convenes the

EXPERT PEER REVIEW PANEL

ATSDR'S HISTORICAL RECONSTRUCTION ANALYSIS

CAMP LEJEUNE, NORTH CAROLINA

VOLUME II

The verbatim transcript of the meeting of the Peer Review Panel, held at 1825 Century Boulevard, Room 1A/B, Atlanta, Georgia, on Tuesday, March 29, 2005, taken by Diane Gaffoglio, Certified Merit Court Reporter.
PARTICIPANTS (in alphabetical order)................. 3
OPENING STATEMENT AND INTRODUCTION OF PANEL
   Dr. Johnson........................................ 4
HOUSEKEEPING RULES:
   Mr. Maslia........................................ 5
OVERVIEW OF ANALYSES OF PRESENT-DAY SYSTEM
   Mr. Maslia........................................ 9
PANEL DISCUSSION OF ATSDR QUESTIONS................ 95
PUBLIC COMMENT
   Dr. Johnson.......................................148
PANEL DISCUSSION ON FOUR CHARGES.....................150
INDIVIDUAL PANEL MEMBER RECOMMENDATIONS.............186
CERTIFICATE OF THE REPORTER..........................203

Legend of the transcript:
[sic]   Exactly as said
[phonetic] Exact spelling unknown
   --   Break in speech continuity
   ...   Trailing speech or omission when reading
           written material
[inaudible] Mechanical or speaker failure
[microphone] Speaker is off microphone

NANCY LEE & ASSOCIATES
PANELISTS

(In Alphabetical Order)

ROBERT CLARK, Ph.D., M.S., D.E.E., P.E.
Environmental Engineering and Public Health Consultant

DAVID DOUGHERTY, Ph.D., M.A., M.S.C.E.
Principal
Subterranean Research, Inc.

BENJAMIN HARDING, P.E.
Principal Engineer
Hydrosphere Resource Consultants, Inc.

BARRY L. JOHNSON, M.S., Ph.D., F.C.R.
Panel Chair
Adjunct Professor, Rollins School of Public Health
Emory University

LEONARD KONIKOW, Ph.D., M.S.
Research Hydrologist
U.S. Geological Survey

ERIC LABOLLE, Ph.D., M.S.
Researcher
University of California, Davis

PETER POMMERENK, Ph.D., M.S., P.E.
Project Manager
AH Environmental Consultants, Inc.

VIJAY SINGH, Ph.D., D.Sc., P.E., P.H.
A.K. Barton Professor of Civil and Environmental Engineering
Louisiana State University

JAMES UBER, Ph.D., M.S.
Associate Professor
University of Cincinnati

THOMAS WALSKI, Ph.D., M.S., P.E.
Vice President, Engineering
Bentley Systems

NANCY LEE & ASSOCIATES
DR. JOHNSON: Good morning. Good morning, one and all, and I hope you all had a restful evening. Before we ask Mr. Maslia for any housekeeping kinds of things, let us welcome one of our panelists, Benjamin Harding, who had airplane difficulties that we've all encountered over our careers. But we welcome you and ask that you, for the record, identify yourself, your affiliation, and lastly to any overarching comments on the materials that you received from ATSDR.

MR. HARDING: I'm Benjamin Harding. I work for a firm called Hydrosphere Resource Consultants out in Boulder, Colorado. And I think, if I had to sum up what I thought in an overarching sense, I would say that the work that's been done here is impressive. One of the things I think that was identified by the other panelists as well is that we need to deal with the issue of uncertainty and try to deal with that in a quantitative way, I think.

DR. JOHNSON: Thank you. Do you know all the other panelists?

MR. HARDING: The ones -- I think I've met everybody at this point, and some of them I knew prior to this time, so...

DR. JOHNSON: Why don't we take a couple of minutes

NANCY LEE & ASSOCIATES
and just go around; name and affiliation, please.

DR. POMMERNK: My name is Peter Pommerenk. I'm with AH Environmental Consultants.

DR. SINGH: I'm Vijay Singh from Louisiana State University.

DR. WALSKI: Tom Walski, Bentley Systems.


DR. UBER: Jim Uber, University of Cincinnati.

DR. DOUGHERTY: Dave Dougherty, Subterranean Research.

DR. CLARK: Bob Clark, formerly with EPA and currently a consultant.

DR. JOHNSON: And I'm Barry Johnson, School of Public Health, Emory University.

Morris, do you have any housekeeping things before we begin today's work?

MR. MASLIA: Just, again, to remind anyone if they've got their cell phones on to silence them or turn them off, whichever you prefer. And again, any of the audience in the back here, your conversation can be picked up by the mikes, even if you're turning to your partner.

And one last thing, more towards the -- for the panelists, Dr. Johnson gave me a homework assignment last night and to see if we could reduce or perhaps modify the
questions and answers for the second day with respect to
the water-distribution systems, and I did some of that. I
handed Dr. Johnson a copy, and I will hand the panel out a
copy when we get to that time so we can go through them
and cover all of them in a little faster manner.

I've combined a couple of them as well. So other
than that, Dr. Johnson, that's it. Oh, the -- if you
haven't deposited your money -- I think it's $5 for the
working lunch. Ann is taking the money outside. Just,
either at break, leave the money there, and they'll go out
and get the lunch. Thank you.

DR. JOHNSON: Okay. Thank you. My purpose in asking
Mr. Maslia to take another look at the list of questions
that bear on the water-distribution systems was that these
questions were prepared some time ago. And he and the
agency have received some information since the
preparation of these questions, and that led to, in my
mind, as to whether all of those questions were still of
importance to ATSDR, and so Morris has reduced the list in
response.

With regard to one housekeeping matter from the
Chair, today's agenda shows, at 2:30, us going somewhere
in executive session. And I gather that that was put in
as an opportunity for the panel to sort of closet itself
and say things, perhaps, in the absence of ATSDR staff.
In fact, that would be the case, and we would have ATSDR staff there initially to answer any early questions, but I would then ask them to leave so that this would be a totally candid kind of executive session amongst the panelists. Do you want to maintain that or forego it and simply continue all of our deliberations here in a public forum? And it's -- really, it's up to the panel to decide. If you feel that need, we'll certainly do it. What is your preference?

DR. CLARK: I don't have any problem with continuing in a public forum.

DR. JOHNSON: Is that all right with all of you? Tom, is that all right?

DR. WALSKI: It's all right with me.

DR. JOHNSON: Okay. Then we'll just continue everything here in public session. And the main thrust of that executive session was to finish our response to the four elements of our charge -- the first two, we will address at the working lunch -- and also to craft some kind of communique.

I asked Mr. Maslia last evening: What did they have in mind as a communique? And his response was: answers to the four charges -- and which we will be preparing as we deliberate this morning and early this afternoon. It seems like we can fill their desire for a communique and
still doing it all in public session.

I also want to alert you that, at the end of our deliberations and toward the end of the meeting, I'm going to ask you the same kind of question I asked you at the beginning, and that is: To what extent are you comfortable with the, what I call, the protocols that are in play, both for the groundwater modeling as well as what you're going to hear today, the water-systems modeling? To what extent are you comfortable? Are you -- do you have something you'd like to sort of red letter as key advice to the agency? But just where are you personally in regard to what you have heard over these two days?

And I don't foresee us taking any kind of vote as a panel. If you feel that that is a need, then let's discuss it. But by voting as a panel, it seems to me to put ATSDR in a bit of a bind and potentially in a bit of a bind. But they will have the benefit of your advice and your recommendations as individual panelists. Does anyone have a problem with the panel, as a body, not taking some kind of vote on whatever, but speaking as individual panelists? Tom?

DR. WALSKI: I'd prefer it that way. It's pretty hard to get this group to agree. I mean, Jim and I probably agree only about 10 percent of the time. So, you know, it'd be pretty hard to get a unanimity on the panel
here, so...

DR. JOHNSON: Well, let the record note that Jim is
smiling (laughter).

DR. UBER: It's one of the 10 percent.

DR. JOHNSON: Which is part of the 10 percent. Well,
if the panel feels, during the course of the day, to
change some of these suggestions, put it on the table, and
we will -- you will debate it as a panel. Okay.

Having said that, Morris, are you ready to begin
updating us on the water -- water-distribution systems
work?

MR. MASLIA: I sure am. Good morning, everybody.
And, Claudia, if we can go ahead and get the overview. My
plan this morning is to give an overview of the approach
for the water-distribution systems analysis and then go
into the field testing that we've done to date on the
present-day water-distribution system.

And as Bob said yesterday, if you would like to
interrupt me or ask a specific question that's either
among the questions that are there or that comes to you as
you're sitting here, please, feel free to do so, and I
will try to answer it as best as I can.

We're all familiar with Camp Lejeune, hopefully,
since yesterday. And again, for the present-day system,
we've got two water-treatment plants and three water-
distribution systems. Just for your information, our piping network has been obtained from data from autoCAD drawings, supplied by Camp Lejeune, as well as through their contractor, AH Environmental, also provided update on piping. And so it's a combination of information as well as us being in the field and observing pipes or asking questions and then defining or having updated information.

This was three bullets of activities based on the entire project, and we talked about, obviously, yesterday, the groundwater issue and some uncertainty issues which still apply to today's issues. But specifically, today we'll look at the potential distribution of contaminants and water-distribution system models.

And let me just add, as Dr. Johnson mentioned, I updated the questions and answers that were prepared a while back based on discussions yesterday. I have not done that with the slide material. So some of the slide material is presented, not in contradiction to your advice or your recommendations, but that they were prepared a while back. And I thought I would just go with what I had prepared.

So again, the chronology, which we still need to refine in some areas. The one point to make here: What is called Montford Point is presently known as Camp Johnson
and is not in existence. It's serviced by the Holcomb Boulevard water-treatment plant. And the treated water goes to Tarawa Terrace ground storage, and that's where it gets its water from.

Basically, we were asked by the epidemiologists to quantify historical exposures for the purpose of their epidemiologic study. And so our understanding is that if the systems are completely separated, completely isolated so you've got three hypothetical systems, they may or may not have any contamination in them. Then, of course, there would be no need to reconstruct the actual distribution system historically, but rather we could assume everyone would receive the concentration based on our groundwater modeling and the source concentration there.

Based on information and talking to people to date, we know at some point in time the distribution systems have not been operated independently or they have been -- a better word is there's been interconnection. Exactly how long that is -- we've heard information on two weeks. We see other data that suggests maybe there were other opportunities for the systems to be interconnected.

And so if that's the case, then we need to do some amount of historical reconstruction to try to get a distribution of contaminants within those systems. So, as
we see it, there are two approaches that we can do the historical reconstruction. One, we can use historical water-distribution system data. This is data from the operators of the system, cycling on and off of wells, flows, demands.

And what we have at least found out, in looking for information, is that the information is sporadic. We talked about that yesterday. There may not be any record specifically of cycling on and off pumps and wells, or it may not be in existence. Bob, did you have a question?

DR. CLARK: Morris, yeah, I had a question on the exposure assumptions. You're assuming that everybody who lives in a system that's independently operated is exposed. Is that --

MR. MASLIA: That would be the assumption.

DR. CLARK: Okay. But you're not taking into consideration things like activity patterns --

MR. MASLIA: No, we're not.

DR. CLARK: -- water use by individual homes and that sort of thing?

MR. MASLIA: No. If you wanted to take into account water use, you would either have to have some measured and demand-type consumption metered information. At a Marine Corps base -- and I assume at military bases in general -- they do not meter household water. We'll actually address
that issue a little later on.

And as such, they do have -- they've got a production meter, obviously, that's going into the system. But because in some of the areas you've got mixed use -- say bachelor housing, industrial. In some of the areas, it's more homogeneous: total family housing. We would have to derive some estimates of that.

DR. CLARK: Right. To the degree that you can't do that, then that constitutes the potential for error, I guess, in the analysis.

MR. MASLIA: Yes.

DR. CLARK: I guess Frank's not here.

MR. MASLIA: Yes. Just as an example, when we were doing the work in Dover Township, we had quarterly billing records for about 18 or 24 months, and we -- I shouldn't say "we." I should say Jason put those in about a month at a time, putting them in by hand.

But they came out. Where we measured, I think it was 7.5 million gallons per day on a test. With the billing records, we came out with 7.6 million gallons. It was right on target. We don't have that here, and it's -- so that's just not available.

DR. WALSKI: Okay. It did bring up the question of historical data. One other source of data is the fact that a lot of the engineering work of this in the past was
done by the NAVFAC IINCOM LANDTIV up in Norfolk, and I haven't heard them mentioned. Have you gone up through their records? They may have some of the construction drawings and such that, you know, they don't have on post. Have you talked to --

MR. MASLIA: Let me talk about that now. We do have historical maps as paper copies, for example, as housing areas expanded. We've actually got maps that tell us how many more housing units were added and which pipelines may have been added. So from that standpoint, we do have that information.

When we've requested, even on the present-day system, say, for example the network drawings, if they haven't had them at Camp Lejeune, they have provided it to us either through their consultant. So I assume if they haven't had it on base, they have gone up to the Navy facilities. We are aware of that.

And, in fact, on the -- which I'll get to a little bit later on. There was a conservation study done. Most -- the Air Force and Navy developed this water-conservation analysis, a software. And we requested it, and I know they did go up to Norfolk to get a copy of that, and actually, that has formed the basis for some of our demand categorizations.

So I'll get into that, but we are aware of that, and
when needed, we have requested. But we personally have not gone up there, but we know someone has gone up there because of the information that we've obtained. Were there any more questions up to that?

The second approach then would be to -- in view of the lack of historical system operation-type information would be to develop a present-day system model; gather information on that; and then to, what we're calling, deconstruct the present-day system: removing pipes as they were removed historically and using the assumption that we've been told that they pretty much operated in a similar manner; use that to do the historical reconstruction or come up with historical systems.

Now, one of the differences we have found out, say, from the Dover Township work, unlike in Dover Township where the network changed at least every year, whether it was addition of pipes, hydraulic devices, or anything, there were just major -- major changes in only certain years at Camp Lejeune, for example, the addition of the Holcomb Boulevard plant.

From what we've been told and what we've been able to find out, they were not adding sections of pipelines every year. That sort of simplifies, at least from a simulation standpoint, where we can make some larger assumptions. So that we have found out, and that is why it's still very
important on this chronology, especially with the Holcomb Boulevard plant. If we can isolate that, the start-up, to a month and year, it will really help us out.

And this is the approach up to this time that we have been using to calibrate models for the present day; get a description of the present-day system in terms of operation, in terms of facilities; and then work backwards in time. The information in front of you and what we've been -- and what we'll discuss today, obviously, is just for the present-day system, but hopefully, we can also get some recommendations for the historical process.

And that's -- so the approach then would be to apply the output from the groundwater model, and that's the arrival of the concentration from the contamination. And then either apply it to Approach A or B, and as I've indicated, we have gone with Approach B because of the lack of information from the historical standpoint.

And that's really a summary of just the approach and what has prompted us to take the next step, which is the field investigation and understanding the present-day system. So at this point, are there any other specific questions on the approach? Yes.

MR. HARDING: Morris, there's a high-level question from the -- going up to 20,000 feet and looking at this for a minute -- and this may have been answered yesterday
since I wasn't here, so if it has, just somebody can take me aside and tell me.

And that is, is that in the event that or in the case where -- these systems were served by a single source essentially. The wells were blended into a water-treatment plant and then supplied to the distribution system, and those systems weren't interconnected then.

MR. MASLIA: Did you say were or were not?

MR. HARDING: Were not.

MR. MASLIA: Okay.

MR. HARDING: So we have independent systems served by a single point of supply. Then there's really no need for any hydraulic modeling in my understanding of the kind of etiology of disease that we're talking about. That is, these are chronic, relatively chronic, exposures.

So we don't need to know, with a precision of hours or even days, when a particular change in concentration occurred. So the calculation -- essentially, everybody in the system -- when you're averaging things out over a period of days or weeks, even that level is going to get the same exposure, the same concentration.

So it seems to me useful to divide this up into the epochs, if you will, of the configuration and operation of the system and decide, you know, what the benefit is of doing the detailed hydraulic modeling and when that
benefit's going to accrue because at some point -- at some points, all of the uncertainty, all of the arithmetic basically falls on the groundwater model. And at that point, once you know the answer to groundwater model and the dispatch of the wells -- if you've got innumerous wells, you have to understand that. Once it gets into the water-distribution system, it's no longer an issue.

MR. MASLIA: Right.

MR. HARDING: So we need to understand that to evaluate when you need to do, if you need to do, the detailed hydraulic modeling.

MR. MASLIA: Our assessment of the water-distribution system, when we were first presented with the opportunity to assist our division of health studies on the epidemiologic study, was really twofold.

First -- and I am not an epidemiologist. I'm probably stepping way off on the plank here. But my understanding on some of the health outcomes, birth defects, there -- they need some information in the first trimester, and I think it's Days 21 through 28 or something like that. So they had mentioned some daily information to us, and Dr. Bove is not here. But David --

MR. HARDING: I probably can answer most of the questions.

MR. MASLIA: Okay.
COURT REPORTER: I need you to get to a mike then.

MR. MASLIA: Oh, okay. But that was our -- one of the questions we had: Could we provide that kind or at least on a monthly, looking at trimesters, monthly information. Yes.

DR. CLARK: I wonder if everybody would be equally exposed because you're talking about people that may -- you know, women who might be in the household, maybe, 18 hours, 16 hours a day with children as opposed to some of the active-duty Marine Corps personnel who are off doing something else.

And I wondered if maybe one way to deal with that is sort of at least classify the percent of population who falls into these different categories who would have different kinds of exposures.

MR. MASLIA: We started down that road, and that's in the next presentation or at least classifying building types and the type of people that occupy those buildings, and that's in the next presentation. And it significantly varies by the different distribution systems, which I will get into. Can I put that off until we get to that? Yes.

DR. UBER: A point of clarification on that, Morris: You're only concerned with exposure of pregnant women.

MR. MASLIA: That's right.

DR. UBER: Okay.
MR. MASLIA: That's right. Women who were living in family housing, although they may have given birth off base --

DR. UBER: Right.

MR. MASLIA: -- because of the movement of the enlisted people, the enlisted men, as they took them off base. Some of them may have been pregnant during the period of exposure while on base, but then actually delivered off base.

DR. UBER: Right. Understood. But the exposure characteristics, the only ones that are of interest, are the exposure characteristics of the women who had been on base at some time during first trimester of pregnancy.

MR. MASLIA: Well, there's Dr. Bove. Let him --

DR. BOVE: What happened?

MR. MASLIA: The question was: We're interested in exposure of women, pregnant women, who were on base during only the first trimester.

DR. BOVE: No. That's for -- well, we have different outcomes, end points. I know I have to get to the mike.

COURT REPORTER: You knew what was coming.

DR. BOVE: Right. We have different end points, and for neural tube defects and oral clefts, it's the first trimester. But we -- because there's some uncertainty as to when the first trimester occurs, we asked for three
months. We asked for the whole year before birth. And we're looking at the first six months of that period: three months before conception, three months after conception because we don't know when conception really is.

So we leave a wide window there to determine exposure for oral clefts and neural tube defects. For childhood leukemia, since we're not sure -- all the evidence seems to indicate prenatal exposure, but we'll ask up to one year of life for childhood leukemia and non-Hodgkin's lymphoma.

DR. UBER: Okay. So you're interested in exposure of women three months before pregnancy, three months --

DR. BOVE: Conception; yeah.

DR. UBER: -- three months before conception --

DR. BOVE: Yeah.

DR. UBER: And three months after conception and --

DR. BOVE: Because we're not sure when conception is. Right.

DR. UBER: Right. And you're interested also in exposure of infants.

DR. BOVE: For childhood leukemia --

DR. UBER: Childhood leukemia.

DR. BOVE: -- up to one year of life.

DR. UBER: But you're not concerned about exposure of
active-duty military personnel who are -- I assume -- I can't remember when that changed, but I assume at that time they were all men.

DR. BOVE: Well, we're going to be asking in -- their drinking-water exposure, no. No. We're concerned about other exposures. We ask a wide range of questions in an interview. Okay.

DR. UBER: But not for drinking water?

DR. BOVE: But not for drinking water, no. I think -- we're really focused on that period of time. Okay.

MR. HARDING: Before you go --

DR. BOVE: Uh-huh.

MR. HARDING: Morris, let me just express --

DR. BOVE: I'm not going. I'll just sit there.

MR. HARDING: -- my understanding of how this system worked and ask a question of both you and Dr. Bove, which is if we go back to the case that I mentioned where we've got a situation where the system operated independently and was served by one water-treatment plant, then what came out of that water-treatment plant was going to reach a home in a matter of days or hours. It would stabilize, given the operation of the tanks. But if we look at the historical data we have a few snapshots here that Tarawa Terrace -- Tarawa Terrace. How do you pronounce it?

MR. MASLIA: Tarawa.
MR. HARDING: Tarawa was pretty stable. The measurements that were made in the water-distribution system were all within the factor of one and a half of each other, it seems like. So my question here is that when the wells -- the major influence then on the concentrations in that system would be the cycling of the wells if the wells in a well field had different concentrations, which might occur three times a day, it sounds like, something like that.

So for the question for the doctor, assuming that understanding is correct, then is: What is your time resolution in terms of understanding? What kind of averaging period is acceptable to you, and what kind of precision on estimates of, ultimately, human intakes that you're going to make as you assess this? What's your level of precision both in terms of time and magnitude that's -- that you need to have in order to make a conclusion?

DR. BOVE: I mean, we're going to be looking at monthly averages. So to do that, you know, at least weekly levels. But beyond that, it's unclear. It depends on how variable the data is, I guess. If there are spikes during a particular time, we'd like to capture that. But if there aren't, then, I guess, by week -- week by week.

COURT REPORTER: Can you go to the mike, please?
DR. BOVE: Yeah. A week-by-week assessment might be sufficient. You know, again, it depends on whether there are peaks. If, in fact, the water that went in was also the first water that came out and there are times when there are slugs going out so that the tap-water sample data that we have is not really reflective of what might occur at the tap. In other words, you know, it may be more closely related to that -- what's in that well actually than -- so there would be, instead of 200 parts that would bring us to the max, something like ten times that much, we'd like to be able to capture that, I guess.

MR. HARDING: That's what my question is: Are we dealing here -- orders of magnitude differences?

DR. BOVE: Well, that would be. Yeah.

MR. HARDING: Right. But, I mean, but you have to answer this because you've expressed this desire to have a six-month window of time. And the question is: Do you need to know what happened in the third week of that six-month window with a precision of two or ten or what? This is what I'm getting at.

MR. MASLIA: Frank was asking me what we did in Dover Township. And in Dover Township, they used the same approach of going zero months, not knowing when conception, to twelve months and the --

MR. HARDING: What was the resolution?
MR. MASLIA: And the resolution -- the model was obviously run on an hourly basis, and then we gave them an average over a month period.

DR. BOVE: But we weren't -- we weren't -- we weren't dealing with concentration at Toms River.

MR. MASLIA: No. No.

DR. BOVE: I mean, it's a tough question because there's so much uncertainly. I'm more concerned about being able to just determine whether people were exposed or unexposed, given some of the things you'll probably hear today about the confusion concerning interconnections and so on.

But if we can get that straightened out, if I can be confident that the people I'm calling unexposed are unexposed and vice versa, which we -- I produced something that -- yesterday that was handed out to you, which goes over what happens when you can't -- when you have some errors in just doing that and the impact on the odds ratio. If we can get that far, then I can live with weekly -- certainly, weekly estimates about resolution.

MR. HARDING: Well, but you're talking about were they exposed in a given week or were they exposed in a six-month period? Yes or no? What is it --

DR. BOVE: Oh, no. We -- there's two things here.

MR. HARDING: It's like two things here.
DR. BOVE: I'm sorry. Well, I'd like to know on a weekly basis whether they were exposed. Okay.

MR. HARDING: Whether they were exposed.

DR. BOVE: Yeah.

DR. LABOLLE: You mentioned at Toms River, you didn't -- you weren't concerned with concentrations. Are you concerned with concentrations here, or are you concerned with mass?

MR. MASLIA: Let me explain here. It's not that -- that's probably a misstatement. It's not that we were not concerned with concentrations at Toms River. We had an alphabet soup of concentrations that we could not separate out or get any definitive single contaminant like PCE coming through there because of the way that the contamination that was on hand.

So because of that -- and this, again, was part of the epidemiologic protocol -- it was decided by the epidemiologist to go after the proportionate amount and look at comparative amounts of water that each of the study cases received or did not receive from various well fields.

DR. BOVE: What I meant was it wasn't part of the analysis.

UNIDENTIFIED PANELIST: Okay.

DR. BOVE: It wasn't part of the exposure assessment.
DR. CLARK: Morris, I had a question. This may be going -- it may address this later on, but is -- have you looked at degradation by-products in the distribution system at all?

MR. MASLIA: No.

DR. CLARK: Okay. Because you've got a lot of cast iron pipe that's going to build up a very heavy biofilm. You've got lots of biological activity going on, and I'm wondering, with the residence times that you have, if it might not be something you might want to take a look at. I assume when the analysis was done -- a lot of them were done with just plain -- just the same volatile analysis using GC, right, back in the early days when they were looking for THMs primarily?

MR. MASLIA: That's what the lab notes indicate, and that's what they indicate why they could not do it when they saw the presence of the volatile or --

DR. CLARK: So there's no attempt to try to, say, differentiate to see if vinyl chloride might be possibly one of the by-products or not?

DR. BOVE: Well, they did later.

MR. MASLIA: Later on, they did.

DR. BOVE: Not during the THM -- not during the THM analysis; no.

DR. CLARK: I know there's a period there when there
were no methods, the standard EPA methods were volatile, so...

DR. BOVE: The issue's about biofilms and residence time.

MR. MASLIA: Not in Tarawa Terrace. It was --

DR. BOVE: Okay. So there's no dead-ends.

DR. CLARK: But there were -- do you know what -- well, you know what the residence times are in terms of the system tanks and so forth; right?

MR. MASLIA: From what we found from our field information -- and I'll present that --

DR. CLARK: Okay.

MR. MASLIA: -- the residence times may be forever.

DR. CLARK: Okay.

MR. MASLIA: And that's one of the issues that we discovered -- or when I say we discovered, during our field testing -- and I'll get to that. I'll just jump to the punch real quick. Even though we allowed fluoride to dilute over a two-week period down to .1 or .2 milligrams per liter, the tanks are still showing one or a little bit above after that.

DR. CLARK: So there is the potential then for very long residence times, biological action, and --

UNIDENTIFIED PANELIST: (Off microphone)

MR. MASLIA: Not the -- I mean, it is.

NANCY LEE & ASSOCIATES
DR. WALSKI: Well, but just the opposite though. When you had the tanks off-line, that means everybody gets very fresh water. The water in the tank just sits there, and so 99 percent of the people get water that goes straight from the plant to their house, which means the residence time on average is, you know, hours only in the system, not days. And the water in the tank just sits there.

MR. MASLIA: Right.

DR. WALSKI: It may dribble back in a little bit --

MR. MASLIA: Talking about in the tanks?

DR. WALSKI: Yeah, right; in the tanks. The tank water doesn't get consumed. So therefore that water is basically almost off-line except during a fire or something is the only time that water gets drained out of the system. So for the most part, the residence time on average is extremely short in a system like this.

MR. MASLIA: Except we've seen both, both cases. And I'll get into that now perhaps. But we've seen in a later test the tanks filling and drawing, and I've got some data to show that. So --

DR. CLARK: Well, I think it depends a lot on what the record shows as far as tank operation is concerned.

MR. MASLIA: So that -- again, our attempt or our concept was, if I can summarize this, is if we felt -- if
we could really understand the present-day operation, that would shed a lot of light on historical operations since we were told they were operated in a similar manner or the operation was in a similar manner, and that was our --

DR. CLARK: So it sounds like there were times when the residents would get water that was fairly aged.

MR. MASLIA: There were times; yes.

DR. WALSKI: But it would be aged in tanks though, not in pipes with contact with the biofilm that much. In the tank, you don't have much contact with the wall.

MR. MASLIA: Right. But again, we've got data to show both cases or at least our interpretation of it, that it shows both cases.

DR. CLARK: You still have biological activity taking place in the tank, too, as you know. So those are just some issues I thought that you might want to at least kind of chalk up and take a look at.

MR. MASLIA: We'll definitely note that down. And, in fact, we're looking at different tank-mixing models, just to let you know. Are there any other questions, suggestions, comments, or -- because what I'd like to do is get into the specificity of the present-day system and the field testing that we've done and perhaps address some of the issues that have been brought up this morning and go from there. Is that okay with the panel?
MR. MASLIA: Okay. Present-day system. Okay. We started preparing to do some field tests. Again, the present-day information, we had some production information from the utility operators, but specificity as far as hydraulics in the present-day system were not available, and we were, again, interested in ultimately travel times of potential contaminants. So we developed a field-testing program. And so we gathered information on pipeline locations.

I've described how we have obtained that for the present-day storage tank locations; high-lift pump data; operational data -- I'll get into the controlling tanks in a minute -- and production data; and what I'm referring to housing data and facilities' use data, classifying the different building types.

The approach was to construct present-day models, and we've done that for the three different areas: for the Tarawa Terrace, the Holcomb Boulevard distribution system, and the Hadnot Point. And the data that we were interested in gathering would be the hydraulic data, pressure, C-factor data for pipeline characteristics, operational data. This is including the controlling tanks and the on-off cycling of pumps, pipe-flow data, and travel-time data.
Primarily, our thought behind the flow data was that since we didn't have individual household meters and household consumption, if we could get an aggregate of small areas where the type of housing were homogeneous, then we could get a present-day per capita use and per diurnal type curves to service for that particular area, and that was our thought behind the flow metering.

So as of right now, we've got three hydraulically independent models. We're assuming here's where the interconnection between Hadnot Point and Holcomb Boulevard are. There are two sets of valves, one here and one here, that are closed off. And so we've got a model for the Tarawa Terrace-Camp Johnson area --

MR. HARDING: Morris, I got an ADH --

COURT REPORTER: Mike please. I didn't get it.

MR. HARDING: I'm color blind. I can't really make out that pointer very well. Can you just linger a little longer or point with your --

MR. MASLIA: Okay. Can I go over there and point to it? Will that be okay?

MR. HARDING: That would be great. Just where the valves are because that's a critical issue for me.

MR. MASLIA: Is there a pointer over here? How about the pointer and that way?

MR. FAYE: Grab the radio mike.
MR. MASLIA: Thank you.

COURT REPORTER: Now you're getting it (laughter).

MR. MASLIA: Is that on?

MR. FAYE: Yeah.

MR. MASLIA: Okay. This is the Hadnot Point area --

MR. HARDING: Right.

MR. MASLIA: -- to the south, and there's an interconnection valve here and one over here or a set of valves actually that they maintain closed for the present-day system. This area up and to here is what we're referring to as the Holcomb Boulevard water-distribution system. And then this pipe right here from the treatment plant provides water to the ground storage at Tarawa Terrace. And then based on demands and the controlling tank right here, that's how water is distributed within the Tarawa Terrace area up north.

Previously, when we mentioned the Montford Point here, that was in this area over there, which is present day no longer in existence at that treatment plant.

MR. HARDING: So when Tarawa Terrace was isolated, it was that pipe that crosses Northeast Creek there right by the 30, TT-30?

MR. MASLIA: Right there. This pipeline comes over there. And if you cross the bridge, you can actually see the pipe tied or bolted underneath the bridge, the bridge
there. And it comes into here. So what's left -- this is where the original or the former Tarawa Terrace treatment plant was. So the pump house is still there. They've got four high-lift pumps there. And the reservoir, underground storage tank, is still there. Just the treatment facility is no longer there.

MR. HARDING: Now, if I recall from the materials, there was a failure in that pipe due to freezing; is that right?

MR. MASLIA: That, I believe -- we discussed this yesterday. And I believe that's this pipe right here, and that is information we're still trying to get some more definitive documentation on. It's a report that was written in 1991 by Geophex out of Raleigh, North Carolina. We've got a contract number. We have no author that's on it.

We're trying to really -- and it makes a statement that two years prior, meaning about '89 or so, which is outside the study period -- but that might be some indication that there may have been other times that there may have been some interconnections, but that's some of the data discovery that we still need to figure out and find a resolution on.

MR. HARDING: Well, there's valves on the pipe -- the systems are isolated or were isolated by valves; right?
MR. MASLIA: This system and this system were isolated.

MR. HARDING: So the indication was that pipe was only constructed after 1985, the one across Northeast Creek?

MR. MASLIA: Yes. That would be correct.

MR. HARDING: Okay.

MR. MASLIA: Okay. In other words, because prior to the closing of this treatment plant, this treatment plant took care of this area here. So there would be no need to --

MR. HARDING: But that can't be right because originally Hadnot Point served the entire system; right?

MR. MASLIA: That was before Holcomb Boulevard plant came into being.

MR. HARDING: So that pipe existed from the very early days of Tarawa Terrace development.

MR. MASLIA: This pipe here?

MR. HARDING: Yeah. The pipe that crosses Northeast Creek.

MR. MASLIA: That, I could not tell you. Joel, would you know about that? Would that pipe have existed prior to the -- no.

MR. HARTSOE: Excuse me. The -- what he's talking about is the --
COURT REPORTER: Excuse me. Microphone.

MR. HARTSOE: At one time, Hadnot Point served the Midway Park area. That's up north, right there. And the connection that he was talking about, there's two separate connections between Hadnot Point and the Holcomb Boulevard distribution system. But, at one time, when the Holcomb Boulevard plant was not there, the Hadnot Point served only up north at the Midway Park area. It did not serve TT.

MR. HARDING: Okay. And so do we have a sense that that pipe that crosses Northeast Creek was constructed after 1985?

UNIDENTIFIED AUDIENCE MEMBER: (Off microphone)

MR. HARDING: Do you know when it was constructed?

MR. MASLIA: We've probably got that information in our --

DR. POMMERNK: Like I indicated yesterday, there seemed to be as-built drawings from 1984. And in discussing a little more, there may have been a temporary line for some time. But, you know, this is, like Morris said, all not clear at this time, when this -- but it probably wasn't -- hasn't gone on-line, you know, before '84.

MR. FAYE: Is this on? The records that we have indicate that that pipeline was constructed by June of
1985 or in that fairly short time frame and it was
operating in June of 1985 or shortly thereafter.

DR. LABOLLE: Is it your understanding that it was
constructed to help mitigate the closure of --

MR. FAYE: Yeah.

DR. LABOLLE: -- Tarawa Terrace?

MR. FAYE: Yeah. There was a recognized -- they --
as I said yesterday, Wells TT-26 and TT-23 were shut down
in February of '85. That -- and Lejeune immediately
anticipated a water shortage for the Tarawa Terrace area,
up into the spring and summer months, because of that
shutdown.

So they expedited this construction of this pipeline,
to the best of my knowledge, so -- and it was -- to the
best of my knowledge, it was supplying water from Holcomb
Boulevard to Tarawa Terrace to supplement their existing
supply by the summer of 1985.

MR. HARDING: Okay. Morris, I have another question.

MR. MASLIA: Sure.

MR. HARDING: And so is the epidemiological study
driven by particular individuals or time frames, or are
you trying to establish the dose-response ratio? In other
words, could you -- after that pipeline is in place in the
situation in Tarawa Terrace, if the wells and the Holcomb
Boulevard supply both served the area -- it gets
complicated.

But, prior to that time, it's not complicated at all. The only complication is how they dispatched the wells and the groundwater modeling. If I understand this correctly -- anybody can jump in if they think I'm wrong here. But prior to that time, you've got a much clearer picture. It's not perfectly clear, but it's much clearer than it's going to be after that pipeline opened.

MR. MASLIA: Yes. The epidemiologic study ends in December of 1985.

UNIDENTIFIED AUDIENCE MEMBER: Last birth; yes.

MR. MASLIA: Yes. Last birth is December of 1985.

MR. HARDING: It's that last -- it's that period from, potentially, 1984, some time in 1984, until December of 1985 that's going to drive 90 percent or 95 percent of your water-distribution effort.

MR. MASLIA: Plus we've got the potential issue, which we've been asked on a couple of occasions now, about the interconnection between Hadnot Point and Holcomb Boulevard.

MR. HARDING: I understand. I'm just trying to get one thing done first.

MR. MASLIA: Okay.

DR. WALSKI: But the point is, though, that if you don't have enough information to know how to do things
without the model, running this bad raw data through a model isn't going to make it any better. You know, because the boundary conditions are what's going to drive the model. And so we're still in the -- you know, back to the fundamental principle of modeling, which is: Garbage in; garbage out.

And you don't know when those things are open or closed. And you aren't ever going to know those things because we can't go back in time and ask people or check these things. So why model it?

I mean, basically, you have to say that in this period we know they got contaminated water. At this period, we know they didn't get it. And this period, we just aren't sure and we just can't do it. And running a model with wild guesses in it isn't going to make it any better is the point, back to this chart here I did yesterday.

DR. BOVE: I don't know if this is pertinent to what's being raised here, but my main concern right now and the problem with the previous study was that we called some people, a lot of people, unexposed when they were really exposed. Is that right? Yeah.

And if you look at the chart I produced, sensitivity, which means correctly calling someone who is exposed -- who truly is exposed, exposed, and not calling them
unexposed is a -- has a bigger impact than specificity of
exposure, correctly identifying the unexposed.

So I'm more concerned right now with being able to
say that these people -- certain proportion of the
population were unexposed and being confident of that
because that was the problem with the last study. And if
there are interconnections, we need to figure out how to
deal with that in our study. So, I mean, you know, I mean
the simplest analysis we can make in our study is simply,
as I said before, unexposed versus exposed and being
confident that we're identifying the people properly.
Okay.

Then, after that, we can talk about the level of
concentration and if we have the numbers. Part of the
constraints of our study is we have small numbers. You
saw the number of cases that we have to deal with. This
is not a large population.

In order to do a birth-defect study, I studied 80,000
births in northern New Jersey, and I still didn't have
enough really to -- I had small sample sizes when you
broke -- started breaking them down into exposure
categories. So this is -- you can't go too far in
categorizing the exposure before you really have very
unstable estimates for the relative risk or odds ratio.
So -- is this --
DR. DOUGHERTY: So to summarize and prioritize, we don't need to worry about the exposures at Tarawa Terrace because we can pretty well guess they're all exposed.

DR. BOVE: Except for that period; right? The later period? Right.

DR. DOUGHERTY: So we have -- I think we heard pretty significant evidence that pumps were operating through the entire study period out of Tarawa Terrace.

DR. WALSKI: Until they shut T-26 down.

MR. MASLIA: Yes.

DR. WALSKI: But until that date; yeah.

DR. DOUGHERTY: Possibly. We don't know the level of concentrations at other wells. TT-25, for example, is very close to TT-26, and it continued to operate, as I understand, at least into '86 or '87. So what we're -- the real issue is worrying about the controls rather than the cases. And that gets us out of Tarawa Terrace. Is that fair?

DR. BOVE: Cases and controls is not the way that I look at it. Exposed and unexposed -- we need to identify who's exposed and not exposed.

DR. DOUGHERTY: Replace my language.

DR. BOVE: Right.

DR. DOUGHERTY: And then, is that a fair summary, a first-order priority?
MR. MASLIA: Yes. Yes.

DR. DOUGHERTY: Okay.

DR. LABOLLE: And that falls on -- on this connection between Hadnot Point, I presume, and the Holcomb Boulevard system, essentially, because the potential for an unexposed population here is Berkeley Manor; is that correct?

DR. DOUGHERTY: What's Berkeley Manor?

DR. LABOLLE: Well, I'm looking at this development here, fed by the Holcomb Boulevard --

MR. MASLIA: Yeah. All the housing that would be served by Holcomb Boulevard.

DR. DOUGHERTY: That's very helpful.

MR. HARDING: Now, let me just point out that on 5 February 1985, somebody sampled somewhere in the Tarawa Terrace system and reports 80 parts per billion of PCE, similar to the sampling that was done in 1982.

So, I mean, the whole system, if you just look at these snapshots -- and we don't know what time of day, what day of the week, what the circumstances were, which wells were cycling. But it looks remarkably stable through that period. It looks to me like even into 1985 you could -- it would be reasonable to think that the people in Tarawa Terrace were all -- I want to make a nomenclature suggestion here -- potentially exposed, in
the sense that the concentrations were available at their tap, should they choose to turn it on.

So it's a potential exposure. The actual exposure occurs when they drink it, they take a shower, they bathe in it. So they may have had personal habits that they drank nothing but bottled water. They may have had -- they may have bathed rather than showered, which would make a big difference in how much they actually -- how much their intakes were. So we have to bear that in mind.

But the -- potentially, that population up there that lived there, if they used the water somehow, then had an exposure, had an intake. So in that whole area, up through 19 -- through at least, it would seem, February of 1985.

MR. MASLIA: Would it be then your suggestion or advice to just use that 80 parts per billion?

MR. HARDING: No. No. That would not be my advice.

MR. MASLIA: Okay. Then what -- then our question would be --

MR. HARDING: I'm going to defer to the groundwater people.

MR. MASLIA: -- is what number do we use?

MR. HARDING: But let me make -- the point is that the water-distribution system is not a substantial factor in what that concentration is. It's the groundwater.
It's reconstructing the historical conditions in the groundwater and then how the wells were cycled because if you had a contaminated well that was used, you know, once -- one day a month it's going to be real different than if that well was running all the time. So you -- we have to understand that. But the pipes, it seems to me, and --

MR. MASLIA: Would you not want -- let me -- let me -- again, so I understand or at least your approach or your understanding is, if we've got three, four wells, one of them is contaminated or whatever and they're mixing. Number one, you're suggesting that we use a simple mixing model. In other words, you pump groundwater however, assuming we get the information on how they're cycling. Then they're -- you use a simple mixing model, and then assume that that mixed mass was distributed equally to everyone in Tarawa Terrace.

DR. LABOLLE: Yes. That's correct. During the time when the systems were not connected.

DR. DOUGHERTY: Right. And to come back to the question that you asked Ben and Ben deferred on, it sounded like the first-order question was potentially exposed or certainly not exposed and that we don't care about concentrations for.

So the first priority out of that list that was given to us by Dr. Bove checked off. The second one -- it seems...
to me -- to come back to the end of the day yesterday, all of the focus should be on the source-release model. That's where the focus has to be. And the attention to how much at which well. That will fall out quite easily.

The hard part is the source term. And so you get the source, and in a relatively simple mixing model, if we need to get the second stage of concentrations, and the first-order estimates based on observed data are that the concentrations are stable, but that's only at the very back of the -- at the very back of the study period. So then we have to do the census work and precipitate perhaps more -- careful analysis of precipitation-induced accretion to get concentrations into the ground. But, you know, it really -- the comments this morning were quite helpful.

DR. LABOLLE: What source are you referring to?

DR. DOUGHERTY: ABC.

DR. LABOLLE: Okay. The ABC source itself.

DR. DOUGHERTY: For handling -- for handling the Tarawa Terrace problem.

DR. LABOLLE: I think, also, as important, in my experience, will be not just the source but the geologic uncertainty. For a given source, different geologic models can yield orders of magnitude, several orders of magnitude difference in arrival concentrations to a well.
And that's the kind of uncertainty that you'd be dealing with there. I mean, granted, we see a few concentrations here at points in time that make this system appear as if it's stable. But then again, we've only got --

MR. HARDING: Two.

DR. LABOLLE: So, that -- there's, you know --

MR. HARDING: I understand.

DR. LABOLLE: -- there's not a lot to go with there to assume stability in the concentration. My experience has been that there's a lot of variability in the arrival to wells based upon their cycling and how the systems are run and variation in the source as David had mentioned, so...

DR. KONIKOW: But the point -- one of the points is that you really -- your study isn't starting until 1965 --

MR. MASLIA: '68.

DR. KONIKOW: '68. That gives you 14 years from the time ABC Cleaner [sic] started. So the value in doing the groundwater flow and transport model will be to, you know, start the -- as best we know, they were introducing contaminants into the soil, at least, through the septic tanks very shortly after they started; maybe a year, maybe instantly, maybe a year, maybe two years at most.

That gives you 12 years for it to reach the water table and spread. The groundwater flow and transport
models, accounting for uncertainty, heterogeneity, and so on, will give you range of arrival times. But I'm guessing that the bulk of your realizations will get contaminant reaching the wells in that 14-year period.

MR. MASLIA: Oh, no question about it.

DR. KONIKOW: I think all of the uncertainty is going to be the range --

MR. MASLIA: Right; range.

DR. KONIKOW: -- is going to be before your 1968 starting time. So it's worth doing those flow and transport models just to demonstrate that, but I --

MR. MASLIA: Let me, again, and I'm not -- I don't want this to come out right [sic] that I'm questioning the panel. But I'm questioning you because we're, from what I gather, at a critical juncture as to how we progress or what direction we take. So I want to make sure, both for the record and for my understanding, that -- and based on what you said, Lenny, and some others.

It's your suggestion then that more of the effort now be focused on understanding the groundwater flow and transport, in fact, from the source characterization through any unsaturated zone to get to arrive at a -- or a reduced level of uncertainty for the concentration that goes into the treatment plant. Is that --

DR. KONIKOW: Well, you have very limited data
against which to calibrate your model. Okay. And you
know, in the period that you were collecting data, the
wells were contaminated. Okay. So if you're going to run
the groundwater model, it's a question of how do you get
from zero to that level of concentration that you're
calibrating. You start with an initial condition of no
PCE in 1954. Okay.

And then you start your model running. And there's
going to be speculation upon assumption built into that,
and you'll get a range of responses. My hypothesis or my
guess would be that all roads will lead to contamination
by 1968. You want to do the modeling to demonstrate it.
Maybe I'm wrong.

But you want -- the only possible outcome that would
differ would be a later arrival, and that may be the first
few years there's no exposure. I think that's unlikely,
but that's what you want to evaluate, and that's probably
the best you could hope from from all of these models.

MR. MASLIA: Would you look at then perhaps putting
some effort into different source characterization or
operation, a continuous source versus pulsing versus
operation five days a week versus seven days a week?

DR. KONIKOW: I don't see the point of doing that. I
mean, the only -- the only possible testing, in terms of
field testing, that might be worthwhile would be a tracer
test to get a handle on travel time in this saturated zone. But I would explore other -- apparently, there were tracer tests done in the Tarawa Terrace area specifically related to ABC Cleaners. And this comes out of a draft, a National Research Council report that I saw.

And they say tracer tests were done there, interwell tracer tests were done there. I don't know what distance the wells were apart. And I don't exactly what the purpose was, but there is some -- somewhere out there is some information, and it would probably be useful to get that. That might help pin down porosity, dispersivity, and travel time.

MR. MASLIA: Okay. We'll look for that information.

DR. KONIKOW: But I'm guessing the outcome is still going to be, from the start of your epidemiological study to the end, Tarawa Terrace residents were exposed, which, if you could support that, it kind of mediates the need for more refined modeling because it's not going to yield anything more than that.

MR. MASLIA: Then from a standpoint of being conservative, from a public health standpoint, let's assume we refine our groundwater understanding and we get it -- get the simple mixing model and get it at whatever concentration we happen to simulate going in. The fact that we may or may not come out with the 80 parts per
billion that was measured at the tap, is that immaterial
or is that of importance or should we go -- again,
supposing we come out with several hundred parts per
billion after that?

MR. HARDING: The 80 parts per billion, these are
five snapshots that we don't -- and first of all, we don't
know the sampling protocols, what time of day, what day of
the week, what the conditions were in the system. These
are just snapshots.

MR. MASLIA: Right.

MR. HARDING: So, I mean, in between those, it could
be 500; it could be two. You don't know. But the point
here is: After you've done the groundwater modeling,
you've got a one or a zero here on the breakthrough curve
having reached a particular well. You still have the
question -- and since I'm in the water-distribution
business, at least at this panel, I want to make sure we
still have a toehold on this; and that is, how the wells
were run.

You know, there's still this operational question of
how they cycled the wells. And if you had -- if I recall
correctly, there was a couple of these, two or three wells
-- groundwater people can may remember better -- that were
really contaminated at Tarawa Terrace. And then there
were several others that were still in operation. So
they'd cycle through these.

So the concentrations were going to vary considerably, depending on which well happened to be in service at a particular point in time. And so personally, right now, based on what I know, I would spend a lot of time, in addition to dealing with the groundwater issue, on trying to understand at least statistically how these wells were operated, getting the statistics of those well operations so you can -- you can do some kind of a calculation of the probability that any particular person was exposed at a particular time. And I have to say right now that a weekly time resolution is probably unreasonable.

You know, I just -- you know, the groundwater -- I don't know. Once it's there, the well's going to be contaminated, but how they ran the wells on a particular day is unknown and probably will never be really known.

DR. BOVE: Right. And I was talking to Bob Faye just a few minutes ago over there, and at the wellhead, we'll have issues of seasonality; right? I mean, there will be differences in recharge, so that I'd like to capture because, you know, that will impact -- if we can categorize exposure more than just yes/no, it will be important to know whether the first trimester occurred at a time of high recharge or low recharge. That would be
important.

MR. HARDING: I don't think the recharge is going to be as big an influence as to which switch on the wall has been flipped.

DR. BOVE: No. There's two --

MR. HARDING: Well --

DR. BOVE: Right. There's two, you know, general sources of uncertainty here, and I'm just focusing on --

MR. HARDING: You ought to pick the biggest one to deal with. Spend most your effort on the biggest --

DR. BOVE: Well, we have a couple of issues here too because we may have to do additional studies. Okay. So we would like to know when that contamination actually got to Tarawa Terrace. So that's why the modeling has to happen, so that we know exactly when that water got there because, if we have do to adult cancer study, for example -- and that's probably going to be recommended -- that we have a notion of how far back in time the exposure --

MR. HARDING: Prior to 1968?

DR. BOVE: Prior to 1968. Yes; absolutely. So that's why it's important to do the groundwater modeling and then determine that. But beyond that, as Bob was telling me, there's variability at the wellhead, which we have to capture. And then there's variability in the system, which you're pointing out, which we have to
capture.

Now, whether -- I just threw out weekly. I'm willing to -- at this point, I'm willing to take what I can get. That's what environmental epidemiologists do all the time. So if monthly is the best resolution that makes any sense, we can work with that.

DR. LABOLLE: When you refer to resolution monthly, temporally, what if I told you that I can give you a range of monthly concentrations and they vary over two orders of magnitude?

DR. BOVE: It wouldn't be unusual.

UNIDENTIFIED PANELIST: For a medical epidemiologist.

MR. HARDING: Yeah; but neither is zero.

DR. WALSKI: The thing is you're going to know they're exposed. In Tarawa Terrace, you know they're going to be exposed from this time until they shut that well off. And after that, they're not exposed.

The real hairy issue is the Hadnot Point to Holcomb one. And I'm afraid there you're not going to get a: Yes, they're exposed; no, they're not. You're going to get: Yes, these people were exposed. No, those people were not. And there's a big chunk of people that we think may have not been, but there may have been a few days that they got it. And that's going to be a chunk of your population. You're not going to get a yes or no for those
people because we just don't --

DR. BOVE: We have to whittle down that chunk because we're going to run out of --

DR. WALSKY: But that's going to be archaeology and not modeling. That's going to be finding out -- finding those people who retired who operated the valves and talking to them. And no amount of modeling is going to make up for that type of uncertainty.

DR. LABOLLE: Have you run the binary analysis already with the epi study? The one saying, you know, under the assumption that Tarawa Terrace is exposed during this --

DR. BOVE: That was the previous study. We said that everyone at Tarawa Terrace was exposed.

DR. LABOLLE: Uh-huh.

DR. BOVE: And everyone at Holcomb Boulevard was unexposed. And we left the Hadnot Point situation aside. And we've been challenged, rightly, that the study had exposure misclassification. The unexposed had a lot of exposed people in them because during -- at least '68 to '72, I don't know, they were getting Hadnot Point water, so they were hardly unexposed. And that really attenuates your odds ratio.

DR. LABOLLE: Couldn't you narrow that, your unexposed population, to a different time frame when they
actually were unexposed?

DR. BOVE: We could. We were going to revisit that study after this effort was done. But we could do that, sure.

DR. JOHNSON: Well, this has been really an outstanding discussion, and at the risk of imposing upon Mr. Maslia once again, I -- would you -- would you put in a capsule statement what you think you've heard from the panel?

MR. MASLIA: Basically, as I think said 15 or so minutes ago, we need to -- my understanding is concentrate on the groundwater issues. And I'll just put that in the issues, including the modeling, the source, what I call source characterization, a source understanding, trying to either narrow or understand the uncertainties associated with the groundwater parameters, infiltration, recharge, things of that nature, well operation, cycling on and off of the groundwater wells, and then assume a simple mixing model at the plant and assume that's what the people in Tarawa Terrace were exposed to.

DR. JOHNSON: Is that what the panel think that you said?

DR. WALKSI: That's right for Tarawa Terrace.

MR. HARDING: In a summary, yes, for Tarawa Terrace.

DR. JOHNSON: Well, thank you. Let's move ahead.
MR. MASLIA: Is there a need then to go over the present-day system, or...

DR. JOHNSON: Yes.

MR. MASLIA: Okay. Okay.

MR. HARDING: I think -- let's get back to this question here. We have an approach to establish an exposed population and within some range of uncertainty quantify those potential exposures and to calculate the intakes that resulted from that once we know what people did. That's the Tarawa Terrace area.

Now, I understand from the discussion that the second need now is to find an unexposed -- populations unexposed to the contaminants that had a similar lifestyle, you know, geographic location. So we're trying to find another population; right? That's our next -- that's our second need here; am I correct?

DR. BOVE: On the base; yeah.

MR. HARDING: On the base being important because we want them to have similar --

DR. BOVE: It's family housing, so they would be similar. It won't differ by -- too much by housing, and we can control for rank if necessary. We've done that before in a previous study.

MR. HARDING: Right. The reason I made that point of similar other exposures is we can't go off the base and
find somebody -- some population. So we have to find some place --

DR. BOVE: Not for this study. Not for this study, we can't.

MR. HARDING: -- some place on that diagram, try to find some place where you can be reasonably certain people were not exposed for some certain period of time, right, specified period of time?

MR. MASLIA: Yes. And I do have, I guess, just another question to understand. Is it your suggestion or understanding then, and going back to Tarawa Terrace, that we would not need to know a diurnal pattern of any type over a 24-hour period as far as to refine periods when they did or did not most likely ingest water?

DR. CLARK: I think you would.

MR. MASLIA: We would need to?

DR. CLARK: That's my opinion. Yes.

MR. HARDING: You would only need it -- you would only need it to try to go back and reconstruct the well operation in my mind. Because once that water gets into the pipe system, assuming there's only one source, it's eventually going to reach every point in the system in a matter of -- if the tanks are really irrelevant, in a matter of hours. And the other -- most conditions. And maybe, if there's dead-ends, it will take a little longer.
But the important part of potentially understanding water demand would be to go back and try to reconstruct how they cycled the wells because they might bring more wells on during the peak hours. They might not. If they're not using the tanks, that's probably what they're doing. I mean, there's only -- if they're not using the tanks then they're matching their supply and their demand quite well. Wouldn't you say, Tom? I mean, that's what you got --

DR. CLARK: I thought they were using the tanks. I thought that was part of what they found out from their study.

MR. MASLIA: Yes. They found out both -- both in different areas.

DR. WALSKI: We can't get rid of that uncertainty, so why try to model it? You know, depending on -- you know, Joe was operating the system in '91 and '92 and he did it this way. And Johnny came in '93 and did it this way. But back in '87, we had Frank did it and he did it this way. And we can't -- we're not going to be able ever to unravel that, I don't think.

MR. MASLIA: You're talking about operating the distribution system?

DR. WALSKI: Yeah; operating the well pumps. You know, we're not going to be able to unravel that it
doesn't appear, other than saying on average from the
USGS data we know they pumped this much out that year of
the well. And that's about all -- that's the level of
resolution we're going to have. So talking about
hourly --

MR. HARDING: Monthly.

DR. WALSKI: Yeah. Okay. Monthly then. But
talking about hourly is just -- we just can't get down to
that resolution.

MR. HARDING: Do we have data for individual wells,
production data for individual wells?

DR. CLARK: Monthly, I think, isn't it?

DR. DOUGHERTY: I believe you said yesterday it was
monthly totals for the system.

MR. MASLIA: Yes.

DR. DOUGHERTY: And then we have snapshots in time of
the individual --

MR. MASLIA: Yes.

DR. DOUGHERTY: -- well capacities from the Tarawa
Terrace --

MR. MASLIA: We've got monthly totals.

DR. DOUGHERTY: -- capacities, not actual rates.

MR. MASLIA: We've got monthly production, raw-water
intake for each of the treatment plants in the eighties.
We're missing a couple of years.
DR. LABOLLE: And some notes.

MR. MASLIA: And then we've got some notes on some other --

DR. UBER: Well, you have an understanding of their methodology of operation.

MR. MASLIA: Yes. Yes. We do have an understanding with also the understanding, although it may be qualitative, that they operated in a similar manner historically.

MR. HARDING: I don't want to sound frivolous here, but when going back and trying to figure out how people have run things in the past, I tend to look at their motivation individually. And in a public municipality kind of situation, that typically is cost.

So they'll typically try to run their most efficient resources first. They will get beat up by the city council or the utilities director to try to cut back on your costs. And I don't know what the motivating factors for the operators here were. But you have to ask that question if you're trying to go back and just come up with some hypothesis about how they operated, which may be the best you can do.

MR. MASLIA: Well, I can, perhaps, from what I've observed, or we've observed, one motivation would be to keep the tanks filled.
MR. HARDING: Yeah. But at which wells would they --
MR. MASLIA: I'm saying --
MR. HARDING: You know, it may be that certain wells were maintenance problems. So they would not run those as often. It could be that they had a particular cycling scheme to avoid biofouling. I don't know. But these are the -- I'm not saying that we can determine that now.

But I'm just pointing out that when you go back you're going to interview people about how they ran this stuff. You're never going to know exactly. But you can refine that a little bit by saying: Well, they typically would run this well first because the switch was closer to the -- to the office. I don't know. They didn't have to walk as far. I mean, things like this happen.

DR. WALSKI: Well, we do have some evidence. Somewhere I read here that they ran several -- they ran each well several hours a day was their usual pattern. So we have at least that guidance that it was fairly uniform, that they didn't operate one for three months and then shut it off for three months.

MR. HARDING: Right.

DR. WALSKI: It was more -- you know, several hour cycles.

MR. HARDING: Right.

DR. WALSKI: So we know that the average, you know,
contribution from each well over a day was fairly steady. It wasn't changing.

MR. HARDING: And that would be, certainly, one model to think about, you know, would be that they continued that operation, one scenario.

DR. CLARK: But back to the 24-hour exposure, I think you do. Particularly if you get into adult cancer studies and other epidemiological studies, you're going to have to have some sense of what people were exposed when. And it seems to me the only way to do that is to come up with some typical 24-hour cycles of exposure.

MR. MASLIA: That was our -- one of our motivations in trying to understand or at least get some system flows, present-day flows in the system. Now, that may or may not be --

MR. HARDING: But it doesn't -- that doesn't -- what matters is what goes into the system; that is, how they operated the wells, the cycling of the wells. That's what matters. Because once it gets into the system, that defines what the profile of exposure is going to be over the next several hours. If they're using the tanks, then it's going to get dampened out some way.

MR. MASLIA: But are you saying we do not need to know that -- and I'm just using throwing out numbers -- at 4 a.m. there's an upswing in demand? So obviously, on the
Marine Corps base, perhaps, because they're showering at 4 a.m. And then it levels off, and then they come home, you know, at 4 p.m., and the upswing goes up. Are you saying -- it's my understanding that you're suggesting we don't need to know that.

MR. HARDING: Let's separate the two issues here. One is the behavior of the potential cases, the people that were exposed, from the operation of the system. And that information might be valuable in trying to figure out how they cycled the wells. But I would -- I would not. And I'm not an epidemiologist. But based on what work I've done related to this, I would not try to infer what people were doing from the water use of the entire system.

What I would do is try to look at the people, the individuals, to the extent that you can interview them or classify them, as to their behavior. And if you can't, then use population-default probabilities that they would shower at this time. If there's only one source, you're never going to know. No matter what, you're never going to know what happened at a particular hour. You won't. You can't know that. You can --

MR. MASLIA: You don't think we can know that because it's a specialized population on a military base?

MR. HARDING: I'm sorry. I was talking about the water-distribution system.
MR. MASLIA: That's what I'm saying. In other words, when we've been on base anyway, at least our observation is that, as we're conducting field tests at 6 a.m., they're all out jogging, doing exercises. Okay. So they're out of the house or out of their quarters at 6 a.m.

If you look at some of our data, you see an upswing in production or whatever at 4 a.m. Well, that would seem to indicate somebody's using water from at 4 a.m. or using more water. Let me qualify that.

MR. HARDING: Well, I would hope you'd see it at about seven, you know --

MR. MASLIA: No.

MR. HARDING: -- when they come back from running.

MR. MASLIA: That's not what we've seen.

MR. HARDING: My point was that, in the water-distribution system, you won't know the concentrations to the hour. You just can't know that. The behavior of the people, you know, you may be able to infer that from other things you know. But I would not infer it from, at least solely from, the water pattern of water use in the system. That's all I'm saying.

DR. CLARK: But it's that combination of use and concentration that's important in terms of exposure.

MR. HARDING: Right. The concentrations represent
what I call the potential exposures.

DR. LABELLE: Are the concentrations important, or is it the total mass --

DR. CLARK: I guess it depends on what you're looking -- I guess if you're looking as adult cancer exposure, I would think the concentrations would be important.

UNIDENTIFIED AUDIENCE MEMBER: We'd like to have that.

DR. CLARK: Every epidemiologist would like to have that, I would think.

MR. HARDING: Yeah. What Eric is saying is that it's the actual mass that enters the body that matters medically, and so it's a combination.

DR. DOUGHERTY: That depends upon the contaminant.

MR. HARDING: The drinking and the -- their behavior because if the water is at the tap and they don't use it, it doesn't --

MR. MASLIA: That's what I'm asking. Not to belabor the point, but I'm trying to understand. If we're saying we want to understand their behavior, short of having activity patterns, would not a surrogate for that be the development, based on data of diurnal patterns for different locations within the base, knowing -- knowing that they -- that you've got a specialized population here. In other words, you've got --
DR. KONIKOW: But is there any hope if you knew the concentration at every well at all times, which you're not going to? But if you did, even given that information, do you know enough about when each well was pumped during the day, how it connected to the distribution system, to the treatment plant, to the tanks, that you could then predict what the concentration distribution within the residential area would be and how it varied with time on an hourly basis? That's just --

MR. MASLIA: No. That, we do not have.

DR. KONIKOW: I mean, it just seems hopeless to try to get hourly exposure data.

DR. CLARK: But you could get typical exposure patterns.

DR. WALSKI: But they're getting the same concentration every hour. So the pattern doesn't really matter.

UNIDENTIFIED PANELIST: Right.

DR. CLARK: Well, it is important. I don't understand your --

DR. WALSKI: If you're getting 80 in the morning or 80 at night, that's not the distribution system.

COURT REPORTER: I need you by the mike; one at a time, by the mike.

DR. WALSKI: Okay. But that's not something you
model. I mean, the model can -- let's say you get 80
during that day, and that's into the epidemiology whether
they drank it in the morning or drank it at night.

DR. CLARK: Well, it would depend on whether you're
using it -- you know, whether you're inhaling it, whether
you're ingesting it. I mean, those are very important.

DR. CLARK: Yeah, those are things are exposure
patterns and you have to be able to have that kind of
information. I think you could get that from a daily
exposure -- a daily cycle of concentration plus
superimposing upon that the pattern of activity.

DR. KONIKOW: Where are you going to get a daily
cycle of concentration from?

UNIDENTIFIED PANELIST: We're not going to get that.

DR. CLARK: I think you can get a typical daily
cycle.

UNIDENTIFIED PANELIST: I don't believe --

UNIDENTIFIED PANELIST: Where? Where would the
variation come from?

DR. CLARK: I believe you can.

UNIDENTIFIED PANELIST: But it goes through the same
place.

UNIDENTIFIED PANELIST: But you don't know that.

COURT REPORTER: Gentlemen (laughter).

UNIDENTIFIED PANELIST: Is she a Marine?
COURT REPORTER: The record is suffering, and you're not getting anything right now. This is for your advice.

DR. SINGH: I think we are playing a little bit of a pundit here. I think the main issue is the water-distribution modeling system here and how does it affect the exposure. That is really the crux of the matter here. That's what he's trying to get at. And as Ben pointed out, as Tom pointed out, I don't think it is really going to make a whole lot of difference so long as we know the concentration and the depth because that is what is going to determine the exposure of the people.

DR. CLARK: But you have to -- I think you have to have the modeling to be able to predict what the concentration of the tap is going to be.

DR. SINGH: Well, I'm not sure really if the water-distribution modeling is going to make that much of a difference to the concentration. I think what we need -- you know, what the groundwater model is giving, that is really the crux of the matter. Once that -- that gives us -- once it goes into the treatment plant, the water comes into the pipes. I don't think the pipes are going to make a great deal of difference unless, of course, as you pointed out, unless we take care of the biology and the chemistry, which they are not --

DR. LABOLLE: Well, that would be another issue.
DR. SINGH: -- which they're not dealing with. Then I'm not sure how it is going to really greatly impact the pollutant concentration which the people will be exposed to.

MR. MASLIA: The fact that -- given that we may not be able to show a difference, you know, day to day or whatever, but rather just come up with a typical day, do we still need to be able to demonstrate that it is insensitive in a formalized way, not just make a statement, but demonstrate in a formalized, approved, or acceptable method, i.e., some kind of model or something, at least running it to some degree to show that this is insensitive and that there's no need to refine it any further?

DR. SINGH: Your snapshot data, on May 27th, 1982, tap water at TT tested: PCE, 80 ppb. Then if you take in the snapshot, February 5, 1985, TT tap water tested: PCB, 80 mpb. And in between, there is a little bit of variation. It seems to me that really that it's not a very wide range of PCE concentration in the water-distribution system.

DR. LABOLLE: It's likely to vary more than, I think, what's indicated by these two snapshots. That's just --

DR. BOVE: Probably an order of magnitude.

DR. LABOLLE: Maybe more; maybe less.
DR. POMMERENK: It's just -- for example, wasn't there a sample point that indicated 12,000 micrograms per liter?

MR. MASLIA: At a well?

DR. POMMERENK: At a well.

MR. MASLIA: At a well, there was 1600 -- almost 1600 parts per billion.

DR. POMMERENK: So hypothetically --

MR. MASLIA: Was that Well 26?

UNIDENTIFIED AUDIENCE MEMBER: (Off microphone)

MR. MASLIA: Well 26 was almost 1600 parts per billion?

UNIDENTIFIED AUDIENCE MEMBER: Uh-huh.

MR. MASLIA: Yeah; 1600 parts per billion.

DR. POMMERENK: Hypothetically, you know, the well could have been turned on in the morning before any other well was turned on, and that got into the tank. And let's say we had some, you know, plug flow there. So the slug of 16,000 -- 1600 micrograms per liter could have reached some consumer within hours or a day. So there is a range of, you know, a factor --

DR. DOUGHERTY: What if it proved less than that because it takes multiple wells to fulfill the demand? Right? Let's say --

DR. POMMERENK: Well, I mean if we assume --
DR. DOUGHERTY: -- three, roughly three at a time, I think, is what we discussed yesterday.

DR. POMMERENK: Well, if we assume that there's always complete mixing and so on. And, of course, there are reserves in the system. So they may draw some water from tanks once in a while, and so...

DR. LABOLLE: But, certainly, that concentration in that well, you know, although we see, you know, a point in time 1600. It could have been 16,000, you know, the month before.

DR. JOHNSON: Mr. Ensminger, do you wish to comment?

MR. ENSMINGER: Let me get up here before I get yelled at (laughter).

There was one test at Tarawa Terrace that did show 215 parts per billion. And that was taken in February of 1985, just prior to the wells being closed down.

MR. HARDING: Was that a test at a well?

MR. ENSMINGER: Yes. And it's in the public health assessment -- no; not at the well. That's at the tap.

MR. HARDING: What was that number again?

MR. ENSMINGER: 215. And it's in the public health assessment.

DR. JOHNSON: I'd like to -- I've been getting sort of a frantic message here from our recorder that she needs to calibrate her recording equipment. So I'd like for us
to take about a ten-minute break. We can return and continue this discussion. And I would like to talk with Mr. Maslia as to what you feel you need to present next, if anything. Okay. So about a ten-minute break.

(Whereupon, a recess of approximately seven minutes was taken.)

DR. JOHNSON: About how much time will you need, Morris?

MR. MASLIA: Three years (laughter).

DR. JOHNSON: And more.

MR. MASLIA: No; probably 20 minutes, maybe. Is that too much?

DR. JOHNSON: We'll give you 15 minutes. Okay?

MR. MASLIA: Okay. I'll --

DR. JOHNSON: So about ten after --

MR. MASLIA: Okay.

DR. JOHNSON: -- try to wrap it up. And then we can turn to these questions.

MR. MASLIA: Okay.

DR. JOHNSON: I'm obsessed by these questions, as you can tell.

MR. MASLIA: That's fine. I appreciate that. What the presentation, continuing from this morning is intended to be, is to go over what we understand about the present-day system. So I'll proceed along that road.
This is an example of the Hadnot Point water-treatment plant. Does anybody mind if I stand out here? Okay. But basically our approach is not to model anything within the treatment plant but basically the flow or the discharge coming out, the assumption being that nothing significant would occur to the -- once the wells are mixed to the concentrations within the treatment plant.

So what we have from a link and node point of view is we're just supplying water at a node or at a point to the distribution model and putting in demands and having our tanks. That's the approach in all three models that we have. And this information we obtained from the water utility or from records that we have -- production records that we have.

MR. HARDING: How complete are those?

MR. MASLIA: We have -- as Bob said yesterday, we've got records in the eighties, except for a couple of years, a couple of critical years. We've got sporadic information, and then we also have some in the nineties.

This is monthly data for each of the -- it's production for the total treatment plant, in other words, not by well, but by what the -- the plant took in as raw water and then produced and put out into the system and then what we measured in the field.

Each system is operated -- each of the three systems
is operated by using what's referred to as a controlling tank. That's the one with the asterisk. And based on the water level in that tank, that triggers high-lift pumps to push water out into the system or fill the tanks.

We've done some C-factor tests. The -- this is an average of all. We did eight C-factor tests. These are the averages. The ones where it says "C-factor tests" is an average of the tests for that particular pipe type. We found that the cast iron pipes had a -- what we thought indicated more of a smooth as opposed to more rough type characteristic in them.

DR. WALKSI: Morris, roughly what percentage of the pipe was cast iron versus PVC? I guess they're the two main ones.

MR. MASLIA: I have that, and that's in the notes.

DR. WALKSI: Just approximately. Was it like half was cast iron or 10 percent or...

MR. MASLIA: I want to say 60 percent, but I'm not -- it's in the report, and I don't have that off the top of my head. But we've got a table. There was a table in the report that listed it, but we can get you that number.

DR. WALKSI: Okay.

MR. MASLIA: Cast iron; yes. It's 34 percent cast iron.

DR. WALKSI: Okay.
MR. MASLIA: That's present day --
DR. WALSKI: Okay.
MR. MASLIA: -- present-day system. We've also
distributed or developed what we're calling demand groups,
and this is based on these group -- groupings are based on
nomenclature from a water-conservation analysis that was
done in 1999 for the Marine Corps at Camp Lejeune.
And we've got this unknown negligible group basically
because there was a large disparity between what could be
accounted for and what couldn't be. It's about 30 percent
difference.
Just to show you the distribution based on our
understanding. This is Hadnot Point, and you can see in
the Hadnot Point -- and I know -- I apologize, Ben, that
this is in color. So let me get a -- I'm trying to think
where I put the pointer now.
DR. JOHNSON: Use the microphone. I was referring to
Ben.
MR. HARDING: Sorry.
MR. MASLIA: Okay. Ben, Hadnot Point -- this is
really the only family housing right up in this area. The
area down in here is bachelor housing. And then the rest
would be more industrial and other offices and things like
that; whereas, in Holcomb Boulevard and Tarawa Terrace --
in Holcomb Boulevard, we've got all this area down here,
down here, and down here. That's family housing. And, of course, in Tarawa Terrace, we've got -- that's nearly 100 percent family housing with the exception of some shopping centers.

The number of nodes that it's referring to in the model is basically for in all pipes. These are all pipe models, although we have also developed the network for skeletonized ones as well. That is basically a short description of the present-day distribution systems, and now what I'll do is go through the field testing that we've done.

DR. CLARK: One question is: Do you have a picture of how pipe replacement took place over time? If it's 34 percent cast iron now, one of the issues is going to be, I think, how much of it was cast iron under previous scenarios, I guess?

MR. MASLIA: Here's a picture of how it took place.

DR. CLARK: Yeah. And do you know how much, for example, in 1985, how much was cast iron?

MR. MASLIA: Not right offhand.

DR. CLARK: Okay.

MR. MASLIA: We don't. But we do know because right now they're replacing -- substantially replacing. They've got a building program, say, at Tarawa Terrace. And so they're, as we speak, replacing -- replacing pipes with
PVC. On the other hand, they replaced a pipe going up to the Naval hospital. That's the asbestos cement pipe that we did a test on, and for whatever reason, when they replaced the pipe, they used asbestos cement not PVC.

So I'm -- for whatever reason, I don't know. I'm assuming that's the way the contract -- whoever bid on the contract replaced it with. Okay. Continuing on, we've conducted -- we conducted a test at -- in the Hadnot Point area from May 24th to 27th through monitored system pressures. We retrieved storage-tank levels and we conducted dual-tracer tests.

We injected calcium chloride, and then we also -- it says injected sodium fluoride. We shut the fluoride off to the -- we didn't shut it off. The utility people, at our request, shut the fluoride off. And they used a sodium fluoride gravity-feed system at both treatment plants.

Just some equipment that we used to monitor: pressure loggers. And these are the water-quality monitoring systems. There's a dual-probe system that's ion specific. In this case, it can measure fluoride and what we specify: fluoride and chloride and then conductivity in the other probe plus pH temperature. The single probe can measure conductivity and -- but is not ion specific.

This is the way we attached it in the field, putting
them in some plastic housing and then strapping it to the hydrant and flowing the hydrant. And then we also obtained grab samples as well and did some QAQC on site as well as sending grab samples off to the federal occupational health lab in Chicago.

This map here shows the monitoring locations, and you've got that in the reports. We had 27 different monitoring locations for the Hadnot Point. We had nine pressure; nine dual-probe locations, where we did fluoride and chloride; and then nine, just conductivity locations.

As I said, pressure ranges basically between about 55 and 65 PSI and fairly constant. And the topography is fairly flat there as well, which gives you very small hydraulic gradients. And realizing that, that was one of the reasons behind us doing tracer tests, as we felt we would not get any kind of unique calibration even on the present day just looking at hydraulics.

This is some -- I'm just going to show you some data from this test. This is injecting calcium chloride. This is at location F-02. The red line is a model simulation, and the -- or the solid line is a model simulation, and the dots are the data recovered by the logger. Here is an example -- this square box is the injection time and at location F-01, which is -- let's see where is -- on my map, it's at Hadnot Point, which is (off microphone) --
MR. MASLIA: Hadnot Point was located right over here. That's F-02. And, of course, what we found out that that's about a 20-hour lag and which greatly exceeded what we predicted in the model even though the model wasn't calibrated. And so we had thought there may have been some closed valves, and post-test auditing by the water utility, in fact, confirmed we had four closed valves.

And you've also got this drawing in the notebook that we gave you. But you can see right over here. Here was the source at the treatment plant. So we've got down here a couple of hour travel time, down in here, down to here about nine hours; but all the way up to here, between 20 and 26 hours, right here. So that obviously shows the effect of the closed valves and low demand as well. So it just stayed in the system there.

This slide shows you the fluoride concentration in the tanks. We had requested that the utility shut the fluoride off on May 15th. The test took place the week of May 24th. And, in fact, water samples, taken by the utility within -- at the distribution-sampling point showed concentrations of fluoride down around between .1 and .2, so it had diluted down.

But the concentrations in the tanks ranged between --
almost averaged about one from these three tanks. This is the controlling tank right down here. So, of course, it's exchanging water back and forth. So it's getting the -- it's diluting; whereas, these tanks over here really did not show much dilution.

This is an example of -- and it caught us by surprise when we -- in the beginning since we didn't understand how they were operating the tanks and that. Here is some grab sample data of the fluoride, and this is a logger that was near the French Creek tank, which is the controlling tank, which was this tank right over here.

And unfortunately, we had to pull the logger for technical reasons. But we still see the grab sample data rising and chloride concentration indicating a slug coming through here. And then 48 hours later, all of a sudden, we see a slug of -- we reconnected the logger, and we see a slug coming through.

And that is sort of what guided us and then in a subsequent test in instrumenting the tanks, putting loggers on the tanks and seeing that the water was not mixing completely in the tanks.

DR. WALSKI: When you say putting a logger in a tank, are you sampling the pipe going into the tank at ground level or are you taking sample from inside the tank and --

MR. MASLIA: Not from inside. We're putting it on
the pipe. And we can see -- and I've got some data to show -- on a subsequent test, you can tell which is the system fluoride and which is the tank fluoride --

DR. WALSKI: Okay.

MR. MASLIA: -- by the spiking, by the spiking of the logger data, in other words, so you know what elsewhere in the system, what the fluoride level is. And then all of a sudden you see a high spike coming through the logger, which is interpreted to be the tank releasing water to the system. And that's when it's -- that's when you're shutting off the fluoride.

Just the opposite is true when you're increasing the fluoride in the system. You'll see low fluoride from the tank now going in the logger as opposed to higher fluoride from the system. And I'll show that in just a few minutes.

Okay. We conducted the hydraulic test in the week of August 25th. Again, this was to determine some C-factors, and we used sort of an innovative fire-flow testing technique where we opened up several -- several hydrants at the -- at different times. We found eight. I think we tested eight different sections of pipe. One of -- because of the piping construction and layout, we were really trying to look at the -- get some information on the Hadnot Point area. But it was just not possible to
find, say, a thousand-foot long section of pipe with three adjacent hydrants.

We scoured the maps and stuff and went out in the field, and that just was not a possibility. So we did fire-flow tests in that area, but -- and for the C-factor test, we used a diffuser, and then we also used a pitot gauge for the fire-flow test in combination with this diffuser.

And this is actually from the summary I showed you before, the three C-factors. These are the actual values that came out, and they were pretty much -- as I said, on the average, they were within the literature published about values. For the fire-flow tests, what we did: We sort of modified the standard approach of putting a gauge on one hydrant and flowing the other. What we did is we used, in this case, two hydrants, flowing -- this is flowing Hydrant 1 here and Hydrant 2 there.

So we would have a static pressure, which you can see basically is about 50 to 53 psi on the observation hydrants. Then we flowed Hydrant 1, which would be this one, 773 gallons per minute. And you can see the pressure drop across all the hydrants. Then we flowed Hydrants 1 and 2. So we'd flow this hydrant and that hydrant, and you'd see a further pressure drop right there.

So that's the total flow coming out. It was about
1300 gallons per minutes. And then, of course, we shut off Hydrant 1 and only flowed Hydrant 2 and then go back to the static case. So that one came out very well, and that was to help us with calibration.

Finally, we conducted, based on our observation of what we saw in May with the Hadnot Point with the concentrations going -- or being delayed and coming in at a later time than we expected at the tanks, we thought we would instrument the controlling tanks.

So we had the water utility put some ports on the pipes leading to the storage tanks, and in this case, we had two controlling tanks. We had one at Paradise Point, which would be right over here. That's controlling -- that's a controlling tank for the Hadnot Point water-distribution system. And then the Camp Johnson tank, which is the controlling tank for the Tarawa Terrace distribution system.

So again, based on the water level in those tanks, that's what triggers the high-lift pumps to turn on or not turn on. And I just -- I showed you there. We monitored the system. We used -- we've got nine of the loggers. So we monitored the fluoride. We shut off the fluoride and recorded as it was diluting.

And then we had the utility turn the fluoride back on and record the increase in fluoride. We did not do any
injection on this test. And so, Tom, you were asking about the storage tanks. This is a picture of the Paradise Point storage tank, the piping. And right over here is piping going in and one coming out. And so our loggers attached right here and on the outside of the actual housing but -- so depending which way the water would go, we would either get inflow or outflow and be able to record the fluoride concentration in the logger.

DR. WALSKI: Is that one tap or two taps?

MR. MASLIA: That's two taps.

DR. WALSKI: It's two taps.

MR. MASLIA: Two taps.

COURT REPORTER: Microphone, please.

DR. WALSKI: Mike. Okay. It's two taps, but how do you know that you're not getting the old -- the wrong water, if you had two taps like that? That it's --

MR. MASLIA: If it's going -- if it's going in -- and I'm trying to remember. I think it goes in --

DR. WALSKI: Usually, it fills through the smaller one.

MR. MASLIA: Going in this way. Right. That's the smaller pipe, going in this way. Then when it comes out, it's going to come out that way.

DR. WALSKI: Okay. But some of the -- well, that's okay. It's probably a very minor thing. Don't worry.
MR. MASLIA: So here are a couple of loggers. F-01 is the source. That was put at the -- on the Venturi meter or near the Venturi meter at Holcomb Boulevard water-treatment plant. So that's essentially your source.

This dotted line here indicates when we shut the fluoride off, which was at 1600 hours on September 22nd. And then we turned it back on at 1200 hours on September 29th. And Logger No. 3 was located down here. So Logger No. 1 is over here. Logger No. 3 is here. And you can sort of see the time it takes between here and here. So that's your -- you know, you could estimate an average travel time from there.

This is the example of the loggers connected to the controlling tanks. F-08 is the controlling tank at Paradise Point, which is this one over here. And F-09 is the Camp Johnson tank. That's basically the end of the distribution system as it is today. So, for example, right here, as the system water is being diluted, the system water -- and that's -- our grab samples show that too is down around .2.

So you've shut off the fluoride here, and by this time the system water is down about .2, but you're getting spikes of high -- high fluoride water, which is coming from what's in the tanks. Okay. And then just the opposite occurs when you're increasing the fluoride in the
system. And, of course, this tank right here, being at the end of the system, shows a much more attenuated effect.

One of the issues we ran into and I believe we resolved -- but this line down here is the flow of water from the ground tank, the Tarawa Terrace ground tank. So if Camp Johnson tank is the controlling tank, when the Tarawa Terrace pumps come on and it's flowing water, we should see changes in the water level in the Camp Johnson tank. And the problem is, I believe, there was some SCADA and/or telemetry issues because Camp Johnson tank is flat-lining. If it's flat-lining, there should be no water flowing from the Tarawa Terrace.

DR. WALSKI: That's not flat. That's about what you'd --

DR. JOHNSON: Microphone. Mike; please.

DR. WALSKI: Okay. It's not going to drop dramatically because it takes a long time. So it dropped 1 or 2 feet --

MR. MASLIA: Over here. This is flat. That's not dropping.

DR. WALSKI: Okay. From there, it's --


DR. WALSKI: There are -- it is --

MR. MASLIA: No. No. No. I'm talking about right
DR. WALSKI: But there's some issues with SCADA, in that just the lag time that SCADA doesn't continuously monitor and you may miss.

MR. MASLIA: Right.

DR. WALSKI: So it's not unlikely to happen, what you see.

MR. MASLIA: Okay. Lack of -- lack of meter data. We were going with the concept of a district metering area. So, in other words, because we did not have -- or we do not have household meters, we were going to meter certain areas and then be able to come up with per capita demand in that area. Sixteen meters have been installed. And we've got eight in the Holcomb Boulevard and Tarawa Terrace area.

So, for example, say, in Berkeley Manor, by knowing the flows and from here, here, and here, we would be able to come up with a per capita estimate or quantity. And this, in fact, there's a paper that just came out in 2004, talking about that. I've got the reference some place. But basically, using this approach and then trying to quantify the stochastic nature of the demand. And that's in the Hadnot Point area, meters in the Hadnot Point area. And that's it, I think. Oh, five minutes early.

DR. JOHNSON: Thank you. Tom.
DR. WALSKI: Well, the question about the metering now, you did full-pipe metering; right? You just tapped whatever size pipe was there?

MR. MASLIA: Yes.

DR. WALSKI: Did you check the model to see what the velocities were at those points?

MR. MASLIA: Actually, we've gotten into that issue, and we have done that now. I mean, we have that now. We've got an upflows. We've got flows. One of the issues that's been run into -- let me just put this up.

One of the issues that we have run into with the flow meters is the calibration process. And our understanding is from the vendor -- of course, these meters are Dynasonics, and they've got plus or minus 2 percent.

And the issue is at what magnitude -- if you calibrate it for a higher flow and then you're actually seeing a predominantly lower flow, you're going to have a much larger error than that. And just the opposite: If you're calibrating it for lower flow conditions and all of a sudden you flow hydrants or whatever, it's not going -- so what we have done, we were just up there in March and based on seeing the attempt for calibration and seeing what we were running into -- and I can pass a couple of these around and just -- if anybody wants a full copy, then we'll just need to run it through our clearance
people.

But this is meter by meter location. And we did use the models as they are right now. They're not calibrated, but we feel they're in the ballpark, in other words. And we did both a table basically giving minimum, average, and maximum simulated flows, pipe diameter and where they are, as well as within each meter giving calibration procedures. And then we also had graphs on some of them. Where hydrants were available, we'd flow that hydrant to change the flow, to check the calibration.

So we also did graphs. So when you go back out into the field to calibrate them, we could know what ranges of flows to expect. You know, basically whether you're looking at flows below 100 gallons per minute or upwards of 600 gallons per minute. So that's where we are with that. We haven't gone back out in the field to do that, but that's the next plan. I'll just pass one around.

(Passing document around)

MR. MASLIA: If the panel would actually like copies --

COURT REPORTER: Mike.

MR. MASLIA: If the panel would like copies, let us know and we'll run it off and get it to you.

DR. WALSKI: Okay. The issue I've run into in these kind of meters is that, typically, the flows in the normal
distribution system are very low because the pipes are sized for fire flow and they're down less than a foot per second and these meters are really lousy at a foot per second.

I mean, no matter what you do, you're going to have a really bad range. Almost -- usually for this type of metering, you've got to go in with a smaller pipe; like if you have a 12-inch line, you put in an 8-inch spool piece or something like that to get the velocity higher so that you get something in a range where it's sensitive because, when you're down less than 1 foot per second, no matter what you do for calibration, they're just lousy for those ranges. So what velocities are you seeing in these pipes?

MR. MASLIA: Claudia, do we have those? We can get those for you.

MS. VALENZUELA: (Off microphone)

MR. MASLIA: Yeah; yeah. If you don't mind showing -- we'll pull that up for you, if that's okay.

DR. CLARK: We had some similar experiences in Cincinnati when we tried.

MR. MASLIA: Are you saying so put them in smaller diameter pipes or...

DR. WALSKI: Well, not so much putting them in smaller diameter pipes, but mike the pipe down. Like, if you have a 12-inch pipe, you don't put in a -- just a
12-inch meter. You put in an 8-inch meter so that the
velocity is higher for a little while and you have -- but
that's a lot more construction cost, unfortunately.

MR. MASLIA: Right.

DR. WALSKI: You want to just tap the pipe.

MR. MASLIA: Yes. They've just been tapped now, and
they've been tapped into a variety of diameter pipes.
I've got the diameters listed.

DR. WALSKI: Yeah. They range from 6 to 12. But in
a 12-inch pipe, to get more than 1 foot per second, you've
got to be really cranking the water through it.

MR. MASLIA: Yeah. In fact, we've got one -- well,
actually that one's not going to be used. We had one in
24-inch pipe, but that one's not being used. There's no
flow in that one. Basically, the majority of them are
12-inch pipes. We've got an 8-inch pipe and then a 16
inch and a 10 inch.

DR. WALSKI: So you need almost -- excuse me. You
need about 500 GPM in a 12-inch pipe to get sensible
velocity.

MR. MASLIA: Yes. Yes. Yes. And --

DR. WALSKI: And in most of the data, you don't have
that.

MR. MASLIA: And we've had to get that by flowing
hydrants.
DR. WALSKI: But then when you measure it, though, the actual flows you're measuring are going to be below --

MR. MASLIA: Right.

DR. WALSKI: -- the sensitivity of the gauge, unfortunately. So it's going to be an issue. So -- it's just going to be an issue when it comes up.

DR. JOHNSON: Please.

DR. UBER: Morris, I've just got a quick kind of a boring clarification question here. I was just looking at some of the hydraulic gradeline elevation in this Table 1. And is this -- this is probably just a typo or something, but the controlling tank in the Camp Johnson tank, which is, I guess, the controlling tank for Tarawa Terrace, that's indicated as having a hydraulic gradeline of 107, roughly. Is that wrong, or...

MR. MASLIA: Which table are you looking at?

DR. UBER: Table 1 of -- in the present day, right after the blue page in mine. The reason why I was asking for -- because I was trying to look at hydraulic gradelines between the different areas and that's -- you know, the controlling tank in Hadnot Point is 160, and in Holcomb Boulevard it's 151, and then this is 107. I can't imagine there's that kind of losses.

MR. MASLIA: Oh, that one.

DR. UBER: I assume that it's a mistake.
MR. MASLIA: No. No.

DR. UBER: No. I guess I just don't understand how it operates then, but -- so -- well, if that's correct, see, there's another. The other tank in Tarawa Terrace, which is just, you know, a little ways away, has a hydraulic gradeline of -- well, 142 plus 32. So, you know, over 170 --

UNIDENTIFIED PANELIST: If you add that to that --

DR. UBER: We can't go from 170 to 107; can we?

DR. WALSKI: That's one of the things I pointed out in my comments too.

DR. UBER: Oh, did you?

DR. WALSKI: It looked inconsistent to me.

DR. UBER: Oh, okay. The only reason why I was asking is that -- I mean, if that were -- I was trying to figure out whether there is any -- any infrastructure information, having not been to this area or anything like that, to indicate likelihood of, if there were interconnections, what might be the possibilities of shipping water between them, you know, sizes of pumps, hydraulic gradeline, you know, that type of thing. And if that were true, that that's a controlling tank, it would seem to be hard to get water out of Tarawa Terrace --

MR. MASLIA: Is that just the tank level or the --

DR. UBER: Well, it's the hydraulic gradeline and the
controlling "controlling tank."

MR. MASLIA: Joel, did you have or Brynn have any --

DR. JOHNSON: Come to a mike, please.

MR. ASHTON: Joel was just telling me -- that was our operator -- that the elevation difference between Tarawa Terrace and the Montford Point or Camp Johnson tank is about 7 feet.

MR. MASLIA: Seven feet.

DR. UBER: Okay. So there's a mistake there.

MR. ASHTON: There must be a mistake there, but there's about 7-foot elevation difference between the two.

DR. UBER: I don't want to belabor the point if it's a mistake. I assumed that it was, but -- okay.

DR. JOHNSON: Well, thank you for the comment. Do you have something else?

DR. WALSKI: Getting back to the graph that Claudia put up on the screen, you're going to have problems with that -- with these meters then. If the velocity is around .1 to .2, you're really down at the very low range of where that meter's good, unfortunately. If that's an average day kind of condition that she's got there, that doesn't bode well for accuracy, unfortunately.

DR. JOHNSON: Okay. Any further points?

DR. CLARK: Just a follow-up that we've even found some cases where we've got negative velocities when we
knew that wasn't the case. So -- yeah -- at those local meters.

DR. JOHNSON: Morris, thank you for your presentation. Why don't you have a seat there at the table? And I would also ask Dr. Bove to join Mr. Maslia at the table. Let us turn to the set of questions and issues that the agency asked that you consider. And there is a revision to this, but the revision is being passed around.

The first question is -- and we've had some substantive discussion on this already, but...

Are the distribution-system tests conducted to date and the one planned for summer 2005 sufficient to provide ATSDR with required data for reliable calibration of present-day models?

Tom, would you like to take a lead on that?

DR. WALSKI: Yeah. It's outstanding. I mean, it's the best data study I've ever seen, probably. And it's probably more than they need for this study because you're not really doing fire-flow analyses. So you don't really need those high-flow tests. So, if anything, it's a little bit of overkill. But they did a great job.

DR. JOHNSON: Other comments from the panel?

DR. CLARK: That was my reaction too, that they're really kind of a state of the art of testing from what

NANCY LEE & ASSOCIATES
I've seen so far.

DR. JOHNSON: Turning to Question 2 then:
Considering the lack of household-consumption data and diurnal-curve characteristics, will applying the "district metering area approach," using the 16 system flow meters, provide adequate and sufficient information to develop per capita consumption data and diurnal-curve characteristics? Are panel members aware of other approaches that could be useful?

DR. WALSKI: Well, the more rudimentary way to do it is just to do a mass balance on the system. You look at flow in, plus or minus changing tank levels, on an hour by hour basis. And that's usually good enough when you don't have submetering because, unfortunately, as I was saying here, the velocities are so low at those points that the accuracy of these gauges aren't going to be that good at those really low velocities. So just the mass-balance approach may be adequate.

MR. MASLIA: Can I ask a qualifying question? Do you not need to then have, you know, reliable SCADA information for that?

DR. WALSKI: Right. Yes.

MR. MASLIA: Okay. And --

DR. WALSKI: And that's --

MR. MASLIA: At least we've been informed that, you

NANCY LEE & ASSOCIATES
know, the SCADA equipment is old at Camp Lejeune. And, you know, at some points in time -- at least some times when we were testing the test, there is some question as to their reliability, that it doesn't have it. So that was one of the issues we had discussed with the folks at Lejeune is -- as to why we decided to go with a metering approach. So -- but you would need the reliable SCADA information then.

DR. WALSKI: Right. The question though is usually it's a lot cheaper to recalibrate the SCADA system than it is to put in all these meters and the vaults and all that. But that's something where I don't know the details. So I couldn't really say.

MR. MASLIA: Okay. I just wanted to clarify that.

MR. HARDING: I think we have to keep in mind the purpose for the estimates of water use. And I'm not completely clear on that. I think in Tarawa Terrace we've decided we probably don't need it, other than to deal with the well cycling. And in this particular circumstance, I -- now, it's referring specifically to the work at Hadnot Point; right?

It isn't clear to me that we're going to -- that a model is required at Hadnot Point if our second objective is to establish an unexposed population. So I think we just need to keep that in mind. But get -- if we do want...
to establish hourly or subdaily water-use characteristics at the water-treatment plant, then I think Tom's right, that it's much easier to measure tank levels and flows at the plant than it is out in the system.

DR. BOVE: Let me just say one thing. We do want to know who was exposed to TCE. So we do want to know not only who's unexposed but who -- how many were exposed to Hadnot Point.

Originally, when we did the earlier study, we had a very small group that we thought were exposed to Hadnot Point. We found an odds ratio of 1.5 for small-for-gestational age, if I remember right. But we would like to also look at trichloroethylene if the numbers are there. And the numbers would be there if we find that some of the Hadnot Point water went to Holcomb Boulevard for any length of time beyond '73 or whatever.

DR. WALSKI: Was there distribution -- or any kind of distribution measurements of TCE at Hadnot Point, or is -- I mean, we talked to Jerry during the break and he says there were well measurements, but were there any distribution measurements of TCE?

DR. BOVE: At Hadnot Point?

UNIDENTIFIED PANELIST: Yeah; at Hadnot Point.

DR. WALSKI: Okay; because I wasn't seeing it in this one list.

NANCY LEE & ASSOCIATES
DR. BOVE: In the old assessment, there were.

DR. WALSKI: Okay.

DR. DOUGHERTY: Let me go back -- the recorder has a question.

COURT REPORTER: Well, the recorder didn't hear what was coming from behind me, and I think it was the answer to one of the questions. So it's not in the record. If you want it in the record, please, identify yourself and get to a microphone.

MS. HOSSOM: Okay.

COURT REPORTER: Thank you.

MS. HOSSOM: Hi, I'm Carole Hossom. I wrote the 1997 Public Health Assessment. And at Hadnot Point, I believe the data shows -- excuse me -- 1400 parts per billion TCE at Hadnot Point.

DR. BOVE: Tap sample?

MS. HOSSOM: Excuse me?

DR. BOVE: Tap sample?

MS. HOSSOM: Tap; drinking-water sample.

DR. WALSKI: Okay. Was that -- so there was one measurement made there historically, or were there --

MS. HOSSOM: No. There were a few, but a handful.

DR. WALSKI: That was the range of numbers because it wasn't on this summary sheet here, and that's why I was asking if we had much.

NANCY LEE & ASSOCIATES
DR. LABOLLE: Okay. Was TCE the principal contaminant there, or was there also PCE?

MS. HOSSOM: For Hadnot Point, TCE was the principle contaminant and degradation products of TCE, not PCE.

MR. HARDING: Okay. While you're there, don't -- because if I recall correctly -- I don't have that open in front of me -- there was also an estimate of vinyl chloride.

MS. HOSSOM: Right.

MR. HARDING: Was that -- was that at any measurement of that, or was that just a calculation based on assumed degradation?

MS. HOSSOM: It was a -- because the laboratory-detection limit was only ten parts per billion, the -- it was estimated at below that to be eight. Although it was not calibrated below ten, it was an estimated measured value.

MR. HARDING: Okay. So it was detected.

MS. HOSSOM: It was detected.

MR. HARDING: But not quantifiable.

MS. HOSSOM: But not quantifiable.

DR. DOUGHERTY: So is that a quantitation limit, you're talking about, and not a detection?

MS. HOSSOM: Correct. It was quantified, but it was below the limit. So that's how it was reported as an estimated detected value as opposed to not detected. Does
that clarify that?

    MR. HARDING:  Uh-huh.

    MS. HOSSOM:  Okay. So Hadnot Point was TCE. Tarawa
Terrace was PCE; majority contaminants and then
degradation products.

    DR. JOHNSON:  Okay. Thanks.

    MS. HOSSOM:  Thank you.

    DR. JOHNSON:  So with regard to Question 2, Morris,
what do you think you have heard?

    MR. MASLIA:  I've forgot to give myself a copy.

    MR. HARDING:  Well, can I --

    DR. JOHNSON:  Please, Ben.

    MR. HARDING:  I'm still not sure we can answer
Question 2 yet because I'm confused again. And forgive
me, but, Dr. Bovey --

    DR. BOVE:  Bove.

    MR. HARDING:  Bove. Sorry. I understand now that,
okay, we're also interested in the TCE exposures in Hadnot
Point, and you talked about also looking for exposures in
Holcomb Boulevard. But it seems to me that -- let me just
see if I can frame this. And I apologize if I get this
garbled. But in doing this analysis, we're going to
compare the exposed populations to an unexposed -- I think
you guys call it -- case control or whatever.

    DR. BOVE:  Just keep with exposed and unexposed.
MR. HARDING: Okay.

DR. BOVE: Because cases and controls are both exposed and unexposed.

MR. HARDING: Okay. So we have to find -- ideally, we'd like to find some populations on the base that were exposed to TCE. We already have established that there's a likelihood, high likelihood, that you can identify populations that were exposed to PCE at Tarawa Terrace. But then we also need to find a population that's unexposed. So that population that's unexposed would potentially be in Holcomb Boulevard during periods when the two weren't interconnected.

DR. BOVE: Right.

MR. HARDING: Okay. Now --

DR. BOVE: But -- but there are interconnections. And that's what I'm concerned about.

MR. HARDING: Well, representing those interconnections is the complicated part of this. So the question I have is -- is that: Can you select your unexposed population from time periods where we're reasonably certain there were no interconnections, where Holcomb Boulevard operated independently of the other water-distribution systems?

DR. BOVE: Well, that's the question, though, I think; isn't it?
MR. HARDING: Well, no. Is it adequate for your purposes? Is what I'm asking. You don't have to -- do you have to -- do you have to have an unexposed population that goes from 1968 to 1985, or can you pick a population that, potentially, let's just say, from 1971 to 1981?

DR. BOVE: No. We have to be able to determine for that whole period who was exposed and who was unexposed. Okay. So -- and if we -- we can misclassify people as exposed or unexposed, but we need to know that.

MR. HARDING: Okay. Well, can we have three groups: exposed with some degree of certainty; unexposed with some degree of certainty; and we don't know, which we put aside? See what I'm saying?

DR. BOVE: See, the design of the study is that you -- we use the whole time period as the -- I mean, the population is all the births during that time period. Okay. We take a sample of all the cases from that time period, and we take a sample of controls. The controls are supposed to give us some reflection of the exposure -- the proportion exposed in that population. That's the purpose of a control series.

We're using that whole time period. So we have cases during that whole time period. We'll have controls during that whole time period. We need to assign exposure properly to those cases and controls. So the -- in the
previous study we didn't do a case-control sample. We can
do what you suggested because we can just -- we took
everybody. So we can decide, all right, we'll just take
this part of the population. But with a case-control
sample, you take a sample of that whole population. You
have to be able to assign exposure for that whole period
of time.

DR. DOUGHERTY: As I recall, the Holcomb Boulevard
came on-line in '73, the treatment plant. Is that --
MR. MASLIA: Between '71 and '73.
DR. DOUGHERTY: Somewhere in that time period.
MR. MASLIA: Yeah.
DR. DOUGHERTY: So -- and then the interconnection
was turned off.
MR. MASLIA: No. We don't know.
DR. DOUGHERTY: We don't know that for sure?
MR. MASLIA: We know --
DR. DOUGHERTY: And we know that --
MR. MASLIA: -- at certain times, we know the
interconnection between Hadnot Point and Holcomb
Boulevard. I believe it's January. There is a date on
the chronology. January of '85, we know there's a period
in there that there was an interconnection because of a
failure of a pump or whatever at Holcomb Boulevard. So
there was an interconnection.
DR. DOUGHERTY: And at the other end, we know that the connection at Tarawa Terrace came in somewhere in the '85 and possibly '84 with a temporary line. Maybe even '83, I think we heard, with a temporary line. So the period prior to 1971, we can say pretty much with certainty that Holcomb Boulevard people received water from Hadnot Point, which makes the classification straightforward. And let's see --

MR. HARDING: Well, you're a groundwater modeler, so you shouldn't be saying that.

DR. DOUGHERTY: No. This is strictly about whether there's a possibility as in a pipeline --

MR. HARDING: Yes.

DR. DOUGHERTY: -- that exists or doesn't exist. And so we can take care of that much of the window. You can fill in the rest of the blanks.

MR. HARDING: Well, but the reason -- I may be belaboring this point. But the reason is is that I'm trying to establish whether there's a way to avoid trying to do the complex and the highly uncertain water-distribution modeling, given the very sparse amount of facts we have about it.

And if -- and I want to put this question to the panel. Maybe you'll tell me to shut up about this. But the level -- we don't need to know a lot about the diurnal
patterns of demand, if we're going to use more of a mass-balance approach to this. And so if we have this period of time -- is it weeks? months? years? -- that we absolutely must include to complete this study, that's a different story than if we can pick the times when we have reasonably good certainty.

If we have to include all of these periods, it's my opinion that we have to be very honest about the very high degree of uncertainty in the periods where we're doing water distribution fate and transport model. So I don't know. I'd like to hear what other people have to say because I've beat this horse pretty hard.

MR. ASHTON: I would just like to clarify one thing.

DR. JOHNSON: Come on up.

MR. ASHTON: There's a little bit of confusion about when the systems were interconnected. After this '72 plant was constructed, unfortunately, the two systems -- Hadnot Point and Holcomb -- they're at different pressures. There are quite a bit of difference in the elevation of the water tanks. So we keep, normally, those belts closed.

The operational procedure now -- and I'm not sure how long this dates back. But we contact the State when we open those valves to get approval for interconnecting the systems. We have two different operating permits for the
systems. And so those systems are separate, and they're kept separate.

The line that we were talking about yesterday between the Holcomb system and the Tarawa Terrace system, that is something that has been confused, and we're in the process of clarifying it with both the construction drawings that we have to install the lines and also the operators that are familiar with the system. And we'll clarify that for you very soon, and that's what we're working on right now. But the people aren't here that have that information. But we feel it's in our construction drawings.

DR. BOVE: Would there be a record of every time you connected Holcomb Boulevard and Hadnot Point then?

MR. ASHTON: That's unfortunate. I don't believe there is unless the State has --

DR. BOVE: But if you record -- that's what I mean.

MR. ASHTON: -- unless the State has a record, which they might.

DR. BOVE: Okay.

MR. ASHTON: And I have no way of knowing what they have. But we'll try to find that out. We've got a request -- there's been some turnover at the State. We have a request through the State to try to get -- see what records they do and don't have, so...

MR. HARDING: Is there a distinct grade difference

NANCY LEE & ASSOCIATES
between the three systems, and if so, can you say 
nominally what the -- what grades they were, what grades 
they ran at?

MR. ASHTON: Yeah. I don't have the exact 
information of the difference in the elevation between the 
Hadnot Point and the Holcomb.

MR. HARDING: Which one was higher?

MR. ASHTON: Okay. I believe -- I believe the newer 
system is higher, if I'm not mistaken.

MR. HARDING: The newer being Holcomb?

MR. ASHTON: Meaning the Holcomb system, I believe. 
But I can verify that. The -- as Joel says, he wasn't 
sure which system -- the tanks, of course, were not -- 
there were quite a bit of difference in the tank levels. 
And, of course, we try to keep our tanks full for fire-
protection purposes, and that is the reason why that valve 
is normally closed and we have two separate systems.

DR. UBER: Just on that point, the data in that same 
Table 1 shows a 9-foot grade difference from -- actually, 
contrary to what you said from Holcomb -- I'm sorry, from 
Hadnot Point to Holcomb in that direction for the 
controlling tanks.

MR. ASHTON: So you're saying that the Holcomb plant 
is lower, you're saying?

DR. UBER: That's what -- just -- that's just what
this data in the table shows --

MR. ASHTON: And I'll verify that.

DR. UBER: -- by 9 feet.

MR. ASHTON: That's -- that's -- we have records of all the differences in elevations. The guys who -- the guys that had that plan, he'd have it off the top of his head, but I don't, unfortunately.

DR. UBER: Yeah. I mean, that would be -- that's, of course, quite useful information to know in terms of interconnectedness. So that would be -- that would be good.

MR. ASHTON: The USGS has took with them all of the elevations. So we have all that information.

MR. MASLIA: We have land-surface elevations at the tanks.

DR. JOHNSON: Ben had put on the table sort of a request for reaction to a proposal. I didn't hear much reaction. Did I miss something?

DR. CLARK: I can give you my answer to Question 2, and I think the answer's yes. I think it's probably the best way you can go about it to develop diurnal patterns using this district metering approach, given the fact that you don't have other data available.

DR. JOHNSON: Okay. Let's move on to Question 3.

MR. MASLIA: Can I ask a question?
DR. JOHNSON: Sure; of course.

MR. MASLIA: And it's sort of encompassing early --
both days, and it's more of a, I guess, philosophical one.
But I'll ask it anyway.

We acknowledge, both on the epidemiologic side as
well as the modeling side, that there's a great deal of
uncertainty. But what I'm hearing is -- or what I'm
interpreting is that perhaps we should just throw our
hands up even if we quantify it or make a gross
assumption, very simplifying assumption, and that is, not
degrading that approach. That may be a valid approach.
But then the agency still has other parties to answer to.

And so my question is: How does the agency go about
saying -- do we go about saying that this is the best we
can do and we can refine it no further, or do we -- that's
what I'm trying to clarify.

DR. WALSKI: Here's what I was going to suggest later
on --

MR. MASLIA: Yeah.

DR. WALSKI: -- but since you brought it up now, I
might as well talk about. It seems like -- my approach
would be is to take what you've got now and say, "Okay.
We know these people were exposed. We know these weren't.
We're not sure about these." And in about six months use
the model as best I can -- in about six months, study,
write your report, and say that we could spend another two or three years on this and we can refine the numbers a little bit.

But, unless you have some hope that two or three years of more work is really going to make the numbers better, I think, you know, wrapping up the modeling part of this in a short time and saying this is -- this is -- call it interim report to cover yourself. But say, "You know, we can -- you know, in a couple of months we can wrap this thing up, give you a good answer, and maybe we can get it 2 percent better if we spend another three years on it or something" is the way, I think, it's going to all play out is my prediction. And I could be totally wrong in it. You probably have some people...

DR. JOHNSON: And my opinion is: Someone who doesn't know much about this whole area of work, they're -- one of the parties you have to be concerned about is the scientific community. And I always found it very useful to try to anchor on those data that you had confidence in. And things that might rise to the level of speculation you discard, unless there's some really good reason for doing otherwise.

And so your response to those other parties who may want you to do God-awful things that may surpass your ability to do, you simply have to say that that's not
possible. The science just doesn't take us that far. And we are going to base our work, whether it's in the area of water modeling or epidemiology, on the most reliable data in which we have confidence. And that's as far as we can go. That's as far as the science will let us -- take us.

DR. BOVE: Well, I still think there's a lot of work that could be done to get other data that's available, both records from the state, if necessary, or other memos and material that might give us a sense of how -- whether these systems were in -- used in an interconnected fashion.

And so I think that that's, more than modeling, is what I would push for. It's a lot more of getting that information from the vault that would help us clarify some of these questions.

DR. JOHNSON: That may be true, but you have to ask the question of: Well, what's it worth? And what am I willing to invest to go beyond what I have now with which I have some confidence? And as Tom characterized it earlier this morning, you're getting into perhaps the area of archaeology and that's -- may be quite appropriate. Do you -- I think you have to do something akin to kind of a cost-benefit effort to determine if it's worth it.

MR. HARDING: I would say that along those lines that the question can be framed as: Where do you want to spend
your resources? And let me first respond to Morris and say, if you're interpreting what I was saying, I'm not saying throw up your hands at all.

What I'm -- what I'm advising is essentially the same thing here is that we ought to ask ourselves: Where can we get the most bang for our buck? And if I had to say right now where that is, it's in trying to refine the understanding of when the contaminants reached the wells at Tarawa Terrace and then -- I don't think we have much of an understanding about what happened at the wells at Hadnot Point if we're looking for exposures to TCE. So those are two areas where more emphasis could be put than on the water-distribution modeling.

And then when we get back to this issue of Holcomb Boulevard, the purpose of the Holcomb Boulevard analysis is to establish unexposed populations. And I think that you have to ask yourself: If we've got these sporadic and poorly defined periods where there was potentially some contamination in that system, think about whether you can exclude those periods from your analysis as a way of saving a huge amount of effort that can be spent better, to my way of thinking, in trying to reconstruct, for example, what happened at Hadnot Point in the groundwater.

So that's my take on it, and that's why I've been asking these questions now because I'm not sure that the
-- if you want to do diurnal-demand reconstruction, there's various ways to do it. But I'm not sure whether you need to or not. That's my point. That's why I was having trouble answering the question.

DR. WALSKI: The impact of your suggestion is (off microphone).

DR. BOVE: We would lose some cases in a situation where we already have a small number of cases, and we would have to take a new sample of controls to fit the new population we're talking about. We've already sampled control, sent them to the vendor. We could -- and the process of interviewing will start, as you heard, next week. But that could be put on hold.

But my problem with this is that we don't know. I mean, we can -- I guess we can -- I mean, we don't know when the interconnections could have occurred, I mean, you know, the water flowing back and forth. So when would you say -- what groups of people, what periods of time should we exclude from our study?

MR. HARDING: Well, let me put the question another way. If you don't know when the interconnections occurred, how are you going to model them? I think you just have to bite this bullet. And you have to -- here is our best determination of when these systems were -- you have to do this no matter what. You have to say when were
they connected and when were they unconnected. And what I'm saying is: Once you've made that determination, don't take the effort to model the interconnections.

DR. BOVE: No. Right. And I'd like to get these records from the state, if they exist.

MR. HARDING: Well, I think that's -- I think that's a good way to spend your money, and I think that doing the archeology in a case like this may well be warranted to figure out what happened. But once you've figured that out, then -- then really you've got to ask the question: Is it worth spending an enormous amount of energy to model these relatively short periods at the expense of doing what I think is more important?

And here, I'm speaking here as a ground -- or as a water-distribution person. But I think that the groundwater case at Hadnot Point is -- am I missing something, or do we know anything about the historical pattern of contamination at Hadnot Point?

DR. WALSKI: I think one of the things we talked about yesterday was, it's so complex that we really can't model it though. We kind of threw up our hands on that one and said, "We know there was contamination, and we know well-monitoring points, but there are so many sources there --

UNIDENTIFIED PANELIST: Yeah. Didn't I hear
160-something?

DR. WALSKI: -- but I can't tell exactly which source went to which well.

DR. DOUGHERTY: That was the limit of information that we've had to review. So the answer is: I don't know. We may have some kind of generalizations.

DR. BOVE: I mean, we've been asked to determine when contamination arrived at Hadnot Point too. I mean, this is -- this was our charge early on. So forget the study for a minute. We were asked that question. And there are people out there who want to know the answer to that. And I don't know if we can provide that, if that's what you're saying, because of the multisources, and we don't have that information on those sources.

DR. JOHNSON: I think Jerry has a point to share, please.

MR. ENSMINGER: As far as the actual contamination of the Hadnot Point water system, you have earlier recorded data at the Hadnot Point system, actual analytical data, than you do at the Tarawa Terrace system. You have a report of October of 1980 from the Army hygienic team that came in there to do the preliminary test for TTHMs that identified chlorinated hydrocarbons in their water, extremely high levels.

And behind that, in parenthesis, he wrote "solvents."
And they had several tests. They didn't find the hydrocarbons in Tarawa Terrace until 1982. But you do have analytical data which shows the actual contamination of the Hadnot Point system in 1980 prior to Tarawa Terrace.

DR. BOVE: But we don't have it before that, and that's --

DR. JOHNSON: Speak in the mike, please.

DR. BOVE: What we're trying to find out, though, is when the contamination first arrived. I mean, that -- that's going to be the difficulty.

MR. ENSMINGER: Well, the hottest well in Tarawa -- or at Hadnot Point was Well 651. We do have the historical data as to when that well was constructed, and it was 1972. And it was constructed at the back corner of the disposal lot, which had been in operation for some 30-odd years at that time. And when it was tested, it was 27,000 parts per billion of VOCs. I mean, it's not hard to figure out that that well was contaminated the day it was sunk.

DR. JOHNSON: Okay. Thank you very much.

DR. WALSKI: But you don't need a model to prove that though. I think that's the point. We can do that without doing sophisticated modeling for that.

DR. CLARK: Frank, what do you think the potential is
for getting more data from the state that may be better to find exposures in the system? Does anyone know that?

Does anyone know that?

MR. MASLIA: Early on, we -- Bob Faye and I went up to Raleigh to look through the historical records, and in the historical records, we found some information for the forties, fifties, sixties, and then nothing after 1969 until the 1990s. There's not a single sheet anywhere.

DR. JOHNSON: Okay. I want to move on to Question 3. Is ATSDR's approach of developing three water-distribution system models appropriate to address answers needed for the epi study?

DR. CLARK: I think it is.

DR. JOHNSON: Lord love you for that. Thank you for that answer.

MR. HARDING: I don't think, based on what I know, that it makes sense to develop models for these systems. That's based on what I know right now is, that in the sense of using a modeling code -- I mean, all of what we're going to be doing is modeling. But a simple mixing model, I think, is appropriate.

The time when you would need to do something more sophisticated is during these periods of interconnection, which we can't even define and potentially will never be able to define. So based on that, I think that, yes,
three models are appropriate. But they aren't -- they don't need to be a fully sophisticated hydraulic water distribution fate and transport model.

DR. JOHNSON: Well, that's a substantive comment. How does the rest of the panel feel?

DR. LABOLLE: I thought I heard something previously regarding the need to go back historically, in a related study or as part of this study or an extended part of this study, and look at cancer risks. And in that context, I think, I see that the Hadnot Point was connected with the Holcomb Boulevard system during the period that you had mentioned.

And if that's the case, possibly in those -- you know, those subtime periods there where there's the interconnection is here, employing. But other than those periods, I tend to concur from what I've heard here that the sophistication in the models may be sufficient at this point to answer some of the questions.

DR. CLARK: I think the sophistication should be at a level that you can create some typical diurnal-exposure curves. That's my opinion.

DR. WAlSKI: Mine is that it's probably not worth the effort, given the amount of data we have here. We'll disagree to --

DR. CLARK: We'll disagree on that.
DR. JOHNSON: James.

DR. UBER: I think that -- so first of all, the issue of interconnectiveness is different from the issue of understanding temporal -- or diurnal variation in concentration. What I'm hearing and what I would agree with is that more archaeology on the interconnectiveness should precede further refinement of the water-distribution system models.

I think that if you found through the archaeology that the interconnections were frequent and of long duration that that would be different from finding out that, you know, there was never any period when Holcomb was putting out less than one MGD. And therefore, from a simple flow balance, you cannot have had significant contribution of water in that area from another system, you know.

So I think that -- I think that the effort needs to be driven by those kinds of factors. I frankly don't think that the information is on the table right now to know -- to answer that question.

DR. KONIKOW: The distribution model -- in terms of when the interconnection was opened, I'm assuming that that connection was not the only source of water to Holcomb Boulevard, or was it? Because if it wasn't, then the distribution model could help refine which
neighborhoods or sections received water from Hadnot Point versus which did not. And that might be very useful.

MR. MASLIA: Right.

MR. HARDING: I might add that the point Jim made earlier on the elevations of these tanks will prove to be critical in that assessment because, if our goal is to isolate the Holcomb Boulevard population, if that ran at a higher grade than Hadnot Point, then we've got the answer. But it isn't clear at this point.

DR. LABOLLE: I think, also, it's important to keep in mind that when you're all done and you're refining, for example, these diurnal curves that the source concentrations to these systems are going to vary over orders of magnitude potentially. And potentially -- and I say "vary in time" -- the actual source may have.

And the uncertainty is potentially an order of magnitude or more, two orders of magnitude, in these concentrations at the wells. And that's due to both geologic uncertainty and uncertainty in the source concentrations, as David has brought up, so...

DR. JOHNSON: So what have you heard, sir?

MR. MASLIA: Well, I go on vacation in about six months. No. The -- I mean, we're still -- we're still talking about two major issues. One is data discovery, and the other, again, is basically using simplified mixing
models.

DR. JOHNSON: I heard a rather strong endorsement that the "archaeology" should be, maybe, pushed before other things -- pushed ahead before other things.

DR. CLARK: Is archaeology the same thing as data discovery?

UNIDENTIFIED PANELIST: I would agree with that; yeah.

DR. JOHNSON: Yes, I think it is. Turning to Question 4: Based on information provided by ATSDR -- to ATSDR by U.S. Marine Corps, pipelines connecting to Hadnot Point water-treatment plant service area with the Holcomb Boulevard water-treatment plant service area were opened for emergency purposes only.

Does the panel agree with the ATSDR approach that, because of this characteristic, these two areas can be and should be modeled as two separate water-distribution systems?

DR. UBER: The answer to that is easy. That's -- if we answer yes to that, then -- then that -- then we don't need to do the archaeology, and we probably don't need to do the distribution-system modeling with -- you know, I know that Bob feels differently. So I would say that -- I would say that the answer to that is that you have to do -- I haven't seen the archeology to support saying yes to
MR. HARDING: The answer is: Challenge the predicate.

DR. UBER: Yeah.

DR. JOHNSON: Excuse me?

MR. HARDING: Challenge the predicate.

DR. JOHNSON: Challenge the predicate. Do others wish to weigh in on this? Peter?

DR. POMMERENK: I can just agree with the previous two speakers. If, for example, during main breaks, those valves were open to supply, you know, a portion of either system and we can -- certain windows occurred and how long, you know, the question would be then: Is that of significance for the epi study, if it's just a one-day interconnection or not.

And, you know, if it's not, then, yeah, there is two separate systems, and we -- I agree you won't need the sophistication of the water-distribution system modeling that is conducted right now.

DR. JOHNSON: Anyone else? I gather this is ATSDR's preferred direction: to consider them as two separate systems; is that right?

DR. BOVE: Not if it's not true, it isn't.

DR. JOHNSON: I don't think that was part of my observation.

DR. BOVE: Sure, that would be the easiest thing.
DR. JOHNSON: What will you need to know in order to make that decision?

DR. BOVE: Well, if it was just one day, you know, we probably wouldn't have to worry about it. But if it was for months at a time that the water was flowing from Hadnot Point to Holcomb Boulevard, then we need to know that. I mean, I don't --

DR. JOHNSON: I understand. Okay. Question 6: An innovative approach for fire-flow testing was employed at Camp Lejeune, using continuous recording pressure monitors simultaneously at several fire hydrants while different combinations of hydrants were flowed. Is this approach technically sound and beneficial? Ben.

MR. HARDING: It seems sound to me. It's better than anything I've seen. So Tom's gone into a moment here. But it's a really interesting approach, and it seemed to work real well.

DR. POMMERENK: We've done a similar approach at a different military base where we had continuous pressure recorders, and it works very well. And I'm glad to see that employed in this study as well.

MR. HARDING: I would make this point, that in terms of calibrating the model you do need to have good data on the tank elevations. And so if you've had doubt about the SCADA system, those ought to be resolved because that's

NANCY LEE & ASSOCIATES
the other boundary condition you need.

DR. CLARK: Did you skip the question on: Should ATSDR consider using probabilistic analyses deliberately, or was that --

MR. MASLIA: I think -- I mean, we answered that. I don't have an extra copy of the sheet I handed out, but is that grayed out?

DR. JOHNSON: Oh, that was my oversight, to be blatantly honest with you. And you can write that off to early dementia. And we will return to that. I thank you for making that observation. Eric, do you have a comment?

DR. LABOLLE: No. It was the same comment about the earlier question.

DR. JOHNSON: Well, with my apologies, let us return then to that previous question: Should ATSDR consider using probabilistic analyses to assess the variability and uncertainty of, one, water distribution-system model parameters; two, nodal demands; and three, system operations? If so, what specific methodologies would the panel suggest or recommend?

MR. HARDING: Well, the answer in my mind is, to the general question of using probabilistic analysis is, yes. We had significant discussions about what needs to be represented here in a simulation.

And -- but what does get represented should be
represented as uncertain variables in a probabilistic
framework, and the most commonly accepted and readily
accessible technique for that is Monte Carlo simulation of
one sort or another.

So I think that ATSDR should not just consider using
probabilistic analysis. They should do that, and they
should frame the resulting intakes -- what I call intakes,
body intakes, of these materials in an empirical or
calculated set of credibility ranges based -- you know,
with probabilities assigned to them. That's my view.

DR. CLARK: I think that it would be great if they
can do that; yes. One technique that they might look at
is the PRP approach that Steve Buchberger is using at the
University of Cincinnati for individual household use and
-- which I think fits your -- within the framework that
you're talking about.

MR. MASLIA: Bob, would that not then require us to
have flow information?

DR. CLARK: You'd have to make some estimates about
individual household use; right. But you could aggregate
those into demands or metered demands.

MR. MASLIA: Well, I'm saying, but we would need some
metered information then.

DR. CLARK: If you had your -- going back to the idea
that you have the metered district approach.
MR. MASLIA: Well, that's the question because at least -- I may be jumping the gun, Dr. Johnson, as to what I'm hearing. But I'll go ahead and take another opportunity. What I'm hearing is that we should not proceed any further with the flow meters because of issues. Tom --

DR. CLARK: I think you should, so...

MR. MASLIA: Oh, okay. That's what -- I want to make sure we get that out and get a clarification on that. Could we have the panel address that issue? Just to give you the status, they're in the ground. Okay. They're operating. They're not calibrated, so...

DR. WALSKI: Well, I think the real source of uncertainty though is the well data. So if I was going to do a Monte Carlo simulation of this, I would not use demands of the houses as my undetermined variable or my C-factors at my variables that I would do statistics on.

I would use which wells are firing at which time. That's the one that I would treat as the stochastic variable because that's the one that's going to have the greatest impact on it is which well.

So you say, "Okay. We roll the dice, and this is the pattern of wells we're going operate, and we roll the dice again and see this pattern." Because I think that's the one that's going to cause the greatest variability in the
results. We do have some data from the installation-
restoration reports and things like that as to well
concentrations.

You know, which wells were on at which time are going
to make the real issue and not which house showered at
this time versus which house flushed their toilet at that
time. It's not going to be what's going to drive TCE.

DR. UBER: I think that this question is connected,
in an obvious way, to all of the other ones, as far as I
can tell, that we've talked about. The only other comment
that I'd have to add is I would be -- I would be all for
doing things probabilistically, assuming that they can be
framed in a way that ends up being meaningful.

And my only problem with this is that I think it's
basically tantamount to rolling back stochastic hydrology
before it existed and just saying, "Should we invent this
over the next two years?"

And I don't -- I don't think that you're starting
from ground zero. I think you have things like, you know,
Buchberger's PRP model and stuff like that. But you have
really no -- you have no existing theory of any weight to
-- with which to say roughnesses are spatially correlated
or demands are -- how -- what their spatial, temporal
distribution looks like. And so I think that, you know,
you could get in trouble there by trying to do that.
DR. LABOLLE: My experience has been that the
geologic uncertainty in the context of the Monte Carlo is
going to swamp out everything else. And that simply just
translates directly into the arrival curves at these
wells, which the sources to these systems. And as I've
mentioned several times, you know, can you tolerate a
couple of orders of magnitude, variability due to
uncertainty in those curves?

Because when you start Monte Carlo-ing geologic
uncertainty, that may be what you find out is the outcome.
And so in my experience, though, it's going to swamp out
other things. That may or may not be the case if you're
actually seeing the exposed and unexposed population
change based on roughness or something -- something of
that sort, depending upon where these interconnections
occur.

DR. BOVE: But -- see how I can phrase this. The
variability you're talking about, it's not a daily
variability. It's not a weekly variability. It's a much
larger time frame.

DR. LABOLLE: Well, we have -- you have two things:
variability and uncertainty. The variability in the
geology, it's spatial variability; and the geometry, the
hydraulic conductivities -- however you want to frame the
geologic characterization. But it's heterogeneity
Then we have uncertainty. What is that? All you have are samples at a few points in space out there. And in particular, this TT-26 at Tarawa Terrace, which appears to be the main source of contamination potentially, although that's uncertain too at this point. And the source location are two points which have been characterized somewhat, I guess, as the source by monitoring well data in Tarawa Terrace by some log there.

But there's, for the most part, subsurface is not sampled. And so all that -- all that material that fills in these points, there's uncertainty there. And it's not layer cake, as the models represented. At least, it's not likely to be. Those are simplifications made for modeling purposes, and that -- the uncertainty in that, if one were to pursue modeling that, one would find, likely find, that that uncertainty would translate to a great deal of uncertainty in the arrival curves, and modeling that uncertainty is a different level of modeling than what's been proposed thus far, than what I've heard. It's not simply twisting the parameters in the existing model.

It could be. I mean, you could approach it that way, but there would also have to be a great deal of spatial refinement in the vertical, potentially in the horizontal, and then the way in which we change those parameters.
We'd have to have some kind of geologic and context and probabilistic context related to the geology and its characterization.

MR. HARDING: I would like to really agree and support the opinions of both Eric and Tom, that the groundwater uncertainty is going to swamp everything else. And then it's the well operation that determines the introduction of the contaminants into the system. So it seems to me these are the two most important factors and that the -- we have to deal with the issue of interconnection and whether you're going to address that, but even so, those are the two most important things. And those should -- and they're really uncertain, so they need to be expressed in probabilistic terms.

DR. JOHNSON: Okay. Are there any other comments on this? Let's finish with Question 7. Is it feasible or necessary for ATSDR to simulate the complete 18-year historical period on a continuous basis? And in red, pink here, Ben, for your -- will monthly --

MR. HARDING: I can see it.

DR. JOHNSON: Just was trying to be helpful. So how do we answer that? Tom.

DR. WALSKI: You don't need distribution modeling on a continuous basis. I mean, it's nice if you want to do it, but I just don't see it as being that important
because essentially we don't have a good way to determine
which wells are operating at which times. So, you know,
why beat the -- this dislinear to death just because we
have nice models that'll solve it?

DR. CLARK: I agree with you, Tom (laughter).

DR. UBER: Our colleagues agree.

UNIDENTIFIED PANELIST: Even monthly simulations are
going to be tough, but I suspect that's what --

DR. LABOLLE: I would like to add something since I
had presented premeeting comments and suggested maybe
averaging exposure over the month would require continuous
modeling because that was my experience in another
modeling effort in which I was involved. But in that
modeling effort, we had multiple entries into the
distribution system, and at the time, I was thinking along
those lines. But this system with the single point of
entry during much of the time periods of interest here, I
don't think it's going to get you much.

MR. HARDING: I want to say that I think the ATSDR
should try to calculate the potential exposures on a
continuous time-series basis, whatever that time step is.
Now, as I've probably said a hundred times here, I don't
believe that in almost every case that requires water
distribution fate and transport modeling, but I think you
should try to reconstruct to your best estimate,
basically, a set of probability, just empirical probability distributions, for the breakthrough curves for the model and for the contaminants that enter the system so that you have a time series that you can then correlate to the activities of the individuals. But that probably doesn't require what we term water-distribution modeling. It does require calculations that are really modeling, but it isn't using a modeling code, continuously or otherwise.

DR. LABOLLE: I don't recommend monthly time stepping in a fate and transport model for the groundwater as an input to your system. I think that's going to end up being a much smaller time scale than the information that's available, simply due to constraints and the way in which these models are run to get a numerically valid result. And that's going to give you something, curves, out of these models that are on a temporal scale, which is much finer than a -- it's probably going to be fractions of day, and that's the kind of output you're going to see from there.

DR. JOHNSON: This completes these questions. Morris, Frank, anything else you'd like to put before the panel in the spirit of this kind of specific questioning?

MR. MASLIA: I'm still unclear on the flow-meter issue. It's a critical issue for the agency and the Marine Corps, and it may be that the panel has differences
of opinion, which is fine. But I think for the record we really need -- if there's any way --

DR. JOHNSON: You want some clarity as --

MR. MASLIA: Yes.

DR. JOHNSON: -- to position.

MR. MASLIA: Position; yes.

DR. JOHNSON: Tom, do you want to start?

DR. WALSKI: Well, if you've installed them already, I would try and get them calibrated and see what I could learn from them. But I wouldn't -- the ultimate impact of that on the final bottom line of the study is going to be really small. It's not -- you know, the fact is though that, you know, it doesn't hurt to know that. But I wouldn't really spend a huge amount of resources on it. You know, try to get them calibrated because, looking at what Peter just showed me, the threshold on those things is like 2.2 feet per second. And most of the time, you're below 2.2 feet per second, so it's questionable whether you're going to get good data out of those things.

DR. JOHNSON: So why do it?

DR. WALSKI: Well, it's in there, so try it.

DR. JOHNSON: No. That's not a reason. Tom, that's not a reason. Why do it if it's not going to give you anything of use?

DR. CLARK: I think it -- I'm a little more
optimistic than Tom in terms of what data you're going to get out of it. I think that plus the flow balancing of the tanks using SCADA data would probably give you a pretty good estimate as to what the demands are in those zones.

DR. JOHNSON: Peter, yes or no?

DR. POMMERENK: Well, I'm not quite sure whether, you know, any background noise, electrical noise, at those low flows will really be able to help us detect significant flows in those oversized mains; that somebody indicated earlier they're oversized for five of those. So, yeah, the question is: Are we going to get any useful data out of it? So if we have to open hydrants to perform the calibration, that is -- it's fine, okay to calibrate it, but in reality, this is not the flow that we usually see. So my expectation is that there may be no useful data coming out of that.

DR. JOHNSON: David, do you want to weigh in on this issue?

DR. DOUGHERTY: No (laughter).

DR. JOHNSON: Okay. That's a very fair response.

Lenny.

DR. SINGH: I think it may be --

DR. JOHNSON: Please.

DR. SINGH: -- it may be opportunity to ask Morris as
to his experience so far with regard to metering the flow.

MR. MASLIA: The -- it goes back -- one of -- the concept of installing a flow-measuring device goes back because of the inconsistency in the SCADA data originally and trying to get at two things: getting a total flow, which you can sum up from the different locations; and at the same time, while you're getting a total flow, you can also do the area, area-type analysis.

One of the issues we ran into is that we've got a report, the conservation study, which admittedly is taken from a water-budget standpoint -- but showed approximately a 30 percent difference in water going in and coming out. Of course, you can just allocate that. You know, one method is just distribute that equally every place. That may or may not be accurate.

So that was another factor, in that we've got a documented approach that summed up water use and was plus or minus 30 percent. From that standpoint -- that was not acceptable from an epidemiologic standpoint. So those two factors taken in combination led us to suggest that by installing flow meters we could accomplish two things at one time.

We would have -- we would be able to quantify by summing up the various flow meters production versus flow, and then really establish is that 30 percent difference
reality, or was that just a method or a consequence of the method that was used, the inaccuracy in that first method? And at the second time -- at the second point also be able to, at that point in time, determine areas, specifically family-housing areas, due to the absence of individual house meters.

At this point in time, as I said, the meters are in. The modeling that we've done to date -- and I'm saying this so you can understand because the comments about the low flow are an issue. We had -- when we did the test last May at Hadnot Point, we had -- I won't say significantly -- we had larger, larger flows. And that model to date, the present day, is probably the best of all three.

The subsequent models for Holcomb Boulevard and Tarawa Terrace, we've attempted to do the calibration based on flow information in levels this fall and winter. And that's, of course, when we've been trying to install or calibrate these meters during a period, which admittedly is a -- even based historically is extremely low, low-demand conditions.

Our attempt or our plan was to have them calibrated in sufficient time so for the peak-demand season, then you would have the higher flows. We're still aiming for that, and that's why I needed some feedback from the panel is
that all our attempts to date have been trying to
calibrate them under exceedingly low-demand conditions.

DR. POMMERENK: One question: Have -- based on your
preliminary data collection, can you tell anything about
the accuracy of those meters, whatever you've measured so
far? Or have you collected any data and compared it with
-- you know, Claudia showed us that graph earlier about
one location. Could you compare, I mean, instantaneous
flow rates and maybe cumulative flow rates?

MR. MASLIA: Well, that's why we prepared the --

DR. POMMERENK: Okay. That was passed on.

MR. MASLIA: Yeah, it was passed on. But the concept
behind that -- so that when we're in the field, we
prepared a minimum, a maximum, and an average, then we
would be able to see immediately -- we have not had that
before -- you know, if the flow meters were somewhere in
between those range of flows. We'd be okay. We'd go
ahead with the calibration.

On one meter, as it turned out -- this was on the
24-inch pipe -- obviously, there's no flow. It turns out
to be a by-pass or a pipe to balance some tanks. And of
course, we're not -- you know, we're pulling the meter and
not using the meter there. As it turned out, that was not
a useful location.

But we do have some preliminary information based on
the model simulation, which we're hoping would guide the
calibration process. However, what we -- what we have
seen is if you assume the meters have been calibrated and
we come up for QAQC, when we do flow a hydrant, you know,
increase the flow from up to, you know, 600, 800 gallons a
minute, there's a substantial difference in what the
meter's recording and what we're flowing.

What they have done for the calibration process, just
so everyone's clear, is they go down into the manhole and
strap an ultrasonic, a trans, which is plus or minus 1
percent. And then you read the Dynasonic, which is
supposed to be plus or minus 2 percent, so we figure, you
know, they should be within a few gallons per minute of
each other, and they're not.

DR. JOHNSON: Does anyone else wish to comment?
Peter.

DR. POMMERENK: Just one more question: You mentioned
the 30 percent difference between a water-conservation
study results and water-production records.

MR. MASLIA: That is -- that is correct. And that's
not a critique of the study. I'm just giving you --

DR. POMMERENK: No. I'm just wondering: What do you
attribute these 30 percent discrepancy to? Is that -- is
that mis -- over- or underestimating household demands or
commercial demands, or is that actually just an estimation
issue so you're not assuming it's leakage, or --

MR. MASLIA: Well, no; no. I'm not assuming it's leakage. It's both the -- what I attribute it to is that methodology is a water budget, adding up, you know, lavatory, sinks, showers, and things of that nature and coming up that way. I don't believe -- it may be a small amount of leakage, but I don't have any knowledge on that so I attribute part of it, at least, to the -- to that methodology.

I don't know if that's a standard, acceptable amount of difference or not. And in the other -- and so what we wanted to, again, determine with the flow meters is we've got on one hand the total production or total delivered water at the plant. Okay? So that's what -- and that was our only other number. So even in the models that we have right now -- for example, Hadnot Point or whatever, you've still got this if you use the water-conservation study.

That's how we spatially distributed building use and all that type of use per building and all that. And we've got a 30 percent difference. We can evenly distribute it or not, and that's another -- again, what we were hoping to obtain with the flow-meter information is a more quantifiable estimate or even areas where you have better estimates than other areas.

DR. JOHNSON: Yes, Peter.
DR. POMMERENK: I'll let Tom go ahead for a while.

DR. WALSKI: Well, first of all, I'm assuming that when you measure the discrepancy the production was higher than the estimated consumption; right? Your estimate was production was up here and what the method says was down here; right?

MR. MASLIA: Yes.

DR. WALSKI: So it was higher. The production was higher. So, yeah, it is likely that there is leakage to that extent. And also, they're thinking about these methodologies that you're using that are based on typical, average customers. And one thing that you learn is that you never have a typical, average system. So that type of discrepancy is not, you know, anything that would alarm me.

You know, they say, "30 percent. My God. That's a lot." But no. It's not really. It's not that bad.

DR. JOHNSON: Peter.

DR. POMMERENK: Yeah; just the other issue. You mentioned you were waiting for higher demands during the summer for doing additional validation of the metering data or --

MR. MASLIA: What we were -- what we -- and we're still anticipating to cal -- we're trying to calibrate the meters during this period -- winter, early winter, fall,
winter -- in anticipation of collecting about six months of metering data to be able to capture the high-demand period.

DR. POMMERENK: But have you -- has your review of past production data indicated that there is substantial -- a substantially higher demand during the summer months?

MR. MASLIA: Yes; yes. The USGS reports show that.

DR. POMMERENK: Okay. I'm just asking the question. We have recently completed a related study, and my recollection -- and I may be wrong -- we didn't really see a pronounced summer. I'm willing to share that data with you, so...

MR. FAYE: It's a difference of -- it's how you define "substantial."

DR. POMMERENK: Okay.

MR. FAYE: But I'm looking -- I have the reports with me; unfortunately, not exactly here in the room. But off the top of my head, I'm looking at -- I'm thinking of perhaps a 20, maybe 25 percent difference between, say, a demand from January through March versus, say, June through September.

DR. POMMERENK: Okay. I would think substantial is if you're maxed is a factor of two or three over the average annual demand, daily demand. So you don't quite see --
MR. FAYE: No.

DR. POMMERENK: -- those.

MR. FAYE: No.

DR. POMMERENK: Okay. With respect to the flow metering, obviously, the increases in flow during the summer are not expected to increase that much; right?

MR. FAYE: Maybe I can invent a different way of saying it, but the average daily demand, for example, during the period -- and this is basewide; basewide, not selective to Holcomb Boulevard or Tarawa Terrace or whatever. The average daily demand during July and August would perhaps be 25 percent higher, greater, more than, the average daily demand during January through February -- January through March. Okay?

DR. POMMERENK: Thanks.

DR. JOHNSON: In summary then, is it fair to say that there -- that some panelists have some concerns about the flow-meter work and would suggest, given limited resources, particularly personnel, that ATSDR look at this in terms of, in effect, what the cost/benefit is? Is this data worth what it's going to cost you to get? Is that a fair statement? Should it be changed? Morris is looking, I think, for a rather clear statement from the panel.

DR. CLARK: Well, given where you are in terms of actually installing the meters, how much more effort would
it take to actually do the next step?

MR. MASLIA: On our part, probably a couple of weeks with a couple of people. That's basically the time to calibrate the meters. And then, of course, on the Camp Lejeune staff, because they assist us in collecting the data, downloading the data -- it's going around to 16 meters once a month. They have the capability of storing more but, say, once a month downloading the data.

So manpower-wise or labor-wise, I don't think it's -- it's the calibration process that's intensive, and it only seems more so intensive because of the past attempts that, obviously, we have made and have not been successful. But now that we've got sort of a step-by-step how-to manual and some estimates of what we expect to see the flows to be based on our model simulation, we're hoping that it will go much -- you know, on schedule. So basically, you're talking about a two-week effort with a couple of people from ATSDR.

DR. JOHNSON: Okay. Last comment from Tom.

DR. WALSKI: Okay. I've got more comments. This is my last (laughter).

DR. JOHNSON: It's the last one on this issue.

DR. WALSKI: Okay. The -- to put this thing in perspective, the calibration data is the calibration of water -- calibrated water-distribution model, which we
aren't sure we're going to need. So first of all, we have that issue to get over.

But, in the meantime, since we have made this investment, I think it's worth getting like a month's worth of data and just looking at it and seeing what does a month's worth of data say. And then we can decide if it's worth doing several months; just for the background information. It may be good for the utilities' people just to have this data to help them manage this system even if you don't use it for calibration.

So I'd say, you know, try it for a month. There's going to be some places where you have shuttling between the tanks where the velocities are going to be high, and you are going to get good information. There are going to be some dead-end areas where you're going to be below the threshold half the time or so, and it's not going to be very useful information.

But get a month's worth of data, and if it looks good and the people from the utility think it's worth collecting, then keep on collecting it. And then if you do have to use it to -- if you decide to do a more detailed model or a more detailed calibration, you'll have it. So that's the way I would put it in perspective.

MR. MASLIA: One point, Dr. Johnson. Actually, it's an answer to Peter that came to mind with respect to
variation in production or flows. When we were doing our
testing in May of 2004 at Hadnot Point, we were seeing on
the average of about 2100 gallons per minute being
produced out of that plant during the week of our test,
more or less.

When we came back in August, although we were not
testing Hadnot Point, I just took the opportunity to go
over to the chart, and it was up at 3,000 gallons per
minute, so...

DR. POMMERENK: Okay.

DR. JOHNSON: Any more? Tom, anything else on this?

DR. WALSKI: Mm-mm.

DR. JOHNSON: Thank you. The panel, I think, has
done an extraordinarily excellent job of responding to
these questions and issues as well as those yesterday.
The work that remains for the panel is to respond to the
four specific charges, and we've talked about almost all
of them. And so that's the work that remains.

I foresee us being able to finish by around 1:30 and
such. That means that a public comment period needs to be
moved up, and I'd like to offer the opportunity now for
any comments from the public. Yes, Ben.

MR. HARDING: Can I just ask one --

DR. JOHNSON: Please; of course.

MR. HARDING: -- question before we do that?
DR. JOHNSON: Yes.

MR. HARDING: We have this amended question, issues, and discussions page, which has explicitly marked out certain bullets. And then on the original sheet, there's issues. On page 3, there was integration of groundwater and water-distribution systems. Did we deal with that yesterday? Was that -- or has that been implicitly X'd out?

DR. POMMERENK: X'd out.

MR. HARDING: X'd out. Okay. It just --

MR. MASLIA: That was my -- that's why I didn't bring it up. I didn't X it out, but, based on the discussion that we've gone today, that becomes a moot point, at least from my interpretation.

MR. HARDING: Okay. That was what I thought, but I just wanted to make sure.

DR. JOHNSON: Okay. Comments from the public. Mr. Ensminger.

COURT REPORTER: I need to go down.

DR. JOHNSON: Oh, excuse me. Let's take about a ten-minute break.

COURT REPORTER: All I need is two minutes, if you just want to continue.

DR. JOHNSON: No. I think the panel needs to have a break. Let's break until lunch arrives.
(Whereupon, a recess of approximately 28 minutes was taken.)

DR. JOHNSON: Okay. The floor is open for comments from the public. Mr. Ensminger.

MR. ENSMINGER: Not so much comments. I had a few questions on some of the things that were brought up during the discussion. It was brought up by one of the members from the Camp Lejeune delegation that North Carolina State requires separate permits for multiple water systems, and I have a question is: How long has that been -- requirement been in place?

MR. ASHTON: I'm not --

MR. ENSMINGER: Whenever you open and close a valve? How long has that requirement been in place?

MR. ASHTON: I'm not sure how long, but I can try to find that out there and also, you know, the -- I can certainly find when we got those permits for the water systems as well.

MR. ENSMINGER: And another thing about the Holcomb Boulevard water system was that it seems that there were a limited number of wells initially assigned to that water-treatment plant. Were the wells that were assigned to Holcomb Boulevard initially able to keep up with the demand for the area that it serviced?

And the question of on the flow meters, there seemed
to be a lot of dissension about that because of the oversized pipes. Would the installation of choke points -- somebody brought that up -- improve the accuracy and the velocity? I know it would increase the velocity of the water going through them. Would it increase the accuracy of the data? I mean, you're talking about 16 flow meters. I don't know if all 16 are on 12-inch oversized lines.

DR. JOHNSON: Tom or Peter or both?

DR. WALSKI: Well, to increase the accuracy, whether or not we need it is still the question. So that's why I'd say: Don't spend this money until we're sure we need that extra quality of data would be the way that I would leave it.

DR. JOHNSON: Peter?

DR. POMMERNK: I agree.

MR. ENSMINGER: And on the Hadnot Point water system, the questions of historical data as far as contamination of certain wells, the installation-restoration program has the accurate data now for each well that was contaminated in the Hadnot Point system. They have the actual contaminants that were in those systems or in those wells, and they know what the sources were. So as far as reconstructing, you know, and doing the historical, there would be some work involved in it, but that data is
available.

DR. JOHNSON: Okay. As I commented before lunch, I think we can certainly be through by 1:30. Some meetings go quicker than anticipated. I have been in many meetings where it's gone the other way and you reach 2:30 on the third day and you realize you're not done. And so this is quite to the contrary. And the preplanning done by ATSDR was really very, very well done, and presenting the issues and questions to the panel has helped us go through some of these tough matters that the ATSDR is going to have to deal with after we leave.

So my goal is to have us out of here around 1:30 or so. I propose to provide a formal response to Questions 3 and 4 in the charge to the panel. I discussed with Mr. Maslia before lunch if all four were still relevant, and he indicated that we had really done a good job discussing questions or Charges 1 and 2. But he asked that we do provide a formal response to Charges 3 and 4. Charge 3 is now on the screen, and so that spares me having to read it to you now. How does the panel wish to react to this third charge?

DR. CLARK: One area that it seems to me that ATSDR might consider is looking at the degradation by-products of some of these oxidated chemicals, and I think there's a potential there that there might be things like vinyl
chloride in the system, which I think would bias their results. And I hadn't gotten a sense of how much of that has actually been done.

DR. SINGH: Number 2 shows that we -- ATSDR already has started with their groundwater modeling. One portends to consent to the analysis, and the other one relates to the accounting for the variability recharge. I think those are the two issues that ATSDR should take into consideration.

DR. JOHNSON: Other advice on this charge?

DR. KONIKOW: Well, the groundwater modeling that we discussed -- and I think that's been focused on the Tarawa Terrace area. And I guess maybe we should talk for a minute about the need for looking at and modeling the groundwater flow and contamination in the Hadnot Point area or the Holcomb Boulevard area. Or do we just accept that Hadnot Point wells are contaminated over the whole time?

DR. LABOLLE: In addition, Lenny, you had mentioned previously -- and I concur with the need to at least demonstrate that contaminants arrive to TT-26 or demonstrate that they may not, depending on the outcome of the models within this for the 14-year time frame, for example, and to the extent that the study period's going to be pushed back further.
In addition, somebody mentioned other periods of time we might be looking at the cancer risk. You may want to actually have a model that's useful for protecting the uncertainty in the arrival curve itself. I'm not sure if you're planning on going back before '68 at Tarawa Terrace.

MR. FAYE: Our intention has always been -- largely due to modeling considerations as well as others, but our intention has always been to begin the groundwater flow simulations at Tarawa Terrace with the beginning of operations of the WTP and the well fields, which would be like 1952, '53, and then simulate that forward to '94, which is the end of our relevant water-level record.

DR. LABOLLE: But the question would be the period of time from '54 through '68.

MR. FAYE: Yeah. To transport -- very definitely. We would do the fate and transport simulations as well for that period of time.

DR. LABOLLE: Well, but are they going to use it in the epi study?

MR. FAYE: That, I don't know. But I would just feel comfortable doing that. If we don't, there's always going to be a question there: Did the contaminants arrive at the wells in one month, six months, five years, or whatever? And I think that's an important consideration.
DR. LABOLLE: I'm not suggesting that it not be done. Actually, I'm suggesting that the degree of effort put into this will depend upon whether or not the epi studies are in the future pushed back to earlier dates, I think.

MR. FAYE: That, I don't know. But our -- as far as the modeling is concerned, I can speak to that, and our plans from Day 1 have been to provide those simulations from the beginning of the WTP operation and the well-field operation, which would be, as I said, 1952 or '53.

DR. KONIKOW: For all three areas?

MR. FAYE: Just for the Tarawa Terrace. Lenny, as we said yesterday, we're using the Tarawa Terrace because it is a "simpler system." But it is a little simpler. So that's our -- what would you say? That's our prototype effort, and if we think we're successful there, then we can advance ourselves to -- if necessary.

I mean, if the epi -- epidemiological demands require that, then we can advance to a more complex system where we have this confidence that we've built on and attempt that, which would be Hadnot Point.

DR. KONIKOW: So is the default option then in the epidemiological study to assume that the Hadnot Point system was contaminated over the whole period of time?

MR. FAYE: I don't know what their default position
would be. But based on the data that I've seen and how the wells are positioned with respect to obvious sources of contamination and whatever, yeah, I would say that there -- whoever said here today that when the particular well was actually opened up and began to be used, it was probably contaminated at that time. I would say that that's an accurate statement with respect to perhaps a number of wells, supply wells, at Hadnot Point.

And also through time -- I mean, the wells may have been -- in 1941 when the wells were constructed, there probably was no problem. And then over the years, as the facility grew and different things were done land-use-wise, why, yeah, they probably became contaminated.

MR. MASLIA: Two issues. If we go into Tarawa Terrace, from a groundwater fate and transport standpoint, if we don't start at predevelopment, then we have some real issues to address with antecedent conditions, and then we're going to have to do some more uncertainty modeling as to the effect of not knowing the antecedent conditions, which I think adds to our effort and, I think, overpowers the amount of additional effort, just by starting from before the -- from predevelopment and running them out. My understanding is we can also -- we can vary the step size in MODFLOW, can we not?

MR. FAYE: Oh, yes.
MR. MASLIA: Yes. So we could use larger -- if we see there's no contamination, you know, for a certain number of years in the beginning after some trial runs, we can make those larger, larger step size, and then when we think it is down to a much smaller -- as you said 15 day or less.

I've actually used even smaller time steps for that previously and do that. And that would be, at least initially, my approach is not to complicate our analyses even more with trying to guess antecedent conditions but let the model do the work; in other words, circuitous development.

MR. FAYE: Yeah. At that -- yeah, the issue then very rapidly moves from a code-capability issue to a number-crunching issue, so that's where you're at there.

DR. LABOLLE: I wouldn't bother corseting the time study, in my opinion, simply because, I mean, you're probably not going to be constrained by the time it takes to run this model. And what that would then do is lead to possible numerical errors and a plume that doesn't look like the plume that the model was intended to solve for.

So you might as well just leave them at the required resolution to obtain a numerically valid solution. I'd be more concerned about the assumptions in the model itself than those kinds of issues and the underlying geologic
characterization, which it looks like, you know, you've done a good job approaching that.

But the -- it's the way in which one deals with the uncertainty in there. And if I were to make any recommendation, I would recommend an approach to dealing with geologic uncertainty be incorporated into the analysis so that one can examine the uncertainty in the geology and its effect on arrival, potential arrival, to these various wells in the vicinity of this ABC's Cleaner there and of the -- some of the wells that are reported -- reportedly clean throughout the periods of interest may have actually seen contamination because they simply weren't sampled continuously.

MR. FAYE: Right. They're --

DR. LABOLLE: And others that -- I'm sorry. Excuse me.

Others, you know, that have seen contamination, we don't know when the contamination arrived. And to the extent that maybe all of these are swamped out by concentrations of TT-26 and the models begin to show that, maybe you can lay these issues aside because the mixing that appears to have been in this system. All wells are mixing.

You may not need to pursue, you know, the groundwater modeling past that point in terms of determining what
arrived at these other wells. But there seems to be another issue -- and I think that you and I discussed during the break -- where if TT-26 is turned off and these other wells have taken over --

MR. FAYE: Right.

DR. LABOLLE: -- and yet there may not be sampling at these wells to assess whether there was contamination arriving to them, and some of them are quite close to TT-26 and appear to be very capable of intercepting the plume.

MR. FAYE: That is a real issue; absolutely.

DR. LABOLLE: And so then you're left with modeling to resolve that.

MR. FAYE: That's right.

DR. LABOLLE: And once again -- I don't mean to belabor the point -- but I think it's geologic uncertainly that is going to swamp out a lot of other uncertainties in all of these modeling efforts of the water-distribution system. And that's going to be one of the main players. That and the source, as David will know.

MR. MASLIA: The other question or issue with respect to the Hadnot Point -- as Bob said, we're using Tarawa Terrace first. But if we assume or can assume that at least some of the wells were sunk into an aquifer upon production that was already contaminated, does that then
not bring the problem in trying to simplify matters to a
materials mass balance where if we knew the cycling on and
off of wells we could calculate the concentration of the
mixture on there? And that might then alleviate also any
detailed numerical modeling of the Hadnot Point area with
the large and nonpoint specific sources.

DR. JOHNSON: Okay. Let's then turn to Charge No. 4.
And as that comes up on the screen, let me ask kind of a
housekeeping detail of Mr. Maslia. Are arrangements being
made for transportation to the airport? I mean...

MR. MASLIA: My understanding is some -- some people
have arranged with the hotel shuttle, and all that needs
to be done is to call the hotel shuttle when they are
ready to board that hotel shuttle. Ann Walker or Joann
can do that once we tell them we're -- we're finished.

DR. JOHNSON: Okay.

MR. MASLIA: If people want -- what?

DR. WALSKI: The shuttle doesn't bring us to the
airport, does it?

UNIDENTIFIED PANELIST: There is a shuttle.

MR. MASLIA: There is a shuttle.

DR. WALSKI: Yeah. But not the -- a different
shuttle; okay.

MR. MASLIA: Right.

DR. WALSKI: Okay.
DR. CLARK: We're better off sharing taxis.

DR. WALSKI: Right. I think so; yeah.

DR. LABOLLE: Along the lines, yeah, I spoke with the driver on the way here, and he mentioned that he's trying to get us a large van to be able to go to the airport from here. And I have to actually have them deliver my bags here, and I mentioned that you --

DR. SINGH: Yeah. My bags are at the hotel.

(Whereupon, a conversation ensued off the record.)

DR. JOHNSON: Charge No. 4 gets under the matter of the project schedule. It seems -- it seems, at least to the Chair, that there have been a number of rather significant suggestions as to perhaps how to reorder the work that is anticipated. That makes it a little bit unclear, at least in my mind, as to how that works out in terms of a project schedule. But I would look forward to the comments from the board -- from the panel.

DR. CLARK: Subject to the comments that have made by the panel, it seems to me that the three-year planning projected cycle is probably a reasonable one to work towards.

DR. DOUGHERTY: Tom had suggested six months.

DR. WALSKI: Yeah. I can see you're getting to the point of beating a dead horse after a while that possibly you can do this in about six months unless you find that
missing notebook. You know, the notebook that was on top of the refrigerator back in '85 that fell behind the refrigerator? And they move it, like, next year, and they find this notebook with all the data in it or something. Unless you find that kind of a notebook, I don't see three years of work giving us a much better answer than we can in probably about six months.

MR. MASLIA: Can I qualify that last charge so that everyone's on the same playing field here?

MR. FAYE: Did you look behind the refrigerator?

MR. MASLIA: I've looked in at a lot of places, including down a manhole. The three years was the total, not three additional years. That was three years of project length, and we have spent length approximately, what, a year or more? Less. So we really are only talking about another year and a half or so.

The initial schedule called to have some preliminary fate and transport modeling results with Tarawa Terrace by this September, which I believe we're on track for that. The question is the additional work, taking the suggestions of the panel. I've been trying to simplify them on the Hadnot Point area, assessing some preliminary flow data from the meters. Would, you know, the three years be sufficient? And the one comment I would have, given a perfect world where, even if you had to look for
data, in six months would probably be acceptable.

Being -- doing the kind of detective work that we have to do with historic data, I would say shortening -- my experience would be on this project that that would really be constraining the agency to shorten it any more than that, but I'm open to some concrete ideas where -- Bob wants to.

MR. FAYE: I don't know, Tom. Maybe there's some pharmaceutical issues related to your comment there, but there's just no way in the world (laughter).

DR. JOHNSON: I don't know what that means. I'll speak. If no one else will speak, I'll speak. What are the pharmaceutical issues?

MR. FAYE: There's no way that I can imagine or devise or anticipate that we could -- we can fulfill the requirements or the suggestions of the panel here with respect to the groundwater-flow models and the fate and transport simulations and provide a comprehensive, complete, technically defensible written product in a six-month time period from today. I think that's a very unrealistic -- that would be a very unrealistic proposal or recommendation. And that's based on 30-some years of experience.

DR. WALSKI: But we have put those -- we've taken out, pretty much, most of the distribution modeling, and
we're taking a fairly major chunk of the scope of work out --

MR. FAYE: Yeah, but the --

DR. WALSKI: -- and also cut out most of the modeling at Hadnot Point, too, for groundwater. So we've --

MR. FAYE: No. Let me clarify that. First of all, the -- there -- the -- as the time-line chart, I guess, that you've been -- that you have -- the groundwater modeling and the distribution modeling were parallel efforts. Okay. They weren't -- they weren't a series situation: One gets done and then the other. So those were all parallel efforts.

And so, I mean, we planned to converge the completion of the two efforts, at a point in time merge the results and then go on from there. So I think, as far as that parallel effort with respect to the groundwater-modeling situation is concerned, we're right on the regional time lines. I think we conformed to them very well.

And as Morris said, the -- we're having -- we're planning to have some fate and transport simulation results by the end of September, this fall. I think that's -- with a bit of work, that's probably doable. So -- and realistic. And so then I would anticipate finishing that project completely: providing the written report, the appropriate peer reviews, et cetera,
et cetera, would still take most of the next year
after that. So --

DR. CLARK: Don't you also have to integrate the
epidemiological studies --

MR. FAYE: Exactly.

DR. CLARK: So you're talking, what, probably another
six months to a year?

MR. FAYE: Absolutely; yeah; yeah. So there's a --
even conforming exactly to what I've heard that you folks
will probably recommend, this three-year time interval
that we're looking at now with about a year and a half or
so left is still extremely ambitious. And I don't know.
I mean, maybe I'm just all wet, but I'd like to hear from
some of my groundwater colleagues on the panel to tell --
to say -- is that -- are you -- have you been smoking
something, too, Bob (laughter)?

DR. JOHNSON: Well, there's a clarification.

DR. DOUGHERTY: What is the terminus of the three
years? Is it the delivery of the water-modeling results,
or is it the delivery of the epi results?

MR. MASLIA: The original schedule was three total
years to deliver the final historical reconstruction to
the epi people.

MR. FAYE: With all of its elements.

MR. MASLIA: And that included another -- another
peer panel to assess the historical or the final report, as we did in Dover Township.

DR. DOUGHERTY: And the Hadnot and --

MR. MASLIA: Right.

DR. DOUGHERTY: -- Holcomb?

MR. MASLIA: That's -- that's correct. I will add --

DR. DOUGHERTY: Or whatever may be done with Hadnot?

MR. MASLIA: Right. I will add that Frank and I and
the epi team had discussed, in fact, with Marine Corps
headquarters, back in February that it was going to be a
challenge, an extreme challenge, if we were going to the
distribution-type stuff to even keep to that three-year
schedule; an extreme challenge.

I think based on some recommendations here that
three-year time frame becomes a more realistic and
attainable goal. And that's really -- but, again, there
are a number of, still, work efforts and implementing
recommendations that you have made even with the
simplifications.

DR. DOUGHERTY: My personal feeling is then that --
and take the comment with a grain of salt because I really
haven't seen the detail of the work plan for the other two
portions of the site in terms of groundwater modeling and
its impact. But I think the schedule is going to be
aggressive because of the additional emphasis on the
archaeology, as we've been calling it, and that really takes a lot of calendar time. It takes a lot of calendar time.

MR. FAYE: Absolutely.

DR. KONIKOW: I think, also, if the goal is to do an advective/dispersive transport model and that hasn't been started yet, that takes time. And that's going to take time.

MR. HARDING: If I might speak specifically about this July schedule --

MR. MASLIA: This is July of? Is that in the July book?

MR. HARDING: It's revised 13 July. It's the current.

MR. MASLIA: Right. There's probably one in September. I don't know if you've gotten it. We've revised it somewhat for -- in September. But you can go ahead. That's probably within a six- or eight-month period.

MR. HARDING: So if you look at this, the geohydrology of groundwater flow, fate and transport work appears to end, roughly, the end of this fiscal year, which is --

MR. MASLIA: Which is September 30th.

MR. HARDING: Of 2005?
MR. MASLIA: Yeah; that's correct.

MR. HARDING: And then what extends beyond that is water-distribution system historical models; actually, water-distribution system present-day models. But we, I think, suggested that you dramatically compress that --

MR. MASLIA: Yes; yes.

MR. HARDING: -- which, I think, may move that out of what appears to be the critical path in this thing. Now, I tend to disagree with Tom because I have been swearing off all my pharmaceuticals recently.

But I think more time is necessary to characterize the Hadnot Point situation, but I don't really know that business. That's the groundwater people's business. But I go down here to this methods' development -- and maybe this was dealt with yesterday. But there's the GA calibration methods, tank mixing, and dynamic linkage of groundwater transport and water distribution models, which I think can be eliminated.

And I think that uncertainty methods in groundwater flow transport and also in terms of water distribution -- or if we want to say integration of exposures and intakes and that stuff. Dealing with this in a -- dealing with uncertainty and quantifying it can be expanded. But that should not affect the overall length of the schedule. But those bars down there on all those methods' developments

NANCY LEE & ASSOCIATES
are really driving the schedule out to the right, and those should be essentially, I think, eliminated.

MR. MASLIA: Those were based, again, when -- were based when the schedule was developed, based on our previous experience, which they did drive the time frame. Although they were -- or at least now we see them as complementary, not driving the schedule.

And from the discussions that we've had here the last couple of days what, again, I see driving the schedule are two issues: the archaeology or data discovery. That is very time-consuming and labor-intensive as well as the methods to better understand the uncertainty with respect to geologic issues at Tarawa Terrace and going to the full-blown fate -- full blown as opposed to the effective full fate and dispersive transport models.

MR. HARDING: Well, I want to emphasize that I think that you can make a contribution, both to the understanding of this situation but also to the practice, if you would, instead of spending your resources on some of these methods that relate to linking the models, if you would spend more of your effort on quantifying and propagating uncertainty through the methods.

That is going to contribute more to a realistic assessment of the epidemiological situation in my view and also to the practice here because this is something that
has been an undercurrent in all the discussions. But the practical matter of how you do this -- it's not like it's unknown, but it's something that could use some effort.

   It would be a good thing for you to shift resources to that area, I think. That's my view, and I think that helps both your schedule, and, also, it puts your resources in a more appropriate area. And I agree with you that the resources should be spent, understanding the Hadnot Point geohydrology; is that right? Hydrogeology transport.

   DR. DOUGHERTY: In my premeeting notes, I had compared the July version of the schedule with the version of the schedule on a preceding page of the handout. And even at that time, last summer, the areas in which the greatest slippage had occurred appear to be in collecting background information and then the development of the historical network information.

   I don't think that we've reduced or accelerated those particular tasks in the last two days. And since those seemed to have been the ones that already grew before we had our two bits to say, they may slip further by as much as the six months that Tom talked about; my gut feeling.

   MR. HARDING: When you say "slip," you mean be compressed?

   DR. DOUGHERTY: No. I mean they've stretched out.
They have been extended when you compare that page to the previous page. And what we're hearing is there continues to be data discovery that has some significance with interconnects, monthly pumping rates that are not yet complete. And their significance to the outcomes -- the requirements for the outcomes seem significant enough that they're going to stretch longer than I thought they would when I walked in here yesterday morning.

DR. JOHNSON: Well, that would seem to conclude our response to these Charges No. 3 and 4. My view of what remains is to offer, indeed encourage, any kind of dialog amongst the panelists on any issue that hasn't been addressed to your satisfaction, any matter that you brought up in your premeeting comments that has not been addressed to your satisfaction, and any points that might represent some differences of view within the panel. Put those on the table to the extent you wish to discuss them.

Following that, it's kind of an open-discussion opportunity. I'm going to conclude the meeting by asking each of you as panelists what you would recommend the agency do in regard to what you've heard about the groundwater work as well as the water distribution work.

And I don't know that -- as I said earlier this morning, that we want to take the individual advice and recommendations and attempt to synthesize them into a
panel product. I don't know that that's in ATSDR's best interest because, to some extent, that may tie your hands. But I think it is quite fair to ask each of you as individuals your comments on what you would recommend for the future. So with that on the table, what else do you want to deal with as a panel? Open discussion on points that haven't been addressed and then closing by asking you your individual comments; vis-à-vis, advice; recommendations; but not going that third step and attempting to compile a panel body of recommendations. How does that resonate with you, Morris? You're the primary user of these deliberations.

MR. MASLIA: I actually would prefer not having a vote, as you say, but rather having everyone's individual opinion or summary of their understanding of what took place today. I think that would be much more beneficial to us.

DR. JOHNSON: Is it fair for the panel to say, as a body, that we consider this work as extraordinarily important for various reasons, certainly in support of an epidemiological study, but for other reasons, as articulated by Ben, as a study that will advance the practice in the field as well? I'm paraphrasing. If I misstate this, please correct me.

But is it fair for this body to go on record, saying
this is pretty important stuff with the epi study and the work that involves water protocol and that we would encourage ATSDR, given the importance of this work, to have resources that are commensurate with that importance? Does anyone want to take issue with that? Are you comfortable with saying that for the public record as a body? Important stuff. Let's get the resources that match the importance and urge ATSDR to provide those resources.

DR. SINGH: I think so. I think this is a very important study. This integrates hydrology, geology, hydraulic engineering, and health sciences. So it's a very important study, and it should be encouraged. And obviously, we would like the agency to provide commensurate resources.

DR. WALSKI: But we also have to be concerned that the marginal benefits exceed the marginal costs. And some of the things I'm still not convinced that they are from my perspective. But, you know, I'm just one voice.

DR. JOHNSON: Does anyone else wish to speak to the issue of importance of study and commensurate resources?

DR. CLARK: I think I would support all of your characterization of the importance of the -- both as sort of the movement for the state of the art, the idea of integrating groundwater and surface water modeling
activities and then tying together with epidemiology.

   I think there's also -- I'm sure the study's going to be scrutinized carefully by the public. And I guess what I'm concerned about is that we appear to do a study that's somewhat short of the best that we can do, then we could be criticized for that, for not taking it seriously and not understanding the public health implications of it because they're very serious because there's a lot of water systems that have similar kinds of problems.

   And I can see that this could lead to, maybe, a further study or a more in-depth study of better understanding of what those exposures might be for other water consumers. So it seems to me that you've got to take it seriously and think of it as scientifically defensible. And I say resources are there. Use the resources to accomplish the end project -- the end goal of the project.


   MR. HARDING: I want to build a little bit on what Tom's saying because I started -- I think I -- I guess I started this ball rolling a little bit. And I want to say that just because something is possible doesn't mean it should be done. And I think that we have to ask ATSDR to really focus on important areas here. And this -- I think this is what we're all going to address individually.
But I think the study is important in the two ways that you mentioned, and if ATSDR puts their emphasis on the areas that will contribute to an understanding of this situation and improving the practice, I think then it can be a very important study. I just want to echo Tom that it's important to make that and not take the resources and use them in areas that are going to just be generating friction.

So I'll make more specific comments, and I'm sure we all will. But I would like to see particularly -- this area of dealing with uncertainty quantitatively is an important one that's moving more into the practice; out of the universities and into practice. And I'd see some more effort spent there.

DR. JOHNSON: Thank you. Eric LaBolle.

DR. LABOLLE: I think this -- it may come back to something I touched on yesterday, which is: What is the role of these models that are being developed? And I think the answer at one point was, well, to provide monthly or submonthly, you know, concentrations, for example, with regards to the groundwater model and its inputs to the distribution system model.

And that may not be the role of the groundwater model. The groundwater model may play a role in simply bracketing the uncertainty in those concentrations that
arrived, and the groundwater model may not be used to even predict the specific inputs used on any realization because that could certainly -- may be so great that one may just want to throw their shot at particular concentration inputs over time.

It depends on how much detail is put into these models and how much more effort is put into them. And I think from what I've heard -- essentially, I think everybody has a valid point hovering. And Tom, essentially, you know, we need to -- they need to make the best effort. You know, you certainly don't want to waste resources. But I think that the role of these models is really critical. You know, at what point do we say we're just, you know, beating a dead horse here?

DR. JOHNSON: Yes, Lenny.

DR. KONIKOW: In terms of the epidemiological study, is there a desire or a capability to look at the role of all the various contaminants? I mean, we were talking about PCE at the Tarawa Terrace. But there was also a benzene pollutant, and there's some TCE and PCE and some vinyl chloride at Hadnot Point and a longer list of contaminants. I mean, is this -- is there enough information to factor this into the epidemiological study?

DR. BOVE: Do you want me to answer?

DR. KONIKOW: I mean, this gets to, you know, what we
might use the groundwater transport models to help define.

DR. BOVE: I mean, I don't think we have any
information as to when Hadnot Point had more TCE than
benzene. We don't have that data so that -- what we'll --
the way that we've characterized Hadnot Point exposure is
to a mixture of VOCs, TCE being the main component. But
if we're going to say -- if we're going to infer from that
-- if we see, for example, an elevated rate of childhood
leukemia or whatever, we will be able to say, at most,
that it's this mixture that caused the exposure, similar
to what we did at Toms River when we said that -- what was
the -- the Parkway well field, which consisted of TCE,
PCE, and this exotic chemical, styrene, acrylonitrile
tramer, and which one was it?

Well, they were all together. You know, or when I
studied trihalomethanes, well, which one caused the neural
tube defect increase? Was it the chloroform? Was it HX?
What -- what was it? That's how Hadnot Point looks to me.
It's a mixture with TCE being the main component, and to
make inferences, I would have to say that TCE is the main
component. But, just as you said, there's benzene.
There's all these other contaminants that could also cause
or be suspected of causing childhood leukemia.

DR. JOHNSON: Okay. The floor is open for things
that you think have been not addressed or not addressed
adequately, things that you commented on in premeeting. So let's put them on the table. David.

DR. DOUGHERTY: I'm just going to return to your suggestion because the panel statement had, in terms of advancing the state of the art, and just comment that what it really looks like to me is the other bookend to Dover Township that really is going to help define the limitations of the methodology because there's such great uncertainty here as compared to a very different case at the other end.

DR. JOHNSON: Thank you. Ben, do you want to start? Anything that's not been put on the table or put on the table to your dissatisfaction?

MR. HARDING: I thought that's what we were doing just now. Then we got interrupted to respond to your charge.

COURT REPORTER: Microphone, please.

MR. HARDING: Again, the issue, monochloride, we've raised it a couple of times, but I think it's something that should not be neglected in our reconstructions.

DR. CLARK: I think that the issues have been addressed pretty thoroughly in an open forum. I'm very satisfied with the discussion.

DR. JOHNSON: David?

DR. DOUGHERTY: (Shakes head)
DR. JOHNSON: James?

DR. UBER: Well, just a couple of very specific, small questions.

DR. JOHNSON: I'm trying to make the point this is an open discussion.

DR. UBER: Open discussion; okay. Just because I had a couple of items here that I didn't have -- obviously, I didn't think I had answers to, and I was just curious. In the -- in the Hadnot Point area, what kind of plant production data is available now and historically?

MR. MASLIA: Basically the same that we have on all the plants. When we have asked for plant introductions, the one that we have monthly data for -- well, it gives us a chart, and it lists all the water-treatment plants.

DR. UBER: Okay. So nothing more than monthly?

MR. MASLIA: I haven't looked at the actual individual well records at Hadnot Point, but for the plant --

DR. UBER: The plant is monthly. So they didn't have to report anything daily or didn't report daily water production?

DR. POMMERENK: Actually, they do.

DR. UBER: They do?

DR. POMMERENK: I mean, in the recent past, I have personally have data from 1998 on this daily production at
each of the plants.

MR. MASLIA: But not for the present -- not for the study period?

DR. POMMERENK: No; not for the study period. The information was also for the current.

MR. FAYE: All of the data that I'm familiar with from Hadnot Point, from the well-construction data to the contaminant data to the supply data, you could probably generally characterize that as at a higher level of quality and number -- somewhat higher level of quality and number than what is available or what was available for Tarawa Terrace.

We can define the relevant issues that we've all talked about in a well-production contamination, temporal distribution of contaminants, spatial distributions, et cetera. We can probably define that somewhat better at Hadnot Point. Historical record: somewhat better at Hadnot Point; not greatly record, but somewhat -- greatly better, but somewhat better than we could at Tarawa Terrace.

DR. UBER: But with regard to temporal resolution --

MR. FAYE: That too.

DR. UBER: Okay. So the subtext of that is that -- the only reason why I'm asking that is because I'm thinking of the issue of interconnectiveness. And I'm
thinking of just, in terms of the simplest model, if one had daily water production and one had information on base, then, you know, conceivably, you could look at a statistical approach that would allow you to say, with some degree of confidence, all of the water was -- all of the water within this area was coming from this plant or, no, there's definitely a shortfall. It had to come from somewhere else. That's why I was asking that.

MR. FAYE: I don't think -- the folks from Camp Lejeune can correct me. But I don't think the actual amount of water available versus need at Hadnot Point is not an issue. Where it was an issue was at Tarawa Terrace for a couple of years.

DR. UBER: I was talking about the Holcomb area, whether or not that ever got water from, you know, the other two interconnects. So it was my recollection you got about .8 MGD here and you got about three down here. That's the data that I saw. And so I'm thinking, you know, does it go down to .4 and go up to 3.4 on a statistical basis? That's what I'm trying to think about.

MR. FAYE: Everything that I know regarding the connection between Hadnot Point and Holcomb Boulevard is that there -- over the years from 1973 to the present, there were possibly some very short-term, intermittent connections between the two systems; i.e., Hadnot Point to
Holcomb Boulevard. Okay? That's that connection.

Between Holcomb Boulevard and Tarawa Terrace, there was a supplemental connection, also possibly intermittent; but a lot more continuous than the previous situation between Hadnot Point and Holcomb Boulevard between 1985 and 1987 Holcomb Boulevard to Tarawa Terrace. Okay?

DR. UBER: Okay. Well, that's just -- that degree of certainty that you just expressed is contrary to what I heard before. I mean, that was our whole -- the whole basis of our discussion of, you know, is Hadnot distribution system a self-contained entity or is there significant -- I'm sorry.

Is Holcomb a self-contained entity, or is there some leakage from a contaminated area? That comment just indicates that, no, or very, very intermittently. And so I'm -- yeah; with the exception of those two years. That's right; with the exception of those two years.

That was so -- we go back to the comments before that was when we were saying, you know, we need to have some archaeological investigation to look at this. So I'm, frankly, uncertain about the degree of certainty, I guess.

MR. MASLIA: Yeah; yeah. We definitely agree with that. And that's my take on the discussion this morning would be to put some effort into trying to reduce the uncertainty or refine the understanding on the
interconnection issue.

DR. UBER: Okay.

DR. BOVE: It's very important because we're going to be telling people and putting it on our Web site that if you lived here at this particular time you were exposed or not exposed. This is going to be information for everybody -- anyone can see on the Web site, so we need to nail this down.

DR. UBER: Okay. So that was my rationale for asking those questions about the production -- production data. The other thing that I was just curious about is I think -- I guess I know the answer to this. But is there any data at all on customer complaints (laughter)?

MR. MASLIA: Well, this past spring I was on the airline, coming back to Atlanta, and one of the Marines that was on there with me -- they knew that we were doing some testing. And he says, "Well, the water tastes fine, but I could use a hot shower."

DR. UBER: All right. You know the reason why I was asking that is -- and I don't know anything about the -- I don't know anything about taste and odor thresholds for the levels of these contaminants. But if they had any kind of record-keeping of complaint data or things or even in terms of surveys of people. If anybody here knows anything about taste and odor thresholds, it might be an...
interesting survey question. You know, did the water smell like gasoline, that type thing.

DR. JOHNSON: Okay. Any unfinished business, Lenny?

DR. KONIKOW: Well, I think yesterday in the modeling we had lots of specific comments and everything, and I'm sure you'll consider them. I have just one residual specific question, which I don't recall was addressed, and it may have been.

But in advective transport, I think Bob said -- or at least I remember reading in the report -- that he placed or seeded particles 600 feet, I believe, east or west of ABC Cleaners; west, I believe. And this somehow led to the conclusion that the source of PCE in TT-23 was not the ABC Cleaners. Am I remembering that right or wrong?

MR. FAYE: Well, you're -- you are remembering it right, but the conclusion is wrong. It was just a poorly written statement, Lenny. What I meant to say was that, yeah, I think ultimately the PCE anywhere in that vicinity, the source was ABC Cleaners.

DR. KONIKOW: Okay.

MR. FAYE: It's just that when the -- when TT-23 was turned on, probably some time in the summer of 1984, and only operated for, maybe, four or five months and in January of 1995 all of a sudden here are these elevated concentrations of PCE found in the well and you're 1600...
feet from ABC Cleaners, the conclusion that I was trying
to draw or make was that, obviously, whatever PCE entered
that well in that very short interval of pumping had to be
much nearer the well than ABC Cleaners.

And then I went on to the explanation of the
overlapping, contributing areas and suggested a
possibility for how that area north, immediately north, of
TT-23 had become somewhat contaminated with the PCE. So
you remembered it right, but I wrote it wrong.

DR. KONIKOW: That's okay. Thanks.

DR. JOHNSON: Tom, unfinished business?

DR. WALSKI: Okay. Well, since I've been accused of
being on hallucinogenics, I might as well continue
hallucinating here and make an observation that I think's
going to happen is: If we sat here today and figured out
about when the plume hit Well TT-26, we could probably --
with the data we have, including the model we've run, we
could probably say it's about the six-month window.

So what we're going to do is take another year and do
-- and possibly do a tremendous job. It's going to be an
outstanding modeling job and put all the uncertainty on,
and I'll bet the answer's going to be about the same
six-month window that we go today. That's my prediction
of probably what is going to come out of the results.

But having said that, I think, you know, the study
team's outstanding. I have tremendous respect for the ATSDR people: Bob and the others. I think they're doing just a super job, and, you know, if anything, they're probably doing a little too good of a job, but that's, you know, not a bad criticism.

DR. JOHNSON: Okay. Thank you. Vijay, anything that's not been addressed to this point that you'd like to bring up?

DR. SINGH: No.

DR. JOHNSON: Peter?

DR. POMMERENK: I don't have anything either.

DR. JOHNSON: Eric?

DR. LABOLLE: I'm clean (laughter).

DR. JOHNSON: This is a government facility, gentlemen. I don't know if there's anyone out there with bottles waiting for us or not.

DR. LABOLLE: But I would like to comment on the six-month factor. I really -- I think that that's -- not the six-month factor, the six-month window of arrival time here.

I think that that's a bit optimistic. Actually, my experience has been if one were to really address the level of uncertainty of the hydrogeology with a method capable of doing that -- and at this point, I don't see that that is in the cards for this, given the time frame
in which they want to complete the job and the approach that's been taken already. I think you're already down a path that doesn't allow for the kind of thing I refer to. But in that context, I think one would find that the uncertainty in arrival would actually be much greater than that, possibly. I mean, TT-26 may be close enough to the source that that's narrowed down some of the six months and is still kind of optimistic.

DR. WALSKI: So I'm even being too optimistic then.

DR. LABOLLE: But it may be -- it may be quite -- it may become clear with a little more analysis that it certainly did arrive prior to the study period beginning in '68. And that's something, I think, that that's another role for the groundwater model in this context.

DR. JOHNSON: Okay. Before I, starting with Eric, ask for your individual recommendations and advice on the groundwater work or the system distribution work, Morris and Frank, are there things that are unfinished in your minds? Are there things that you want the panel to address now that haven't been addressed?

MR. MASLIA: No; only, Jim, you did ask about water quality complaints, and Jerry just brought this document here. Under Item No. 37, it says, "There have been complaints concerning water quality residents aboard Camp Lejeune." And that's dated -- I don't have the exact date
on this, but it's one of the released Camp Lejeune documents. So there apparently have been complaints. But other than that -- '93. It's 1993.

Other than that, we've gotten -- or at least I've gotten some clear indications and clear assessment of what we've done and what we need to do. And I'd just like to thank each one of the panel members. I think it's always better to have internal discussions as opposed to, as Frank said, putting it out on our Web site and then hearing the discussions.

DR. JOHNSON: Don't be too conciliatory. You've not heard their final recommendations. Frank, anything that's not been discussed to your satisfaction?

DR. BOVE: Thank you very much.

DR. JOHNSON: Okay. Starting with Eric and then working our way around, I'd ask for your individual recommendations as to how ATSDR should proceed, given this day and a half of discussions, and you can give that advice, make those recommendations any way you wish: specific to groundwater, specific to the water-distribution systems, or both. So here's your -- at least for this meeting of this expert panel. What are your recommendations?

DR. LABOLLE: I suppose I'd begin with regards to the water-distribution system, parsing out this chronology, as
has been suggested by members of the panel, and focusing on those times when we know there was contamination in the system and there wasn't interconnection and assessing the need for the water-distribution system during those times -- the water-distribution system model during those times.

And I think the model itself that's been constructed to date may be useful in this for showing that, you know, what comes in this one line into the system reaches the tap. It may be obvious to those of us sitting here, but it may not be obvious to the public. And I think that I would recommend at least demonstrating that to the effect that it can be demonstrated and then identifying those other areas where the water-distribution system model may be important.

And I think if there's effort to be put into that that's what I would focus on in terms of the water-distribution system model. In terms of the groundwater model, as I mentioned several times, you know, my principal concern is with the geologic uncertainty and the source terms to the system and how they're modeled and a way to the uncertainty within the context of the model.

If there isn't the plan to do that in a realistic way, a geologically realistic way, then one should acknowledge, you know, the outcome of what they're seeing and the uncertainty in the outcome with regards to the
pre-existing characterization that kind of went into it
and the inability to modify that within the context and
the constraints of the modeling approach.

And I think that that's important because that's --
what it's going to do is constrain the model outcome to
kind of a precondition, you know, range of exposure
estimates that don't necessarily encompass the degree of
uncertainty that we really have about this system.

DR. JOHNSON: Thank you. Peter.

DR. POMMERENK: Yeah. My recommendations follow
almost exactly on that line. I think the focus on the
groundwater modeling should be on determining the range of
concentrations and times that the contaminants may have
arrived or may not have arrived at the wells. And as the
panel has, in my opinion, fully stated that's the driving
force for everything that is downstream of that.

So again, yeah, the focus should be -- you know,
several suggestions have been made, you know, for example,
Monte Carlo simulations and so on, to derive a measure of
the uncertainty of those values that come out of the
groundwater model.

With respect to the water-distribution modeling, if I
understand this correctly at this point, the main
uncertainty that we have right now left over is the degree
of interconnection between Holcomb Boulevard and Hadnot
Point. Although we have heard -- we've heard two opinions that think that these interconnections were only intermittent and short-term connections. It would be good to just be certain of this fact, if possible, and go from there.

If, indeed, these interconnections don't have any effect on the epidemiological study, then we can essentially proceed and say, you know, whatever comes into the plant goes out everywhere in the distribution system, and that would essentially eliminate the need to, you know, develop further sophisticated distribution-system models.

My recommendation is not to continue on the field efforts at this time until these issues have been resolved. That's all I have.

DR. JOHNSON: Thank you. Vijay.

DR. SINGH: Essentially, I would just reiterate what has already been said earlier as well as this morning and yesterday. First of all, I would like to take this opportunity to state on the record that the ATSDR group, especially Morris Maslia and his group, have done really an outstanding job, and I have nothing but admiration for their work, both quality-wise as well as scientific rigor-wise.

Having said that, coming back to the groundwater
modeling area, as we have cited so many times, I think it's important that there is a clear statement and a clear discussion of the model assumptions, the hypotheses, as well as the model limitations because no model is a perfect model. That's why we call it as a model. 

And from the standpoint of public, I think it's very important to say very clearly what the assumptions are and what the model limitations are and which model hypotheses are, which directly would reflect on the quality and the reliability of the model.

And then the issue of uncertainty and risk analysis that we have been discussing since yesterday -- I think in the groundwater modeling area -- this issue has to be clearly, explicitly taken into consideration, and then there has to be a better accounting of the recharge, which really has not been done so far. Recharge has been taken as an average value on a yearly basis, which in my view is a very gross estimate of the rainfall water that goes into the ground.

After all, it is the rainfall water which enters into the ground which is responsible for transporting the contaminants into the groundwater body. And so it is, to me, of importance that the water percolation and the water recharge are more accurately estimated and included in the groundwater modeling area.
In terms of the water-distribution network, I think what Peter and Eric have said, I tend to concur with. The original effort on water discovery, I think, will be well worth the effort because there is no substitute for data, for data is the only source through which we get the information through which we communicate with nature. So I would strongly suggest that they continue their effort in terms of discovering or rediscovering the archaeology of the data as far as they can go.

But I also tend to concur with Tom in terms of the water-distribution modeling. I think the important point here is once the groundwater model produces water contamination through which we can quantify the water contamination at the wellhead and we can also have some data on the water contamination in terms of time and the depth. I think that is what is essentially going to be primarily responsible for determining the exposure from an epidemiological viewpoint. And I think that, to me, is essentially the central issue, which is what all this interval is meant for.

And so I'm not quite certain if a very detailed water-distribution modeling is really necessary. I think a simpler one might suffice, but if they have already done it and they're doing it, it certainly it's not going to hurt.
DR. JOHNSON: Thank you. Tom.

DR. WALSKI: Okay. Thanks. Yeah. I want to just second, I guess, what other people have said, and we have an excellent study team here, and they've done a very high-quality job. And it's just really ironic to find myself in the position of not selling modeling because usually that's what I do for a living is sell models and try to get people to use them. So I find myself, kind of, in an odd position here of saying, "Don't put too much emphasis on the models, but go for the real data."

And trying to -- I think, maybe, you might remember things better if I could just tell a story here. There's a guy walking down the street and sees another fellow on the ground on his hands and knees, looking around. The first guy -- he goes, "Well, what are you doing down there?" And he goes, "Well, I lost a $50 bill. I can't find it.

So the second guy comes and helps the guy look for the $50 bill, and after about five minutes, he says, "Well, how come you haven't found it? I mean, where did you lose it?" And he goes, "Well, I lost it over there in that vacant lot." And he goes, "Well, why aren't we over there looking?" And he goes, "Well, it's dark over there, and there's broken glass and rats and stuff. I don't want to go over there."
And I think that's kind of the position that the study team is in. It's kind of nice to work with models, but I think they're going to have to spend their time in the archives with the rats and the broken bottles, looking for data because that's where you're going to get the most for your effort is not being under a light in a nice area, but going to the archives and digging. And I think they're a qualified team, and they're going to do a great job with this.

DR. JOHNSON: Great. Thank you. Lenny.

DR. KONIKOW: Well, again, I second all the comments that have been made up to now. I again just reiterate with the groundwater modeling and the transport modeling that ultimately we're limited in what we can do in terms of the available data. I mean, you know, we don't have concentration data before 1980 or '82. And so everything we do for looking at distribution before then is going to be a little fuzzy.

We'll do the best we can with the flow model. You'll do the best you can with the flow model based on the distribution of pumpage, and that may be about the best you can do. In terms of, you know, the modeling approach and sensitivity analyses, this is all stuff that should be done. And one of the things to keep in mind is that your hydraulic heads in your flow model may be relatively

NANCY LEE & ASSOCIATES
insensitive to certain things to which the concentration distribution is highly sensitive.

And so there's not necessarily a direct transfer value in terms of the sensitivity analysis and uncertainty analysis between the flow and transport model. So it's just something to be aware of.

DR. JOHNSON: Jim.

DR. UBER: I'll leave it to the groundwater colleagues to talk about the -- what particular elements to include any probabilistic analysis or whatever form that may take, and I think that's clearly appropriate. My only reason for mentioning that is that I would have a suggestion that -- about the way the results of those analysis be portrayed. And specifically, for me, I focus on the precise connection between the groundwater resource and the water-distribution system, which is this pipe header that comes from the well field and goes into the distribution system.

And I think that the results of that stochastic analysis should be expressed in terms of the uncertainty or some type of interesting plot of the variability or uncertainty or both in that concentration that it is delivered to the distribution system, considering not only the uncertainty and the geohydrologic variables and the model set-up, but also the uncertainty in how the wells
are operated.

I believe that if the uncertainty in that quantity is quantified within some bounds, then what we have been talking about today, which is to, maybe, allow data discovery to drive the train for a little while longer. If we continued on data discovery and then you had the results of that uncertainty analysis, then between those two, I think it would become clear what to do, if anything, more with the water-distribution system model. And I would just leave it at that.

DR. JOHNSON: David.

DR. DOUGHERTY: Well, yeah, I think people have hit a lot of the points, and we could repeat them several times, as we have through the past couple of days. I think the summary that I have is that the model complexity is too far out in front of the data in the characterization of the uncertainties. It's something that can be corrected, I think, and reasonably without major correction. It's just a correction.

The three issues that come to mind, and two of them are on the groundwater side and one's a general, easy observation about the archaeology, about interconnects. And so that's number three, but the first two are about the things that drive concentrations in the groundwater delivered by the wells.
There's the pumping schedules that, I think, keep coming up, but I'm not sure we can do very much about them. The things that we haven't characterized enough are source, the mass loading, and the accretion; not just the reinfiltration but the septic returns and making sure we've got those in a time -- reasonably timed; very, very consistent with the climate.

And finally, making any statements about the groundwater issues for Hadnot, I don't feel comfortable about it. I don't think we've had enough conversation or information about that, and that may be something that you may need your next panel to tend to.

DR. JOHNSON: Thank you. Robert.

DR. CLARK: Well, I don't think I'm going to say anything new or original, but I am generally supportive of the current plan. But I think with any project of this complexity and magnitude, there always adjustments that take place in the process. And it seems to me that a couple of those are the re-emphasis on data discovery, which I think is a very important issue.

But the uncertainty issues with regard to the model parameters and the stochastic nature of demand and then the consequences of those yield in terms of the output and data reliability. It seems to me that the real issues surrounding this study are really going to come in the
public health and public policy area in terms of the
epidemiological results. Excuse me. That's going to be
the one that the public is going to look at, and the
public health community is going to look at very, very
carefully.

So I would suggest anything that needs to be done to
support effort to make it more scientifically defensible
is an important aspect to the project with the only
comments, which I've made before, that the issue of
transformation by-products is an important one to look at.

And also, what has actually been measured, I guess,
in terms of some of the samples that have been taken prior
to the establishment of the MCLs or vault organic
chemicals, and this concludes an excellent team. I'm more
impressed after listening to the presentation than I was
before when I read the background data.

DR. JOHNSON: Thank you. And Ben.

MR. HARDING: I want to thank Morris and ATSDR team
for the opportunity to sit in on this panel. I'm very
impressed with what you guys have done. It's an eye-
 opener to see some of the kinds of efforts you guys have
made.

What I want to suggest is that now you sort of step
back and take a higher level look at this again. Take a
little break. Reassess the requirements, starting with

NANCY LEE & ASSOCIATES
the epidemiological study. Just say, "What is it that we absolutely have to have and what are the things that are just sort of nice?" And probably just toss the latter. But certainly prioritize your requirements, and then make a decision based on a prioritization how you want to use your resources best.

With regard to the groundwater, which I can only kibbutz about, but I think it seems clear that the Hadnot Point situation requires some more understanding and possibly some more quantitative work modeling simulation.

I think in support of that and in support of, also, the water-distribution system, it's appropriate to do more of what we've referred to as data archaeology and continue in parallel while you're assessing your requirements. It seems that the groundwater work should express the uncertainty of, at least, the arrival time quantitatively and in a probabilistic framework.

With regard to the water-distribution system, the big issue here, it seems to me, is -- well, it may not be the biggest issue, but it seems to be the most contentious -- is to understand these interconnections. And I would suggest that if it turns out the systems are interconnected and they're interconnected in such a way that water flows from Hadnot Point into Holcomb Boulevard based on the grades that you consider excluding those
periods of time and those populations that are affected, if you could possibly do that, rather than trying to model that particular situation.

Otherwise, in the other periods where the systems can be viewed as operating independently, I think the simple mixing models are adequate. And there, the most important issue, aside from the groundwater arrival time, is the dispatch of the wells. And that might be supported by the data archaeology.

And then, finally, and I think this would be a big contribution to the practice. Again, I've said this over and over and over again. But to apply methods of propagating your uncertainty quantitatively. Typically, Monte Carlo is the way people do that. It doesn't mean you have to run your groundwater model in a Monte Carlo framework. There's other ways to do.

I think it's practical, and I'd take a real hard look at that because it's very clear from our discussions there is a lot of uncertainty here. And again, thanks for the opportunity. I've very much enjoyed this. I'm very impressed with what you guys have accomplished.

DR. JOHNSON: Okay. Any reaction from the agency representatives before we close?

MR. MASLIA: Only to thank everyone for spending the time going through the material. Obviously, it was not a
polished report by any means. But, again, to emphasize, we do take your recommendations and suggestions very seriously. It will, I believe, help guide us. We were at a stage where we needed, at least, some external input and guidance and just to thank everyone for their time and effort.

DR. JOHNSON: And in closing, one observation from the Chair. I've mentioned the term "cost benefit." And I think, as Ben and others said, I think you -- there's time now, and I think there's need now for the agency to step back and reflect and digest what you've heard over the past day and a half.

And I think you need to ask yourself, in the vain of getting data in which you have confidence, what benefit is it going to be toward that goal if other activities are done or not done? What's going to be the cost of some of these things you've put on the table? And perhaps, as a result of the last day and a half, some suggestions have been to perhaps reorder those activities? So take a hard look at the cost of what you're proposing to do in the future, factoring in the advice you've heard here from this panel.

And with that, I'd like to close by thanking, as the Chair, this panel. I've been in public health for about 40 years, and so I've attended lots and lots of meetings.
I've been on lots and lots of committees. Some committees, I've chaired. Some other committees, I simply chewed on as a member. But this is, certainly, in my experience, the most able and the most helpful committee of which I've had the privilege of being associated. So really, accolades to the panel.

I'd also like, on behalf of the panel, to thank the agency representatives: Morris, Frank, Bob Faye, and others who really in an exemplary way represented the agency and interacted with this panel and with the public representatives.

On behalf of the panel, I also would like to thank the public input and the public representatives here. And what was added was really important insights that we would not have had otherwise brought forward and were very valuable.

A special thanks to our reporter, who kept us all in line, starting with the Chair. So many thanks for your expert work. And lastly, many thanks to the administrative staff, Ann Walker and her colleagues, who have made much of what has been brought to you happen in terms of materials, arrangements, et cetera, et cetera.

So with that, using the prerogative of the Chair, I declare us adjourned. Thank you.

(Whereupon, the proceeding was adjourned at

NANCY LEE & ASSOCIATES)
approximately 1:35 p.m.)