DR. CLARK: Is this something we might want to take up after the break?

DR. HILL: Yeah, that's fine.

MR. WILLIAMS: The wells, there's a State standard for ^ [abandoning –ed.] wells [; -ed] fill them with generally with bentonite and so that there won't be an interconnection between the possible transportation of contaminants between layers. So we did abandon those wells according to the State standards.

DR. DOUGHERTY: That's really not very definitive because it doesn't say that you, because there are various stages of abandonment. One of them is simply pulling the pump and leaving it in reserve. Another one is filling the existing casing with bentonite cement, and another one is yanking the casing and actually making sure you've grouted the entire annulus because we had, I think we have well water records that say that the annulus is open. So if you just filled up the casing, which I don't know North Carolina State standards so please tell me. Did y'all yank the casing or --

MR. ASHTON: No, we did not yank the casing. And typically these are gravel-pack type wells. And, no, we did not yank the casings. Typically, how these wells are constructed is about a 50-foot grout to prevent surface influence. Then, of course, they go down between 150 to, in some cases, we have some wells that are 250, some that are even deeper --

DR. DOUGHERTY: (Off microphone)

MR. ASHTON: Pardon me?

DR. DOUGHERTY: How were they installed here?

MR. WILLIAMS: Oh, those were all rotary.

MR. ASHTON: Yes.

DR. CLARK: Why don't we take this up after the break and give you a chance to get together and talk about it?

MR. WILLIAMS: Yeah, and the other question that was unanswered is what's the state of the water system now. And we can take that up whenever you want.

DR. CLARK: Why don't we address all this after the break?

(Whereupon, a break was taken between 10:20 a.m. and 10:33 a.m.)

DR. CLARK: We're going to change the format just a little bit and change the order a little bit. I think that maybe we're not giving ATSDR the kind of advice that they need to continue on with their work.

So what I've asked Morris to do and Frank to talk a little bit about what they think they would do for the future and what kind of advice and input they would like to have from the panel. We've got you guys here, an expert panel, tremendous input, tremendous help, but I'm not sure they're getting the kind of advice that ATSDR really needs to continue on with their work.

So, Morris, why don't you go ahead?

PANEL DISCUSSION: WATER-DISTRIBUTION SYSTEM MODELING (RECOMMENDATIONS FROM THE PANEL)

MR. MASLIA: What we would like to focus really on is, and at the end of the day when you make your recommendations, besides the details is the big picture. Because what we have to be able to do is go back, or if any of our management is here, and also go back to the Navy and say, yes, we're going to finish in this timeframe or, no, here are the steps we need to take to accomplish to provide the epidemiologists with an estimate of exposure.

And to be able to do that I think we need to step back or go back to the bigger picture recognizing that the details are important; however, what I've noticed is we were, I thought, getting down to so much detail that we lost sight of the big picture in terms of the distribution of water historically at Hadnot Point and Holcomb Boulevard.

So I just put up, just real quickly here, from 1941 when the system came online, Hadnot Point supplied everything until Holcomb Boulevard came online approximately in June of '72. During that period you have one system, and you have all the wells contaminated, non-contaminated going into a water treatment plant so we can go back to what we did at Tarawa Terrace and use a simple mixing model. So that takes the distribution system water dynamics and water quality dynamics of a distribution system out of the picture completely, and we just have to concentrate on, yes, important factors, but the well cycling and from a groundwater standpoint.

From 6/72 when Holcomb Boulevard came online to ’87, from August through March there’s no indication that there are any interconnection, the booster pump or the Marston Pavilion valve was turned on. So again, we still have simple mixing because the wells are feeding into storage tanks, combining into storage tanks. So again, that takes the detailed water quality dynamics of a distribution system out of the picture.

So that leaves us basically this time period in here for April, May, June and July with an interconnection issue a couple of days during the month. So the question or the idea would be can we use, can we come up with a typical day, a typical day that we could say during a typical day -- with bounds on it.

I mean, I’m not throwing out the uncertainty, but with bounds on it that we could then say during a typical day to the epidemiologists, this is what the exposure would be at different locations in the distribution system given what data we have, given that we have a two-week period where we've got test data or sample data or whatever when the line broke, given that we also have field data that we collected in terms of calibration or seeing that the system operated realistically from a diurnal pattern. And that's --

I guess, Frank, is that stating I guess the big picture?

And that's what I'd like to throw out to the panel here to see if we could focus the discussion really on that so we can get, hopefully, some direction as to how we should proceed on that.

Frank.

DR. BOVE: The other big picture is can we get monthly averages? Does that make sense given the complexity of the situation? Can we get quarterly, should we move to a quarterly situation where we get just quarterly data averages? So that's another question that the epidemiologists, I would like to know.

DR. KONIKOW: Doesn't that hinge also on how well we do in predicting what the wellhead concentrations were?

MR. MASLIA: Yes, absolutely, absolutely.

MR. HARDING: You can't model at those longer time steps in the water distribution system. You have to do it on an hourly basis or a sub-hourly basis. The model will choose the time period that it needs. But what you can do then -- I'm thinking out loud here, but Walter and I had a discussion in the hall here.

What we've done in the past, because as the water distribution people are always the tail of the dog, and the groundwater people deliver their stuff to us at the last second, and then we have to make our calculations. And so we adopted as a matter of convenience, but it happens to be good in other ways though, using the method of super-position to provide a fast way to make the calculations of nodal concentrations to the concentrations of the source in use.

And we have -- my brain isn't completely functioning here, so correct me if I'm wrong. But we have two sources of water at Holcomb Boulevard during the interconnections. We have the Holcomb Boulevard water treatment plan, and we have one, possibly two, interconnections. I think the second one is when the booster pump is running is going to prove to be a drain, but you could do the modeling during those actual interconnection periods.

The hydraulic modeling will calculate, just like Jason did up there, and use a hundred part per billion or use the source of water function in EPANET and calculate the percentage of water from each source and each node, average that over a rolling three-month period, which is your resolution that you needed, and will help avoid overconfidence in what you're predicting because you're going to be wrong on any particular day. You know that.

But over an average of a period of three months, and that's usually what I felt I had some confidence in, you should be getting close. And then keep those coefficients there, and then you can do whatever you want. You can load them however you want with what comes out of the Hadnot Point mixing model.

MR. MASLIA: I'm in absolute agreement with you. In fact, we took a similar approach, not contaminant-specific, but in Toms River. In other words put a hundred units in and did it that way as well. And that's I think what I was trying to hopefully get to here is to try to simplify that in that --

MR. HARDING: And in the Hadnot Point system the memory in the tanks is going to be important if the wells, if the contamination's going on and off. If it's more smooth ^ but if you've got contamination going on and off, then the memory of the wells becomes significant.

But you can use the same approach. You can use the, what we call transfer coefficient super-position approach to run it once, and then use it to force it with a Monte Carlo or whatever you come out of a resampling from your groundwater results, just thinking out loud. Walter had some thoughts as well.

DR. GRAYMAN: You were talking about temporal averaging period. Spatially, under most circumstances we'll be able to say, well, we can treat Hadnot Point as a single unit just as we did Tarawa Terrace. Holcomb Boulevard, hopefully, we may be able to just do it by ~~assume~~ [assuming –ed.] Berkeley Manor is homogeneous. And that can be tested in the water distribution system model to see if that’s the case.

DR. WARTENBERG: I have a question about this temporal averaging. One of the things that would be helpful for an epidemiologic analysis is to know the variability of your predictions. And I don’t know where in the process you’re doing the averaging and whether or not it’s possible to give us more fine scale data that epidemiologists would average using rolling averages or some other approach or finally give us some sense of that.

DR. GRAYMAN: Finer scale temporally or --

DR. WARTENBERG: Temporally.

DR. GRAYMAN: -- probabilistically?

MR. HARDING: You can do it, but you have to then use it in a longer timeframe because you’re going to be wrong. You’re not going to have it exactly the right time. But if you want to calculate frequency information, I think you could do it.

DR. WARTENBERG: Well, all I'm saying is if you asked me what's the right temporal increment? Should it be one month, three months? I don't know the answer. But if you gave me the data, say, daily data, then I can average it different ways and look at it.

MR. HARDING: It scares me if you're going to use it and on a daily basis.

DR. WARTENBERG: No, I wouldn't use it on a daily basis, but I could look at how it changes and aggregate it weekly, monthly. Otherwise I don't see that variability. That's what I'm saying.

DR. KONIKOW: Look at the first page of Table C-7 that they handed out this morning and look at the wellhead concentration in the first well, 602, over a two-week period. It hit a high of 1,600. The next sample is 540 and the next was 300.

DR. WARTENBERG: Those are still going to be the data, right? Those are the data, and you're going to have to --

DR. KONIKOW: You want to know what the variability is on a less than a mean monthly, well, there’s the information we have. Whatever we reconstruct in the model to feed into the water treatment plant isn’t going to be any better than this. And this is your sample, and you know, you say, well, there’s three samples in two weeks. What’s the odds of actually hitting a peak? Well, pretty small. Somewhere close to this time it was probably much higher than 1,600. There you have an example of the range in a contaminated well, and if you go to the really bad well, 651, you see similar things over basically a two and a half week period it went from 3,200 to 18,000. Well, there’s your sample of a local area --

DR. HILL: And I really agree with that, but the model’s going to give you a very smooth representation of what that system was doing. The actual variability is just what Lenny said. You’ve got it there, and that’s the best information you’re going to get.

MR. HARDING: It won’t be smooth in the water distribution model. It will be step functions. It’ll be on and off. It won’t be smooth. But when you average it, you -- but it will be wrong on Tuesday, or Wednesday.

DR. DOUGHERTY: Right, but if we do a multiplicity of scenarios and then provide those averages across the scenarios on a sub-daily basis, which way do you want to, it just becomes risky.

DR. KONIKOW: Well, there's no way in terms of the wellhead concentration according to the plan modeling scenarios, there is no way that you could possibly reproduce the observed variance in what gets fed to the water treatment plant.

MR. HARDING: I can't even speak to what gets fed to the water treatment plant. That's your business not mine, but I'm saying that what happens in the water distribution systems is going to be way more dynamic. That's the point I'm making. And let me just ask this question about objectives here.

All the work that I've done in the past, we've been looking at chronic effects, and we haven't been looking at acute impacts. And so what we looked at was what we called either whole body dose or intake of a particular contaminant, typically TCE, vinyl chloride or chloride sometimes. And so you would be looking at the accumulation by on an annual basis.

And the reason that you looked at it on a shorter basis was because people moved in and out and things like that. Now, in this case we've got to look at it on a shorter basis because somebody, because we're worried about these trimesters. But is it really necessary to know that, or even useful to know, that that occurred in the first month or the third month? See what I'm saying?

Because I'm very, I think you're going too far if you break this down more than a quarterly basis, but you could do a rolling three month summarization. And I'll leave it to the statisticians to figure out just how much structure you could put into that summarization. Typically, we've used the mean.

DR. GRAYMAN: Let me ask you a couple [of – ed.] questions and interpret how you'd use the information. Would it be different if you were to get the information, let's say, on a monthly basis or on a three-month basis that the average concentration in the water was 300 micrograms per liter. If you had that information, but if we were to tell you that during that same period, the concentration varied between zero and 1,500, would you use that information? But on average it was 300. Would that impact your study?

DR. WARTENBERG: I guess I don't know enough about what people think the mechanism might be in terms of how the causation works, but there's certainly been studies where people looked at maximum exposure levels or percent of time above some level. In other words how many days were they exposed above, and I don't think there's good theory behind it.

What I was trying to get a sense of if you're telling me the data are, I don't care if they're not reliable for that day, but are they really representative of the variability, then that's useful. If they're not, then obviously it's not useful.

But for things which people can actually measure over time, sometimes people have taken these daily numbers and then looked at different ways of summarizing the exposure not assuming that the average is what makes sense.

MR. HARDING: I think that's okay. There's some technical or mechanical issues that have to be resolved. I mean, this is not going to fall right out of EPANET as it comes off the shelf and you pull the shrink wrap off it. So there's some mechanical difficulties, but that's why we pay Morris the big bucks and Jason the big bucks, right? I'd be happy to describe the way we've modified it, but, yes, you can do that.

And you can basically -- leave to the statisticians to figure out just which of these things would be valid. But I would think that days above a threshold would be valid and a mean. The problem is that if you don't do this right, you're going to have to go back and re-run the model to get it again with a different threshold.

So I would suggest figuring out a way that you can run it on these short timeframes and store your transfer coefficients on a short period and then be able to run it through a subsequent processing step to --

these are technical details, but I think it can be done.

DR. WARTENBERG: Yeah, but I don’t know if Frank’s thought about this at all. Just listening to you talk about the different timeframes just occurred to me.

DR. BOVE: When it comes to, say, neural tube defects, we’re talking about a time window here of vulnerability of a few days during the fourth week of gestation. We can’t, of course, know when those four days occurred based on what the birth date of the child or even if we have LMP, last menstrual period, where a clinician decides on gestational age. I’m not sure we could pinpoint those four days anyway, or five days. But that’s how tiny the window is for neural tube defects.

For clefts we’re talking more of a week or two, a two-week period for each of the clefts, cleft lip and cleft palate. So we’re talking small timeframes of window of vulnerability, but there’s also uncertainty as to when those two weeks occurred given what we know about the child’s birth and the mother’s

LMP. So those are issues.

MR. HARDING: Well, I think the best you can hope for would be this percentage of time above certain thresholds, and I think that would be a valid statistic to calculate. I'm looking for support here from somebody that knows more about this, but I think you can get that, and then from that you could probably make some inferences about what the odds would be that this particular causative factor was a factor in that particular.

DR. BOVE: Where are these thresholds coming from?

MR. HARDING: Well, let's say that you'd say that during this particular three-month period the concentration was above 300 parts per billion for sixty percent of the time or something like that. And if your threshold for impacts a hundred, I mean, we could do a hundred, too. Maybe it's 100 percent of the time. And so you've got a clear answer there. It's going to be diceyer [dicier –ed.] if your threshold is, say, 200 and the percent of time above 200 is 30 percent. I don't know.

I can't answer that question for you,

but I think you need to step back. I wanted to go back to Walter's point here. You need to just climb up to about 20,000 feet for a minute and look at this, and you guys need to look and ask for your endpoint what you need, and then talk about how you're going to try to get the best estimates of those things you can from the models.

DR. GOVINDARAJU: I'm seeing two kinds of variability right now. First is if you have a model run which has all these behavior fluctuations and ^ [temporal –ed.] fluctuations, if you want to average them or do the moving window of let's say one week or ten days or three months, then you'll get fluctuations within one single model run.

But if you want to incorporate the variability you're getting from wellhead concentrations and so on, then you're talking about doing many of these model runs to try and capture that variability as well. So there is almost like an internal, intra-model variability, and somehow we have to combine all this information to answer questions like what is the likelihood that you will exceed a certain value over a continuous ten-day period.

Or what would be -- and so some of those we can, I think those could be done, and we could perhaps attach some probability of what is the likelihood, what is the probability of this kind of event happening.

MR. HARDING: In fact, what you're dealing with in the water distribution system is variability. And what Rao's talking about is uncertainty, I think. And I would suggest bringing Owen Hoffman who's a guy we've worked with before on the, to help frame this team. He's a really excellent person on risk out in Oak Ridge. But, yeah, that's the issue.

You've got variability in the water distribution system, which is more profound than in the groundwater system, but just happens faster a little bit. There's still variability in the water distribution system, and then there's a profound imperfection in our state of knowledge about this, which is the uncertainty we face. And that's going to be represented by different iterations of a Monte Carlo, for example.

DR. HILL: So we have this range of things that epidemiologists might want. We have just give me bulk, high, low, medium exposure or no, medium and high exposure. And then we're getting into these ideas of, well, if I had more detail, this is how I would use it so that I could use it.

And we've talked about different strategies for creating more accurate concentrations at the wellheads and whether or not those are worth it and maybe they're not worth it if you're just trying to get rankings. But maybe they're well worth it if you're trying to dig any deeper.

So it seems to me like there's a goal of this groundwater model that's a bit of a moving target as of these last couple of days. And I'd be interested in, and I don't know what you think about this, but it seems to me like the design and effort in the groundwater model depends very much on these priorities.

DR. GOVINDARAJU: The answer is yes, but just to bring the discussion back, I think we're talking about just the water distribution system right now. Is that correct?

MR. HARDING: I don’t want to limit it to that.

DR. HILL: It seems like the, it may be that the water distribution system impact dominates. I don’t know, but I wouldn’t think entirely.

DR. GRAYMAN: I think it’s time to broaden this discussion back.

MR. HARDING: But certainly it only dominates for, it may not even dominate, but it’s ^ [important –ed.] in this relatively small piece of a relatively small piece probably of Holcomb Boulevard. Unless the wells are going on and off and there’s big step functions in the forcings [? –ed.] from the contaminants, which I think is probably unlikely, then the tank memory in Hadnot Point will become important. But if it’s not, it’s not important.

DR. CLAPP: I’d like to just respond to Mary’s laying out of the range of opinion that’s been made by us epidemiologists. I sort of staked out the three-category thing yesterday. But it’s definitely true that the more, especially for Frank’s birth outcome studies, the more detail the better.

I guess what I’m worried about is that we’re getting to a point where we publish an effect estimate that has so much uncertainty bound or bundled up in it that the confidence bounds go off the page, and you’re left with just a big fuzz ball. So if we can narrow the bounds of uncertainty to the point where it’s useful on a monthly basis, fabulous, and not just a guessing game.

MR. HARDING: Don’t expect -- I keep saying this. Think in log space. Think in terms of astronomical framework. I mean, when I’ve done this before, the medical causation people think that way. I mean, if the exponent doesn’t change, we don’t have a significant difference. I mean, you’ve got to be to that point. I mean, you’re talking -- we never did get to the calibration standards, but you’re talking about a half an order of magnitude plus or minus, so you’ve got an order of magnitude range just in your calibration standards. So how can you expect to be conceptually better than that in --

DR. HILL: And that was heads.

MR. HARDING: Yeah. But I just think if you can't use it for an epi study in the log space, maybe you can't answer the epidemiological question. But there's a lot of other questions certainly that can be answered or be thought about.

DR. WARTENBERG: I don't know if the ^ will fix that or not but I mean, some epidemiology has really ^ [had –ed.] horrible exposure data and worked. There are countless occupational studies where if you worked in a given profession versus not, there are really clear associations with disease.

And then it goes off in the other extreme where people have very fine-scale, accurate estimates of exposures and can show associations. So in something like this where I think it's less, there's less data to say what the association is, it's a little hard to say what we really need to show an association if one exists.

MR. HARDING: But if I've learned anything here, the one thing you want to avoid is misclassification, right? So if we can get that right, then we've made a step forward.

DR. WARTENBERG: Right, misclassification will just blur the whole thing.

DR. HILL: So let me go back to -- it seemed to me yesterday there were three ways to deal with the wellhead, developing wellhead estimates of concentration. One was just to take the measured concentrations that we already have. Say, okay, I'm going to project back in space or in time that this really contaminated well had some kind of average value back, almost a step function or exponential or something.

And just say, okay, based on measurements here, I'm just going to project it back. No physics, no nothing, just a direct, and then feed it through the mixing system of the well distribution system and get exposed node, high-level node, whatever in three categories.

That would be like level one. Level two or level three, whatever, the other two options that were discussed were doing some linearization of the system and doing what Professor Aral said. And then the third one is to go through the whole groundwater model.

And so if this is level one, it seems to me that then you want to think really closely about, okay, if I can start with this level, what do I want to get out of those next two levels, and very specifically. Because I think if you have very specific objectives on what you want to attain from those given the data you have and given what you have a hope to, then you can make some progress.

But I'm a little concerned that the charge being given for the groundwater model isn't focused and defined enough, and it's just like, well, just represent the system accurately. Well, given this data what does that mean? So I'd be interested in a discussion that kind of address those three things and what to get out of it.

DR. ASCHENGRAU: So I think with going further would be to get a more accurate ranking of those study subjects, that that's what all of that effort would do would be to boost at that accuracy and get a more accurate ranking that would be possible with the first method. So and it just seems as though there's a huge amount of effort that needs to take place in order to do that.

DR. BOVE: I think Mustafa's approach does not take a whole lot of effort and may still give us some of what we got for Tarawa Terrace, if I'm not mistaken. So I think that's the approach we've been thinking about all along. That that approach might give us a good answer, a good answer for the epi study.

And then if we need to move beyond that, we could use that part, step two, to help us with step three if we wanted to go to step three. But we could try step two to try to get the monthly averages like Tarawa Terrace. And then if that was sufficient, we could stop. Does that make any sense?

MR. HARDING: How do you know it's sufficient?

DR. BOVE: We make a judgment. I mean, --

MR. HARDING: That's fair, but the concern I have -- and I'm not a groundwater --

DR. BOVE: Not by -- we make a judgment without looking at the outcome, blinded by the outcome, of course.

MR. HARDING: Coming out of the world of litigation I know there's a huge weight put on trying to acceptance and I think that it's a novel idea, and it seems to conform to Clark's law about a sufficiently developed technology. It really is cool what it does though I have a problem thinking that people are going to accept this very much when they can't get in and dig around and look at the physical underpinnings and say that these make sense.

Do the constraining layers, you know, we've gone into all these details, and that's a real pain for the modelers. And some people focus on little details that are their specialty, but on the other hand that's the way you're going to develop confidence with this is that does it look reasonable. And unfortunately, you can't do that with a matrix that's got 16 elements in it or 25 elements.

DR. GRAYMAN: Yeah, building on that, again from the legal standpoint or at least my observation of it, is a lot of reliance is on has this model been used before. So if you go in and you say I've used MODFLOW. MODFLOW's been used for 25 years all over the world. It develops a certain confidence. If you use something else that’s new and innovative, then you, the burden of proof is on you that that is valid. It’s a tough thing to prove.

DR. KONIKOW: Well, in this case if you get to the point of trying to develop a history of wellhead concentrations using this full-blown modeling approach, deterministic approach, it’s really going to be difficult to defend it in a litigation requirement. I mean, there are just so many weaknesses in assumptions and uncertainties in it that it really will be very difficult. I mean, you get very open to attack.

MR. HARDING: It is, but it’s been done many times.

DR. GRAYMAN: Is it more so than other situations? Is it more --

DR. KONIKOW: In this case more so than other situations.

MR. HARDING: I’ve seen some really messy situations with not nearly as much data.

DR. DOUGHERTY: But is it more than the linear control approach?

DR. KONIKOW: Well, no, I think they both hinge on what do we know. And what we know is very limited. And so whichever, it’s a question of how do you want to extrapolate back. For the wellhead what we really need to know are two things. One is the pumping history of each well. That’s important to know if the modeling will not give us a clue about that. We have to tell the model what that is, not the other way around. So that’s one thing that’s needed.

The other thing is the concentration in the well or in the well discharge, the history of that. Now that we could try to get that starting from a very deterministic approach. And I’m not saying it’s not worth doing, but I’m saying we better have something to compare it against such as Mary’s level one and just see how they compare. I think we could do a little bit better and still keep it very conceptually simple but key into the history that we have even though as limited as it is, those are the knowns.

And then there were all kinds of questions about what causes this variability. Look at the contaminated wells. It shows a peak. You know, you’ve got five data points, it goes up and then down. Well, is that variance, is that just representative of a saw-tooth pattern or was this the real peak in the whole full-blown history.

But what I would say, and you will have to reconstruct something about the mass loading history to do the transport model, so you will have some estimate of that information. Well, take that information, use your flow model in MODPATH analyses from each well to each source and reconstruct the distribution of travel times.

Use that then to lock in the starting points in growth history of a concentration curve, and then just bring it, just use a thick pencil and bring it up, if you want to work on a log scale exponentially or on an arithmetic scale, try them both, then just bring it up, use your MODPATH to get you a starting point, an initial curve, and then bring it up to your known history. And then feed that into your mixing. Do that for each well.

You still need as good a groundwater flow model as possible, but you use MODPATH instead of -- but then you'll still have other complications. Do you want to retard the movement field or retardation factor to catch, but at least you have a starting point, and it'll be much simpler and more defensible and easier to explain conceptually than the full-blown transport model. Do the transport model also, but I think have this simple, I'll call it level 1.5, as a way to get at the numbers you really need and --

DR. CLARK: What about linear control?

DR. KONIKOW: I don't understand that well enough to know that it's any different from the drawing with a thick pencil.

DR. CLARK: Dr. Aral.

DR. ARAL: I think Mary wanted to say something before I --

DR. HILL: Oh, no, all I had just wanted our discussion to progress further before Dr. Aral talked, but if this is the appropriate time for that, that's fine.

DR. CLARK: That's an issue, I gather, is how appropriate the use of linear control theory would be.

DR. KONIKOW: If the linear control theory is as good as it looked, then fine. Do it for the wells where there's enough data to do that, then great, but I don't understand about the ^.

DR. BAIR: To me the shortcoming of it is not in where it can be applied, it's where it can't be applied. And do you go forward with something that is an incomplete picture of the whole thing from 20,000 feet, which would be the linear control model at three or four places, where you have sufficient data to go forward with it.

Can you ignore -- I don't know -- 70 percent of the area or 60 percent of the other production wells? And how do you enter that missing 60, 50, 40 percent into the water distribution model? And if you're missing 40 percent, how do you analyze that in an epidemiological way when you're missing 40 percent of the possible source terms because you didn't address all the wells in the flow system?

DR. KONIKOW: Yeah, one of the things that the transport model could do for you that the data don't is that at least within the framework of the conceptual understanding of things, it may show you some surprises. It may show you a pulse of contamination going by one water supply well where you have no records of contamination because it came and went before the period of observation. So things like this could be gleaned from this. You just don't know whether to believe it or not. You don't know what to do except to say, well, there's a possibility.

MR. FAYE: Let me just say that Lenny has pretty well articulated what we have discussed in our planning conversations amongst the staff. And in terms of the deterministic model about the approach, the methods and how to do it. And somewhere I hope there's a verbatim transcript of that because it lays out very well, as I said, what we have looked forward to doing.

The issues with the linear model, the difficulty there is what Dr. Bair talked about is that you need concentration data at the supply wells, and there's very little concentration data for all of the abandoned supply wells through time, and there is none.

And unless you have something going on at that well that represents in the linear model, there's no way to construct anything from that in terms of a monthly concentration, quarterly concentration, whatever.

DR. HILL: Okay, now I'm confused. Because it seems to me that you have been advocating the use of that approach, and now it seemed to me that that was a very clear explanation of why it was really pretty limited and so now I'm confused.

MR. FAYE: Why are you confused? Because it was totally presented yesterday as a screening tool. I mean, well, it was, as I heard --

DR. BAIR: Twenty minutes ago it wasn't.

MR. FAYE: -- as I heard it was to be used as a screening tool, as an adjunct to developing our deterministic model.

DR. HILL: I have definitely been getting mixed signals about how it would be used exactly.

So, and Frank, some of your comments, particularly made me think you were thinking of it in a more, in a broader perspective. So maybe you can --

MR. MASLIA: Let me clarify because we've got some objectives here that need to be mutually compatible. And that is that we need to give the epidemiologists results that they have some confidence in. And at the same time we do not have an infinite amount of time or resources. So what we need to try to do -- and I'm not necessarily talking about the December date that we had thrown out. I'm just saying in realistic, you know, we can't go on for another five years like that.

With that said we were looking to develop a screening-level method that could initially give us some rough cut or estimate to give us some handle on what the concentrations would be back in time, and at the same time, as Lenny and Bob said, perhaps help us avoid from going to the full, dispersive fate and transport approach and using a much smaller sized advective transport model.

DR. GOVINDARAJU: Well, I think one of the things that we could consider is from what Professor Aral explained yesterday, his method is allowing us at least to have an idea of what happened in the past for the wells that we have observation. For wells that we have observations recently, it can also reconstruct some of the stuff in the past.

So we could use that information and then have that also constrain the full-blown groundwater model. Because the groundwater model as it is has too many unknowns, too many things that we aren't able to pin. So having some other guidance to perhaps pin it at these locations and for wells which have no data, you're right, we have no data, let the groundwater model, full-blown model, do its best.

It'll already have a lot to do just trying to capture that. So if it is outcome guided in some other way with some other information, I think we should use it.

DR. DOUGHERTY: Okay, I'm an engineer so I'm trained to be conservative and have big safety factors on things. So with that as a preface, I'd like to move on. I'm in agreement with Lenny in many respects here. I like the idea, the linear control, the black-box model, whatever you want to call it, I think it's intriguing, and I think it should be explored in parallel.

I think hanging your hat on it is inappropriate because you're going get too many hits once the first document goes out the door. I do think it's very intriguing, and I think it should be explored in parallel in those locations where they are appropriate.

But I think we need to move past it and get on with the other significant things to deal with, which are the sources of uncertainty that drive it, pumping schedules, source locations and release times and mass loadings and all the other things that we've talked about.

DR. HILL: One thing that I'd be interested in talking about is what groundwater transport model to use. Because there's -- and I brought this up in my comments as well -- there are widely used transport models that I believe simulate the processes that are being simulated, that are of concern for this model and instead of a relatively, new untested model that's being used. In this highly political situation, I really wonder about that decision.

MR. MASLIA: We used MODFLOW and MT3DMS.

DR. HILL: Yes, but for the reactive transport.

MR. MASLIA: For the degradation, one of the reasons we went there is we thought we might need to get into the unsaturated zone.

DR. DOUGHERTY: So the plan here moving forward is to stick with MT3DMS --

MR. MASLIA: Or MODFLOW/MODPATH.

DR. DOUGHERTY: Or MODFLOW/MODPATH.

MR. MASLIA: Yes, that is correct.

DR. DOUGHERTY: So we don't foresee the unsaturated issue showing up here? I mean, this because I have a hard time --

MR. FAYE: Actually, it could because there's issues with vapor from PCE, BTEX into the buildings, particularly at the HPIA. We didn't really even anticipate a problem of that nature with Tarawa Terrace. It did show up with respect to one of the schools there, and we had, it was a good thing that we had the unsaturated zone model. So all I can say is we just don't know, but it would be handy to have because there are issues out there where it would be useful.

DR. DOUGHERTY: So do you see that in this particular study or other studies that are in planning --

MR. FAYE: Well, as it happened in Tarawa Terrace, it turned out to be a secondary thing, a post-modeling thing, but it did happen, and we did have the model there to attempt to deal with it. And so who knows? If the very same, as Mary said, this could be a highly litigious situation, and it could come up just right out of the blue as it did at Tarawa Terrace.

DR. KONIKOW: Well, in terms of informing the calculated wellhead concentrations, I'm not sure I see the connection.

MR. FAYE: No, there is none. It would just be an ability to simulate the unsaturated condition.

DR. KONIKOW: So in terms of the objective maybe that's going a bit astray then.

MR. FAYE: In terms of the objective as it's stated now, yeah. I would agree with that. But like I said, at Tarawa Terrace it was the same issue. I mean, it was a kind of a research thing to do. It worked out nicely, and we did the whole degradation scheme with it.

It happened to have an unsaturated zone component. And from the point of view though of doing the degradation, the complete degradation pathways, Lenny, that was a model that we used. It just happened to have an unsaturated zone component that came in handy later on.

DR. KONIKOW: Yesterday when we were talking about the models we, I mean, we're kind of at a disadvantage here projecting where the transient flow model and MODPATH and the MT3DMS will get us, we really never talked about them, but you were having some experience with Tarawa Terrace. And looking at some of the documents in the three-ring binder, there are still many -- maybe we need a day or two, you know, eight months from now to talk about this.

But I'm really particularly concerned about projections of degradating calculations of degradation rates or decay rates in there. Because I saw preliminary estimates using observed concentrations assuming that there’s no advection, no dispersion, no nothing else going on and ignoring the fact that there were remediation efforts going on, just using the best fit to get a decay rate. And then saying --

MR. FAYE: It wasn’t even a best fit. It was just two points at a time.

DR. KONIKOW: And then saying that that’s the rate you should use in the transport model, and this is circular reasoning that I think will be difficult to defend. So I mean, there are many issues on the transport modeling, and that’s just one example that really will leave the whole thing open to severe criticism. I don’t see any easier way around it.

DR. DOUGHERTY: Those particular pages I, those should be red-lined right now. Throw them out. I’ll be direct. They’re terrible.

MR. FAYE: Which ones are you talking about?

DR. DOUGHERTY: The biodegradation reaction section in -- I forget which tab it was under -- there are two pages ^, and they’re not biodegradation or reaction fittings.

DR. ARAL: Morris, they have to log on.

MR. FAYE: All I can say is with respect to that, Lenny, you're right. There's all kinds of limitations. We have on the one hand, we have a lot more opportunity because of data to compute degradation rates in this study from field data. But they're still limited by the same caveats that you describe regardless.

And then the other choice is literature data. All I can say is you know we'll do the computation so we'll take the field data out. We'll take the literature data and look at it and make our best judgment and defend it as well as we can. We know that. We're aware of the limitations of using those field data, for sure.

DR. HILL: Just coming back to the transport model, having the capability to deal with the unsaturated zone is fine, but usually to deal with the unsaturated zone you need a fairly fine grid. So you might consider using a very fine grid, a much finer grid usually than you need for the saturated zone. So you might consider using the more sort of tested and accepted model for some of your simulations and bring in the model with the unsaturated zone for those simulations that have that requirement.

MR. FAYE: Yeah, I think that the point's well taken. The application of that model would only be with respect to what Rene was talking about yesterday was the child models, you know, where the --

DR. HILL: Right, I understand.

MR. FAYE: And that would be a very high grid resolution.

DR. HILL: Let me just finish. I just wanted to mention that the name of that model is RT3D, which you know I'm sure.

DR. CLARK: Right.

DR. ARAL: I'm not going to defend any model or any procedure. I'm just going to summarize probably what has been said in this group this morning.

As a technician in this field in developing models and as a technician in this field in applying models, we all know that the model sophistication can be put forward in terms of its ability to model this and that and other things in the field that we observe in any which way we want.

In other words technically we are capable of developing a mathematical representation of a physical system and then computationally discretizing it and solving it. We are technically capable of doing that. And I'm summarizing that in this slide here. This is one sophistication level that we can look at. We can go beyond this. We can go backwards from this. So model sophistication from a technical point of view can go forward from that in any direction that we would like to go.

However, in an application the model to be used should be a function of availability of data in the field. We cannot go to a more sophisticated model than that if we don't have available data for the parameters that we introduce at that sophistication level because as we go forward in sophistication, we are adding additional parameters. If we don't know the parameters then the uncertainty that we introduce into the outcome is going to be greater than the capability of the model to represent the physical system.

So this is what has been discussed in this group all morning. I mean, basically, we have limited data. We have to accept that. Can we go to a daily pattern in a water distribution system? Yes, I have worked in that. Yeah, I can put a daily pattern in. But do we have that data? No. So the discussion has to concentrate and focus on what we have and what the model can do in that arena.

The other aspect of all this in my opinion, what is the outcome that we are after? Yes, the data is limitation. The model can be of any sophistication level, but what do we want as an outcome? That is the other consideration which is also discussed in this group that we need to address. The outcome is what the epi people want. Do they want monthly data output of concentrations? Do they want daily output or quarterly output? So that needs to be a driver. All of this I think has been discussed, and all I'm saying is let's summarize that, and let's look at it from that perspective.

The other concept that has been discussed here is in litigation we should use established models. Well, if you put me to a litigation desk, I can always criticize MODFLOW. I can always criticize MT3D because they are not sophisticated enough for certain applications. And we have discussed why they are not because vapor exposure. They don't address that.

So if there's a model which does an additional analysis over what other models can do, if it is available, why not use it? If it is available in terms of duplicating what MODFLOW does, why not use it? Just because MODFLOW has an earlier history doesn't make it better.

So I just want to leave it at that. I think the summary here is we have to look at the data. We have to look at the output required. The models are just tools. We can choose A, B or C if it helps us getting from A to Z, then that's okay. That's all I have to say.

DR. ASCHENGRAU: Dr. Aral, have you validated your methods against the other methods or against data --

DR. ARAL: The new method that I have talked to you today or yesterday? No, that's a totally new method. The only validation that you have seen is on the Tarawa Terrace application. That's a totally new application.

DR. ASCHENGRAU: But this third thing, the matrix, it may be --

DR. ARAL: Oh, yes, this solution that we have, I think it's the name was not mentioned but ^[TechFlowMP –ed.] ~~FLOW MP~~ is a new 3D model -- not new, started in the '90s we are working on it -- does solve these equations similar to the way MODFLOW and MT3D solves. On top of what they do in MT3D, it looks at the unsaturated zone and the vapor transport.

DR. DOUGHERTY: I think there's some confusion about which model's being discussed in terms of questions and answers. So I think Ann was asking about the linear control where it has been validated against other methods in any particular way. A majority of your comments, I believe, are on the multi-phase, multi-media.

DR. ARAL: My comments were referring to groundwater flow, contaminant transport analysis aspects. Those models can get to be as complicated as we want. But in application we are limited, as we are hearing all day yesterday and today, we are limited by the data. So the complicated nature of the model doesn't make it better in terms of an outcome if the data is not available to use that complicated nature of the model. We have to accept that.

DR. ASCHENGRAU: But it's just people who have been expressing their discomfort with some, with what I perceive as some new method that other people haven't used yet. And so I'm just trying to figure out is if we can be more comfortable with it because that new method has somehow been compared to the existing methods. And so they shouldn't be as comfortable about it. That's all I'm --

DR. ARAL: That's a very good point. We are not proposing this black-box model to be used which was developed three months ago. We accept that. We developed this three months ago. And we are not proposing to use this without extensively validating it in other areas, in other databases, so that it establishes a footing in the field. We are not proposing that. We have to test this model over and over again to have confidence on its outcome.

DR. GRAYMAN: Getting back to your comment when you referred to when you were in court testifying. I think we'd all agree as scientists we want to use the best, most appropriate method, and that sometimes is not totally in line with what you see if you're in a court case, and it just isn't. I mean, court cases aren't necessarily about the best science. They're about whatever they're about.

But it would almost be like if you were doing climate modeling and you'd developed some new climate model that had some additional processes. And you felt that this was definitely much better than what the established methods were that were tested by the IPCC and had gotten the Nobel Prize for it.

And you're in court and you're trying

to say, well, my model is better because -- and they ask you, well, has this been validated. Has it been used other places. And you say, no. You're going to be probably a lot better off in convincing the court by using one of the established models. And then so we are in a situation of science versus a legal situation, and I don't know where this whole thing is going to go to.

DR. ARAL: Well, I fully appreciate that, but --

DR. BAIR: There's a huge change in the law for expert testimony in the mid-'90s between the Frye Rule and then the Merrill-Dow Pharmaceutical lawsuit where the judge now sits as the gatekeeper of what is acceptable science. And it is up to the scientist prior to the trial and the expert witnesses or the engineers to convince the judge, who's the gatekeeper, that what they're doing is not junk science that just appeared, but it has foundations and validations in the steps that people have been talking about.

So I just, I don't know where this is headed one way or the other for lawsuits. It seems like everybody's walking around the hat without ever putting it on. But I think that effort that you've talked about has to be way up front before you put any of the effort into looking at a Camp Lejeune.

DR. ARAL: Oh, I agree with that.

DR. CLARK: Morris, you wanted to make a comment.

MR. MASLIA: Yeah, I wanted to make the point again after we completed, essentially completed the Tarawa Terrace -- and you need to, I guess, put your administrative organizational hat on --

DR. CLARK: Doesn't fit.

MR. MASLIA: -- I know, that's a problem for us. We saw the effort that it took -- and there's still a question about it, I mean, looking at all sides and all questions, the effort that it took to get the answers that we got to give to the epidemiologists.

And we were looking for an approach to speed us up to get some initial results. And we wanted an alternative because you know the amount of effort and multiply it by ten for Hadnot Point. That's at least by ten if not by a hundred. And if we do that, December 2009 is not even in the question. Probably December 2012 is not in the question given the discussion here.

So we have to, I think, look at some alternative ways. One way, as they said, let’s cut out for the time being the dispersive transport and all that and look at a flow path approach to get some indication. Another approach is where we have the information and see if we can reconstruct the concentrations from that. It does not in my opinion invalidate the use of either one. It actually may add some additional insight for us to maybe enhance the more sophisticated modeling.

And that’s what I asked Georgia Tech to do because I only had one tool in my toolbox, and we knew it was too heavy at this point to pick up and try to fix the second part of the problem. So that’s really our objective is to see what results, does that give us some additional insight while not expending as much effort and resources.

DR. CLARK: To get back to Ann’s point, are you thinking in terms of using Tarawa Terrace as a validation tool? Because you've done traditional groundwater modeling in Tarawa Terrace. Could you use that example as a validation tool for the linear control theory model?

MR. MASLIA: Well, Dr. Aral's used that already. In other words he's tested the method out on Tarawa Terrace, but again, that is assuming that the simulation mean values or whatever are, in fact, quote, surrogates for real data. Now what needs to be done, and we can go to other sites, do a literature search or go to other sites, let's test it out on some other site data, not necessarily Camp Lejeune, and see if we get similar results or results that build further confidence in it. The fact is that this approach does not take a lot of effort to run on subsequent datasets.

DR. CLARK: Do you have some datasets that you can [use to –ed.] perform those validation tests?

MR. MASLIA: I can't. I don't have them in hand or know of them at this point.

DR. ARAL: Just a few comments on what I have heard just now. Obviously, the judge is the gatekeeper and established models have to be used in court cases because they are established. That's the only reason. But that shouldn't hinder the science.

In other words science has to go forward in bringing new ideas, new models, new concepts into the field. And in the next 50, 60 years maybe they will be the accepted models to be used in the court cases. Can you imagine a world which is stuck to MODFLOW? And a hundred years from now that will be extremely limited because the science is advancing. We have to bring that new science into MODFLOW.

DR. WARTENBERG: But it seems to me that they're two different issues here. There's no question that science needs to go forward, but that doesn't necessarily address why we're here and what we're looking at. And it seems that's that's --

DR. ARAL: I know. I'm looking from a, to this problem from two perspectives. I will continue with this method. I will publish technical papers, and then it will be applied or not at Hadnot Point is a different story.

DR. CLARK: I'm going to suggest that we go ahead with our lunch break. I do have a question.

Scott was in the process of giving a presentation, and we cut him off due to ~~technological~~ [technical –ed.] error problems. Do you want to try to do it during the lunch period, [or –ed.] at the end of the lunch period?

DR. BAIR: I'd rather do it later than now. I just think the demeanor in the room will refresh itself over lunch.

MR. MASLIA: Bob, if he wants to, just before the end of the lunch break, because I am concerned --

DR. CLARK: Yeah, after you have the lunch break.

MR. MASLIA: -- because we have to meet our 2:30 to start summarizing because some people have planes.

DR. CLARK: Does 12:15 work?

MR. MASLIA: That's fine.

DR. BAIR: So I can be here at 12:15? Yeah, and I think what you're going to see are some of the comments that Ben made about what the step functions are going to look like when you get to the end of this.

(Whereupon, a lunch break was taken between 11:40 a.m. and 12:30 p.m.)

DATA DISCOVERY – ADDITIONAL INFORMATION AND DATA

DR. CLARK: We're reconvening. We're going to modify the agenda again just a little bit. From about 12:30 to 1:30 Morris and I guess Frank are going to talk about data discovery issues and new [, –ed.] additional informational data.

MR. MASLIA: And I'm basically just opening it up and let the panel also obviously join in and all that. But as you see the data that we have gone through, and there's a lot of it to consider. And we mentioned yesterday this data that are in the notebook represents the IRP Program on the base. And there is about another 100-plus documents that represent the above and underground storage tank data.

And what our proposal is or our approach to do with that is to actually separate this report that you have or the collection of, the draft report that you have, and have two sets of reports, one strictly with the IRP data, and then pull out any UST data from that report. And then have a separate report with the UST data. That's the, I think, straightforward approach to dealing with that.

As far as from a modeling or use of data in whatever form of modeling we want, whether it's calibration, verification or whatever, our thoughts at this time are probably to try to use that second set of data as almost a verification stage. In other words sort of treat it as if we don't know about it right now. Use what we have.

And then if we get to the point of where we have some confidence in model simulation in terms of concentrations or whatever, see how it compares to this other set of data. I say that because to add, put this into, quote, a calibration set or whatever, still does not get us over this hurdle of uncertainty, variability or anything else.

So I think it's maybe limiting the use of some data that could maybe even help improve our confidence in the model. That's just my thoughts right now. And I think that also helps us in terms of resources expended, people, time, money and stuff like that.

And it'll help us learn with the model what the models may be doing or may not be doing with an existing dataset that we've gone through pretty thoroughly at this point. And save that other dataset in terms of modeling that may, as I said, help improve our confidence which may be more of an advantage for us and then lumping it all together.

And I'll just throw it out and see what the panel thinks about that approach or any other approach you may have. But that's our thoughts right now as to how to handle that.

So anything else, Frank?

DR. POMMERENK: Morris, let me get started on a couple comments. And I also appeal to those panel members who were here in 2005. You know, there were several recommendations made in 2005, and if I recall it correctly, and I tried to focus the discussion back on this, was the whole uncertainty analysis and you addressed with Tarawa Terrace some of those issues where you acknowledge the model results and so on.

We saw this was at least piece-wise brought up by panel members, you know, the overly optimistic narrow band in the Tarawa Terrace concentrations that we need to address also uncertainty in other things which will be for Hadnot Point no doubt be greater. We saw it with the mass computations. So I just would like to recall from the 2005 panel meeting that one of those key recommendations was, if I recall correctly, the focus should not be on so much on the little details in the groundwater model and hydraulic model versus trying to quantify uncertainty because in all the little errors that we may make in a non-representative model or whatever, may be swamped out by uncertainties upstream. For example, in this case the mass was disposed in the first place. So I think I should throw out this just to refocus the discussion. I hope that the other --

MR. MASLIA: I think that your point is very well taken to incorporate what the previous panel said. And that was I think impacted two things. One, why a lot of effort and emphasis both the Marine Corps and Navy in going out and hiring a company to go through their records. And we spent an additional amount of time going through data and information. And then the second thing is, and this brings us back to this morning's discussion, is why -- I'll say I -- I asked Georgia Tech to try to come up with a simpler method because that was one of the recommendations out of the panel in 2005 is to look maybe at the bigger picture, but a simpler representation because of all these factors. So your point is very well taken, very well taken.

DR. POMMERENK: Yeah, just as an aside on that. You know that linear control theorem, we may not care about what the individual coefficients of that matrix or the matrices represents because we may have sources of uncertainty elsewhere that would ~~swap~~ [swamp – ed.] out any little issues that we may have with the groundwater flow model or the hydraulic model or when interconnection was there or not.

And that’s why the panel and again in my recollection, recommended the increased efforts in data discovery where they have actually hired a company to go through all the records on base. That just is a reminder. And I believe that is all documented recommendations of the expert panel.

MR. MASLIA: Yes, it’s in the yellow-color folder report there that’s available both -- yeah, that one. It’s in Section 6 of the report. That summarizes it, and then if you want the detailed actual final recommendations you can pull out the verbatim transcript that’s included on the CD there. But the report just summarizes that in generalizations. But that is correct.

And I know we focused, I mean, as an Agency we did. We hired more people and obviously tried to go through more, and I think that’s how some of this discussion on the interconnection came about as well. Because if you recall at that meeting or the generalization was made that, well, if there’s no very limited interconnection, well, simple mixing will do the trick. And that worked correctly for Tarawa Terrace.

That was, we looked, and we could not find any instances of, I used a rule of thumb of a two-week period just at Tarawa Terrace, and that was correct. But in looking further and actually understanding what was written in the logbooks, which takes some doing, you know, how they make notations and what it really means. And in discussing with the present and former operators, we came across the short intervals but pretty much consistent, but that they would turn it on in dry late spring or early summer months.

So again, I think what we do in your recommendations here are adding to the recommendations of the 2005 panel. But we do have a much more complex issue, and that's hopefully y'all can put some recommendations down that we can take to both our management and the Navy and tell them what our plan is for concluding the study. I think that's really what Frank's looking at is an exit strategy that's satisfying.

DR. BOVE: Maybe not as quickly as some.

MR. MASLIA: Well, not as quickly as some.

I didn’t mean to imply that we’re walking out the door today and that’s our exit strategy. But, no, and that’s why I think it’s motivating me to say with the additional data that we have, let’s not be quick to just use it or throw it in for model calibration right away. Let’s see what we can understand about it first, and then maybe help us improve or reduce maybe some of what we perceive to be as uncertainty or build confidence in whatever model or modeling approach we take for Hadnot Point.

DR. HILL: And just one comment on that. In terms of a simpler modeling approach, it can be a simpler physical-based model. That’s an option instead of, so there’s a lot of ways to ^.

DR. GOVINDARAJU: I just wanted to, you know before lunch we were talking about what if it were to do a court case and so on. And when you’re given this charge and when I started looking at the document, I was not preparing myself by trying to advise people by what one should do in case of litigation. And maybe if that is the case our objective functions should be somewhat different. I thought we were going to be doing this to see how we can reduce uncertainty and stuff like that. So I just want us to be able to explain that if we should be thinking in terms of what would fly in a court of law or see what we can do --

MR. MASLIA: Well, the answer is anyone can sue or sue anyone at any time of the day, but for anything, so no, we're not gearing our study for that. What we're gearing our study for is for to be able to provide the epidemiologists and the epidemiologists to be able to assess epi results.

DR. BOVE: Maybe I should say this. There is not much in the literature about the health effects of these chemicals from drinking water exposures. But there's even less about birth outcomes in these. So the main reason we embarked on these studies was to add to the scientific literature. I mean, that was the primary goal here. People want to know what the effects are of these chemicals. Well, we have occupational data, but we have very little drinking water data. We have a birth defects, one study in New Jersey looked at birth defects that so far has been published. We have a few studies looking at cancers and these chemicals. And so that's what we have that are published, a few studies out there, and some of them may not even agree with each other or they do to some extent with very little good exposure information as well. So that's what the literature is out there. We want to add, make a major contribution if we could to that literature. That's the primary goal here. It's not litigation. It has nothing to do with litigation.

DR. CLARK: Dick, you have a comment.

DR. CLAPP: I was just pointing at Dr. Aschengrau, who's done some of the studies.

DR. DOUGHERTY: I have two things. One is I took the litigation court of law as a metaphor for other courts of opinion that bear on reliability and judgments of reliability.

Second was a question. In the data that we're talking about, do we know the contents of these tanks?

MR. MASLIA: You mean the contents of the database?

DR. DOUGHERTY: No, what materials were in these, what chemicals are we talking about?

DR. CLARK: In the new information.

MR. MASLIA: Oh, in the new information.

DR. DOUGHERTY: Yes.

MR. MASLIA: Bob, I haven't looked at it. I just catalogued the information, but Bob can generally describe what's there.

MR. FAYE: Some of the tanks were just pure gasoline, diesel fuel, heating fuel, waste oils, that's pretty much the gamut of the contents.

DR. BAIR: What else could you wish for?

DR. WADDILL: Would you like me to clarify that?

MR. MASLIA: Yes, please.

DR. WADDILL: In regards to the new documentation, this is all leaking underground storage tank program studies, records of decision. Clean up information related to the leaking underground storage tank program per NCD nuregs*. So it's all POL contamination. Any solvent contamination falls under the IR Program per ~~CIRCLA~~ [CERCLA –ed.].

DR. DOUGHERTY: What about the waste oil?

DR. WADDILL: Waste oil if it's solely benzene or BTEX or POL falls under the [UST program –ed.]. If it has solvent co-contamination it usually goes into the IR Program.

DR. DOUGHERTY: Thank you.

DR. ROSS: I have a comment that that information may be useful because of all of the compounds, the BTEX compounds are going to serve as good fruit for the bugs for one thing to break down the solvents over time.

DR. POMMERENK: Okay, since nobody else is saying anything, I just want to make one comment so it's in the record. Because we've been talking all day today and yesterday about the groundwater flow model and then the water distribution system model, and the one thing that I would like -- that's why I want it in the record -- there's a big five ~~entity~~ [MGD –ed.] treatment plant in between, between the groundwater collection system and the distribution system.

It consists -- and correct me if I'm wrong -- of a ^ [ground storage –ed.] tank. I don't remember what the size is, but it's probably a million gallon or larger. The

Hadnot Point plant has a pump station that pumps water from that water collection tank into what are called catalytic softening units or ~~spiracteristic (ph)~~ [spiractor –ed.] cones to which ^ ~~line~~ [lime –ed.] is injected to facilitate softening and it overflows into a central pipe.

It goes from there through ~~a currently still through~~ [–ed.] a rectangular basin that used to be a re-carbonation base, and I'll get back to that. And from there into gravity filters and you know after chlorination and fluorination into a finished water clear well.

Obviously, in this facility there's several quiescent or not so quiescent surfaces from which ^ [volatile –ed.] organic compounds can escape. And that kind of depends on the physical properties of these compounds, PCE more so than TCE and so on. We made an estimate a few years ago, a rough estimate, that probably PCE and TCE, we didn't look at BTEX, removal would be incidental, minor, probably. The tanks are covered so there's no way effluents could stir up things.

However, what was not looked at that

was, because of lack of information is the re-carbonation basin. The re-carbonation basin serves to, it's typically a small, flow-though basin to which you inject carbon dioxide that is generated from a propane generator or from gas bottles. And carbon dioxide is an ~~asset~~ [acid –ed.] in water and ~~increases~~ [decreases –ed.] the pH which has been pretty high prior to, because of lime addition.

So that's how this whole softening process works. You bring the pH up you're still going to have calcium carbonate. Bring the pH back down within the allowable limits. So as far as I know, and as far as I can recall, I've never seen this basin in operation. It was just water flowing through.

However, it was put in for a purpose originally some time in the '40s, and nobody can tell me exactly if it ever has been operated and how long it has been operated. Because if it has been operated, it could have ~~been~~ [caused –ed.] substantial removal of PCE and TCE. It would have been in the 90 percent removal.

And it kind of depends on the gas flow

rates. It kind of depends on the turbulence that got generated. So there's a variety of factors that would have presented. But it could have affected removal of these compounds in the plant. And again, we just looked at PCE and TCE as from volatilization from the basins that are there, not ~~re-carbonization~~ [re-carbonation –ed.] because we didn't have any additional information.

But it might be worth looking into BTEX volatilization from the basins, you know, whether that as a source is uncertainty again. And I'm not trying to get exact numbers or anything, but it's another source of uncertainty for the exposure calculations for what could potentially be the removal of these compounds from the plant, A. And B, finding out whether this has ever been online, this re-carbonization basin.

MR. MASLIA: Hopefully, we're sending five, six people up to Lejeune this month, sometime this month, because in the BAH when they indexed the records that were there, we looked at the Tarawa Terrace stuff knowing that we would be back to look at Hadnot Point. And so there may be some information on that in those records. I don’t know in other words. So we have not gone through the ~~BAX~~ [BAH –ed.] information index and then told, you know, requested that those documents be pulled, if in fact, there are documents in that index that would be useful.

DR. POMMERENK: You may want to look first in any purchasing records of propane or whatever they used. You may want to start talking to Bernash* [sic –ed.] when you get down next time with him. I can’t imagine it has never been used because it’s still comparable, softening plants operated by the Navy or Marine Corps. Kings Bay, Georgia, they still use re-carbonation basin. Guantanamo Bay has recarb basins, you know, it’s not uncommon. So if you look for these kind of records. I always find these kind of things.

DR. DOUGHERTY: So, Peter, when you were there and there was not ^, were they not dropping the ~~TH~~ [pH –ed.] or was there some other procedure that they were doing?

DR. POMMERENK: As far as in dealing with that plant, they've always softened just below -- well, this is the secondary MCL anyway. The ~~TH~~ [pH –ed.] leaving the plant should be below nine, and they're always, eight-eight, eight-nine, fluctuating. Of course, you know, you have a certain ~~goal treatment~~ [treatment goal –ed.], the soft pH, its hardness, and if they get within their 60-to-80 milligrams per liter ^ carbonated range with that pH, that's -- in fact, Holcomb Boulevard is operating in the exact same manner and so is New River across the river when it was still operational as a lime softening plant. So it's not uncommon with that type of water that you would soften at a somewhat lower pH and not adjust it finally. So that's not uncommon to do that.

DR. DOUGHERTY: I just wanted to know if there was a different process that they had temporarily used or if it was just as he's described, and they just bumped it up just enough and left it there.

MR. MASLIA: The pH throughout the system was fairly high. It was higher than I've seen in other distribution systems. Because when

Jason and I were there, we were doing the field test, we first thought the instruments were out of calibration because it was always well over eight, 8.5, 8.8, I mean.

And that's why we thought there was something, you know, we had to go back and recalibrate the instruments or whatever to make sure. But then we checked with them inside, so it's a pretty high pH.

DR. POMMERENK: With a gain in precipitation.

DR. ROSS: Downstream?

DR. POMMERENK: I can't say. I mean, you know they have had problems. I have pictures, in fact, one of my memos that I sent to you a while ago ~~it picks up~~ [depicts –ed.] the ~~spiroactors*~~[spiractors –ed.], so they get pretty badly encrusted downstream. So all the softening is not done in the ~~spiroactor~~ [spiractor –ed.]. Softening's going to go on throughout. That's been one of the hassles that they've always, ^ has been complaining about. Now, I cannot say for sure what, how much precipitation's going on in the distribution system, but, yeah, it will happen. And now to bring up a point here. How does that affect ~~POCs~~[VOCs –ed.].

DR. CLARK: Is it possible that they had cast iron pipe in the system at one time?

DR. POMMERENK: Yeah, you should be able to see. We inventoried that system.

MR. MASLIA: No, the system is cast iron, and then when they would replace them, now presently when they replace them, they presently replace them with PVC. They've got a few lines of ductile iron and very little AC pipe at all. So it's mostly cast iron and PVC now. And one would think it was historically then cast iron.

DR. POMMERENK: Two years ago we had excavated some pipe, four-inch pipe, in New River which is across the river on the other side where they also until 2007 operated a lime softening plant in a similar manner. And they got water from wells in what is called the ∆ [Verona Loop –ed.] area which is, you know, you can see it west of New River, you know the left, top corner. Left top, left, left, left, left, left. All the way on the left is --

DR. DOUGHERTY: The ~~N~~[M –ed.]-C-A-S, Morris.

MR. MASLIA: Oh, here, okay.

DR. POMMERENK: Right down there, ^ wells from a hardness standpoint a similar composition as the wells at Hadnot Point. And again coming back to those pipes that we excavated, I don't know exactly where they came from in the system, but they didn't show any large amount of scale. There was ~~tuberculation~~ [precipitation –ed.] and you could clearly see on there ~~tuberculation~~ [precipitation –ed.], various layers of all the different iron oxides and ^ mixtures of that. But there was not a distinct calcium carbonate layer.

DR. DOUGHERTY: Do we know the frequency of well rehabilitation just as another indicator of this?

MR. FAYE: We have some records of actually a lot of records in the early '50s and perhaps up to '65, '66, '67. Then there's a gap, and then beginning in '78 up through '85, '86, '87 we have records of gross rehabilitation. On the one hand the records may indicate things like notes in the margins, well down May, bearings replaced in pump. Or well down in October, air line replaced. Things like that. So you have to make a judgment. Was it down for three days or three weeks? So that's kind of the extent of that kind of information.

DR. DOUGHERTY: So there's no direct information that the well was acidized or ^ [cleaned –ed.] up or something?

MR. FAYE: In some of the records that are quite detailed, I've never seen those kinds of activities take place or have no indication that those activities took place.

DR. KONIKOW: I wonder if some of the local well drillers would have that information more readily available than the Marine Corps base, maybe foot work there might.

MR. FAYE: Well, that's a good question, Lenny, and it's a possibility based on my experiences with drillers, some of them do keep really good records. On the other hand a lot of folks that work for government, and particularly the military, I think they took their training from squirrels. They take care of everything. They hide everything, and so I got a strong hunch if those records were available, we'd know it.

DR. CLARK: Anybody else have any more comments at this point?

(no response)

DR. CLARK: Well, one thing that occurred to me, [and –ed.] I think Frank maybe alluded to it at one point, is the possible extension of the study to include something other than birth issues. Some of the levels that were being distributed in the finished water almost ~~looks~~ [look –ed.] like occupational exposure levels and could [have –ed.] inhalation and dermal effects.

And I think you've mentioned that you're giving some consideration to extending the study to include that, but I didn't know whether you wanted to talk about it now or not.

DR. BOVE: Just briefly, we have two studies that we're going to embark on this summer. One is a mortality study of adults obviously which will take into account hundreds of thousands of Marines at the base plus a comparison group at Camp Pendleton population. And with that, monthly data, of course, isn't as relevant in that kind of a study as it is with a birth outcome study, the small for gestational age study or the case-control study we were talking about all day.

The other study is a health survey which is going to ask people about their, any cancers they may have had and other diseases that we think are related to solvent exposure that we see in the occupational literature as well as any information from the drinking water literature, which I already said was very sparse. And then we'll confirm those diseases as well as we'll confirm the deaths and find out the cause of death.

So that's roughly, without going into too much detail, what we plan to use this data for as well as the current case-control study and the re-analysis of the small for gestational age study. So any questions about those two studies I can answer them, but just so you know that what we produce here in the water modeling will be used for additional studies.

DR. CLAPP: I don't think he's talking about dermal or inhalation exposure as part of the extension. He's talking about different study types.

DR. BOVE: Right, what we assume -- well, in the health survey as well as the case-control study, we do ask about people's consumption habits, how long they shower, for example. So that we start getting at some of those routes that way. But really, we assume that everyone's pretty much getting the same kind of exposure. They're showering roughly about the same amount. They're getting the same kind of dermal exposure, and they're ingesting roughly about the same amount of water.

MS. RUCKART: Frank, we don't ask about that on the health survey.

DR. BOVE: We don't ask about their consumption at all?

MS. RUCKART: Just the case-control.

DR. BOVE: Okay, I'm getting confused between studies. That's right. For the case-control study we ask that question. Actually, as I said yesterday, the usefulness of that information is not that good.

There are also civilian employees who were exposed and there we're going to take into account their occupational exposures as well as -- and also the military have occupational exposures, too, and also where they drank water at their occupational sites, workplaces. So these are things that we're going to take into account in the future studies.

So does anyone have any questions about that? I don't want to get into that because we have so much to discuss about the modeling and wanting to get advice. We had an epi panel actually a year ago discuss these two studies and the issues there.

DR. CLARK: Any reaction to ~~your~~ [the –ed.] comments or thoughts on that?

(no response)

DR. CLARK: I know when we were ~~doing,~~ [-ed.] setting a radon standard ~~in~~ [for –ed.] drinking water, we looked at some of those kinds of issues. So there is some literature in terms of --I think it's the University of Pittsburgh that actually has a physical shower where you can go and measure the transfer of water of the radon from the water into the air. And I would assume that [at –ed.] some of those levels [, –ed.] that eventually the household would be basically saturated with~~,~~ [–ed.] volatilized ~~with~~ solvents [BELJIN –ed.], which would apply not only to the Marines, but also their dependents and children.

DR. BOVE: Right, and then there's also some concern, for example, cooks at the, in the Hadnot Point area getting heavily exposed.

DR. CLARK: Yes.

DR. HILL: Laundry workers?

DR. BOVE: Laundry workers, yeah. So we'll be looking at them in the future studies.

DR. CLARK: I ~~gathered~~ [gather –ed.] from what Mr. Ensminger was saying, that he has had contacts from people who'd been on the base and adults who've had follow-up health issues that kind of were linked to that sort of exposure.

DR. BOVE: That's why we have to do these studies.

DR. CLARK: This is the quietest I've ever seen this particular group.

PANEL DISCUSSION: INCORPORATING AND USING ADDITIONAL INFORMATION AND DATA

DR. HILL: I don't know if we want to get into this now, but Lenny and I were talking at lunch about looking at the model fit, and methods to do that and some of the results.

Lenny, am I interpreting our discussion correctly and did you want to start with that? So it was model fit and the use of the sort of preconceived criteria for measuring whether or not the future model fit was going to be good enough. And I'm not quite sure, this is a discussion that's sort of better done with a bunch of maps on a table and pointing at this and this and saying why is this ^.

So I'm not exactly sure how much of this can be done in this kind of format, but a couple of general things I'll start with was there's -- and I'll start with the head data just as a beginning -- and essentially what head data gives you is sort of the pipes of the groundwater system, kind of what are the directions of flow. It's sort of similar to topography on a land surface, but it's fully 3D, and you can't see it. And it's hard to figure out.

And so the heads and the geology are

essentially what we have to constrain that and also where concentrations go. And so in this model there were two kinds of head data. The data in pumping wells essentially taken with air lines, which are known to be extremely problematic.

And so one of my concerns was even that they were put on the same graph with the other kinds of head data. It seemed like it should be analyzed separately. And one of the things that allows you to do better, too, is to look for patterns within the, so the residuals are the observed minus simulated. And ideally, they will be random spatially in the system, and any distinct non-randomness suggests bias in the model.

And when you had observations like those air line observations that have so many known problems, it’s really unclear whether, what they represent and how much you can depend on them. And it could be that some of them should not be considered at all and others have good information.

But we have to look at where they are in the system and what trends they might have.

Does it make sense? If the pumping from the well is greater, do they actually -- you know, do they make sense? And a thorough analysis of that was perhaps outside the realm of some of these reports, but really, without that analysis, my feeling was there was just a lot of data kind of thrown in, and it didn't fit very well, and there were some patterns in that set of data.

In particular, if I looked at the graph, there's a band that goes through a certain, I think it's observed versus simulated, and I think the simulated range is 13 to 15 or something like that. So you have a band that goes through. So there's issues related to that. Maybe I'll stop there. You were looking like you wanted to say something.

MR. FAYE: No, actually, I agreed with almost everything you say. And also, I don't take exception at all to your comment that we threw everything in there but the kitchen sink. You're exactly right. And it just came down to a choice of on the one hand we felt that we would be severely criticized if we didn't try to deal with the data, and on the other hand we felt we would be severely criticized if we did deal with the data. So we came down on the side of inclusiveness and did our best. In fact, I appreciate your comments very much about the air line measurements because, frankly, there are some people that just don't believe you and me that those measurements are totally perfect, but be that as it may.

DR. BAIR: Who are those people?

MR. FAYE: Well, I can mention a few that I'd rather ~~now~~ [not –ed.] ~~of~~ [have –ed.] met, but I won't. But anyway, your thoughts, I've read your notes about the residuals and the variability of the accuracy of the data. Very well taken, and we definitely have already decided to do some major analysis of the data before we try to use it in this next model, and so I accept that.

The only point I would take exception to is the, I think it's your notions about the graph and the boundary lines on there. I thought I was doing a good thing when I copied that directly out of the USGS report, but so be that as it may, it is what it is and I appreciate your comments very much.

DR. HILL: In other studies I've been involved in if you don't have every data point somewhere, someone will come and say did you pay attention? Did you do this? Did you do that? But my thought is that it could be, that some of those points, I think this is consistent with what you're saying. Some of those points can appear in graphs that are used to determine a trend, and then the trend is used in the model calibration so it appears in the report just not as a verbatim --

MR. FAYE: Yeah, in the report obviously we tried to have our cake and eat it too. We did not deliberately, explicitly attempt to weight the data, weight the head data. The real accurate data was fine, but what do you weight the other data as? Is it a 1:2, 1:1, we just didn't know.

So we didn't deliberately, explicitly attempt to weight the data from a formal analysis point of view. But then on the other hand we did spend a lot of time explaining why one set of data was better than the other. We tried to have our cake and eat it too, and, yeah, I'll take that. I'll take a hit for that.

DR. HILL: That's all right. I don't mean to hit.

Let's see. Another aspect of that is the idea of sort of pre-processing the data, thinking about it spatially and stuff and getting trends. It could be that there are situations where, for example, that vertical thing we were talking about where there's a three-foot decline at head. It might be better to use that difference it had and have some observations that are changes with depth, changes at head with depth.

And specifically, and basically take your data and -- on the one hand that's three feet. On the other hand you are saying you think your variability is plus or minus three feet. Okay, so then that begs the question do you have faith in that three-foot change.

Is the situation such that because of where the well is or blah-blah-blah-blah-blah, that you really think it is pretty close to a three-foot decline which means when you take that difference, you're getting a small, you're getting rid of errors that might be constant in some manner and actually the difference, you have more faith in the difference than the actual values.

MR. FAYE: No, actually, those are very accurate measurements. So, yeah, I can answer both because that's a bona fide well cluster for the State of California. So it's good data. I mean State of North Carolina. My dreams have overtaken reality there for a second. But we really didn't have data like that to that detail, Mary, at Tarawa Terrace.

But we've got gobs of data in the Hadnot Point-Holcomb Boulevard area where we have well clusters, vertical gradients and both at substantial depths even. So we can really identify those issues in some pretty good detail using actual field data. And it would be typically like you would suspect.

In the Berkeley Manor area they're sort of in the center of Holcomb Boulevard, which is a highland area, your vertical gradients are downward. You're close to the Wallace Creek and other major drainages. You've got your heads coming up. HPIA is a similar area. It's in a highland area. You know, your vertical gradients are downward, et cetera, et cetera. So it all fits a pretty good conceptual ~~Hubbard~~ [Hubbert- –ed.] type model of the flow system, so it works pretty well.

DR. HILL: Yeah, it's the graphs ^.

DR. BAIR: I was just going to say I think that that's a really worthwhile calibration target under a transient flow because you're going to have certain pumping conditions that either exacerbate or mitigate that vertical gradient. And if you incorporate that as a calibration target, that in turn, helps you pin down the hydraulic conductivity to the confining layers which so far one foot per day because it's the confining nature that's going to give you that large gradient, only a small grade.

MR. FAYE: Absolutely, and also from a limited number of aquifer tests, and again you have the scale issues that you have to deal with in terms of point data versus extrapolating it out to a large model cell and all that. But we do have some fairly decent data, ~~Noyman~~[Neumann- -ed.] Witherspoon* and where we've been able to apply some nice aquifer test analyses and determine ~~leak-ins~~ [leakance –ed.] of confining unit. So for whatever it's worth on a scale issue or a scale-dependent value, we do have some of those data.

DR. KONIKOW: Well, this also gets to, I mean this first modeling phase, which developed a steady state, full model representative of pre-development conditions. And that's part of our concern, I think, on the data that you use in the calibrations is that much of the data is so influenced by transient conditions that it just probably shouldn't have been in there.

MR. FAYE: That's really not true. And that wasn't true at Tarawa Terrace either although I think one of you gentlemen might have, someone might --

DR. KONIKOW: I thought you were saying that some of these, some of the data used from all those measurements were influenced by --

MR. FAYE: They are. They are. But those data were not used, to the best of my knowledge, in determining the pre-development surface. And also at Tarawa Terrace I think there were like 50 or 60 measurements that I listed in the report that I said, okay, these were estimates of pre-development heads. And someone did mention that they were possibly influenced by pumping, and that is correct. Six of those 60 were perhaps influenced by pumping, but I --

DR. KONIKOW: I'm talking about the 5,000 or so observations that were --

MR. FAYE: A number of those, Lenny, if you've got ten years of data, and you can see how it varies over time and the data are near a pumping well, and you can see -- or a supply well, and you can see some or infer that they are being, that the heads are being influenced even though the screens in the supply well are rather deep, and you're looking at shallow, et cetera, et cetera.

But you have ten years of data to look at. So you can either select a data point that seems to be the highest point or the one that isn't influenced if you really, really, really want to use that point as a control point or you can disregard it.

But obviously 5,000 measurements, hundreds of sites distributed throughout the study area, you have an opportunity to filter your data pretty readily. And at most of the sites there was no, virtually no influence except seasonal influences. And if you got 20, 30, 40 measurements over ten years, you take an average, et cetera.

So that's pretty much the way those control points were developed. There was a pretty serious effort to filter out influences from anything other than seasonal variations.

DR. KONIKOW: Okay, I didn't gather that there was, but okay.

DR. DOUGHERTY: In the ~~permutation~~ [presentation –ed.] it said that there were some obvious ones to pull.

MR. FAYE: Pardon me? Oh, Rene said yesterday that he needed to look at some of the data in addition. If he said it, I believe it, but it wasn't a pervasive issue with respect to the representation of the potentiometric surface that he's showing. I'm pretty sure of that, that he showed.

DR. KONIKOW: Now, when you go from the steady state model ultimately you'll be going to a transient model. I think you have to be open to the idea that your boundaries and boundary conditions and discretization, particularly the vertical discretization, that may be adequate for a steady state model, might prove inadequate for a transient model. And you may have to go back and revisit.

MR. FAYE: Absolutely. Those are, that's good advice, and I believe that we've got our arms around that issue pretty well.

DR. KONIKOW: On a more philosophical level perhaps, I'm not sure I saw the value of setting, you know, pre-determining calibration targets in terms of accuracy and fitting. I'm not sure I saw any outcome.

In other words it's just something to measure against and one of the values of doing that is you're assessing the accuracy of the observations. But beyond that saying that your goal is to come within plus or minus three feet or 12 feet, I don't see the value of that if you don't meet the target and then don't do anything about it.

MR. FAYE: Well, that’s not true because it’s a target that you meet as well as you can. So what you see as far as Tarawa Terrace is concerned is our best effort to meet the target. So you don’t know what the worst --

DR. KONIKOW: You’re always making your best effort to do the best that you could.

MR. FAYE: That’s right. But before I get to the issue though of calibration standards, good or bad, though, you didn’t see what our worst effort was. So we progressively got better and better and better. So you saw our best effort in terms of the calibration standard.

And, frankly, I agree with you a lot, and I agree with what Mary’s comments were and her notes as well. From a practical point of view I think having some explicit standards up front at the initiation of calibration are kind of a good idea. It gives you sort of a target to shoot for based on your best judgment about the quality of data, et cetera, et cetera, et cetera, but at the end if you, whether you really represent it as such or don’t, I don’t really see it as a major issue.

DR. KONIKOW: Well, I mean, I'm just getting at what does it mean.

MR. FAYE: It was more of a tactical tool to provide some guidance perhaps I could say during the calibration process rather than something that we, and I think Mary made the point that you might focus too much on appeasing the standard rather than on the conceptualizations and all the other things that relate to a good calibration process. But I don't think --

DR. KONIKOW: I mean, my concern is it's not a standard. There's no standard approach for doing that and picking a number ahead of time really is rather on the arbitrary and subjective side and doesn't lead to any action afterwards when, I think, in the steady state there were, if I recall, 55 percent of the wells or the observations fell outside the pre-determined calibration limits. And so that's not a very good, you didn't meet the target.

MR. FAYE: Well, I would also say that that effort is, as Morris said this morning, that that effort is somewhat to substantially incomplete right now. I mean, it was just a point in time that the staff said, okay, this is as best as we're going to do up to this time to get a notebook ready to send out to the peer review panel.

Your point's well taken. I'm not really arguing with you at all. I'm just saying that in terms of what I did, what I personally did and what I personally used it for was, like I said, sort of a tactical tool to make me feel warm and fuzzy if I got close to it during calibration.

DR. BAIR: I guess what I'm hearing is the panel people saying that philosophically that they don't really care for that type of criterion. And we would recommend that you kind of drop it. I'd much rather not meet a really stringent requirement than barely meet a very loose one myself. And I think a more accepted calibration target might be the mean absolute error over the total relief in the water table surface. So if you're at 100 feet of relief and your mean absolute error is ten, you've got about a ten foot error over that distance. If you're in a mountainous terrain, you have 1,000 feet of relief, a 100 foot error is ten percent. You're in a very flat terrain --

MR. FAYE: Well, we have -- if you look at our good data, you know, the what we call the monitor well data, I think our mean absolute error for almost 300 of those data points was less than two feet. And we have a total topographic, i.e., water table drop of about 30 feet. On the other hand if you look at the air line data --

DR. BAIR: Yeah, dump the air line data. They're ruining you.

MR. FAYE: Your notion of being ruined might be my notion of saving my ass, so that's kind of a relative thing. But it is what it is, and I accept the philosophical, it's really not a philosophical difference of opinion. As I said, I agree. And how we apply that, and how we use it will hopefully be more pleasing to y'all the next time around.

DR. HILL: I think, just one thing I want to say is when you publish a standard, when you, I don't mind you having that in the back of your head and feeling warm and fuzzy when you make it, but when you put it out front in the beginning, you set an expectation up. And I think it’s that disappointment of expectation that you’re having trouble with.

MR. FAYE: I agree, no problem.

DR. CLARK: We have a comment. Randall has a comment.

DR. ROSS: Just a question. Out of the 5,000 or so historical measurements you had, it seems like you said a minute ago you took the average, but I seem to recall you tried to take the highest elevation. And in a situation where you have precipitation ranging from less than 40 inches to 80 inches between years, would the high measurements kind of bias?

MR. FAYE: Yeah, there’s no question about that. And if you’re referring to the Tarawa Terrace, we only had less than a hundred compared to the 5,000 or so there. So we really didn’t have an opportunity to select through a lot of data for Tarawa Terrace. I can’t even recall now. I think there was something like 60 measurements that we actually ended up using to estimate a pre-development surface. Some of those were earliest in time, and some of those where we might have had two or three multiple measurements at the most other than the air line data. Again, let's not deal with that.

DR. ROSS: I'm with Scott. Bag the air line data.

MR. FAYE: Yeah, bag the air line data. But the good data, and those were all what I would call high quality data that we used there for that potentiometric surface. Where there were two or three measurements that we actually did have at the same point, I might have used again the highest there, not necessarily the earliest in time but the highest. It was a --

DR. ROSS: And something that we see at sites all over the place is the lack of good survey data for the wells. It's, for god's sake given the cost of surveying the monitoring points is nothing compared to the other efforts that are going on at the site.

MR. FAYE: Again, most of those data that are in that table for that use, those points were surveyed in. And I don't know whether it's actually explicitly noted in the report or not, but it's true with all the tables in Chapter C, if you happen to see head data reported to the tenth of a foot, those were all surveyed-in points. If you happen to see data published to the nearest foot, those were estimated from topographic maps or something like that. I don't know that it's explicitly said in that report, but that was the protocol that was used.

MR. HARDING: Dr. Faye, let me ask a question on that because I thought I saw in there -- I'm poaching on the groundwater folks -- a plus or minus two and a half foot standard for those ground surfaces that were taken from the topographic maps. Why can't that be refined at low cost nowadays? I'm just curious. Is that worth the effort to go refine that since you've got this N-square error of two feet? It seems like it's a pretty big chunk of it.

MR. FAYE: I think it might be mixing some apples and oranges there.

MR. HARDING: It could easily be.

MR. FAYE: To answer your first question, no, I don't think it would be worth the cost of refining those data at all. Second of all, most of those 5,000 measurements that we talked about for Holcomb Boulevard/Hadnot Point, 5,000 plus measurements, I would say, well, certainly the vast majority of those relate to wells that are surveyed in.

And your two and a half foot issue there is kind of a, I don't know whether it's ever been formally recognized, but in 30 years of work sort of a standard rule of thumb that I've always used to estimate that altitude using topo maps was plus or minus one-half the contour interval. And the standard contour on these maps that we were using was five feet, i.e., the two and a half plus or minus rounded off to make it simple to three feet. And that's where the three-foot standard came from.

DR. DOUGHERTY: Just to follow on, first, I'm working on a project with some reasonable data of questionable quality for reference elevations, and we used a similar topographic approach. So I'll just give you some validation on that. But, and you can do it, because it's not that expensive, but sometimes it is. The thing I was going to talk about was where these calibration curves, and again this single plot that we're looking at, the Q-Q plot or the one-on-one plot. If I didn't have the units' ~~little blanks~~ [unit slope – ed.] to guide my eye, I would not get a one-on-one slope for this. I would say this is on an inclined line that has a break point and the slope of each leg, neither one has a slope of one. So this is a fine type of plot, but if you did the residuals versus the head, I think you'd find that the errors are not homoscedastic, and it would lead you to, the residuals are not constant with the observed heads.

MR. FAYE: I'm not sure there's a sexual preference to the points but --

DR. DOUGHERTY: It's more political because you've got red points and blue points. I did notice that. Where are the purple points? If you looked at these residuals as a function of observed head, I think you'd find that there is a structural issue that might inform you how to go forward from here.

MR. FAYE: No argument. I think Mary articulated those issues I think really, really well in her notes and we acquiesced on behalf of the project. I'll just say that we acquiesced to those sentiments and heartily agree, and we'll follow through on that. No problem.

DR. HILL: So we have yet the concentration data to discuss? And are we ready to go on?

DR. CLARK: I'm going to suggest we take a break. A couple of housekeeping things. Who has flights that are going to be tight?

(multiple responses)

DR. CLARK: Anybody else?

(no response)

DR. CLARK: Liz, can we make sure that they get some better transportation?

(Whereupon, a break was taken between 1:40 p.m. and 1:55 p.m.)

DR. CLARK: First, Mary would like to start a discussion on the concentration calibrations. And then after that, we'll do that for about ten minutes, and then we're going to go around the panel, and I'm going to ask for every panelist to give his opinion and summarize for the record. And I think Walter and Ben are tight on time. Who else, somebody else was going to go with you in your cab. Dan, okay, so three, so when we start out I'm going to go with Walter, Dan and Ben.

MR. HARDING: I don't think we're that tight.

MR. FAYE: That's really famous last words.

DR. CLARK: Well, let's start the discussion that Mary wanted to have.

MR. HARDING: Then we have a three o'clock cab.

DR. HILL: This will be real quick because Lenny's laid all the foundation or the foundation I was interested in. And that is to take the concentration data and first calibrate, use it to derive effective transport paths and use those to calibrate first to get yourself in the right direction and then obviously, and then really manage your water table non-linearity to your advantage.

Don't let it, because that can add 50 percent to a project. It's amazing. And then when you do bring the concentrations in you can weight them so that you can consider your heads at the same time and your stream flow, we talked about the stream flow gains. I'll open it up if anybody has questions or comments about that.

DR. KONIKOW: You kind of mentioned earlier that you have quite a lot of variability over short periods of time in the observed concentration. And that's really going to be a big obstacle to calibrating the model.

MR. FAYE: It was and it is.

DR. KONIKOW: Look at Figure F-16 in your Tarawa Terrace report. You have this simulated curve that's coming up, a nice smooth curve, and then there's one point in, I guess, 1985, where you have five frequently, samples collected over a short period of time --

MR. FAYE: I know.

DR. KONIKOW: -- and they have a range much greater than the long period of the --

MR. FAYE: I know. I know, Lenny. Let me make a comment on that, and in part of my comment I'll reference, for example, the Table C-7, if you want to check that out.

DR. KONIKOW: Yeah, I've got it right here.

MR. FAYE: There's a lot of reasons for variability of the concentration data. I'm not going to go over all that again. We know sampling, et cetera, et cetera. And the point that I'm about to make I also make in Chapter F, perhaps not well, but I attempt to make it anyway.

My belief is that the major variability that you're looking at in terms of TT-26, I think in about a 28-day period, there's a two and a half order of magnitude difference in the water quality that was as a result of sampling at this well. The highest measurement and the earliest measurement, I think which was about 1,580 micrograms per liter, that's the greatest measurement, and that's the earliest measurement.

That was sampled actually when that well was probably still operating routinely before they formally shut it down or was very, very, very close to the time that they actually shut it down. And the subsequent samples there that were compressed within about a three- or four-week period of time were, my guess is -- this is my supposition --

were probably sampled with perhaps the well turned on to evacuate maybe two or three casing volumes or something like that.

And as a consequence, the result was the fact that there was not a lot of contaminants solute in the well at that time at a concentration that would have been there if the well had been operating for 12, 13, 16 hours, whatever, and more that mass of, from the center of mass of a plume had been attracted toward the well at the time.

And we see that. I give an example with respect to TT-23 in Chapter E, I believe, and Chapter F where indeed TT-23 was operated for two hours and sampled and then operation continued for another 22 hours so it was operated for a total of 24, and the contaminant concentrations doubled in that period of time.

So my point is, after this long and drawn out craziness, is that there’s an issue of how these supply wells were sampled in terms of the length of time that they were run prior to sampling. And I think that accounts for a large amount of the variability that we're seeing.

And you can look at 602 is another example on page C-7 that the analysis there on November 30th, 1984, that well was still operating routinely at that time. And it was very shortly after that shut down, and then subsequently sampled quite frequently at week intervals or several day intervals after that. But it was not operating routinely at that time.

Well, the latest data, water quality data, that we have for the supply wells, I think as far as data that I have, is for the year 2000, and there was a massive undertaking on the base as well as over at the air station to sample supply wells at that time. And the protocol observed for sampling at that time was to let all of the supply wells run for 24 hours and then sample them. So I think finally the issue, the sampling protocols, were catching up to the real world finally by the year 2000.

DR. KONIKOW: So this gets at really a basic issue of when you get to the calibrating the ~~solu~~ [solute –ed.] transport model, what are you calibrating it against?

MR. FAYE: We made a point in Chapter F, I believe, that we, again, perhaps we tried to have our cake and eat it too, and maybe got a ~~stomach ache~~ [stomachache –ed.] over it, but we made a point that we say that we believe these data are more realistic in so many words than other data. And again, it was this earliest in time data.

DR. KONIKOW: Shouldn’t you say that before you calibrate the model though?

MR. FAYE: Pardon me?

DR. KONIKOW: Shouldn’t that, I mean, in keeping with your setting of pre-calibration targets, shouldn’t your decision about which data are more reliable for a calibration bracelet[bracket –ed.], that assessment should be made before you decide to see which fit match better.

MR. FAYE: We did. Those statements are made in Chapter E which is a summary of all the water quality data, and that was clearly before we attempted to do any model calibration or anything like that.

DR. HILL: But yes but, you didn’t then use that information and perspective to inform how you actually conducted your calibration. And let me just provide an example of that -- and there's a bunch of things that come in here.

One is that you have this very long in time kind of base model. And that's your goal is to get this as accurate as possible. But you end up having detailed concentration information at different times along that path. Now, you're using a methodology because you have to sort of degrade your model and because it's a long time period, you're using a solution method for your transport that has a lot of numerical dispersion, but it's fast.

Okay, so that's fine for your sort of long-frame model, and when you get to that point in time where you're trying to match information at that well, it's probably a higher concentration I would say that's going to be consistent with that methodology. But you could also take your model as calibrated and for a fairly short simulation use a methodology, a method that has very low numerical dispersion.

You're going to have to figure out

your initial condition, your initial concentration conditions. And then compare that simulation, basically, what your short-term, temporal data is telling you is that once that well stops pumping, that it's the pumping of the well that's making the plume come over there. That if you stop pumping the plume's going to recede. And you could test to see if that occurs given the flow field you have.

DR. BAIR: On a short-term basis.

DR. HILL: On a short-term basis. So there might be some combination of kind of this long-term calibration and then some short-term simulations that test certain hypotheses.

MR. FAYE: Yeah, we did that at Lenny's suggestion for another reason, basically, to look, not to test the retreat of the mass, contaminant mass in the plume, but we did that to test the possibility of numerical dispersion. We came right down to one-day stress periods, so that's easy to do. And that's a good idea. We can give that a try.

DR. HILL: And you can use one of the solution methods then that's --

MR. FAYE: Oh, not only that. We can actually use some of the field data that we have to test that out.

DR. CLARK: As worthwhile as this discussion is, I'm afraid we're going to have to cut it here, but first off let me thank, in case I don't get a chance to do this and they have to leave in the middle of this discussion, I'd certainly like to thank everybody for their input, attention, perseverance and patience for putting up with us. It's been very interesting, and I hope it's been very useful for ATSDR. I think it has.

CHAIR SOLICITS RESPONSE TO CHARGE FROM EACH PANEL MEMBER

Why don't we just start with Walter. We'll go around the table with Walter. I guess Walter, Dan and Ben might have to leave before we're finished. So, Walt, we'll start with you.

MR. MASLIA: If you would, obviously all comments are welcomed and desired, but if you could try also to specifically address the questions --

DR. CLARK: That were in the charge?

MR. MASLIA: -- that would help us out. And anything else above that, that's also fine. It would help us out if you focus.

DR. GRAYMAN: I'll start by seconding Bob and just say it's been quite a privilege in working with this distinguished group. And I think this has been an excellent and hopefully very useful to ATSDR. Thank you, Morris; thank you, Liz, for organization, and the rest of the group.

I'm going to concentrate on the area of water distribution system analysis in my comments. First of all, the previous work that ATSDR has done in developing a detailed water distribution system model has put them in a good position to move forward in analyzing the Hadnot Point and Holcomb Boulevard during the interconnection periods.

Second, the water distribution system analysis is going to be needed for analyzing the impacts on Holcomb Boulevard, primarily the Berkeley Manor area during the interconnection periods with Hadnot Point. For other times in the areas the mixing model approach used in Tarawa Terrace should suffice.

I think that the analysis of the Holcomb Boulevard system during interconnection can be separated into two types of analysis, first of all the groundwater wellhead, water treatment plant type of analysis that was done in Tarawa Terrace and second the distribution system analysis, and I think it’s important that they can be separated. And it can take place by using the distribution system model to calculate the percentage of water from Hadnot Point reaching points in Holcomb Boulevard. In other words for each node in Holcomb Boulevard you calculate the percentage of the water reaching it at any time that comes from Hadnot Point. Subsequently, the concentrations reaching the customers can be estimated by overlying that percentage of water from Hadnot Point with the calculated concentrations leaving the Hadnot Point water treatment plant.

For assuming the concentrations leaving the Hadnot water treatment plant can be estimated probabilistically on a monthly basis, then with a manageable amount of effort in the distribution system area, I think that a monthly probabilistic estimate of concentrations reaching the Holcomb Boulevard, Berkeley Manor customers can be made. And my question for the epidemiologists is, is this an acceptable form of results for them to analyze.

And finally, the detailed data that was available for that 1984-'85 period when Holcomb Boulevard water treatment plant was offline should be studied and used at least as a partial validation exercise. However, it really is not that useful as calibration because of the operation during that period was so different. That's all. Thank you.

DR. CLARK: Thank you.

Mary.

DR. HILL: Let's see. One thing I did want to mention that I hadn't mentioned previously was that, Morris, you had spoken about a timeframe of 2012 for the modeling at one point. And I think really that you can, I actually do think the November deadline is tight, but that something like next May is plausible. So that's the kind of extension that I might consider if recommending.

So that's one issue. The other issues I've really, we've just been talking about them, and I'm going to focus on the groundwater model, but the issues of being more strategic and more hypothesis testing kind of focused in some of the testing that's done with the model and that comes into working with the observations in a more kind of strategic way, having observations that represent more solidly specific kinds of dynamics in the system including vertical flow, maybe even flows in different directions you could have or have differences in different parts of the model.

You might break it down geographically. It'll depend on draw-downs over time. That's another option. But having graphs of residuals that make a little bit more physical sense so it can be interpreted better. Observations of any kind of stream flow gain and loss that you can get your hands on is just a really great cross-check.

In connection with that as well, you might define, you might keep track of the flows going in and out of the conson (ph) [constant –ed.] head boundaries along the rivers. Not that you have a very good handle on what the values should be, but you might be able to say that value's ridiculous.

And in terms of the concentrations, I think we've spoken quite a bit about that. Since we just did it I won't repeat. In terms of the parameters for the model, obviously we've talked a lot about over-fitting and trying to avoid that because usually an over-fitted model doesn't have great predictive capability. And you can demonstrate that to yourself with your model, using suppressed validation exercises and stuff.

And being a geologist in my undergrad and engineering in my grad, in grad work I tend to really want to constrain models with geology a lot, so I tend in that direction. And I think this system has potential for perhaps doing that more than has been done. And that's all I have. Thanks.

DR. CLARK: Thank you.

Dave.

DR. DOUGHERTY: Here again, it's been a very interesting couple of days, and I know I’ve put a little bit of water from the fire hose on the end. I suspect I’m not alone. I guess my reactions are kind of mixed because in some ways I feel we’re coming in quite early in this process, and in some ways we’re coming in a little bit late in the process. I’m not sure exactly where the balance is.

But to try to answer the basic questions, there seems to be a reasonable possibility of delivering data useful to epidemiologists with some periods of time where that[data –ed.] may be less reliable than others. And this interconnect time I think is one that’s going to be a little testy.

We’ve talked about the data analysis somewhat, some things to do with taking the January ’95 period data and doing a very simple mixing model to make sure we have some sense of measurement errors, either, not sure of the treatment plant or to the production well, but it will give us some sense of one measure that we can use that constrains or informs concentration measurement errors because I don’t feel we have a very good handle on that.

In terms of calibration we talked about looking at different ways of representing the residuals so that we can extract some information rather than just saying we’ve made it, -- and I haven’t seen Mary’s notes, so I don’t know the details of what she’s given, but I’m sure she’s given them all, all the various plots.

On the concentration calibrations looking forward, we didn’t get into a discussion of the treatment of non-detects in, lower bounds of non-detects in the calibration process. But they are, as I read it for Tarawa Terrace, they’re set at one microgram per liter no matter what the detection and/or reporting limit may be. That seems to me inappropriate.

Think about it, another way to do it if you’re limited by taking logarithms, take the log of one plus the concentration so that your variable can be logged without blowing up on you. Do something, use the data better where it’s limited.

~~Simpler by~~ [Simplified –ed.] physically-based models are the way to go. I like the idea of pursuing a second path that's totally data driven, but it can't be used in preference to before the physically-based modeling systems. I don't think it's worthwhile spending a lot of time on fancy transport systems. Try to keep them relatively simple. The approach that Lenny talked about earlier really simplifying, grossly simplifying the transport processes and getting some representation of early arrival times makes a lot of sense to me.

With respect to arrival times, I would note that in the documents at Tarawa Terrace that both densities seemed out of line. There may be a nomenclature issue. Both densities were around 2.8 or 2.9 because I calculated them. It seemed a little like one too high. So it may be a nomenclature issue. It just needs to be clarified and get it right so we're not retarding excessively. Thank you.

DR. CLARK: Ann.

DR. ASCHENGRAU: Well, I just want to say from an epidemiologist perspective, and it might seem strange given the discussion of the last two days, but that this is really state-of-the-art, even beyond the state-of-the-art epidemiologic study of drinking water pollution. And what’s been done here just goes way beyond what’s typically done in most epidemiologic studies that have been able to find effects and associations. So I have in spite of all the problems we’ve heard about, I have every confidence that the study has a very good shot at finding an association if it’s there.

My problem comes more from the size of the case control study, that that’s a limitation. But I’m heartened to hear also that the great efforts that have been undertaken will be used to reanalyze the prior analysis of small for gestational age in the two planned studies. So that’s really excellent.

That being said I also want to reiterate the point that I made yesterday that the Department of Navy should make every effort to identify and give to ATSDR all of the relevant data that they need to do the best job possible and that they need to do this immediately. I think it’s a real shame that they now have to go back and reanalyze the study data from before because they didn’t have all of the necessary information.

I do think that the goal should be to try to get monthly data for the current study, so monthly exposure data that should be the goal that people are aiming for. And that, you know, if you don’t reach it, that’s okay. Epidemiologists have never been stopped by having imperfections in their data. It doesn’t stop us.

And the other impression I’ve had is just that there are sort of lots of possible sensitivity analyses that can be done with the groundwater modeling, the distribution water modeling. It just seems like a huge, huge job, but that somehow some plan has to be made for developing what needs to be done, and it needs to be done strategically. And that the goal should really be to keep the epidemiologic study in mind and not spend a lot of time on things that really won’t make such a difference in the exposure assessment for the study.

In terms of just some particulars, they’re not so much to do with the exposure modeling, but for the case control study of cancer, I do think that the exposure assessment should go beyond the first year of life and that it should go up to the time of the diagnosis of the cases and some comparable date of the controls. That that may end up being a large source of error if that’s not done. So you may have to go back and get supplemental data from the study subjects or somehow get that data from records.

And the other thing, well, is the school. That really high value at the school is problematic. And so I think that you should monitor or assess the exposure, not just at the residences but at the schools. And so that would only be really relevant for the cancer study I think at this point. And that that source of exposure should be taken into account.

And then my last point has to do with the behavioral data so it’s the water consumption habits of the study participants.

Frank has said a couple of times he doesn't think the data are very good. So I think that the goal would be to try to pick up the extremes so the people that take like long hot showers basically, and drink a lot of tap water and to try to distinguish them from the other study subjects if that's possible.

DR. CLARK: Ann, thank you very much.

Scott.

DR. BAIR: Yes, I guess I'd like to also thank people for inviting me. This has been a very worthwhile and educational process for me. I think the discussions over the last two days have probably convinced those who already recognize it at the table and elsewhere and those of you in the audience that all models are wrong. There are some models that are useful.

So the goal here is to incorporate enough uncertainty and analyze enough sensitivity aspects that we come up with a useful model that can be used by the epidemiologists. So I don't want all the discussion of the nitty gritty that went into the making of the sausage to discourage people that this can’t be done. Because I, like Ann, share a positive idea that this can be accomplished. Having read the Tarawa Terrace and the other reports that we were sent before we got here, I was a little skeptical about the amount of data that was available.

And through the discussions with Bob and others there are a fair amount of data that are present that can be used to help constrain the models that I don’t think have been mined to their greatest extent yet. For example, the grain size analyses, I think more can be squeezed out of that just looking at the percentage clay or looking at something as simple as a uniformity coefficient or ratio between D-60 and D-10.

I think being the geologist that Mary mentioned, all three of my degrees, anything that is deposited in water because of particle size differences and settling through water, is going to be anisotropic inherently. So I think there’s an anisotropy within each year model layers that you may need to consider. These are stacked channel deposits so they are deposited in water. So I’d encourage you to try to glean as much as you can.

The grain size data, there are actually geophysical logs that we didn't get to mention, SP logs and resistivity logs that are giving you information that can be interpreted to show that these are not continuous layers, and they're in some of the older wells, but I think that, too, needs to be incorporated into the model either as an uncertainty analysis, a what would happen if this data point is correct and there's a hole in the confining layer here or not. Getting at the pumping test data, the slug test data that Bob talked about and incorporating that in the model I think is essential to get the velocity fields pinned down a little bit.

Having said that, that y'all have a lot of data to squeeze yet, I do think that there are some simple pieces of data that you can add within your timeframe to help you lower the uncertainty in your model by adding a couple monitoring wells and locations there where water levels are sparse and then just using that to help guide your model even though you're going backwards in time, the water level in the sparse areas probably has not changed that much because it’s not in the middle of your well fields, and I’m thinking specifically on the northeast border of the model area.

Perhaps getting some tritium/helium data would be useful to help get another full velocity measurement like Mary talked about getting stream discharge data to help corroborate -- calibrate, corroborate -- what’s going on. I think MODPATH is an essential target of your future work, and it wasn’t in the Tarawa Terrace report, but I think it should be an essential part.

And then the last thing I have, and we really didn’t get too great a discussion on it, is the source term issues. For me one of the biggest problematic areas you have is how you’re going to treat all these different source terms. Are they going to be pulse sources or are they going to be continuous sources? If they’re continuous sources, is there known DNAPL at depth that can continue to shed off dissolved phase TCE or PCE? What are the initiation dates of those and how are you going to bracket those in some sort of uncertainty analysis?

That's about -- oh, yeah, one last thing. Dump the air line measurements.

DR. CLARK: Scott, thank you.

Dan.

DR. WARTENBERG: I'm also going to thank everyone. I found it fascinating to hear about all the inter-season groundwater modeling and the complexity and the difficulty in obtaining accurate estimates. But as Ann said, as epidemiologists we're used to complicated problems and data that's not as good as we want and are still able to move forward.

But that having been said, I think we've seen maybe the best data that can be provided for this study because the better the data, the more accurate would be the epidemiological results, the more sensitive the study will be. And also, fine scale data are important in helping us resolve some of the epidemiologic issues in terms of how the exposed were related to outcomes.

I think that just speaks to the notion

of if it's at all possible to get the monthly data to get an opportunity to try and see at what stage in the pregnancy there is this effect would be very important, although I recognize that's going to be harder. And there's always the opportunity to aggregate it back up to whatever timeframes if needed to do the analyses.

I think one of the other things that would be useful to do which hasn't been talked about as much is also to do some sensitivity analysis from the epidemiologic studies in terms of if they're different estimates based on different assumptions. Those also can be explored epidemiologically to see if there are associations in different ways.

One of the challenges here is, I guess there are a few challenges, there are a moderate number of studies looking at TCE and PERC and vinyl chloride in terms of cancer, but there's much less in terms of reproductive outcome. And being able to get a better handle on that's pretty important. So I think that trying to complete that picture, even the cancer data right now is still very controversial. But I think, again, it just speaks to how important this study is in doing as good a job as is possible.

I guess a couple other things to say are that I support Ann's statement about really asking the Navy to provide whatever data are being requested and available to help inform the study that that would be an important component to try to understand what's going on and trying to understand the epidemiology of these compounds that we know definitely affect people's health and to try and better understand that.

I guess those are my main comments. I just think again, just to reiterate, the better data we can get the better the epidemiologic data will be and the more retrievable and reliable. I think that's an important thing to try and strive for. Thank you.

DR. CLARK: Thank you.

Peter.

DR. POMMERENK: Well, I'll say thank you again for having me a second time on this panel. I find a certain new perspective that

I hadn’t heard about groundwater modeling before, and I also heard some things that we spoke about last time. And instead of repeating again, I just want to keep it short and want to reiterate that it appears critical to this study that uncertainty is included from the get-go.

From every aspect, starting upstream from the mass that was deposited, when it was deposited to have some measure of uncertainty in all these estimates and how they propagate through our model and whether it’s the simplified physical model or linear control theory model or highly complex transport model, the uncertainty that is upstream will propagate ~~for~~ [through –ed.] the model and will possibly skew it.

In the end we need to be, a logical study needs to be able to distinguish certain levels of exposure, whether it’s not exposed versus exposed or whether it’s a little exposed, medium exposure, high exposure and just providing a number will not help that cause. So it needs to be accompanied by some level of certainty in those numbers.

So with that in mind from my perspective certain things that will have to be addressed in Hadnot Point is the pumping schedules, having a well operate 24/7 over a month at a reduced apportion [proportional – ed.] flow rate may not be appropriate, and you may want to look into at least a cursory analysis of how using 12-hour stress periods may affect the outcome.

For the Holcomb Boulevard wells you may want to use 12-hour stress periods because that's the typical amount of time they operate versus Hadnot Point, those wells seem to operate in ^ [continuously –ed.] for a week or two or even a month. Anyway, it would be worthwhile looking at how this type of model or approach will affect the outcome and uncertainty in the study.

And then secondly what I mentioned earlier, we need to look at some of the issues of volatilization up at the treatment plant. You know, just a cursory analysis and say it's significant or not. But it should be on record somewhere because that question may come up at one point.

And I think moving downstream from there, again, it’s a lot about uncertainty. We need to wonder how much detailed modeling we have to do in the distribution system. Will that increase certainty in our, in the end or is it not worthwhile by the time we get to what [we want –ed.]^. Anyway, that’s all I have.

DR. CLARK: Peter, thank you.

Dick.

DR. CLAPP: Thank you all for teaching us a lot. I think some of you mentioned yesterday there are boundary layers between the engineers here. Well, there are tribal differences I think between ^ [various –ed.] epidemiology tribes. It’s fascinating to listen and learn from you all.

To me, I would like to reiterate the points that Ann and Dan made from the point of view of an epidemiologist. When you get the final number that you’ll use to assign a dose or an exposure to a particular subject in a study, that’s the result of a lot of phenomenal work, and it will have error bars around it.

But there is still going to be a central tendency for that number. I know it's a sort of probability density function that goes along with that number. Our goal is to see that that's as peaked as possible, not as flat and as compatible with anything as sometimes happens. So that's the goal here, and I think everyone has established that that's what the modeling effort is going to lead to. So anyway, I think that's in good hands. As Ann said it's state-of-the-art work, and I commend the ATSDR folks for doing it.

I'd like to mention I think there is a particular problem which is this Hadnot Point to Holcomb Boulevard interconnects during four months for a period of years from 1972 to 1987 where the problem is or a lot of the problem is in the distribution system at least. And so that seems to me to be a tractable problem, that it's not as big as or hopeless as some of our discussion today or yesterday might have made it seem, especially today, I guess.

So I'm optimistic. I think this is going to work. I think that the process that we've engaged in is going to have a fruitful outcome. I think it will be useful to veterans, the people who lived and worked at Camp Lejeune, and that we shouldn't lose sight that that's what this is all about. And I think some aspects of this we learned, for example, there may be a simpler solution than we realized, one of which can be done this weekend. We may have data next Monday I think from him, Dr. Aral. Without being too silly, I'd like to say I think this is a useful exercise that's going to lead to an important finding and glad to be a part of it.

DR. CLARK: Thank you.

Ben.

MR. HARDING: Thanks, Bob. I want to thank ATSDR for allowing me to have this opportunity. I really learned a lot in the first pass, and I've learned a lot from this one. I thank all the panelists, too, for allowing me to poach on your territory and talk about things I don't really know that much about.

And I want to say how remarkable Morris is. I don't know what, does he drink

Tension Tamer Tea or something like that? Your ability to stay calm in the face of all this is really impressive.

MR. MASLIA: Thank you.

MR. HARDING: I'd like to know what it is.

Bob, I'm not going to say anything about, or not much, about what happens below the ground here. I do think it's feasible for this work to contribute a lot of important knowledge, at least at the exposure level. And I'll leave it to the epidemiologists to work from there. So I think there's a good foundation, and it's feasible to complete this successfully.

I would suggest, and I think you probably already intend to do this, that you step back and re-scope your remaining efforts at this point. And from the program scenario I think Walter laid out the components that you need to think about quite well: wellhead concentrations, the interconnection scenarios, water use and then the system operation rules.

And with regard to the water distribution, both the large view I agree with Morris' breakdown and essentially the difficult problem is the interconnections, which others have mentioned here. In doing that I suggest that you should use a detailed hydraulic network model, an extended period simulation of that.

There’s no sort of technical or cost problem with doing that. You already have it essentially. That you will need to extend your scenarios over potentially several months depending on what you see in the tanks because it can be a long time before the tanks clear out.

In all of the phases of the work above ground, we’re going to need to have what you call a simple mixing model, but it’s actually more complicated than that as Peter has mentioned. So we need to have what I call a well operation well supply model that will take into account if there are hydraulic effects on particular wells.

And I think you should develop an informed model of well operations, as informed as you can make it. It’ll probably have to be stochastic at some point, but you should inform it as best you can with what you know about the way they operated, the wells.

I think you should use the super-position approach that Walter mentioned. It’s essentially similar to the Murphy method that was portrayed. You know, he called it an exposure index. We call it transfer coefficients.

But that approach will allow a low cost and rapid recalculation of the exposure statistics which will happen because the groundwater people will come up with new numbers, and then the epidemiologists will ask for new thresholds. I know. I’ve been to the rodeo before so, and being able to recalculate this in a short time is really important.

I think it’s okay. I think it’s feasible and proper to be able to calculate your exposure statistics over a one-month period. That’s been a real request from the epidemiologists, but I wouldn’t go any shorter than that. I think you have to model a water distribution system on an hour to even -- EPA did a model of minutes if it has to get the convergence.

But you have to model in a short

period to get the dynamics of the system. You can roll it up to a month but no shorter, I think. ^ a quarter but because you need the resolution as long as you bear in mind there's some additional uncertainty.

With regard to the control theory approach, I thought it has a lot of use for developing confidence in the physically-based model, but that we should use a physically-based model for the basic work. And I think there's other reasons why the control theory approach isn't appropriate because we can't get a complete set of wellhead concentrations. But it really was sort of nice to see how well it agreed with the physically-based model. That was interesting.

Echoing what Peter said, you should focus on uncertainty at every step from start to finish. I won't try to tell you how to do that, but I think ultimately it has to be some kind of Monte Carlo numerical approach. At least make an analysis of sensitivities if you're not, if you're going to treat things as point values.

Overall, I want to say this. There's

hundred of thousands of people, and I guess Frank said potentially up to a million people that may have passed through this site during this period that are interested in this event and potentially exposed. And it’s a bad thing that’s happened, but we should do our best to learn from what happened and not repeat this mistake. And whatever we can gain medically and scientifically we should do that.

If this is done well, future people will make medi-analyses of these results with new information about the populations. So I think it’s really, really worth committing the time and effort that are necessary to get this done right, whatever right means, but to get a good foundation in every spurt or step. I mean, the flow model is going to be the fundamental foundation that probably won’t change all that much. And as you build up from it maybe some things will be refined, but I really do think it’s worth it.

You need to take the time and the money to do that. With respect to time, I think a year for the water distribution modeling should be enough, and maybe you could do it faster. I mean, we've done similar things in a year. I think if you set your mind to it, you could do it faster, but there's a real value in rethinking things every once in awhile.

But do focus on the essentials, just what you essentially need to do to get the end result. Try to avoid digression into details where they aren't relevant. But I think you've done a real good job, and I really do appreciate the opportunity to be here with all the panel members and your tolerance.

DR. CLARK: Thank you, Ben.

Rao.

DR. GOVINDARAJU: I, too, would like to thank ATSDR and all of you for contributing to my learning. I really enjoyed all this. I have some recommendations, but they're not necessarily out of the charge that was laid down to us.

First, I would like to say that I found out that more data has become available very recently, 200 new ~~USG~~ [UST –ed.] reports and many other data coming online. And this data is not likely to be immediately ~~be~~ [-ed.]

used in a model. It's not in spreadsheet form and all cleaned up. So by the time all that data discovery from all this takes place, I suspect it will take some time and I do not know how large a team you have, how many person hours you can throw at it. So I'm going to suggest that December 2009 does not look likely to me, at least one more year and maybe more. But that's something I wouldn't be able to tell. So that is in terms of the timeline issue.

I'm also not comfortable, I would not like to answer the question and say can we promise a plus-minus half magnitude for concentrations, which actually may not be possible for such a complex system even with the best methods available and even if we had a lot of very good data. So I think what the focus should be on is trying to reduce one certainty to the extent possible using whatever that can be done. Use the best methods and so on. I think that would still be useful even if it did not meet this plus-minus half magnitude target.

I'd also like to say that I do not

think that all quantities that are produced, all the things that are predicted or hind-casted, let's say, they will be done equally reliably. Some things will be done better, and some things will not be done as well. So renewed concentrations I'm not sure we'll ever reproduce, but perhaps some we need to drop the averages or different averages you could do perhaps more reliably. So I feel that all the information that we have should be used ^ uncertainty which has been pointed out as being very crucial.

So right now we have uncertainty from the groundwater models which is reflecting, which is trying to predict concentrations in these wellheads, and then this is going to be translated or propagated into the distribution network. But in between there's a step at the treatment plant. I do not know how these concentrations ^, and I do not see much -- and we talked about it -- but I do not know what work has be done about that, but that's potentially useful.

Regarding the models I think the models that you have selected, which is

MODFLOW, MPT, [MT3DMS –ed.]. the ^ [Ga. Tech –ed.] code for solu [solute –ed.] transport, EPANET, ^ [and –ed.] what have you. I think these are all fine models. I have no, I guess I have no objection to these models. Any simpler model you want to use that is fine, too, if it does the job well.

Now for the EPANET water distribution model, when you are trying to get concentrations at the endpoints, I think one of the greatest challenges is going to be to try to reconstruct how to disaggregate this one-month quantity that is being given to you from the groundwater side to a daily or an hourly time schedule like has been mentioned. ^ calibration work and with the expectation that patterns haven’t changed, I feel it should be possible to reproduce the variability within the month.

I mean, you can consult that volume within a month but you appropriate so that you reproduce some of this variability. And then looking at this variability over time and perhaps over the front realizations which come from different concentration values from the groundwater, if you look at all of these, then I think some meaningful decisions can be made about what the exposure was, how likely the concentrations to have been exceeded over different time windows and so on. So a good statistical analysis I think could be done and could be quite revealing to the epi people.

Well, I think those are my oral comments. I see there is a lot of hard work that has been done by the ATSDR team, and I have a feeling there's quite a bit more to come also. Thank you.

DR. CLARK: Lenny.

DR. KONIKOW: Thank you. I'm going to keep my comments from the ground level down and focus basically on the one test. How do you get or reconstruct the concentrations unloading from the wellheads? And what I see is the task at hand is enormously difficult, and it's a challenging one, but it's very important.

And it's very important that you succeed, and I think you can succeed, but there'll be some errors and uncertainty associated with that. But if you recognize that I think we can pass that information on and let the next group above ground, they can do something with that.

As you go forward and develop the models and develop the insight, I think it's very important that you clearly indicate all the assumptions that underlie it and conceptual models that we use to formulate that. And I think that will help in your defense of it in the future, and it would help enable people to understand it.

Now, I've spent quite a bit of time in Scott's proverbial modeling sausage factory so I tend to see all these difficulties, and I get very concerned about them because they do affect the answers, and I have a few detailed comments related to that.

But the other kind of big picture thing I see here is that you've essentially completed the work at Tarawa Terrace, and I could nit pick a lot of little things in there, but basically, I think that was a successful effort. You did a good job there within its own right was a very complicated problem.

What concerns me here is that the Hadnot Point-Holcomb Boulevard I see another one or two orders of magnitude of complexity here, and so I do get concerned. Is this whole thing doable? And that’s a reasonable question to ask. I don’t have the definitive answer, but I do think you can do something. I think what you do can be useful.

I think basically, I think you can succeed within a certain framework, but maybe keeping in mind what was done and what was able to be done at Tarawa Terrace, what’s able to be done and our success in groundwater science with groundwater flow modeling. Transport modeling again just is another level of complexity. So as I tell some people, the secret to successful ~~solu~~ [solute –ed.] transport modeling is to lower your expectations.

And I think that’s something we have to do. We’re just not, all the difficulty in groundwater flow modeling will have that, but we could do it. We’re not going to be able to do as well with transport. There’s too many other processes involved and there’s too many additional unknowns. So what this gets at then, and I’ve worked in the sausage factory, but I’m also a sausage salesman, so I don’t want to discourage you from this, and I’m trying not to discourage you.

I think it is a valuable path to follow, and you will learn a lot and on. But be that as it may, with this complex approach, as several of us have said earlier, it has to be supplemented with simpler approaches both to see if they could provide the necessary information as well as to provide cross-checks against the very concas (ph).

As we said again many times, no matter what we do with the models, there’s still a very limited set of observations of concentrations against which we could compare the model results. So we have this enormous field of a couple of decades of no data on concentrations. So we’ve got to take a couple, you should take a couple of different paths.

The linear control theory I think is certainly worth pursuing and get as much out of that as you could. Other simple ways that we’ve talked about which would encompass some coupling of groundwater flow modeling with MODPATH modeling and with very simple interpolation extrapolation I think would be very useful also, and I think you could do a lot with that.

I think you could learn a lot from using MODPATH more than was done in the Tarawa Terrace approach. With this lack of data I think you have to keep mining, searching, doing what you can to get more data if it’s out there, and if it’s available. Because one extreme, and again, I don’t want to sound like an academic researcher who just always wants more data, but one of the difficulties I’ve had in doing this review in constructing my comments was -- I think it was Dave mentioned -- it’s very early in your phase.

And my focus really has been on the wellhead concentration, how we get there. And yet we’ve had no document on the hydrogeologic framework yet, no transient flow model yet, no transport model yet. So it’s hard to comment on them because that’s what’s going to get us to the wellhead concentrations.

So one recommendation that was mentioned was that somewhere down the line when you get further into that, but not too far into it, get maybe a smaller group of expert peer panel to look over your shoulder and give you some advice and help maybe guide you in a more efficient -- and by more efficient I mean you're always going to have some deadline facing you. So you want to get this done as well as possible and in as short a time as possible. And I think peer review is a very useful way to help you do that.

On the data picture a lot of people don't like to hear this, but consider getting more data. I mean collecting more data so, but before you do that you've had an enormous amount of money spent on installation restoration programs there. Have you mined that for all the data that's available?

In the report I saw there was a 40-day tracer test done at one of the sites, which I can't remember. I mean, that should have gotten you some effective porosity and dispersivity data if they did it well. Is that data available to you and have you looked at it? They must have to do the kind of work they do, and they must have taken some cores. They must have looked at some of the clays and the confining layers.

Did they measure any hydraulic conductivities or porosities?

MR. FAYE: Was that rhetorical or do you want an answer?

DR. KONIKOW: I don't want an answer right now, but it wasn't rhetorical either. These are things I want you to think about, and I'm sure most of you've already thought about it, but these are things that are just kind of popping out of my mind now.

On the modeling and the work that's done so far, again, I'm very concerned about up to now -- I know it's preliminary still -- it's locking into one foot per day as a hydraulic conductivity for the clays and for all the clays.

I mean, that bothers me. One of the things we talked about doing sensitivity analysis. In your steady state, pre-development flow model, those heads are not going to be sensitive particularly to those values, but your transient flow model it will be, and in your transport model even more so, that value is so few.

Rely on locking it into those values based on the sensitivity test in your steady state flow model, you may be making a big mistake. And again, that's something I mentioned before is when you go beyond the steady state, you may have to re-examine almost everything because what worked there may not work for transport.

In a transport analyses again one of the things that has certainly been highlighted in the last 20 years or more is the control and the importance of spatial heterogeneity in the formations. And you're dealing with models at the moment.

You're assuming each layer, each unit, is homogeneous, and I'd like you to explore the data to see if there are ways to not only get at the spatial variability but other aspects of heterogeneity including channeling and connectivity of the sediments because every study where there was detailed data showed that this was the controlling factor on

~~solu~~ [solute –ed.] transport. So if at all possible, pay a little more attention to that.

Then there's all the uncertainty with reaction, ~~absorption~~ [adsorption –ed.], fate, you know, ~~absorption~~ [adsorption –ed.], decay and all those other terms which we don't want to get into right at the moment. But again, like I think it was Scott mentioned his concern about estimating the source terms. Again, what's more critical for ~~solu~~ [solute –ed.] transport model than how much gets in and when and where.

And I didn't see all the answers yet in the presentations here or how the approach that was taken and described will actually get to an estimate for the source term in the model and how they'll be done. At Tarawa Terrace you did a mass loading which I would much rather see defining a source concentration associated with the fluid that goes in the model. Because otherwise you get some conceptual inconsistencies that I think need to be explained. So this gets into other issues, but again be careful with that source term because that's very critical and very important.

So with that I guess I'll pass the mike.

DR. CLARK: Lenny, let me thank you very much.

Randall.

DR. ROSS: First, I'd like to thank ATSDR and Morris for the opportunity to come and be with such a talented group of individuals and learn. And I had a professor that once said water level maps are a figment of the artist's imagination. And I'd say the same could probably be said about groundwater modeling results. But with that in mind it's also the best that can be done. I don't want to say a necessary evil, but it is. It's the best answer that one can come up with with confidence. And I think that's true.

One of the things about data gaps, modeling, one of the benefits of modeling is it forces you to look at your data, look at what you have and identify your data gaps. And I think Scott hit on this a little bit. There may be some data gaps that come up in the initial parts of the modeling exercise that tell you where you need more information, have better control on the situation.

With regards to the charge, with respect to the question did the methods provide an adequate level of accuracy and precision, using Dr. Faye's definitions of precision and accuracy, I'd say for precision probably, for accuracy at Tarawa Terrace probably, for Hadnot Point I'll refrain from a final answer on that.

I'd say that the Tarawa Terrace exercise represented one of the best case scenarios that we've had an opportunity to see with respect to coming up with concentrations for exposure that will keep you folks happy. And that's one thing I have written down here is listen to the epi folks.

If you have another meeting like this I'd say the first 15 minutes should be the epi folks re-impressing upon all the people that work below ground and above what they're looking for. If it's enlightening to me to hear that high, middle and low are acceptable. And with that in mind I'd say whether or not you could reach the accuracy, probably. And that's a good thing.

Looking through the previous panel's comments after we made our comments I noticed there were a few things that we commented on that in particular Dr. Konikow identified in the last panel meeting that didn't seem to be fully addressed. And that leads me to the question of exactly what will become of the comments that were submitted today and how that will be addressed I guess.

Then I have a note here that says listen to the geology. To go back to what Dr. Hill said, basically. And this with respect to including two marginal aquifers and a confining unit in the same layer. I mean, that's a no-no, and I think pretty much all the modeling folks here, the hydrogeologists, kind of cringed when they saw that. And there was a reason for that because it flooded, the nodes were flooded I understand. But as Dr. Hill also said, don't do that. Fix it some other way I guess.

I would say it's, I had a comment here about the plus or minus three feet and the plus or minus 12 feet, and I'd say that if there are wells that haven't been surveyed, I think it's well worth surveying them now. These could provide valuable data in the future.

And along those same lines one of the recommendations that people get tired of hearing me suggest is the implementation of or deployment of pressure transducers. Yeah, they produce a whole lot of data, but at the same time they can provide a lot of insight into how the system reacts to pumps shutting on and off.

You can't do it in hindsight, but hindsight being 20-20, we can look ahead and say that might be a useful tool that could be deployed. Pressure transducers in select locations to give you a better understanding of how the system reacts, hydrogeologic system in general.

There were several comments I guess that I included in our written comments, but something then to consider with the, more of the worst-case scenario I guess which would be the whole Hadnot Point modeling exercise, not worst-case scenario but certainly not as friendly and nice. The looking at leakage from your domestic production lines, the water lines. Ten percent’s not an uncommon number that you hear batted around the modeling community, but which could be a significant number.

Likewise for sewer lines, they pump a boatload of water out of the aquifer, well, if you lose ten percent or 20 percent of that usually the sanitary folks don’t really care if they don’t see it and if nobody’s complaining that they’re basements are flooding. That could be a significant input into the model as well and nobody measures it or likes to.

Degradation rates, you’ve got to be careful there. It’s going to be completely different I believe than the exercise at Tarawa Terrace. There you really don’t have evidence that the bugs were really happy. There’s not a large quantity of -- at least I haven’t seen -- VC, DCE and compounds like that, nor of the geochemical data that indicate that the bugs were happy for reducing conditions. I think there’ll be a lot more of that associated with the DNAPL sites as I'm sure they are.

And that leads right into the source term. You've got bugs that are munching away at the dissolve[d –ed.] phase, but there's no doubt in my mind just looking at the numbers in a cursory manner that, I mean, you've got a -- I've used the term boatload three times now because I like it. There's an unknown, yet probably very large quantity of dense ^ [nonaqueous phase –ed.] ~~disphase*~~ liquid TCE and PCE in the subsurface especially below the dry cleaner. How that will be handled as a source, that'll be interesting, and I think will have a significant impact maybe. Maybe. It has an impact with respect to the longevity of the source and remediation talk, but maybe not necessarily on the high, middle and low concentrations that you folks are really looking for.

Echo what was said earlier about the bulk density issue. It looks like there was an error early on that was carried through. It could be a nomenclature issue, but going back to that original article and tracing it through the documents, I think there's a, the retardation factor in the model would be modified by about 25 percent probably, just a ballpark, back-of-the-envelope kind of calculation.

Source issues we've talked about transducers. Thank you for the opportunity to participate in this.

DR. HILL: Can I say three words?

DR. CLARK: Sure.

DR. HILL: Two significant digits.

DR. CLARK: Words to live by.

I'd really like to thank all the panel for your participation and your outstanding insights. It's been a pleasure to work with all of you. I'd certainly like to thank the audience, too. We had some very good input from a lot of the people who've been here and observers, Dr. Aral. We certainly appreciate the ATSDR staff and Liz, for all your help. So it's made it possible to do this.

Morris, would you like to say a word or two?

MR. MASLIA: Are you giving your recommendations?

DR. CLARK: Well, I can. I didn't know if I was allowed to do that as a panel member.

MR. MASLIA: Yes, definitely.

DR. CLARK: Very few. I thought everybody did an outstanding job in recommendations, and I support all that was said. The only things that I thought were worth maybe re-emphasizing for the fact that it seems to me that the epidemiological study should probably go beyond just child [and –ed.] in utero studies. That [and –ed.] there's significant exposure to adults and that's just almost totally unknown.

And some of the levels that adults have been exposed to are almost unbelievable. I was looking at some of the vinyl chloride levels that were pumped from one of the wells in there, and when I was working on this sort of thing with EPA, this would have been frightening stuff. So I think that's something that probably needs to be explored.

I still think that some of the degradation byproducts issues have not been explored thoroughly and should be. I think it's, like the degradation rate [rates –ed.]

shown in the manual are a lot slower than would be of concern in [–ed.] [- ed.]in a distribution system, but it depends on where you start from.

And I think it’s something we were always concerned with in our studies is just how fast did some of these compounds degrade the vinyl chloride in it. What would the implications for that be? It wouldn’t take, wouldn’t be [–ed.] very much vinyl chloride to really have an impact on the outcomes in an epidemiological study.

Another thing I wanted to mention was the fact that I think you’ve missed an opportunity to look at some direct exposure data in terms of CHMs [THMs]. I know I gave up on that earlier because I know Dave and ^ looked at it, and they didn’t have the GC traces so they sort of pushed it aside. But looking at some of what I’ve seen, it seems to me that’s an opportunity to actually look at direct exposure and transport in the distribution system. I would encourage you to go back and look at that very carefully and see if there isn’t some way to reconstruct that. And I certainly would help you with some of my contacts at EPA when you get into some of the analytical chemistry issues. So with that I'll conclude and thank everybody.

And Morris, you want to make a few comments?

MR. MASLIA: I wanted to thank all the people who participated in the panel. It's obvious even the preliminary work is a large volume of information for you to digest in the short period that we gave you and then provide us with feedback that we can implement and use to carry the project forward to a successful completion, so thank you very much for your time and effort.

I also wanted to thank Bob Clark for stepping in at the last minute and chairing and guiding the panel, which he was not expecting to do just a couple of days ago. So that was a benefit to us. And I do agree. I think was it Lenny that made the suggestion and actually I was going to bring it up, but since you said it, it's good is to reconvene perhaps a smaller group as we get to different aspects or phases, and sort of looking over our shoulder and critiquing those aspects rather than waiting a whole long time and bringing a larger group together. And I think that probably will provide us with much more valuable input in a shorter time period. So I thank you for bringing that up. It's a very worthwhile suggestion.

And to answer Dr. Ross' question about what happens is we will put a draft report together similar to the one that we did. We tried to, I think actually this panel was much more succinct in their final recommendations than the first panel, which is easier for us to, and then we tried to implement it to the best of our ability both in technically as well as time and effort and money.

But again going back to Lenny's suggestion I think if we do have smaller groups of technical experts looking over every so often that's easier to make sure we don't miss anything or overlooking something that's important. So thank you to everybody, and thank you to all the administrative staff for assisting us and thank you to our technical staff who have spent at least the last months just administratively putting the panel together with all the material.

DR. BOVE: I want to thank all of you. I think this has been very valuable to the epidemiologists, both myself, Perri and I think the epidemiologists on the panel learned quite a bit today. So thank you very much. I think your input was great and I think will help the study immensely.

DR. HILL: Thank you.

DR. CLARK: With that the bus from the hotel comes at 3:30, doesn't it?

(Whereupon, the meeting was adjourned at 3:12 p.m.)

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I, Steven Ray Green, Certified Merit Court Reporter, do hereby certify that I reported the above and foregoing on the day of April 30, 2009; and it is a true and accurate transcript of the testimony captioned herein.

I further certify that I am neither kin nor counsel to any of the parties herein, nor have any interest in the cause named herein.

WITNESS my hand and official seal this the 19th day of July, 2009.

Steven Ray Green, CCR

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